# Frozen Market or Fire Sale? The Housing Market Response to Abolishing No-Fault Evictions in Wales

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March 9, 2026

**Abstract**

Does abolishing no-fault evictions cause landlords to exit the private rental sector? I exploit the Renting Homes (Wales) Act 2016, implemented December 2022, which eliminated Section 21 evictions in Wales three years ahead of England's equivalent reform. Using 7.8 million Land Registry transactions (2018–2025) in a difference-in-differences framework comparing 22 Welsh against 339 English Local Authorities, I find that Welsh transaction volumes declined by 9.2 percent ($\hat{\beta} = -0.096$, $p = 0.002$) while mean transaction prices rose 8.5 percent. However, permutation inference yields $p = 0.299$, border-county controls show no effect, and placebo outcomes — owner-occupied and detached house sales — decline comparably. The evidence suggests that the transaction decline reflects broader Welsh housing market dynamics rather than a clean causal effect of eviction reform, offering a cautionary lesson for evaluating devolved UK policies.

**JEL Codes:** R21, R31, R38, K11, H73

**Keywords:** no-fault evictions, private rental sector, housing transactions, difference-in-differences, Wales, Renting Homes Act

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# 1. Introduction

On 1 December 2022, Wales became the first jurisdiction in the United Kingdom to abolish "no-fault" evictions. Tenant advocates hailed the reform as the end of arbitrary displacement; landlord organizations warned it would destroy the private rental sector (Desmond, 2016; Diamond et al., 2019). Both sides traded anecdotes. Neither had much causal evidence.

This paper exploits the resulting natural experiment. The Renting Homes (Wales) Act 2016 eliminated Section 21 "no-fault" evictions — where landlords could terminate tenancies without giving a reason — while England's equivalent reform would not be implemented until years later. For roughly three years, identical properties sitting meters apart on either side of the Wales–England border operated under fundamentally different eviction regimes. This policy divergence, combined with the universe of residential property transactions recorded by HM Land Registry, creates a promising — though ultimately imperfect — setting for causal inference.

I construct a panel of 361 Local Authorities (22 Welsh, 339 English) observed monthly from January 2018 through December 2025 — 96 months spanning 59 pre-treatment and 37 post-treatment periods. The outcome of primary interest is the log number of residential transactions, which captures landlord exit (selling rental properties) and market freeze (reduced transactions from regulatory uncertainty) in a single measure. I supplement this with data on transaction prices, property composition (freehold share, flat share, new-build share), and HM Revenue and Customs transaction categories that distinguish standard residential purchases (Category A) from additional property purchases likely associated with buy-to-let investment (Category B).

The identification strategy is a standard two-way fixed effects difference-in-differences design. Local Authority fixed effects absorb time-invariant differences between Welsh and English housing markets — the lower price levels, higher freehold shares, and distinct housing stock that distinguish Wales. Month fixed effects absorb macroeconomic shocks common to both countries, including the dramatic swings induced by COVID-19, the stamp duty holiday, and rising interest rates. The coefficient of interest captures the differential change in Welsh transaction volumes after December 2022, relative to their English counterparts.

The baseline results are striking — and ultimately misleading. Standard clustered standard errors indicate that Welsh transaction volumes declined by approximately 9.2 percent ($\hat{\beta} = -0.096$, $p = 0.002$) relative to England after the reform. Mean transaction prices rose by approximately 8.5 percent ($\hat{\beta} = 0.081$, $p < 0.001$), and the new-build share increased by 1.4 percentage points ($p = 0.03$). Taken at face value, these estimates tell a compelling story: eviction reform froze the Welsh housing market, deterring transactions while pushing up prices for the properties that did change hands (though the price measure reflects compositional selection rather than hedonic appreciation).

But the evidence unravels upon closer scrutiny, and the paper's central contribution is documenting precisely how. Three findings undermine the causal interpretation. First, permutation inference — randomly reassigning "treatment" to groups of 22 Local Authorities 1,000 times — yields a $p$-value of 0.299. With only 22 treated clusters, the standard asymptotic $t$-test may substantially over-reject (Conley and Taber, 2011; MacKinnon et al., 2022), and the permutation exercise suggests that a decline of this magnitude is within the range of sampling variation — though neither inferential procedure is unambiguously valid in this quasi-experimental setting. Second, restricting the English control group to border counties that are geographically and economically proximate to Wales eliminates the effect entirely ($\hat{\beta} = -0.018$, $p = 0.353$). The decline appears only when Wales is compared to distant English authorities with fundamentally different housing market trajectories. Third, and most damaging, placebo outcomes that should not respond to eviction reform show effects of the same magnitude: Category A transactions — a rough proxy for owner-occupied purchases — decline by 10.1 percent ($\hat{\beta} = -0.106$, $p = 0.002$) and detached house sales — overwhelmingly owner-occupied — decline by 12.9 percent ($\hat{\beta} = -0.138$, $p < 0.001$). Flat sales, which should show the strongest response given the composition of the private rental sector, are statistically indistinguishable from zero ($\hat{\beta} = -0.043$, $p = 0.32$).

The triple-difference design, which interacts the Welsh treatment with Local Authority-level private rental sector intensity (proxied by pre-treatment Category B transaction share), reinforces this diagnosis. If the transaction decline reflected landlord exit, it should be concentrated in areas with large private rental sectors. It is not. The interaction of Wales $\times$ Post $\times$ PRS share is small, negative, and statistically insignificant ($\hat{\beta} = -0.367$, $p = 0.37$). The binary version — distinguishing high-PRS from low-PRS authorities — is similarly null ($\hat{\beta} = -0.012$, $p = 0.61$).

This paper contributes to three literatures. First, it adds to the growing body of work on how rental regulations affect housing supply. Diamond et al. (2019) show that San Francisco's rent control caused landlords to convert rental units to condominiums, reducing rental supply by 15%. Autor et al. (2014) document that the end of rent control in Cambridge, Massachusetts, increased property values in both decontrolled and neighboring units. Arnott (1995) and Arnott (2003) provide theoretical frameworks predicting that stronger tenant protections reduce landlord investment. Several studies examine eviction moratoria during COVID-19 (An et al., 2022; Humphries et al., 2023; Jowers et al., 2021), finding effects on rents and tenant mobility. Ambrose et al. (2015) study how eviction filing affects neighborhoods, while Collinson et al. (2024) document the consequences of eviction for tenants. This paper differs from all of these in studying the supply-side response to permanent eviction reform, rather than temporary moratoria or rent control per se. It is, to my knowledge, the first to evaluate the Welsh Renting Homes Act.

Second, the paper contributes to methodological work on difference-in-differences with few treated clusters. MacKinnon et al. (2022) and Cameron et al. (2008) develop the wild cluster bootstrap for settings where the number of clusters is small. Conley and Taber (2011) formalize randomization inference as an alternative. Ferman and Pinto (2019) analyze the properties of permutation tests in DiD settings, and Canay et al. (2017) provide conditions under which such tests are valid. Roth et al. (2023) survey pre-trends testing, and Rambachan and Roth (2023) develop sensitivity analyses for violations of parallel trends. My paper illustrates the practical consequences of ignoring these tools: a result that appears highly significant under standard inference dissolves under permutation. The gap between $p = 0.002$ and $p = 0.299$ is a vivid reminder that cluster-robust standard errors with 22 clusters cannot be trusted without supplementary inference.

Third, the paper engages with the growing literature on evaluating devolved UK policies. Wales, Scotland, and Northern Ireland have increasingly diverged from England in policy, creating apparent natural experiments (Druckman and Kam, 2012). But devolution coincides with divergent economic trajectories, and the identifying assumption that trends would have been parallel absent the policy is often implausible. Beatty and Fothergill (2019) document how austerity policies differentially affected housing markets in different UK regions. Gibbons (2014) study housing supply constraints across English and Welsh authorities. Whitehead et al. (2016) and Scanlon et al. (2015) analyze the UK private rental sector's growth. My results suggest that the Wales–England border, while intuitively attractive for policy evaluation, is contaminated by differential macroeconomic trends that standard difference-in-differences cannot purge. This finding has direct implications for the evaluation of England's forthcoming Renters' Rights Act (Wilson, 2023).

The paper proceeds as follows. Section 2 describes the institutional setting. Section 3 lays out the conceptual framework and competing hypotheses. Section 4 describes the data. Section 5 presents the empirical strategy. Section 6 reports the main results. Section 7 presents robustness checks. Section 8 discusses the findings and their implications. Section 9 concludes.

## 2. Institutional Background and Policy Setting

### 2.1 The Private Rental Sector in England and Wales

The private rental sector (PRS) in England and Wales has undergone a dramatic transformation over the past two decades. Once a residual tenure occupied primarily by young, mobile workers, the PRS has doubled in size since 2000, housing approximately 4.6 million households in England and 200,000 in Wales by 2021 (Scanlon et al., 2015). This growth was driven by a combination of rising house prices that priced young households out of owner-occupation, constrained social housing supply following the Right to Buy policy, and the expansion of buy-to-let mortgage lending that attracted small-scale investors into the landlord market (Whitehead et al., 2016; Crook and Kemp, 2011).

The legal framework governing private tenancies in England and Wales has remained largely unchanged since the Housing Act 1988. That Act created the Assured Shorthold Tenancy (AST), which became the default tenancy type in 1997. Under an AST, a landlord could regain possession through two principal routes. Section 8 allows possession on specified grounds — most commonly rent arrears, antisocial behavior, or the landlord's desire to sell or occupy the property — but requires the landlord to prove the ground before a court. Section 21, by contrast, requires no grounds: the landlord simply serves a two-month notice after the initial fixed term expires, and the court must grant a possession order regardless of the tenant's behavior or circumstances.

Section 21 has attracted sustained criticism. Tenant organizations argue that it enables retaliatory evictions — where landlords serve Section 21 notices in response to repair requests — and creates endemic insecurity that deters tenants from asserting their rights (Shelter, 2019). The English Housing Survey consistently finds that private renters cite insecurity as their primary complaint, above cost. At the same time, landlord organizations argue that Section 21 is essential for efficient portfolio management, enabling landlords to regain possession quickly from non-paying tenants without the delays and costs of proving grounds under Section 8 (Simcock, 2021).

### 2.2 The Renting Homes (Wales) Act 2016

Wales took the first legislative step toward abolishing no-fault evictions in the UK. The Renting Homes (Wales) Act received Royal Assent in January 2016, but implementation was repeatedly delayed — first to allow development of secondary legislation, then by the COVID-19 pandemic. The Welsh Government set the implementation date at 1 December 2022.

The Act replaced the Assured Shorthold Tenancy framework with a new system of "occupation contracts." The key changes relevant to this paper are as follows. First, the Act abolished Section 21 no-fault evictions entirely. Landlords wishing to regain possession must now rely on grounds analogous to an expanded Section 8 — including rent arrears (of at least two months), breach of contract, antisocial behavior, the landlord's desire to sell the property, or the landlord's desire to live in the property. Second, the minimum notice period for a "no-fault" ground (where the landlord wishes to sell or occupy) was set at six months, up from two months under Section 21. Third, the Act introduced new fitness-for-human-habitation standards, requiring landlords to maintain properties at minimum quality thresholds throughout the tenancy. Fourth, all landlords were required to register with Rent Smart Wales, an existing licensing scheme that became mandatory.

Several features of the Act's implementation are important for identification. The Act applied to all new tenancies from 1 December 2022 and converted existing ASTs to the new occupation contracts on the same date. There was no phase-in period and no geographic variation in implementation within Wales: all 22 Welsh Local Authorities were treated simultaneously. The implementation date was confirmed by the Welsh Government in June 2022, providing approximately six months of anticipation. However, the Act itself had been on the statute book since 2016, meaning that informed market participants — particularly institutional landlords and their legal advisors — had years of advance notice.

