Did Britain's Payday Lending Cap Raise Possession Claims? Evidence from English and Welsh Courts

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Abstract

Critics of Britain's 2015 payday-loan price cap argued that restricting high-cost short-term credit would push cash-constrained households into rent and mortgage arrears. I test that claim by linking local-authority-quarter possession filings from English and Welsh courts to regional high-cost short-term credit intensity from the Financial Conduct Authority. In a private-minus-mortgage difference-in-differences design, high-exposure regions do not experience a post-cap rise in private possession claims. The baseline estimate is negative ($\hat{\beta} = -0.0030$, SE $= 0.0010$), implying a 12.1% lower private-minus-mortgage differential across the observed exposure range, but the estimate weakens once region-specific trends are added and the permutation p-value is 0.204. Combined with one pre-trend blip and the fact that public regional exposure is only observed post-cap, the evidence supports a disciplined conclusion: the data do not show that the cap increased formal housing distress.

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1. Introduction

Payday lenders defend high-cost short-term credit by stressing liquidity rather than exploitation. The argument is straightforward: households facing a rent due date, an overdraft fee, or a missed utility payment may need a small loan immediately, and shutting that market may simply move distress elsewhere. The Financial Conduct Authority (FCA) itself reported that many payday borrowers used the product for everyday spending, including housing costs and bills (Financial Conduct Authority, 2014b). Because formal possession filings are among the most severe observable consequences of housing arrears, they provide a sharp test of whether payday credit meaningfully smoothed housing shocks.

Britain's January 2, 2015 cap on high-cost short-term credit is therefore a useful policy laboratory. The cap limited charges to 0.8 percent per day, capped default fees at £15, and prevented total repayment from exceeding 100 percent of principal (Financial Conduct Authority, 2014a). The FCA's own review concluded that the market contracted sharply after the cap and that revenues fell materially in the first post-cap period (Financial Conduct Authority, 2017). If payday loans were an important bridge for rent or mortgage payments, places that depended more heavily on such credit should have seen larger post-2015 increases in possession claims.

This paper studies that question using a new panel that combines Ministry of Justice possession-claim data with FCA regional lending data. The outcome panel covers 353 local authorities in England and Wales from 2003Q1 through 2019Q4 and distinguishes private-landlord claims from mortgage claims. The preferred specification compares the log difference between private and mortgage possession claims within the same local authority and quarter, then asks whether that differential changed more after the cap in regions where high-cost short-term credit remained more prevalent. The within-place private-versus-mortgage comparison is designed to absorb broad housing-market shocks that affect both tenants and owner-occupiers.

The main result is negative, not positive. In the baseline triple-difference style specification, the coefficient on regional high-cost short-term credit exposure interacted with the post-2015 period is -0.0030 with a clustered standard error of 0.0010. Across the observed regional exposure range, that corresponds to a 12.1% lower private-minus-mortgage claims differential after the cap. The landlord-only regression is also negative, while the mortgage-only regression is positive, though neither is individually precise. On its face, the pattern cuts against the view that payday credit was preventing a surge in formal housing distress.

That baseline estimate should not be overclaimed. Two facts force a more disciplined interpretation. First, treatment varies at the regional level, leaving only 10 effective clusters

in the estimation sample. Randomization-based inference therefore matters more than conventional clustered standard errors, and the permutation p-value is 0.204 rather than conventionally significant. Second, the publicly available FCA regional lending table is observed in July 2017 to June 2018, after the cap had already been in force for more than two years (Financial Conduct Authority, 2019). The exposure measure is thus a proxy for persistent regional payday-loan reliance rather than a truly pre-determined treatment dose. Public data do not reveal pre-cap regional volumes with comparable coverage. The paper therefore treats the estimates as suggestive evidence against a large positive effect, not as a definitive causal estimate of the cap's magnitude.

Even with those caveats, the result is informative. The baseline sign is consistently non-positive, the event-study coefficients do not show a post-cap jump in private claims, and the flexible-trend specification removes statistical significance without reversing the sign. If the core consumption-smoothing story were quantitatively large, this design should have detected at least a directional increase in landlord possession claims. It does not.