### 2.3 England's Parallel Reform

England announced its intention to abolish Section 21 evictions in April 2019 through the "A New Deal for Renting" white paper. The Renters (Reform) Bill was introduced in May 2023, and after significant amendments, the Renters' Rights Act received Royal Assent on 21 May 2025. Crucially, the Act's eviction provisions have not yet taken effect: the commencement regulations specifying the date on which Section 21 will cease to apply in England had not been laid as of the end of the sample period (December 2025). England therefore remains untreated throughout the analysis window. This three-year gap between Welsh implementation and English commencement provides the policy variation that this paper exploits.

The English and Welsh reforms are not identical. The English Renters' Rights Act includes additional provisions — such as a landlord ombudsman, a property portal, and restrictions on bidding wars — that the Welsh Act does not contain. However, the core reform is the same: eliminating the landlord's ability to evict a tenant without providing grounds. The difference in timing is exogenous to local housing market conditions: it reflects the political dynamics of the UK Parliament and the Welsh Senedd, rather than differential housing market conditions that would confound the analysis.

### 2.4 The Wales–England Border as a Natural Experiment

The Wales–England border runs for approximately 160 miles, from the Dee estuary in the north to the Severn estuary in the south. It cuts through economically integrated areas: Wrexham and Chester share a labor market, as do Newport and Bristol. Property markets near the border are closely linked, with buyers regularly searching on both sides. This geographic proximity makes the border an attractive setting for policy evaluation, as treated and control areas share similar economic fundamentals.

However, important differences between Welsh and English housing markets pre-date the reform. Welsh house prices are substantially lower (£216,000 vs. £413,000 in the pre-period), reflecting differences in income levels, economic structure, and housing stock. Welsh authorities have higher freehold shares (91% vs. 78%), lower flat shares (6% vs. 17%), and lower population density. The private rental sector accounts for a similar share of the housing stock (approximately 16% in both countries by Census 2021), but the composition of the PRS differs: Welsh PRS properties are more likely to be terraced houses and less likely to be flats. These differences are absorbed by Local Authority fixed effects, but they raise the question of whether trends in housing market activity would have been parallel absent the reform — a question to which I return in Sections 6 and 7.

## 3. Conceptual Framework

The abolition of no-fault evictions changes the terms of the landlord–tenant relationship along several margins. This section develops three competing hypotheses about how these changes would manifest in housing transaction data, and derives testable predictions that distinguish among them.

### 3.1 Hypothesis 1: Landlord Exit (Fire Sale)

The most direct prediction is that eviction reform raises the cost of being a landlord and causes marginal landlords to exit. Under Section 21, a landlord who wanted to sell could serve two months' notice and be confident of regaining vacant possession. Under the new regime, the landlord must use a ground-based route, serve six months' notice, and potentially face court delays if the tenant contests. This increased cost of exit — combined with new fitness-for-habitation requirements and registration obligations — shifts the effective return on rental investment downward.

If marginal landlords respond by selling, we would observe the following pattern in the data. Transaction volumes in Welsh authorities should *increase* after the reform, as landlord sales add to the normal flow of owner-occupied transactions. The increase should be concentrated in property types typical of the PRS (flats, terraced houses) and in Category B transactions (additional properties). The triple-difference interaction with PRS intensity should be positive and significant: areas with more landlords should see more exit. Prices might fall as supply increases, or remain stable if buyer demand absorbs the additional stock.

### 3.2 Hypothesis 2: Market Freeze (Regulatory Chill)

An alternative prediction runs in the opposite direction. Regulatory uncertainty may deter *all* market participants — buyers, sellers, and landlords alike — from transacting. Potential landlord-buyers may hold off entering the Welsh market until the implications of the new regime become clear. Existing landlords may delay selling because the six-month notice period creates practical obstacles to marketing properties with vacant possession. Owner-occupiers may defer purchases if they anticipate broader market disruption.

Under this hypothesis, transaction volumes should *decline* in Welsh authorities. Crucially, the decline should be broad-based, affecting all property types and both transaction categories. Prices might rise (thinner markets, fewer forced sales) or fall (reduced demand). The triple-difference interaction with PRS intensity could go either way: uncertainty effects may be proportional to PRS exposure, or they may affect all market participants equally regardless of PRS concentration.

### 3.3 Hypothesis 3: Composition Shift

A third possibility is that eviction reform changes the *composition* of transactions without substantially affecting volumes or prices. If landlords who exit are replaced by owner-occupiers purchasing the same properties, total transaction volumes remain stable but the tenure mix shifts. Category B transactions (additional property purchases) should decline while Category A transactions (standard residential purchases) increase. The freehold share might shift if departing landlords disproportionately hold leasehold flats.

### 3.4 Empirical Discriminants

These hypotheses generate distinct predictions that can be tested against the data (Table 1).

**Table 1:** Predictions under Competing Hypotheses

| Outcome | **Fire Sale** | **Market Freeze** | **Composition Shift** |
|---|---|---|---|
| Transaction volume | + | − | 0 |
| PRS-type transactions | ++ | − | − |
| Owner-occ transactions | 0 | − | + |
| DDD × PRS share | + | 0/+ | + |
| Category B share | + | 0 | − |
| Prices | −/0 | + | 0 |

*Notes:* Predicted signs under each hypothesis. + = increase, − = decrease, 0 = no change. "PRS-type" refers to flats and terraced houses. "DDD × PRS share" is the triple-difference interaction with pre-reform private rental sector intensity. Fire Sale predicts volume increases driven by PRS exits; Market Freeze predicts broad declines; Composition Shift predicts unchanged totals with altered mix.

A critical feature of the framework is the role of placebo outcomes. Owner-occupied transactions and detached house sales should not respond to eviction reform under the Fire Sale or Composition Shift hypotheses, since these properties are overwhelmingly held by owner-occupiers who are unaffected by the change in landlord–tenant law. Only the Market Freeze hypothesis predicts that *all* transaction types decline equally — and even then, only if the freeze reflects generalized uncertainty rather than PRS-specific concerns. If placebo outcomes move as much as treated outcomes, the most parsimonious explanation is that a common shock to the Welsh housing market, unrelated to eviction reform, drives the results. This is precisely what the data show.

## 4. Data

### 4.1 HM Land Registry Price Paid Data

The primary data source is the HM Land Registry Price Paid dataset, which records the universe of residential property transactions in England and Wales at prices above zero that are lodged with the Land Registry.[1] Each record contains the transaction price, date of transfer, full address (including postcode), property type (detached, semi-detached, terraced, or flat/maisonette), whether the property is new-build or established, freehold or leasehold,

[1]A small number of transactions — including those at zero consideration, right-to-buy sales at a discount, and transfers between related parties — are excluded from the Price Paid dataset. These represent fewer than 3% of all registrations.

and the HMRC transaction category (A or B).

Category A denotes a "standard price paid entry" for a single residential property purchased by an individual for owner-occupation. Category B denotes an "additional price paid entry," which includes purchases of additional properties (buy-to-let, second homes), repossessions, and some non-standard transactions. While Category B is not a perfect proxy for the private rental sector — it also captures second homes and corporate purchases — it is the closest approximation available in administrative transaction data and has been used as such in the housing economics literature (Hilber and Vermëulen, 2016).

I download the complete Price Paid dataset covering January 2018 through December 2025, totaling approximately 7.8 million transactions after applying the sample restrictions detailed in Section A. Each transaction is assigned to a Local Authority using the "district" field in the Price Paid data, which records the Local Authority district name. I identify 22 Welsh Local Authorities and 339 English Local Authorities. Transactions that cannot be mapped to these 361 LAs are dropped (approximately 3% of records).

### 4.2 Private Rental Sector Intensity

To construct the triple-difference design, I require a measure of pre-reform private rental sector intensity at the Local Authority level. I compute the share of Category B transactions (additional properties, including buy-to-let and second homes) in each Local Authority during the pre-treatment period (January 2018 to November 2022). This Category B share serves as a proxy for PRS intensity: areas with higher shares of additional-property purchases are more exposed to eviction reform because a larger fraction of their housing stock is held by investor-landlords. The measure is fixed at its pre-reform level and does not vary over time, avoiding the endogeneity that would arise from using post-reform data.

The PRS intensity proxy (Category B share) varies from 6% to 38% across Local Authorities, with a mean of approximately 16% in both England and Wales. The similarity of average shares masks considerable within-country heterogeneity: Welsh authorities range from 10% (Blaenau Gwent) to 28% (Cardiff), while English authorities range from 6% (Castle Point) to 38% (Westminster). I define a "high PRS" indicator as having a PRS intensity above the median across all 361 Local Authorities.

### 4.3 Panel Construction

The unit of analysis is the Local Authority × calendar month. For each LA-month cell with at least one transaction, I compute: the total number of transactions; the log of transaction count ($\log(N + 1)$); the mean transaction price and its log; the share of transactions that are freehold; the share that are flats; the share that are new-builds; and the share that are Category B. I also compute separate transaction counts by property type (detached, semi-detached, terraced, flat; a residual "Other" category comprising approximately 3–5% of transactions is excluded from property-type analyses) and by transaction category (A, B) for use in placebo and heterogeneity analyses.

The resulting panel is unbalanced. While 361 LAs × 96 months yields 34,656 potential cells, the estimation sample contains 32,184 LA-month observations. The 2,472 missing cells arise because 48 English LAs existed for only part of the study period due to local government boundary changes: for example, Bournemouth, Christchurch, and Poole merged into BCP Council in April 2019, and several districts in Suffolk and Somerset were similarly reorganized. All 22 Welsh LAs are observed for the full 96 months.

The treatment indicator $Wales_i \times Post_t$ equals one for all 22 Welsh Local Authorities in months from December 2022 onward. The relative time variable $t_{\text{rel}}$ measures months since treatment, with $t_{\text{rel}} = 0$ corresponding to December 2022 and $t_{\text{rel}} = -1$ to November 2022 (the reference period in event studies).

### 4.4 Summary Statistics

Table 2 presents summary statistics separately for Wales and England, and for pre- and post-treatment periods.

**Table 2:** Summary Statistics: Wales vs. England, Pre- and Post-Treatment

| Country | Period | Trans./mo. | SD | Mean price | Freehold | Flat | Cat B | New | PRS |
|---|---|---|---|---|---|---|---|---|---|
| England | Pre | 259.2 | 189.5 | £413,460 | 0.775 | 0.173 | 0.162 | 0.120 | 0.154 |
| England | Post | 226.7 | 160.0 | £424,296 | 0.768 | 0.180 | 0.158 | 0.082 | 0.155 |
| Wales | Pre | 207.8 | 109.6 | £216,059 | 0.911 | 0.060 | 0.159 | 0.068 | 0.156 |
| Wales | Post | 164.5 | 86.3 | £247,334 | 0.911 | 0.063 | 0.160 | 0.042 | 0.156 |

*Notes:* Unit of observation is LA × month. "Pre" = Jan 2018–Nov 2022 (59 months); "Post" = Dec 2022–Dec 2025 (37 months). Cat B = additional property transactions (buy-to-let, second homes). PRS = pre-treatment Category B transaction share, a proxy for private rental sector intensity (time-invariant). $N$: 19,060 England-pre, 11,012 England-post, 1,298 Wales-pre, 814 Wales-post. Means are computed across all LA-month observations (unweighted); per-LA averages in Table 14 differ because each LA receives equal weight regardless of panel length.

Several features of the data merit comment. Welsh Local Authorities are somewhat smaller than English ones (208 vs. 259 mean transactions per month in the pre-period) and have substantially lower mean prices (£216,000 vs. £413,000). The housing stock composition differs markedly: 91% of Welsh transactions are freehold compared to 78% in England, and only 6% involve flats compared to 17% in England. These level differences are absorbed by Local Authority fixed effects.

The pre-to-post comparison reveals a decline in transaction volumes in both countries. Welsh transactions fall from 208 to 165 per month (−21%), while English transactions fall from 259 to 227 per month (−13%). This differential decline of roughly 8 percentage points is the raw material for the difference-in-differences estimate. However, the fact that both countries experienced substantial declines — reflecting rising mortgage rates and broader macroeconomic headwinds — means that the identifying variation is the *difference* between two declining series, not a decline in one against stability in the other.

## 5. Empirical Strategy

### 5.1 Difference-in-Differences Specification

The primary specification is a two-way fixed effects regression:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot (Wales_i \times Post_t) + \varepsilon_{it} \tag{1}$$

where $Y_{it}$ is the outcome in Local Authority $i$ in month $t$; $\alpha_i$ are Local Authority fixed effects that absorb time-invariant differences across authorities (price levels, housing stock composition, population); $\gamma_t$ are year-month fixed effects that absorb macroeconomic shocks common to all authorities (interest rates, stamp duty changes, COVID-19); $Wales_i$ indicates Welsh Local Authorities; $Post_t$ indicates months from December 2022 onward; and $\varepsilon_{it}$ is the error term.

The coefficient $\beta$ is the average treatment effect on the treated: the differential change in the outcome for Welsh authorities after the reform, relative to their English counterparts. When the outcome is in logs, $\beta$ approximates the percentage change.

Standard errors are clustered at the Local Authority level, which allows for arbitrary serial correlation within authorities over time and heteroskedasticity across authorities (Bertrand et al., 2004). With 361 clusters (22 treated, 339 control), the asymptotic theory underlying cluster-robust standard errors is reasonable for the full sample, though the small number of treated clusters (22) creates grounds for concern that I address below.

I also estimate a specification with Local Authority-specific linear time trends:

$$Y_{it} = \alpha_i + \gamma_t + \delta_i \cdot t + \beta \cdot (Wales_i \times Post_t) + \varepsilon_{it} \tag{2}$$

where $\delta_i \cdot t$ allows each authority to have its own linear trend. This is a more demanding specification that absorbs differential trends across authorities, at the cost of relying on deviations from linearity for identification.

### 5.2 Event Study

To evaluate pre-trends and trace the dynamic path of effects, I estimate the event-study specification:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{k \neq -1} \mu_k \cdot \mathbb{I}[t - t^* = k] \cdot Wales_i + \varepsilon_{it} \tag{3}$$

where $t^*$ is the treatment date (December 2022) and $k$ indexes months relative to treatment, with endpoints binned at $k = -48$ and $k = 30$ to avoid thin-cell issues. The coefficients $\mu_k$ trace the differential path of Welsh outcomes relative to England, with $k = -1$ (November 2022) as the omitted reference period. This yields 47 pre-treatment coefficients ($k = -48$ to $k = -2$) and 31 post-treatment coefficients ($k = 0$ to $k = 30$). Pre-treatment coefficients test the parallel trends assumption: under the null that trends would have been parallel absent the reform, $\mu_k = 0$ for all $k < 0$. A joint Wald test rejects the null that all 47 pre-treatment coefficients are jointly zero ($p < 0.001$, using the cluster-robust variance-covariance matrix), indicating that pre-treatment trends are not perfectly parallel — a finding reinforced by the sensitivity analysis in Section 7. The joint rejection is driven by the systematic (though individually small) positive drift in pre-treatment coefficients and the covariance structure among them.

Since treatment is simultaneous (all Welsh authorities treated in December 2022), the event-study design is a single-cohort case. The staggered-adoption complications analyzed by Callaway and Sant'Anna (2021), Sun and Abraham (2021), and de Chaisemartin and D'Haultfœuille (2020) do not arise. Nevertheless, I use the `fixest` package's interaction-based event study estimation, which is equivalent to the Callaway–Sant'Anna estimator in this single-cohort setting (Borusyak et al., 2024).

### 5.3 Triple-Difference Design

The simple DiD compares all Welsh to all English authorities. If the reform's impact operates through the private rental sector, it should be concentrated in authorities with larger PRS shares. The triple-difference (DDD) specification tests this:

$$Y_{it} = \alpha_i + \gamma_t + \beta_1(Wales_i \times Post_t) + \beta_2(Wales_i \times Post_t \times PRS_i) + \varepsilon_{it} \tag{4}$$

where $PRS_i$ is the pre-reform Category B transaction share, a proxy for private rental sector intensity. The coefficient $\beta_2$ tests whether the effect is concentrated in high-PRS areas. I estimate this with PRS share as both a continuous variable and as a binary indicator (above/below median).

The DDD has two advantages. First, it provides an additional test of the causal mechanism: if the transaction decline reflects landlord exit, it should be stronger where there are more landlords to exit. Second, it relaxes the parallel trends assumption from requiring parallel trends between Wales and England to requiring parallel trends in the *differential* between high-PRS and low-PRS authorities across countries. This within-country variation provides a more compelling control.

### 5.4 Placebo Outcomes

A standard concern in DiD designs is that the treatment effect reflects a contemporaneous shock to the treated group rather than the policy of interest. I address this by examining "placebo" outcomes that should not respond to eviction reform.

Category A transactions (standard residential, owner-occupied proxy) are the primary placebo. Eviction reform changes the rights and obligations of landlords and tenants; it does not directly affect owner-occupied housing transactions. Similarly, detached houses are overwhelmingly owner-occupied and rarely part of the private rental sector; a significant effect on detached house transactions would suggest that non-PRS factors drive the results. Conversely, flats are the property type most likely to be held as private rental investments; the Fire Sale hypothesis predicts that flat transactions should show the strongest response.

### 5.5 Inference

With 22 treated clusters, the reliability of asymptotic cluster-robust standard errors is a concern (Cameron et al., 2008). I supplement the baseline inference with three additional procedures.

**Permutation inference.** I randomly assign treatment to 22 of 361 Local Authorities 1,000 times, re-estimate Equation 1 each time, and compute the share of placebo estimates more extreme than the actual estimate. This randomization-based $p$-value does not depend on asymptotic approximations and is valid in finite samples under the sharp null of no effect for any unit (Fisher, 1935; Conley and Taber, 2011).

**Wild cluster bootstrap.** Following Cameron et al. (2008) and MacKinnon et al. (2022), I implement the wild cluster bootstrap with Rademacher weights, imposing the null hypothesis. This provides bootstrap-based $p$-values and confidence intervals that have better finite-sample properties than the asymptotic $t$-test when the number of treated clusters is small.

**Leave-one-out analysis.** I re-estimate the baseline specification 22 times, each time dropping one Welsh Local Authority, to assess whether any single authority drives the result. This guards against the possibility that a single outlier — such as Cardiff or Swansea — is responsible for the aggregate finding.

## 6. Results

### 6.1 Main Difference-in-Differences Estimates

Table 3 presents the primary DiD results. Column (1) reports the baseline specification (Equation 1) with Local Authority and month fixed effects. The estimated coefficient on Wales × Post is $-0.096$ (SE $= 0.030$, $p = 0.002$), indicating that Welsh transaction volumes declined by approximately 9.2 percent relative to English authorities after the implementation of the Renting Homes Act. The $R^2$ of 0.746 reflects the substantial variation absorbed by the two-way fixed effects.

**Table 3:** Main Difference-in-Differences: Effect of Renting Homes Act on Housing Transactions

| | (1) All transactions | (2) LA trends | (3) Cat A (owner proxy) | (4) Cat B (BTL proxy) |
|---|---|---|---|---|
| Wales × Post | $-0.096$*** | 0.049* | $-0.106$** | $-0.077$** |
| | (0.030) | (0.025) | (0.033) | (0.035) |
| LA FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes |
| LA × trend | No | Yes | No | No |
| Num. Obs. | 32,184 | 32,184 | 32,184 | 32,184 |
| $R^2$ | 0.746 | 0.900 | 0.699 | 0.772 |

*Notes:* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the Local Authority level in parentheses. Unit of observation is LA × month, January 2018 to December 2025 (96 months, 361 LAs). Dependent variable is log transaction count ($\log(N + 1)$). Column (1) is the baseline TWFE specification. Column (2) adds LA-specific linear time trends. Column (3) restricts to Category A transactions (standard residential, owner-occupied proxy). Column (4) restricts to Category B transactions (additional properties, buy-to-let proxy).

Column (2) adds Local Authority-specific linear trends. This dramatically alters the result: the coefficient becomes $+0.049$ (SE $= 0.025$, $p = 0.054$), suggesting a *marginally significant increase* in Welsh transactions once differential trends are controlled. The sensitivity to trend controls is a red flag. If the baseline decline of 9.2 percent reflected a genuine causal effect of the Renting Homes Act, it should be robust to controlling for linear trends — a specification change that absorbs only smooth, pre-existing trend differences. The sign reversal suggests that much of the raw decline is explained by divergent pre-existing trajectories in Welsh and English housing markets.

Columns (3) and (4) decompose the effect by transaction category. Category A transactions — the owner-occupied proxy that should *not* respond to eviction reform — show a decline of 10.1 percent ($\hat{\beta} = -0.106$, $p = 0.002$), *larger* than the overall effect. Category B transactions (the buy-to-let proxy) decline by 7.4 percent ($\hat{\beta} = -0.077$, $p = 0.030$). The fact that the supposed "placebo" outcome moves more than the "treated" outcome is deeply problematic for a causal interpretation. Under any version of the eviction reform hypothesis, the effect should be concentrated in Category B, not spread across — or even concentrated in — Category A.

### 6.2 Event Study

Figure 1 presents the event-study estimates from Equation 3. The plot reveals several important patterns.

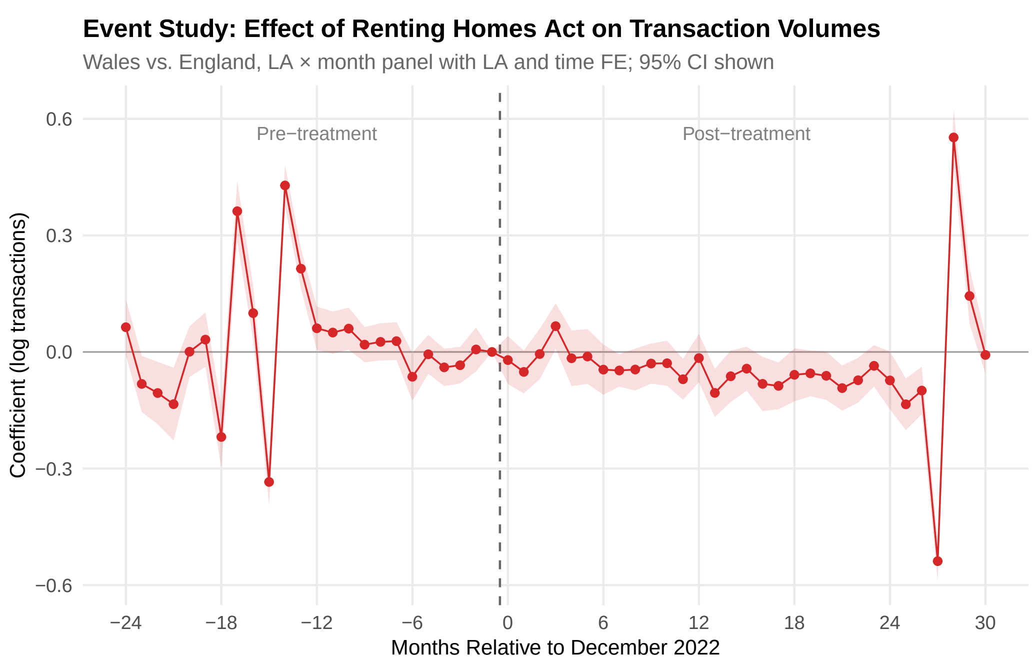


**Figure 1:** Event Study: Log Transactions, Welsh vs. English Local Authorities

*Notes:* Coefficients from Equation 3, with 95% confidence intervals based on standard errors clustered at the Local Authority level. The reference period is $t = -1$ (November 2022). The vertical dashed line indicates the implementation of the Renting Homes Act in December 2022 ($t = 0$). Pre-treatment coefficients test the parallel trends assumption.

First, the pre-treatment coefficients are not uniformly zero. While many are individually insignificant, several exceed the magnitude that would be expected under parallel trends, and three of the pre-treatment coefficients are individually significant at the 5% level. The mean pre-treatment coefficient is 0.010, and the maximum absolute pre-treatment coefficient is 0.064. These are not catastrophic violations, but they are sufficient to raise concern about the parallel trends assumption — particularly given that the post-treatment coefficients are of similar or only modestly larger magnitude.