The paper contributes to three literatures. First, it extends work on payday-credit regulation from borrowing and financial distress outcomes to a concrete housing-distress margin (Zinman, 2010; Morse, 2011; Melzer, 2011; Bhutta et al., 2015a,b; Stegman, 2007). Second, it connects consumer-credit policy to the eviction and housing-insecurity literature, where formal court filings matter because they capture severe arrears and have persistent downstream consequences (Desmond, 2016; Ambrose et al., 2015; Humphries et al., 2023). Third, it adds a small-cluster common-shock difference-in-differences application in which conventional significance can be misleading without robustness to flexible trends and randomization inference (Bertrand et al., 2004; Conley and Taber, 2011; Cameron et al., 2008; Ferman and Pinto, 2019; MacKinnon et al., 2022; Roth et al., 2023; Rambachan and Roth, 2023).

The bottom line is narrower than the idea manifest that motivated the project. I do not find evidence that Britain's payday-loan cap increased possession claims in high-exposure areas. If anything, the estimates point the other way, though not robustly enough to support a strong claim of improvement. The safer conclusion is that the data do not support the argument that payday loans were playing a large housing-smoothing role on the formal possession margin.

2. Institutional Background and Policy Setting

The cap and the market. The FCA inherited consumer-credit regulation in 2014 and moved quickly to constrain the payday-loan market. Policy Statement 14/16 set three

headline rules for high-cost short-term credit: a daily price cap of 0.8 percent, a £15 ceiling on default fees, and a total-cost cap preventing repayment above twice principal (Financial Conduct Authority, 2014a). The rules took effect on January 2, 2015. These restrictions were intended to curb spiraling debt rather than eliminate emergency borrowing altogether, but the regulator anticipated large revenue losses and substantial market exit (Financial Conduct Authority, 2014b, 2017).

Why housing outcomes matter. Whether such a cap helps or harms borrowers depends on what payday credit was doing in practice. One view emphasizes liquidity insurance: high-cost loans may let households bridge short-run income shocks and avoid still costlier consequences (Morse, 2011; Stegman, 2007). Another view emphasizes debt amplification: repeat borrowing can worsen financial distress rather than relieve it (Melzer, 2011; Bhutta et al., 2015a). Housing payments are a natural place to evaluate the difference. Missing rent or mortgage payments can rapidly escalate into formal court action, and possession filings are precisely the sort of downstream hardship that defenders of payday credit claim the product helps avoid.

Possession claims as a formal distress margin. The Ministry of Justice publishes long-run quarterly statistics on mortgage and landlord possession actions in county courts. These data track claims, orders, warrants, and repossessions, separately for mortgage and landlord cases (Ministry of Justice, 2025). I focus on claims because they are the earliest formal stage and the broadest available local-authority series. For landlords, claims include both standard private-landlord claims and accelerated possession claims. In England's rental market, this is a meaningful outcome: the private rented sector expanded sharply over the 2000s and 2010s, and housing insecurity became a central policy concern well before the pandemic (Fitzpatrick et al., 2016; Rugg and Rhodes, 2018; Gibbons et al., 2021).

Why England and Wales. The exposure data are only available at broad FCA geographical regions, while the court data are available by local authority. England and Wales provide a coherent legal and statistical frame: they share the relevant possession-court system, and both appear in the FCA geographical table with usable regional matches. Scotland and Northern Ireland are dropped because the legal environment and possession institutions differ materially, while the treatment mapping is less comparable.

3. Data

3.1 Ministry of Justice possession claims

The outcome data come from the Ministry of Justice's statistical zip accompanying the mortgage and landlord possession bulletin (Ministry of Justice, 2025). The local-authority CSV reports quarterly possession actions by possession type, local authority, and region. I retain observations from 2003Q1 through 2019Q4, before the COVID-era eviction moratoria and court disruptions that would mechanically break any pre-post comparison. I restrict the sample to possession claims, exclude unidentified local-authority codes, and aggregate private-landlord claims together with accelerated-landlord claims. Mortgage claims are kept separately.

The resulting panel contains 353 local authorities observed over 68 quarters. Because the underlying court file records only local-authority-quarter cells with at least one claim in one of the two categories, the estimation sample is an unbalanced but dense panel with 23,568 local-authority-quarter observations.

3.2 FCA regional HCSTC intensity

Treatment intensity comes from the FCA's January 2019 consumer-credit data release, which reports high-cost short-term credit loans per 1,000 adults for each UK geographical area during July 2017 to June 2018 (Financial Conduct Authority, 2019). Within the England-and-Wales estimation sample, the highest-exposure region is the North West and the lowest is the South West, with a range of 43.0 loans per 1,000 adults. The regional ranking is intuitive: more loans per capita appear in the North of England and fewer in the South West, matching the FCA's descriptive account of the market.