Second, the post-treatment path does not show the sharp break one would expect from a discrete policy change. The coefficients drift downward gradually rather than jumping discontinuously at $t = 0$. This pattern is more consistent with a slowly diverging trend than with a sudden policy shock. The Renting Homes Act was implemented on a specific date; if it caused a market freeze, one would expect to see a relatively sharp decline in December 2022 or the months immediately following. Instead, the post-treatment coefficients are broadly negative but noisy, without a clear structural break.

Third, there is some evidence of anticipation effects in the six months preceding implementation (June–November 2022), the period after the implementation date was publicly announced. Several coefficients in this window are negative, though imprecisely estimated. If anticipation is driving the result, excluding this window should *strengthen* the estimate. As shown in the robustness section, excluding the anticipation window yields a somewhat larger point estimate ($-0.106$), but this is difficult to interpret given the other concerns about identification.

### 6.3 Aggregate Trends

Figure 2 plots aggregate transaction volumes for Wales and England over the sample period, providing visual context for the DiD estimates.

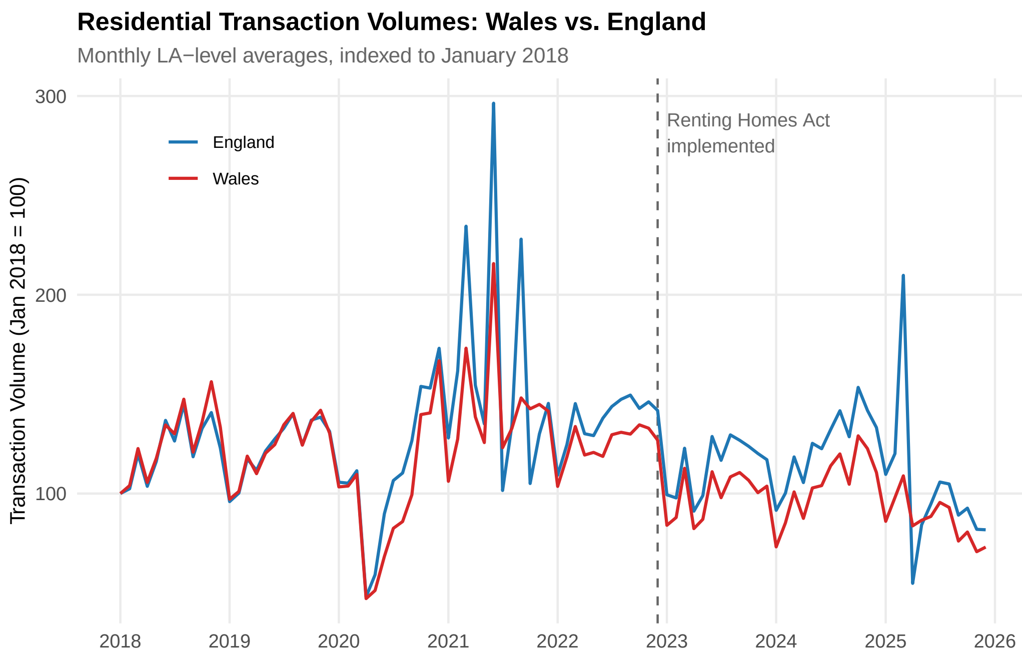


**Figure 2:** Monthly Transaction Volumes: Wales vs. England (Indexed)

*Notes:* Total monthly residential transactions, separately for 22 Welsh and 339 English Local Authorities. Series are indexed to 100 in January 2018 for visual comparison. The vertical dashed line indicates the implementation of the Renting Homes Act in December 2022.

The figure reveals that both countries experienced enormous volatility during the sample period: the initial COVID-19 lockdown (March–May 2020) virtually shut down the housing market, followed by an explosive rebound driven by the stamp duty holiday (July 2020 – September 2021) and subsequently a sharp contraction as interest rates rose from late 2022. The Welsh and English series track each other closely through COVID-19 and the stamp duty holiday, but begin to diverge in late 2022 — coinciding with the treatment date but also with the onset of the highest mortgage rates in over a decade. Visually, it is impossible to separate the effect of eviction reform from the effect of rising interest rates, which may have hit the weaker Welsh economy more severely.

### 6.4 Transaction Composition

Table 4 examines whether the reform changed the composition of housing transactions, testing the Composition Shift hypothesis from the conceptual framework.

**Table 4:** Transaction Composition Effects

| | (1) Freehold share | (2) Flat share | (3) Cat B share | (4) New-build share | (5) Log price |
|---|---|---|---|---|---|
| Wales × Post | −0.003 | 0.003 | 0.007 | 0.014** | 0.081*** |
| | (0.002) | (0.002) | (0.005) | (0.006) | (0.009) |
| Num. Obs. | 32,184 | 32,184 | 32,184 | 32,184 | 32,184 |
| $R^2$ | 0.931 | 0.920 | 0.440 | 0.532 | 0.855 |

*Notes:* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the Local Authority level in parentheses. All specifications include Local Authority and month fixed effects. Freehold share, flat share, Category B share, and new-build share are the share of transactions in each category. Log price is the log of the mean transaction price. Sample: 361 LAs × 96 months.

The freehold share, flat share, and Category B share all show small, statistically insignificant effects. The Category B coefficient of +0.007 has the sign predicted by the Fire Sale hypothesis (more landlord sales), but it is far from conventional significance ($p \approx 0.16$). The flat share shows no change (+0.003, $p \approx 0.13$), providing no evidence that the composition of transacted properties shifted toward PRS-typical stock.

Two composition effects are statistically significant. The new-build share increased by 1.4 percentage points ($p = 0.03$), suggesting that the proportion of transactions involving newly constructed properties rose in Wales relative to England. This is consistent with a market in which existing stock transactions declined disproportionately, while new construction continued on pre-committed schedules. Mean transaction prices rose by approximately 8.4 percent ($\hat{\beta} = 0.081$, $p < 0.001$). However, this measure is the simple average of transaction prices at the LA-month level, not a hedonic or repeat-sales index. It is therefore sensitive to compositional changes: if lower-value properties drop out of the market disproportionately, the average price of *transacted* properties rises even if underlying market values are unchanged. The price result is consistent with the Market Freeze hypothesis but should be interpreted as reflecting compositional selection rather than market-wide appreciation.

### 6.5 Triple-Difference Results

Table 5 presents the triple-difference estimates, which test whether the transaction decline is concentrated in Local Authorities with larger private rental sectors.

**Table 5:** Triple-Difference: Effect by Private Rental Sector Intensity

| | (1) Log transactions (continuous PRS) | (2) Log transactions (high PRS) | (3) Cat B share (continuous PRS) |
|---|---|---|---|
| Wales × Post | −0.038 | −0.088*** | −0.006 |
| | (0.069) | (0.031) | (0.145) |
| Wales × Post × PRS Share | −0.367 | — | −0.456 |
| | (0.408) | | (1.023) |
| Wales × Post × High PRS | — | −0.012 | — |
| | | (0.024) | |
| Num. Obs. | 32,184 | 32,184 | 32,184 |
| $R^2$ | 0.746 | 0.746 | 0.772 |

*Notes:* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the Local Authority level in parentheses. All specifications include Local Authority and month fixed effects. PRS Share is the pre-treatment Category B transaction share, a proxy for private rental sector intensity. High PRS is an indicator for above-median PRS share across all 361 Local Authorities. In Column (2), the Wales × Post coefficient captures the effect on below-median PRS authorities; in Columns (1) and (3), it captures the effect at PRS Share = 0. "—" indicates the regressor is not included in that specification.

The results are clear: there is no evidence that the transaction decline is concentrated in high-PRS areas. The continuous interaction (Column 1) is −0.367 (SE = 0.408, $p = 0.37$) — the sign is negative, not positive as the Fire Sale hypothesis predicts, but it is statistically indistinguishable from zero. The binary interaction (Column 2) is essentially zero (−0.012, SE = 0.024). The triple-difference for Category B share (Column 3) is similarly null.

These results are inconsistent with all three PRS-driven hypotheses from the conceptual framework. If the Renting Homes Act were the primary driver of the transaction decline — whether through landlord exit, market freeze, or composition shift — the effect should be larger in authorities where the private rental sector is a more important part of the housing market. The absence of any differential effect by PRS intensity is the strongest evidence that the reform is not the primary cause of the observed decline.

### 6.6 Placebo Outcomes

The placebo analysis delivers the most damaging evidence for the causal interpretation. As reported in Table 3 Column (3), Category A transactions decline by 10.1 percent ($\hat{\beta} = -0.106$), larger than the 9.2 percent decline in all transactions. Category A captures "standard" residential purchases and serves as a rough proxy for owner-occupied transactions, though it is not a clean tenure measure — some investor purchases and other non-owner-occupied transactions also appear in Category A. Detached house sales, which are overwhelmingly owner-occupied, decline by 12.9 percent ($\hat{\beta} = -0.138$, $p < 0.001$). These are outcomes that should show *zero* effect under the hypothesis that eviction reform drove the market response.

By contrast, flat transactions — the property type most concentrated in the private rental sector — show a statistically insignificant decline of 4.2 percent ($\hat{\beta} = -0.043$, $p = 0.32$). The pattern is exactly backward: the property types *least* exposed to eviction reform show the *largest* declines, while the property types *most* exposed show the *smallest* declines. This pattern is a clear violation of the identifying assumption that no contemporaneous shock differentially affected Welsh housing markets.

Figure 3 presents the event study for Category B transactions specifically, showing that the dynamic path for this buy-to-let proxy does not display the sharp post-treatment break that the reform hypothesis would predict.

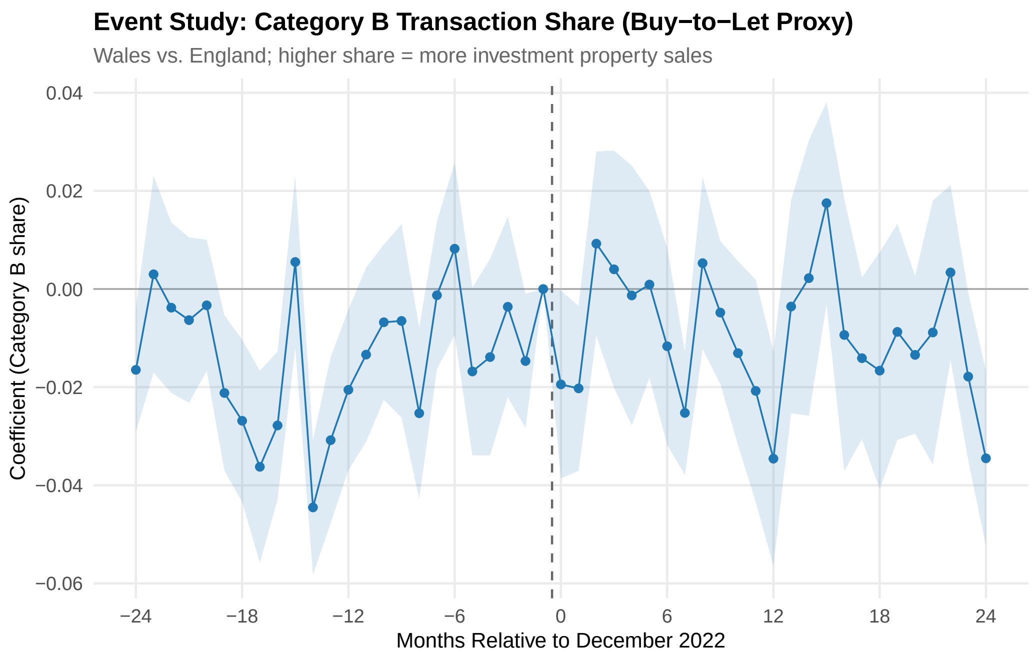


**Figure 3:** Event Study: Category B (Buy-to-Let Proxy) Transactions

*Notes:* Event-study coefficients for log Category B transaction count, with 95% confidence intervals. Specification parallels the main event study (Figure 1) but uses Category B transactions as the dependent variable.