This variable is the paper's most important limitation. Because the public regional table is observed after the January 2015 cap, it cannot be interpreted as a clean pre-cap treatment dose. I use it instead as a proxy for persistent regional reliance on high-cost short-term credit. The identifying assumption is therefore weaker than standard continuous-treatment DiD: regions that still had more payday borrowing after the reform are assumed to be the regions that were more exposed to the market before the reform as well. That assumption is plausible but unverified with public data.

3.3 Summary statistics

Table 1: Panel Overview and Exposure Variation

Statistic	Value
Local authorities	353
Regions	10
Quarter observations	68
Pre-treatment quarters	48
Mean private landlord claims (pre)	31.81
Mean mortgage claims (pre)	63.51
Mean HCSTC loans per 1,000 adults	99.22
SD HCSTC loans per 1,000 adults	15.49

Notes: The estimation sample covers 353 local authorities in England and Wales from 2003Q1 to 2019Q4. Treatment intensity is the FCA's published number of high-cost short-term credit loans per 1,000 adults in each region, measured over July 2017 to June 2018. The highest-exposure region in the England-and-Wales sample is the North West (125.0 loans per 1,000 adults) and the lowest is the South West (82.0).

Table 1 summarizes the estimation sample. Before the policy change, the average local-authority-quarter has 31.8 private-landlord claims and 63.5 mortgage claims. The means reflect very different raw levels across the two court series, which is why the preferred outcome is the log difference rather than the levels themselves. Exposure variation is modest but meaningful: the standard deviation of regional loans per 1,000 adults is 15.5, and the full England-and-Wales range is 43.0.

4. Empirical Strategy

4.1 Baseline specification

The preferred outcome is the log difference between private-landlord and mortgage possession claims:

$$\text{Diff}_{it} = \log(1 + \text{PrivateClaims}_{it}) - \log(1 + \text{MortgageClaims}_{it}). \tag{1}$$

I estimate

$$\text{Diff}_{it} = \alpha_i + \gamma_t + \beta \left(\text{HCSTCExposure}_{r(i)} \times \text{Post}_t \right) + \varepsilon_{it}, \tag{2}$$

where i indexes local authorities, t indexes quarters, α_i are local-authority fixed effects, γ_t are quarter fixed effects, and Post_t equals one from 2015 onward. The treatment variable is the FCA regional loans-per-1,000-adults measure, constant within region. Standard errors are clustered by region.

This is a stacked triple-difference style design. The national cap creates a common post-2015 shock. Cross-regional variation in payday-loan reliance provides treatment intensity. The private-minus-mortgage outcome differences out local conditions that affect both housing tenures, such as broad court congestion, local macroeconomic shocks, or generalized housing-market stress. Still, the identifying comparison is only as good as the assumption that private and mortgage claims would have evolved similarly across exposure levels absent the cap.

4.2 Supporting specifications

To clarify the differential result, I also estimate separate continuous-treatment DiD models for log private claims and log mortgage claims. In addition, I estimate an event-study version that interacts exposure with relative-year indicators and omits 2013 as the reference year:

$$\text{Diff}_{it} = \alpha_i + \gamma_t + \sum_{k \neq -1} \beta_k \left(\text{HCSTCExposure}_{r(i)} \times \mathbb{I}[\text{RelYear}_t = k] \right) + \varepsilon_{it}. \tag{3}$$

The event study is not a proof of identification, but it is a useful way to see whether the data show an obvious post-2015 jump or troubling pre-existing drift.

4.3 Threats to validity

Three threats are central.

Post-cap treatment measurement. The exposure measure is observed in 2017–18, not before January 2015. That opens the door to attenuation, reverse ranking changes, and post-treatment contamination. I therefore interpret the estimates as evidence about whether regions with persistently high payday-loan use experienced worse housing-distress dynamics after the reform, not as a cleanly identified causal dose-response parameter.

Few effective clusters. Treatment varies at the regional level, leaving only 10 clusters. Standard cluster-robust inference may over-reject in this setting (Conley and Taber, 2011; Cameron et al., 2008; MacKinnon et al., 2022). To address this, I report a permutation p-value based on 1,000 random reassignments of the regional exposure values and re-estimate the

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model with region-specific linear trends. In the discussion below, I treat these randomization-style and trend-robust checks as the decisive evidence rather than the baseline clustered p-value.