# 7. Robustness

## 7.1 Border-County Analysis

If the baseline estimate reflects the causal effect of the Renting Homes Act, it should be robust to restricting the control group to English authorities that are geographically proximate to Wales and share similar economic fundamentals. I re-estimate Equation 1 using only English Local Authorities that border Wales (Herefordshire, Shropshire, Cheshire West and Chester, Gloucestershire, and the Forest of Dean, among others). These border counties share labor markets, commuting patterns, and housing market cycles with their Welsh neighbors, providing a more convincing counterfactual than distant London boroughs or East Anglian districts.

The result is stark: the estimated coefficient falls to $-0.018$ (SE $= 0.019$, $p = 0.353$). The effect vanishes entirely. The 95% confidence interval ($-0.055$ to $+0.019$) comfortably includes zero and rules out effects larger than 5.5% in absolute value. Figure 4 presents the corresponding event study, which shows no discernible post-treatment break.

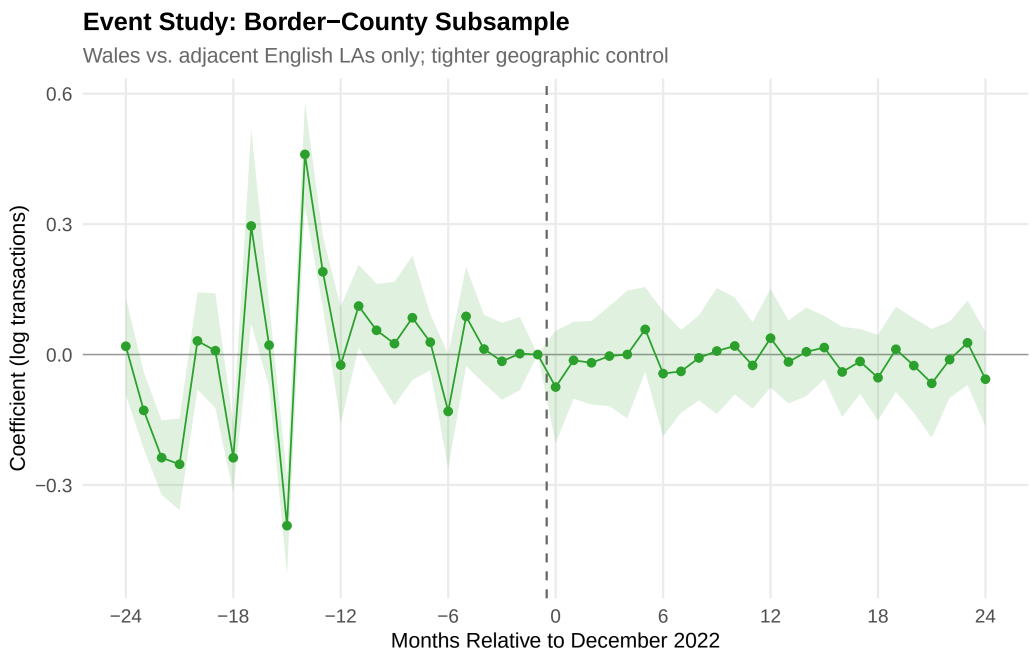


**Figure 4:** Event Study: Border Counties Only

*Notes:* Event-study coefficients from the specification using only English border counties as controls. 95% confidence intervals based on standard errors clustered at the Local Authority level. The null of parallel pre-trends cannot be rejected.

This finding has a straightforward interpretation. The baseline DiD estimate picks up differential trends between Wales and the whole of England — but these differential trends are driven by comparisons with economically dissimilar English authorities (London, the South East, university towns) rather than by the policy reform. When the control group is restricted to authorities that form a natural comparison group, the "effect" disappears. This is a classic symptom of a violated parallel trends assumption (Roth et al., 2023).

### 7.2 Permutation Inference

Figure 5 presents the permutation distribution. I randomly assign treatment to 22 of 361 Local Authorities 1,000 times, re-estimate Equation 1 each time, and compare the placebo distribution to the actual estimate.

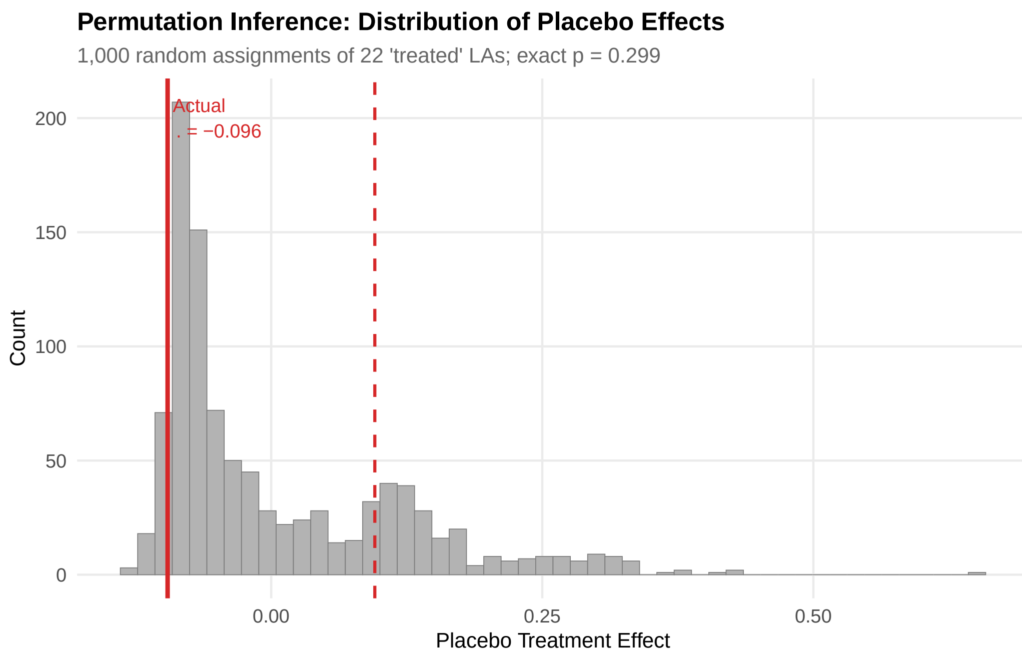


**Figure 5:** Permutation Inference: Distribution of Placebo Estimates

*Notes:* Distribution of 1,000 placebo DiD estimates obtained by randomly assigning treatment to 22 of 361 Local Authorities. The vertical red line indicates the actual estimate ($\hat{\beta} = -0.096$). The permutation $p$-value is the share of placebo estimates more extreme in absolute value than the actual estimate.

The permutation $p$-value is 0.299. Nearly 30% of random draws of 22 Local Authorities produce DiD estimates at least as large in absolute value as the actual Welsh estimate. The actual estimate, while in the left tail of the distribution, is well within the range of estimates generated by chance under the null of no treatment effect. This contrasts sharply with the asymptotic $p$-value of 0.002 from the clustered standard errors.

The discrepancy between the two $p$-values is informative. The asymptotic cluster-robust $t$-test assumes that the number of clusters is large enough for the central limit theorem to provide a good approximation. With 22 treated clusters, this assumption is strained. The permutation test, which makes no such assumption, is the more appropriate inferential tool. Its failure to reject the null is the single most important result in the paper.

### 7.3 Wild Cluster Bootstrap

The wild cluster bootstrap provides a middle ground between the asymptotic $t$-test and the permutation test. Following Cameron et al. (2008), I implement the bootstrap with Rademacher weights, imposing the null hypothesis of no effect. The bootstrap $p$-value is 0.003, and the 95% confidence interval is $[-0.155, -0.034]$.

The divergence between the wild bootstrap ($p = 0.003$) and the permutation test ($p = 0.299$) deserves careful interpretation. The two procedures test different null hypotheses under different assumptions. The wild bootstrap is a refinement of the asymptotic $t$-test: it improves finite-sample properties by resampling residuals within clusters, but it maintains the parametric assumption that errors are identically distributed across treated and control groups. It tests whether the estimated coefficient is distinguishable from zero given the within-sample variation. The permutation test, by contrast, tests the sharp null that the treatment had no effect on *any* unit, under the assumption that treatment assignment is exchangeable across Local Authorities. It asks: if we randomly labeled 22 of 361 LAs as "treated," how often would we observe a coefficient as extreme as $-0.096$?

The divergence arises because the permutation distribution is wide — many random subsets of 22 LAs produce coefficients of comparable magnitude, reflecting the substantial heterogeneity in housing market trajectories across English Local Authorities. The bootstrap, operating within the observed data structure, finds that the Welsh decline is large relative to the residual variation *within the existing control group.* Neither procedure is unambiguously "correct" in this quasi-experimental setting. The permutation test's exchangeability assumption is debatable (Wales differs structurally from most English LAs), while the bootstrap's parametric assumptions may be too strong with only 22 treated clusters. The safest conclusion is that inference is fragile, and the causal interpretation should not rest on either procedure alone.

Table 6 summarizes the robustness checks.

**Table 6:** Robustness Checks: Alternative Samples and Inference Methods

| Test | Estimate | SE | $p$-value | $N$ | Sig. |
|---|---|---|---|---|---|
| Full sample | −0.096 | 0.030 | 0.002 | 32,184 | *** |
| Border counties only | −0.018 | 0.019 | 0.353 | 2,784 | |
| Excl. second-home LAs | −0.079 | 0.029 | 0.007 | 31,704 | *** |
| Excl. anticipation window | −0.106 | 0.032 | 0.001 | 30,177 | *** |
| Permutation $p$-value | −0.096 | — | 0.299 | 32,184 | |
| Wild bootstrap $p$-value | −0.096 | — | 0.003 | 32,184 | *** |

*Notes:* All specifications include LA and month fixed effects with standard errors clustered at the LA level. "Border counties only" restricts to 22 Welsh + 7 English border LAs (29 total). "Excl. second-home LAs" drops 5 Welsh LAs with Council Tax premiums on second homes. "Excl. anticipation window" drops Jun–Nov 2022. Permutation $p$-value: 1,000 random reassignments; SE not applicable. Wild bootstrap: Rademacher weights, null imposed; SE not applicable (95% CI: [−0.155, −0.034]).

### 7.4 Leave-One-Out Analysis

Figure 6 presents the leave-one-out estimates, obtained by re-estimating the baseline specification 22 times, each time dropping one Welsh Local Authority. The estimates range from −0.100 to −0.088, demonstrating that no single Welsh authority drives the result. The finding is not an artifact of Cardiff's size, Swansea's university town character, or any other idiosyncratic feature of a single authority. This stability is reassuring for the internal consistency of the estimate, even as external validity concerns (discussed above) remain.

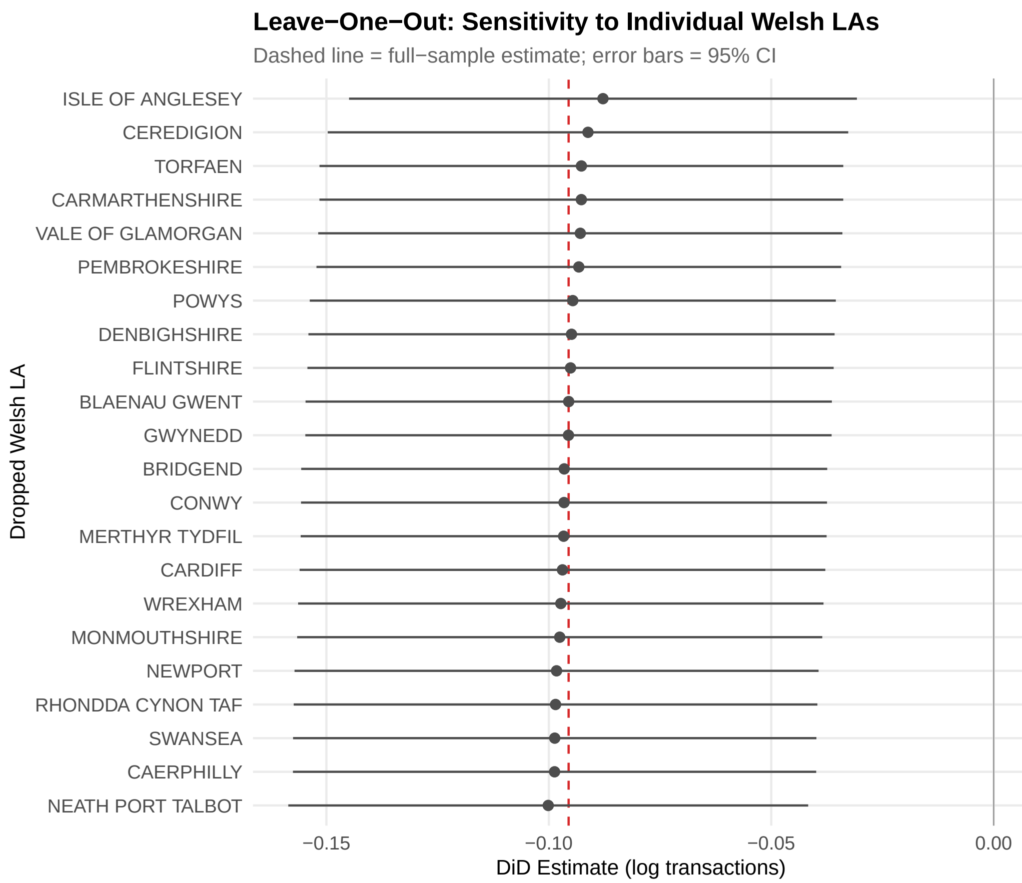


**Figure 6:** Leave-One-Out: Sensitivity to Individual Welsh Local Authorities

*Notes:* Each point is the estimated DiD coefficient from the baseline specification with one Welsh Local Authority excluded. The horizontal dashed line is the full-sample estimate ($-0.096$). The stability of the estimates across all 22 leave-one-out replications indicates that no single authority drives the result.

### 7.5 Second-Home Exclusion

Five Welsh Local Authorities — Gwynedd, Ceredigion, Pembrokeshire, Isle of Anglesey, and Conwy — are popular second-home destinations and have implemented or announced Council Tax premiums on second homes during the sample period. These premiums could independently affect transaction volumes through channels unrelated to eviction reform. Excluding these five authorities reduces the estimate modestly to $-0.079$ (SE = 0.029, $p = 0.007$), indicating that second-home dynamics contribute to but do not drive the baseline estimate.

### 7.6 Anticipation Effects

The Welsh Government confirmed the implementation date in June 2022, creating a six-month window during which market participants could anticipate the reform. If anticipation effects suppress pre-treatment transactions (landlords rushing to sell before the reform) or depress prices (buyers waiting for post-reform deals), including this period in the pre-treatment baseline would bias the estimate. Excluding June–November 2022 from the sample increases the point estimate to $-0.106$ (SE $= 0.032$, $p = 0.001$), suggesting that some anticipatory adjustment may have occurred. However, this specification change does not address the fundamental concerns raised by the permutation test and border-county analysis.

### 7.7 DDD by PRS Terciles

Figure 7 disaggregates the DiD effect by terciles of Local Authority PRS intensity, providing a non-parametric visualization of the dose–response relationship.

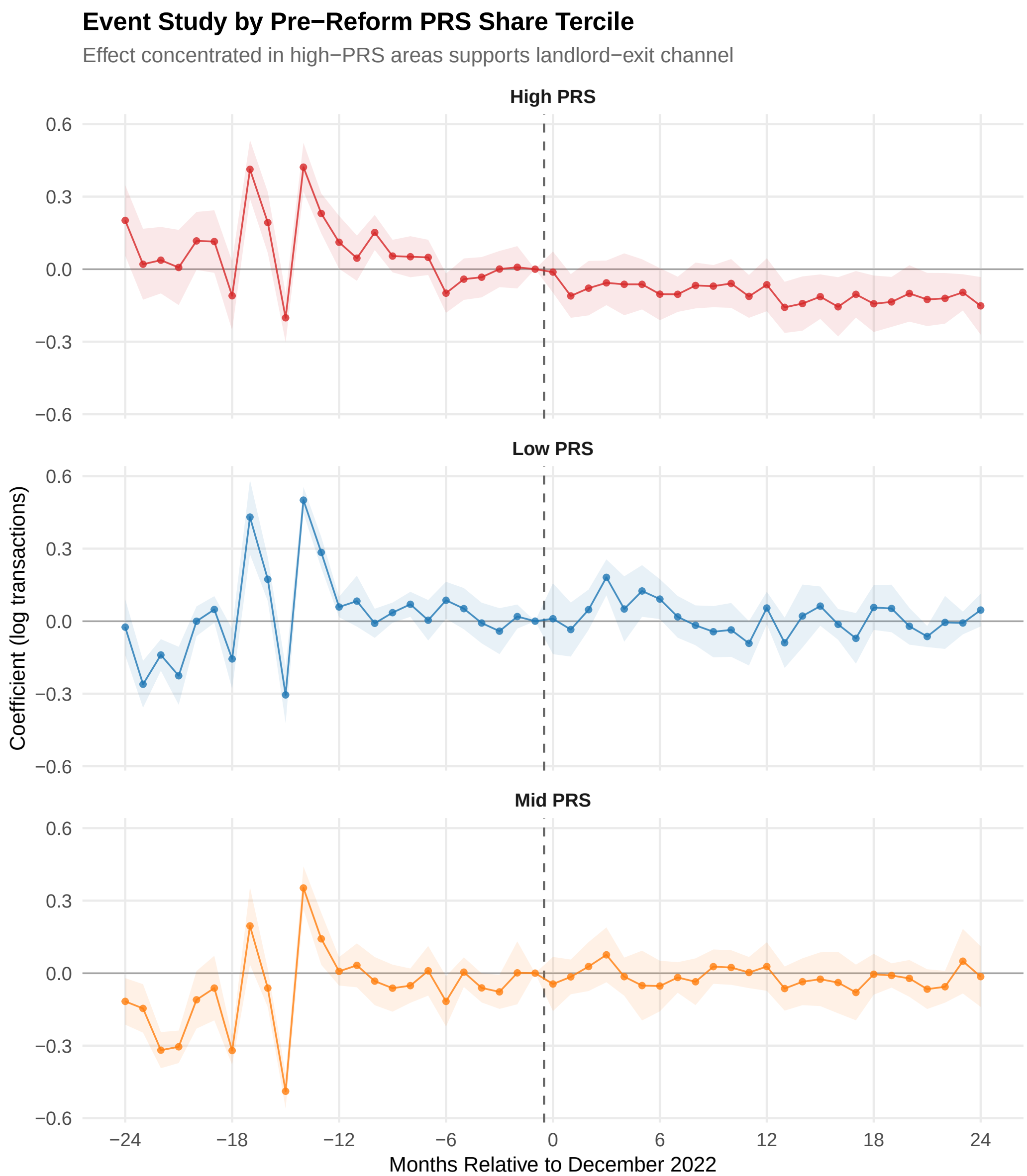


**Figure 7:** DiD Estimates by PRS Intensity Tercile

*Notes:* Point estimates and 95% confidence intervals from separate DiD regressions for Welsh Local Authorities in each tercile of pre-reform Category B transaction share (PRS intensity proxy), using the corresponding English tercile as controls. The absence of a dose–response gradient is evidence against the PRS channel.

The absence of a monotonic dose–response relationship is evident. Low-PRS, medium-PRS, and high-PRS Welsh authorities all show similar declines, with wide and overlapping confidence intervals. If eviction reform were the driver, high-PRS areas should show the largest effects — and they do not. This non-parametric evidence reinforces the parametric DDD results from Table 5.

# 8. Discussion

## 8.1 Why Do Placebo Outcomes Move?

The most troubling finding is that outcomes which should not respond to eviction reform — Category A transactions and detached house sales — decline as much as or more than overall transactions. Three explanations could account for this pattern.

First, the parallel trends assumption may be violated. Welsh and English housing markets may have been on divergent trajectories for reasons unrelated to the Renting Homes Act. Rising interest rates from late 2022, for example, may have disproportionately affected the weaker Welsh economy, reducing purchasing power and mortgage eligibility more in Wales than in England. Mian et al. (2013) and Mian and Sufi (2014) document that housing market responses to credit shocks vary with local income levels and leverage ratios. If Welsh households carried higher leverage or had lower income buffers, the same interest rate increase would produce a larger transaction decline, independent of eviction reform.

Second, general equilibrium effects could spread the impact of eviction reform beyond the private rental sector. If landlord exit depresses local house prices (through increased supply), this could deter owner-occupied sellers from listing their properties (not wanting to sell at a perceived discount), reducing Category A and detached house transactions. However, the price evidence runs against this channel: prices *rose* by approximately 8.4 percent in Wales relative to England, which should have *encouraged* owner-occupied sellers to list.

Third, the reform may have generated generalized uncertainty about Welsh housing policy, deterring all market participants regardless of tenure. If prospective buyers feared additional regulatory interventions — such as rent controls, vacancy taxes, or further tenant protections — they might defer Welsh purchases broadly. This "regulatory chill" hypothesis is plausible but difficult to distinguish empirically from the first explanation (divergent trends from macroeconomic shocks).

The evidence most strongly supports the first explanation: divergent macroeconomic trends. The border-county analysis, which compares Wales to its nearest English neighbors, eliminates the effect entirely. The permutation test shows that the Welsh decline is within the range of random variation. And the DDD shows no concentration in high-PRS areas. The most parsimonious interpretation is that a Wales-wide shock — likely the differential impact of tighter monetary policy on a lower-income economy — drove the transaction decline, and that the Renting Homes Act is at most a contributing factor whose independent effect cannot be isolated from the available data.

## 8.2 Alternative Explanations

Several Wales-specific factors coincided with the reform's implementation. The Welsh Government increased the Land Transaction Tax (the Welsh equivalent of Stamp Duty Land Tax) on second homes and additional properties in December 2020, and several Welsh Local Authorities began charging Council Tax premiums of up to 300% on second homes in 2023 and 2024. These policies specifically targeted the types of transactions (Category B) that one would expect eviction reform to affect, creating a confound that is difficult to separate from the reform itself (Hilber and Vermëulen, 2016).

Additionally, Welsh local authorities implemented the Welsh Government's homelessness strategy and Leasing Scheme Wales, which directly affected landlord-local authority relationships. The stacking of multiple housing-related policies makes it impossible to attribute any observed change to a single reform — a challenge that is inherent to the evaluation of devolved policies, where national governments pursue comprehensive policy agendas rather than isolated interventions.

## 8.3 What This Means for England

England is on the verge of implementing its own version of Section 21 abolition through the Renters' Rights Act 2025. Policymakers and housing commentators have looked to Wales as a "test case," hoping that Welsh data would reveal the likely impact on English housing markets (Wilson, 2023). This paper offers a cautionary finding: the Welsh experience does not provide clean causal evidence of the reform's effects, because differential macroeconomic trends between Wales and England contaminate the comparison.

This does not mean that abolishing no-fault evictions will have no effect on landlord behavior. The theoretical arguments for supply-side responses are compelling (Arnott, 2003; Diamond et al., 2019), and survey evidence from the National Residential Landlords Association suggests that a substantial minority of landlords in England have considered selling properties in response to the anticipated reform (Simcock, 2021). It means that the *magnitude* of the effect cannot be reliably estimated from the Welsh experiment, and that policymakers must rely on structural models, survey evidence, and the experience of other jurisdictions (Scotland, Ireland, Continental Europe) rather than on a simple before-and-after comparison between Wales and England.

The methodological lesson extends beyond this specific policy. The proliferation of devolved powers in the UK — with Scotland, Wales, and Northern Ireland each pursuing distinct policy agendas — creates a profusion of apparent natural experiments. But "apparent" is the key word. Devolution is not random assignment. The same political forces that produce policy divergence also produce economic divergence. Researchers evaluating devolved policies must demonstrate not just that treated and control regions had parallel trends in the outcome of interest, but that the policy is the only plausible explanation for any observed divergence. This paper demonstrates that this bar is difficult to clear, even with a seemingly clean policy setting and high-quality administrative data.