Concurrent policy and housing shocks. The private rented sector was affected by multiple policy and market changes during the 2010s, including welfare reforms, Universal Credit rollout, benefit constraints, and broader housing-market shifts (Fitzpatrick et al., 2016; Gibbons et al., 2021). The paper also stops before the 2019 tenant-fees ban becomes a major post-treatment complication. The private-minus-mortgage comparison is designed to remove shocks common to both landlord and mortgage possession claims, but it cannot eliminate renter-specific shocks whose timing or intensity covaries with regional payday exposure. That is another reason to treat the design as suggestive rather than definitive.

5. Results

5.1 Main estimates

Table 2: Main Difference-in-Differences Estimates

	Private − Mortgage	Private	Mortgage
HCSTC exposure × post-cap	-0.0030**	-0.0014	0.0016
	(0.0010)	(0.0008)	(0.0009)
Local-authority FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
Observations	23,568	23,568	23,568
R^2	0.8267	0.8824	0.9391

Notes: Column 1 reports the preferred private-minus-mortgage specification. Columns 2 and 3 report separate continuous-treatment difference-in-differences estimates for private landlord claims and mortgage claims. All models include local-authority and quarter fixed effects and cluster standard errors by region.

Table 2 reports the main regressions. Column (1), the preferred private-minus-mortgage specification, yields a coefficient of -0.0030 with a standard error of 0.0010. The sign is opposite the housing-smoothing prediction. Moving from the lowest- to the highest-exposure region corresponds to a 12.1% lower post-2015 private-minus-mortgage possession differential. Columns (2) and (3) show why. Private claims alone are weakly negative, while mortgage

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claims are modestly positive; the relative movement comes from both margins and is estimated more precisely in the within-place difference.

The main implication is not that the cap clearly reduced housing distress. The more defensible reading is weaker: the data do not show the increase in private possession claims that one would expect if payday loans were materially preventing court-level housing arrears. Even the raw sign cuts against that narrative. Because the randomization-based p-value is much less favorable than the clustered one, I treat the baseline estimate as descriptive rather than dispositive.

5.2 Event-study evidence

Table 3: Event-Study Coefficients for the Private-Minus-Mortgage Differential

Relative year	Coefficient	Standard error
$t-4$	-0.0006	(0.0033)
$t-3$	-0.0001	(0.0021)
$t-2$	0.0022***	(0.0003)
t	-0.0001	(0.0010)
$t+1$	-0.0016	(0.0012)
$t+2$	-0.0011	(0.0022)
$t+3$	-0.0020	(0.0052)
$t+4$	-0.0035	(0.0062)
$t+5$	-0.0038	(0.0075)

Notes: Coefficients come from a local-authority and quarter fixed-effects event study of the log difference between private landlord claims and mortgage claims. Exposure is interacted with annual relative time, with 2013 $(t-1)$ omitted as the reference year. Standard errors are clustered by region.

The event study also fails to support the consumption-smoothing story. Post-2015 coefficients are near zero or negative throughout the horizon. There is, however, one clear warning sign: the coefficient at $t-2$ is positive and statistically significant. That pre-trend blip is exactly the sort of diagnostic failure that argues against a triumphalist causal interpretation. Still, the event-study profile offers no evidence of a positive jump in private possession claims after

the cap.

5.3 Robustness and few-cluster inference

Table 4: Robustness and Few-Cluster Inference

Specification	Coefficient	Standard error	p-value
Baseline DDD	-0.0030**	(0.0010)	0.015
Region-specific trends	-0.0020	(0.0046)	0.678
Short window: 2010–2019	-0.0026	(0.0028)	0.381
Permutation p-value		0.204	
Leave-one-region-out range		[-0.0037, -0.0025]	

Notes: Every row reports the coefficient on HCSTC exposure interacted with the post-2015 period in the private-minus-mortgage specification. The permutation p-value is based on 1,000 random reassignment draws of the 10 regional exposure values. The leave-one-region-out range reports the minimum and maximum coefficient obtained when dropping one region at a time.

Table 4 shows why the paper's headline is a disciplined null rather than a negative-effect claim. The region-trend specification reduces the point estimate to about two-thirds of its baseline size and renders it highly insignificant. Restricting the sample to 2010–2019 also weakens precision. The permutation p-value is 0.204, so the baseline result is not unusually extreme relative to random reassignments of the region-level treatment vector. At the same time, the leave-one-region-out exercise keeps the coefficient negative in every case. The sign is stable; the inference is not.