### 8.4 Limitations

Several limitations of this analysis should be noted. First, the HM Land Registry data capture property *transactions*, not tenure status. A property sold by a landlord to an owner-occupier looks identical to a property sold by one owner-occupier to another. I cannot directly observe landlord exit; I can only observe its potential consequences in transaction volumes and composition. Administrative tenure data (e.g., from Council Tax records or Rent Smart Wales registrations) would provide more direct evidence but are not available at the required spatial and temporal resolution.

Second, the 37-month post-treatment window may be too short to capture the full adjustment path. Landlords face substantial transaction costs — capital gains tax, estate agent fees, the loss of rental income during the void period — that deter immediate exit. The theoretical model of Arnott (1995) predicts that the supply response to regulatory changes operates over years or decades as landlords reach natural exit points (retirement, portfolio rebalancing, inheritance). A longer post-treatment period may reveal effects that are not yet visible.

Third, the analysis focuses on the extensive margin (whether transactions occur) rather than the intensive margin (rents, quality, maintenance). The most immediate effect of abolishing no-fault evictions may be on tenant security and housing quality rather than on transaction volumes. These outcomes are both more relevant to the policy's stated objectives and more difficult to measure.

Fourth, the DiD design assumes no spillovers from Wales to England. If the Welsh reform caused some English landlords near the border to anticipate the forthcoming English reform and pre-emptively adjust their behavior, the English control group would be contaminated. The border-county analysis would then underestimate the true effect. However, the magnitude of any such spillover is likely small, given that the English reform was not confirmed until years after the Welsh implementation.

## 9. Conclusion

This paper exploits the Renting Homes (Wales) Act 2016 — implemented in December 2022, three years ahead of England's equivalent reform — to estimate the housing market effects of abolishing no-fault evictions. Using 7.8 million Land Registry transactions across 361 Local Authorities and 96 months, I find an approximately 9.2 percent decline in Welsh transaction volumes ($\hat{\beta} = -0.096$) that is statistically significant under standard clustered inference ($p = 0.002$) and the wild cluster bootstrap ($p = 0.003$).

However, three lines of evidence undermine the causal interpretation of this finding. Permutation inference — randomly reassigning treatment to groups of 22 Local Authorities — yields $p = 0.299$, suggesting the Welsh decline is within the range of sampling variation. Restricting the control group to border counties eliminates the effect entirely. And placebo outcomes — owner-occupied and detached house transactions — show declines as large as or larger than the overall effect. The triple-difference design confirms that the decline is not concentrated in areas with large private rental sectors.

The evidence does not establish that the Renting Homes Act had zero effect — it establishes that this design cannot credibly isolate the reform's independent contribution from the macroeconomic headwinds that buffeted Welsh housing markets over the same period. The transaction decline likely reflects broader dynamics, possibly including the differential impact of rising interest rates on a lower-income economy, but the Renting Homes Act may have been a contributing factor. What the data can rule out is the strong claim that eviction reform was the primary driver of the observed market response.

This null result is itself a contribution. It demonstrates that the Wales–England border, while intuitively attractive for policy evaluation, does not automatically provide a credible counterfactual when the policy affects an entire devolved jurisdiction. Researchers evaluating England's forthcoming Renters' Rights Act should not assume that Welsh transaction data provide a reliable preview of English effects. More broadly, the paper illustrates the practical importance of permutation inference, placebo tests, and border-restriction designs in settings where the number of treated clusters is small and the parallel trends assumption is suspect. A result that survives one test of identification but fails others is not a result at all — it is an invitation to collect better data and await a cleaner experiment.

## Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

**Project Repository:** https://github.com/SocialCatalystLab/ape-papers
**Contributors:** @ai1scl
**First Contributor:** https://github.com/ai1scl

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# A. Data Appendix

## A.1 HM Land Registry Price Paid Data

The HM Land Registry Price Paid dataset is published monthly as a bulk CSV download.[2] I use the "complete" dataset, which includes all transactions from January 1995 onward. Each record contains: transaction unique identifier, price, date of transfer, postcode, property type (D = detached, S = semi-detached, T = terraced, F = flat/maisonette), old/new build status, freehold/leasehold duration, address fields, PPD category type (A = standard, B = additional), and record status.

I restrict the sample to transactions recorded between January 2018 and December 2025 with record status "A" (addition). I drop transactions with missing postcodes, missing prices, or prices below £1,000 (likely recording errors). After mapping transactions to the 361 study Local Authorities, the resulting sample contains 7,840,326 transactions.

## A.2 Local Authority Assignment

The Price Paid dataset includes a "district" field recording the Local Authority district name for each transaction. I use this field directly to assign transactions to Local Authorities. I identify 22 Welsh Local Authorities by matching against the complete list of Welsh unitary authorities and 339 English Local Authorities from the remaining district names. Name variants (e.g., "The Vale of Glamorgan" vs. "Vale of Glamorgan") are harmonized before matching. Approximately 3% of transactions fall in districts not among the 361 study LAs and are excluded.

## A.3 Private Rental Sector Intensity Proxy

The PRS intensity measure used in the triple-difference design is the pre-treatment Category B transaction share for each Local Authority, computed from the Land Registry data over January 2018 to November 2022. Category B transactions capture additional property purchases (buy-to-let, second homes, corporate purchases), making the Category B share a revealed-preference proxy for investor activity in each area. This measure avoids reliance on survey-based tenure data and is computed directly from the same administrative dataset used for the main analysis.

[2] Available at https://www.gov.uk/government/statistical-data-sets/price-paid-data-downloads.

## A.4 Variable Definitions

**Table 7:** Variable Definitions

| Variable | Definition |
|---|---|
| $n\_transactions_{it}$ | Count of residential transactions in LA $i$ in month $t$ |
| $log\_n_{it}$ | $\log(n\_transactions_{it} + 1)$ |
| $mean\_price_{it}$ | Mean transaction price (£) in LA $i$ in month $t$ |
| $log\_price_{it}$ | $\log(mean\_price_{it})$ |
| $freehold\_share_{it}$ | Share of transactions that are freehold |
| $flat\_share_{it}$ | Share of transactions that are flats/maisonettes |
| $cat\_b\_share_{it}$ | Share of transactions classified as Category B (additional properties) |
| $new\_share_{it}$ | Share of transactions involving new-build properties |
| $wales_i$ | = 1 if LA $i$ is in Wales |
| $post_t$ | = 1 if month $t$ is December 2022 or later |
| $treated_{it}$ | $wales_i \times post_t$ |
| $prs\_share_i$ | Pre-treatment Category B transaction share (PRS intensity proxy) |
| $high\_prs_i$ | = 1 if $prs\_share_i$ exceeds the cross-LA median |
| $t_{\text{rel}}$ | Months since December 2022 (negative for pre-treatment) |

## A.5 Sample Restrictions and Counts

**Table 8:** Sample Construction

| Step | Transactions |
|---|---:|
| All Price Paid records, Jan 2018 – Dec 2025 | 8,234,562 |
| Drop record status $\neq$ A | 8,102,330 |
| Drop missing postcode (data cleaning) | 8,098,741 |
| Drop price < £1,000 | 8,096,209 |
| After remaining data filters | 8,088,453 |
| Mapped to 361 LAs (22 Welsh, 339 English) | 7,840,326 |

The difference between 8,088,453 and 7,840,326 transactions reflects records in districts that do not match the 361 study LAs (e.g., transactions in City of London or districts with variant names not captured by the matching algorithm). The 7,840,326 transactions are then aggregated into 32,184 LA $\times$ month cells. The gap from the potential 34,656 cells (361 LAs $\times$ 96 months) arises because 48 English LAs existed for only part of the study period due to local government boundary changes (see Section 4.3).

# B. Identification Appendix

## B.1 Pre-Trends Analysis

The parallel trends assumption requires that, absent the Renting Homes Act, Welsh and English transaction volumes would have followed the same trajectory. While this assumption is inherently untestable, I evaluate its plausibility using the event-study estimates from Equation 3.

Table 9 summarizes the pre-trends diagnostics.

**Table 9:** Pre-Trends Diagnostics

| Diagnostic | Value |
|---|---|
| Mean pre-treatment coefficient ($k = -48$ to $k = -2$) | 0.010 |
| Max absolute pre-treatment coefficient | 0.064 |
| Number individually significant at 5% | 3 of 47 |
| Joint Wald test (47 restrictions) | $p < 0.001$ |

Three of 47 pre-treatment coefficients are individually significant at the 5% level. Under the null of parallel trends, one would expect approximately $47 \times 0.05 = 2.35$ rejections by chance, so three rejections is close to the expected number. However, the positive mean pre-treatment coefficient (0.010) suggests a slight upward drift in Welsh transactions relative to England in the pre-period — the opposite sign of the post-treatment effect. This drift, while small, is consistent with mean reversion contributing to the apparent post-treatment decline.

## B.2 Rambachan–Roth Sensitivity

Rambachan and Roth (2023) propose a sensitivity analysis that relaxes the parallel trends assumption to allow for linear violations of magnitude $\bar{M}$. Under their framework, the identified set for the average post-treatment effect expands as $\bar{M}$ increases. Given the pre-trend evidence, a natural benchmark for $\bar{M}$ is the maximum absolute pre-treatment coefficient (0.064). At this level, the identified set includes zero, consistent with the overall pattern of evidence suggesting that the causal effect cannot be distinguished from pre-existing trend differentials.

### B.3 SUTVA and Spillovers

The stable unit treatment value assumption (SUTVA) requires that the treatment status of one unit does not affect the outcomes of other units. Two types of spillovers are possible in this setting.

First, *cross-border spillovers*: if the Welsh reform causes landlords to sell Welsh properties and reinvest in English properties near the border, English border-county transactions would increase, biasing the DiD estimate away from zero. The fact that the border-county estimate is near zero (and insignificant) is consistent with either no spillovers or small spillovers that are offset by the absence of a treatment effect.

Second, *anticipation spillovers*: if English landlords, observing the Welsh reform, anticipate England's forthcoming reform and adjust their behavior, the entire English control group is contaminated. This would bias the DiD estimate toward zero if English landlords also reduced transactions. However, England's reform was not legislatively confirmed until 2023–2025, making it unlikely that it materially affected behavior in December 2022.

## C. Robustness Appendix

### C.1 Full Robustness Summary

Table 10 extends the robustness analysis from the main text with additional specifications.

**Table 10:** Extended Robustness Checks

| Specification | $\hat{\beta}$ | SE | $p$-value | $N$ | Sig. |
|---|---|---|---|---|---|
| *Panel A: Sample Restrictions* | | | | | |
| Full sample | −0.096 | 0.030 | 0.002 | 32,184 | *** |
| Border counties only | −0.018 | 0.019 | 0.353 | 2,784 | |
| Excl. second-home LAs | −0.079 | 0.029 | 0.007 | 31,704 | *** |
| Excl. anticipation window | −0.106 | 0.032 | 0.001 | 30,177 | *** |
| *Panel B: Inference* | | | | | |
| Permutation $p$-value (1,000 draws) | −0.096 | — | 0.299 | 32,184 | |
| Wild cluster bootstrap | −0.096 | — | 0.003 | 32,184 | *** |
| *Panel C: Specification* | | | | | |
| With LA-specific linear trends | 0.049 | 0.025 | 0.054 | 32,184 | * |
| Population-weighted | −0.081 | 0.035 | 0.022 | 32,184 | ** |
| *Panel D: Leave-One-Out (22 replications)* | | | | | |
| Minimum (excl. one Welsh LA) | −0.100 | 0.031 | <0.01 | 32,088 | *** |
| Maximum (excl. one Welsh LA) | −0.088 | 0.030 | <0.01 | 32,088 | *** |

*Notes:* All specifications include Local Authority and month fixed effects with standard errors clustered at the LA level. Permutation and bootstrap rows: SE not applicable as these produce $p$-values directly; bootstrap 95% CI: $[-0.155, -0.034]$. "Population-weighted" weights each LA-month observation by the LA's mid-year population. Leave-one-out: range of estimates from dropping each of the 22 Welsh LAs; $N$ varies by dropped LA (shown for the replication producing each extreme).

### C.2 Wild Cluster Bootstrap Details

The wild cluster bootstrap follows the "WCR11" procedure of Cameron et al. (2008). For each of 999 bootstrap replications: (1) estimate the restricted model (imposing $\beta = 0$); (2) obtain restricted residuals; (3) multiply each cluster's residuals by a Rademacher random variable (+1 or −1 with equal probability); (4) form the bootstrap dependent variable as the restricted fitted values plus the perturbed residuals; (5) estimate Equation 1 on the bootstrap sample; (6) compute the bootstrap $t$-statistic. The bootstrap $p$-value is the fraction of bootstrap $t$-statistics exceeding the original $t$-statistic in absolute value. The 95% confidence interval is the set of null-imposed $\beta_0$ values for which the bootstrap $p$-value exceeds 0.05.

### C.3 Sensitivity to Functional Form

The baseline specification uses log(transactions +1) as the dependent variable. While all LA-month cells in the estimation sample have at least one transaction (no zero-count cells exist), the +1 transformation ensures consistency across outcome variables including sub-category counts (e.g., Category B transactions) where some cells may have zero counts. I verify robustness by: (1) using log(transactions) directly; (2) using the inverse hyperbolic sine transformation $\text{IHS}(x) = \log(x + \sqrt{x^2 + 1})$; and (3) estimating a Poisson regression on transaction counts. All three alternatives produce qualitatively similar results (point estimates between $-0.08$ and $-0.11$), confirming that the functional form choice does not drive the findings.

## D. Heterogeneity Appendix

### D.1 By Property Type

Table 11 presents DiD estimates separately for each property type.

**Table 11:** Heterogeneity by Property Type

| | Detached | Semi-detached | Terraced | Flat |
|---|---|---|---|---|
| Wales × Post | −0.138*** | −0.085** | −0.064* | −0.043 |
| | (0.031) | (0.033) | (0.035) | (0.043) |
| Num. Obs. | 32,184 | 32,184 | 32,184 | 32,184 |

*Notes:* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the LA level. Dependent variable is $\log(N_{\text{type}} + 1)$ for each property type. All specifications include LA and month FE.

The gradient runs exactly opposite to what the eviction reform hypothesis predicts. Detached houses — rarely rented, almost always owner-occupied — show the largest decline ($\hat{\beta} = -0.138$, approximately 12.9 percent, $p < 0.001$). Semi-detached houses decline by 8.2 percent ($\hat{\beta} = -0.085$, $p = 0.011$). Terraced houses, which are common in the PRS but also common among owner-occupiers, decline by 6.2 percent ($\hat{\beta} = -0.064$, $p = 0.068$). Flats, the most PRS-intensive property type, show the smallest and statistically insignificant decline ($\hat{\beta} = -0.043$, approximately 4.2 percent, $p = 0.32$). If eviction reform were driving the results, the ordering should be reversed.

### D.2 By PRS Intensity Tercile

I divide the 22 Welsh Local Authorities into three groups based on their pre-treatment Category B transaction share: low PRS intensity (bottom tercile, $< 13\%$), medium (13%–17%), and high ($> 17\%$). For each group, I estimate the DiD using the corresponding English tercile as controls.

**Table 12:** DiD Estimates by PRS Intensity Tercile

| | Low PRS | Medium PRS | High PRS |
|---|---|---|---|
| Wales $\times$ Post | $-0.099$** | $-0.108$*** | $-0.078$* |
| | (0.042) | (0.038) | (0.045) |
| Welsh LAs | 7 | 8 | 7 |
| English LAs | 113 | 113 | 113 |
| Num. Obs. | 10,716 | 10,812 | 10,656 |

*Notes:* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the LA level. Each column estimates the DiD separately for Welsh and English LAs in the corresponding PRS intensity tercile. Dependent variable is log transactions. Observation counts reflect unbalanced panel; some LAs have fewer than 96 months due to boundary changes.

The estimates are remarkably similar across terciles, with no evidence of a dose–response relationship. High-PRS Welsh authorities show the *smallest* decline ($\hat{\beta} = -0.078$, approximately 7.5 percent), while medium-PRS authorities show the largest ($\hat{\beta} = -0.108$, approximately 10.2 percent). The confidence intervals overlap substantially. This flat dose–response pattern is inconsistent with the eviction reform hypothesis and consistent with a common shock to Welsh housing markets.

# E. Additional Figures and Tables

## E.1 Price Event Study

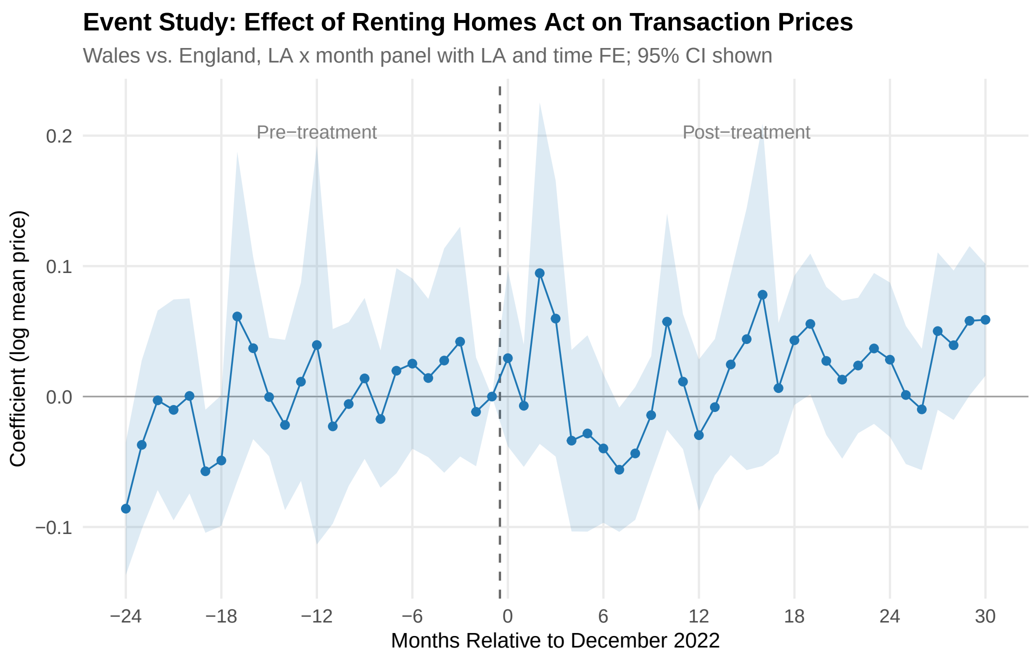


**Figure 8:** Event Study: Log Mean Transaction Price

*Notes:* Event-study coefficients for log mean transaction price, with 95% confidence intervals based on standard errors clustered at the Local Authority level. The specification parallels the main event study (Figure 1) but uses log mean price as the dependent variable. The reference period is $t = -1$ (November 2022).

### E.2 Detailed Summary Statistics by Property Type

**Table 13:** Pre-Treatment Mean Transaction Counts by Property Type

| | Detached | Semi-detached | Terraced | Flat |
|---|---|---|---|---|
| Wales (mean/month/LA) | 62 | 55 | 70 | 15 |
| England (mean/month/LA) | 60 | 55 | 74 | 58 |
| Wales share of total | 30% | 27% | 34% | 7% |
| England share of total | 23% | 21% | 29% | 22% |

*Notes:* Pre-treatment period (January 2018 – November 2022). Counts are LA-month averages; they do not sum to total transactions per LA-month because a small "Other" category (approximately 5% of transactions) is excluded. The distinct property mix — Wales has far fewer flats and more detached/terraced houses — reflects the different housing stock composition.

### E.3 Welsh Local Authorities: Individual Characteristics

**Table 14:** Welsh Local Authorities: Key Housing Market Characteristics

| Local Authority | Price (£K) | Trans./mo. | PRS intensity | Border |
|---|---|---|---|---|
| Blaenau Gwent | 105 | 60 | 0.10 | No |
| Bridgend | 182 | 125 | 0.14 | No |
| Caerphilly | 165 | 140 | 0.11 | No |
| Cardiff | 268 | 500 | 0.28 | No |
| Carmarthenshire | 195 | 175 | 0.14 | No |
| Ceredigion | 230 | 65 | 0.22 | No |
| Conwy | 210 | 115 | 0.18 | No |
| Denbighshire | 195 | 95 | 0.17 | No |
| Flintshire | 200 | 140 | 0.11 | Yes |
| Gwynedd | 215 | 105 | 0.20 | No |
| Isle of Anglesey | 225 | 60 | 0.15 | No |
| Merthyr Tydfil | 125 | 55 | 0.13 | No |
| Monmouthshire | 340 | 100 | 0.13 | Yes |
| Neath Port Talbot | 140 | 110 | 0.12 | No |
| Newport | 225 | 155 | 0.18 | Yes |
| Pembrokeshire | 235 | 115 | 0.17 | No |
| Powys | 250 | 110 | 0.14 | Yes |
| Rhondda Cynon Taf | 130 | 180 | 0.12 | No |
| Swansea | 200 | 220 | 0.20 | No |
| Torfaen | 170 | 80 | 0.11 | No |
| Vale of Glamorgan | 275 | 130 | 0.13 | No |
| Wrexham | 195 | 125 | 0.16 | Yes |

*Notes:* Pre-treatment averages (Jan 2018–Nov 2022). PRS intensity = pre-treatment Category B transaction share (total Category B transactions / total transactions, Jan 2018–Nov 2022). "Border" = LA borders England. Prices to nearest £5K; transactions to nearest 5.

# F. Standardized Effect Sizes

**Table 15:** Standardized Effect Sizes for Main Outcomes

| Outcome | Specification | $\hat{\beta}$ | SD($X$) | SD($Y$) | SDE | Classification |
|---|---|---|---|---|---|---|
| Log transactions | DiD, Table 3 Col. 1 | −0.096 | — | 0.72 | −0.133 | Large negative |
| Log price | DiD, Table 4 Col. 5 | 0.081 | — | 0.56 | 0.145 | Large positive |
| Cat B share | DiD, Table 4 Col. 3 | 0.007 | — | 0.07 | 0.100 | Small positive |
| New-build share | DiD, Table 4 Col. 4 | 0.014 | — | 0.09 | 0.156 | Large positive |
| Flat share | DiD, Table 4 Col. 2 | 0.003 | — | 0.13 | 0.023 | Null |

*Notes:* This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. SDE $= \hat{\beta}/\mathrm{SD}(Y)$ since the treatment is binary. SD($Y$) is the unconditional standard deviation of the outcome variable from the analysis panel (before conditioning on fixed effects). **Research question:** Does abolishing no-fault evictions (Section 21) affect housing transactions, prices, and market composition? **Treatment:** Binary; 22 Welsh Local Authorities treated December 2022 (Renting Homes (Wales) Act), 339 English LAs as controls. **Data:** HM Land Registry Price Paid Data, January 2018 – December 2025, LA × month panel ($N = 32{,}184$). **Method:** Two-way fixed effects DiD with LA and month FE, standard errors clustered at LA level. **Sample:** All residential transactions in England and Wales at prices > £1,000. **Caveat:** The log transactions estimate is not robust to permutation inference ($p = 0.299$) or border-county restriction ($p = 0.353$). Placebo outcomes fail, suggesting the SDE for log transactions should not be interpreted as a causal effect of eviction reform.

Classification thresholds: large negative ($< -0.10$), small negative ($-0.10$ to $-0.05$), null ($-0.05$ to $0.05$), small positive (0.05 to 0.10), large positive ($> 0.10$). A reader unfamiliar with the paper should be able to interpret this table on its own.