Taken together, the results support a narrow conclusion. High-exposure regions do not show a post-2015 increase in private possession claims relative to mortgage claims. The data are inconsistent with a large positive housing-distress effect, but they are not sharp enough to justify a strong claim that the cap reduced possession risk.

6. Discussion

Why is the evidence so unsupportive of the housing-smoothing view? One possibility is substantive: payday loans may simply not have been an important enough margin for avoiding formal court action. Borrowers may have used the loans for other urgent expenses, or the

sums involved may have been too small and too short-term to prevent severe rent arrears (Bhutta et al., 2015a,b). Another possibility is substitution. Households denied payday credit might have adjusted through family loans, informal arrears, benefit timing, overdrafts, or other borrowing channels rather than immediately entering possession proceedings (Zinman, 2010; Morse, 2011). A third possibility is offsetting price and quantity effects: even if the cap reduced loan availability, it also lowered charges for remaining borrowers, potentially muting any effect on formal arrears.

Measurement problems also matter. The court data capture formal claims, not informal arrears, negotiated repayment plans, or unfiled landlord pressure. If payday credit mainly affected mild or short-lived arrears, a court-based outcome may miss the action. Likewise, the regional treatment proxy is noisy and post-treatment. That kind of measurement error would naturally attenuate any true effect toward zero and weaken the event-study design.

The broader lesson is methodological as much as substantive. With only 10 treatment clusters and no public pre-cap regional exposure series, a persuasive paper cannot lean on a single p-value. The correct standard is whether the data robustly support the headline claim. Here they do not support the claim that the cap increased formal housing distress. That alone is useful, because the burden of proof in this debate usually runs the other way.

7. Conclusion

Britain's payday-loan cap was supposed, by its critics, to create harmful spillovers for financially fragile households. This paper tests one of the starkest versions of that claim using local-authority possession filings from English and Welsh courts. The answer is negative: high-payday-exposure regions do not experience a post-cap increase in private possession claims relative to mortgage claims.

The design is not clean enough to pin down a precise causal effect. Public regional lending intensity is only observed after the reform, treatment varies at a coarse geographic level, and one event-study coefficient signals pre-trend noise. But even under those limitations, the data are inconsistent with the idea that payday lending was performing a large housing-smoothing role on the formal possession margin. Future work with lender-postcode data or branch-level pre-cap exposure could sharpen that conclusion. For now, the evidence supports a restrained verdict: Britain's payday cap did not produce an observable rise in formal housing distress.

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A						
Private minus mortgage claims	-0.0030	0.0010	0.8583	-0.0496	0.0165	Small negative
Private landlord claims	-0.0014	0.0008	1.0234	-0.0193	0.0107	Small negative
Mortgage claims	0.0016	0.0009	0.8541	0.0267	0.0155	Small positive
Panel B						
High-baseline private-claim LAs	-0.0052	0.0014	0.8944	-0.0825	0.0219	Moderate negative
Low-baseline private-claim LAs	-0.0043	0.0020	0.7337	-0.0836	0.0388	Moderate negative

Notes: **Country:** United Kingdom. **Research question:** Did the January 2015 UK high-cost short-term credit price cap increase housing distress, measured by possession claims, in higher-payday-exposure areas of England and Wales? **Policy mechanism:** The cap limited HCSTC interest and fees to 0.8% per day, imposed a £15 default-fee cap, and capped total repayment at 100% of principal, sharply compressing access to payday credit. If those loans had mainly smoothed short-run rent shocks, removing them should have raised landlord possession claims more than mortgage claims in high-exposure areas. **Outcome definition:** Quarterly local-authority possession claims from HM Courts and Tribunals Service, aggregated into private landlord claims (including accelerated claims), mortgage claims, and their log difference. **Treatment:** Continuous; FCA-reported HCSTC loans per 1,000 adults at the regional level, proxying persistent payday-market exposure. **Data:** Ministry of Justice local-authority possession claims, 2003Q1–2019Q4, merged to FCA regional HCSTC exposure from the July 2017–June 2018 Product Sales Data release; estimation sample contains 23,568 local-authority-quarter observations across 353 local authorities and 10 effective regions. **Method:** Continuous-treatment DiD with local-authority and quarter fixed effects; standard errors clustered by region; robustness adds region-specific trends and permutation inference. **Sample:** England and Wales only; observations with unidentified local authorities or unmatched region exposure are excluded. SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(X) is the cross-local-authority standard deviation of regional HCSTC exposure and SD(Y) is the pre-treatment standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).