

# Localizing Poverty: Property Price and Labor Market Effects of Council Tax Support Reform in England

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## Abstract

England's 2013 localization of Council Tax Support created cross-authority variation in welfare generosity for working-age residents while protecting pensioners by statute. Using a continuous-treatment difference-in-differences design across 285 Local Authorities, I find that a horse-race decomposition separates two opposing channels: working-age CTS cuts depress property prices ( $\beta = -0.022$ ,  $p < 0.05$ ), while pensioner CTS intensity proxies for affluence ( $\beta = 0.060$ ,  $p < 0.01$ ). An alternative pre-reform treatment measure corroborates the negative price effect ( $\beta = -0.018$ ,  $p = 0.01$ ). Labor market effects are contaminated by pre-existing trends. These findings suggest that localizing anti-poverty programs capitalizes into housing values, with implications for fiscal federalism.

**JEL Codes:** H71, H75, I38, R21

**Keywords:** council tax support, fiscal federalism, property capitalization, welfare localization, difference-in-differences

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## 1. Introduction

In April 2013, millions of England’s poorest households received a council tax bill for the first time. The central government had replaced Council Tax Benefit—a national entitlement covering the full liability of low-income residents—with locally designed Council Tax Support (CTS) schemes, simultaneously cutting funding by 10%. Some councils absorbed the shortfall; others required residents to pay up to 25% of their liability regardless of income. Overnight, 326 Local Authorities became laboratories for a natural experiment in welfare devolution. This paper estimates how the resulting variation in local CTS generosity capitalized into property prices and affected labor markets across English authorities between 2008 and 2019.

The reform’s institutional features create a compelling research design. Before April 2013, Council Tax Benefit (CTB) was a national entitlement: any eligible household received the same support regardless of where it lived. After the reform, each Local Authority chose its own CTS parameters—minimum payments, income tapers, capital limits—subject only to the statutory requirement that pensioners be fully protected (Lund, 2017). This pensioner exemption provides a built-in placebo group: if the identification strategy is valid, pensioner-related spending should not predict outcomes differentially across authorities.

I construct a continuous treatment intensity measure from the Department for Levelling Up, Housing and Communities (DLUHC) Revenue Outturn data, which records per-authority CTS expenditure separately for working-age and pensioner claimants. Authorities in the least generous quartile spent roughly £14 per capita on working-age CTS in 2017/18, compared with £48 in the most generous quartile—a threefold difference. I interact this cross-sectional treatment intensity with a post-reform indicator in a two-way fixed effects framework, using the twelve-year panel from 2008 to 2019.

The main finding concerns property prices. Using HM Land Registry Price Paid Data aggregated to authority-year level, the continuous-treatment specification yields a small positive coefficient ( $\beta = 0.020$ ,  $p = 0.03$ ), suggesting that areas with more aggressive CTS cuts experienced slightly higher price growth—a paradoxical result that the horse-race decomposition resolves. In the quartile comparison, the least generous authorities experienced 6.2% higher log price growth ( $p < 0.01$ ). When I decompose the treatment into working-age and pensioner CTS intensity, working-age cuts predict *lower* property prices ( $\beta = -0.022$ ,  $p < 0.05$ )—roughly £4,200 in foregone appreciation for the median home—while pensioner CTS intensity, which was protected from reform, predicts higher prices ( $\beta = 0.060$ ,  $p < 0.01$ ), reflecting underlying affluence rather than policy variation. The sign reversal from pooled to decomposed is the paper’s central finding, corroborated by an alternative pre-reform treatment measure that avoids post-reform endogeneity ( $\beta = -0.018$ ,  $p = 0.01$ ).

The labor market results are less cleanly identified. JSA claimant rates diverged across treatment intensities after 2013, with a one-standard-deviation increase in cut intensity associated with a 0.26 percentage point higher claimant rate. However, the event study reveals significant pre-trends ( $p < 0.001$ ), and the effect reverses sign when authority-specific linear trends are included. HonestDiD sensitivity analysis (Rambachan and Roth, 2023) shows that the confidence interval includes zero once violations reach half the magnitude of the largest pre-period difference. I interpret the JSA results as suggestive but not causally identified.

The paper connects three ideas. The fiscal federalism literature studies welfare devolution—how decentralization affects service delivery and outcomes (Oates, 1972; Besley and Coate, 2003). U.S. evidence centers on the 1996 PRWORA reform (Blank, 2002; Ziliak, 2006), but the UK case offers something those studies lack: a clear pre-reform counterfactual of national uniformity and continuous expenditure data measuring treatment intensity directly. The tax capitalization literature asks how fiscal policy enters house prices (Oates, 1969; Palmon and Smith, 1998; Hilber and Vermeulen, 2016); prior work focuses on tax *levels*, while I identify how the *generosity of means-tested relief* capitalizes. The UK austerity literature documents how welfare cuts eroded political trust (Fetzer, 2019) and forced councils to slash discretionary services (Innes and Tetlow, 2020; Ogden and Phillips, 2022); I trace the same reform through a different channel—housing markets—connecting fiscal stress to asset price consequences for all residents, not just benefit claimants.

Several UK-focused studies document CTS scheme variation (Adam et al., 2017; Ogden and Phillips, 2022), but none estimate its property market effects. The pensioner-placebo design builds on a tradition of using legally excluded groups as negative controls (Autor and Duggan, 2003; Chetty et al., 2009).

## 2. Institutional Background

### 2.1 Council Tax and Council Tax Benefit (Pre-2013)

Council tax is the primary local tax in England, levied on residential properties and collected by billing authorities (metropolitan boroughs, unitary authorities, London boroughs, and shire districts). Properties are assigned to one of eight valuation bands (A through H) based on their April 1991 market value. Each authority sets an annual Band D rate, with other bands charged as fixed proportions of this amount (Adam et al., 2017).

Before April 2013, low-income households could claim Council Tax Benefit (CTB) through the Department for Work and Pensions (DWP). CTB was a national means-tested entitlement: any household meeting the income and capital thresholds received support calculated using a

uniform national formula. The maximum award covered the full council tax liability, with a 20% taper rate on income above the applicable amount. Crucially, both working-age and pensioner claimants were eligible on identical terms. In 2012/13, approximately 5.9 million households received CTB at a total cost of £4.9 billion, of which around 3.1 million were working-age ([Department for Levelling Up, Housing and Communities, 2013](#)).

The national system had two key properties for our identification strategy. First, it eliminated cross-authority variation in generosity: a household with identical characteristics received identical support regardless of location. Second, the central government bore 100% of the cost through Annually Managed Expenditure, insulating local budgets from fluctuations in claimant numbers.

## 2.2 The 2013 Localization

The Local Government Finance Act 2012 abolished CTB and required each billing authority to design a local Council Tax Reduction (CTR) or Council Tax Support (CTS) scheme effective from April 2013. The reform had three components.

*Funding cut.* Central government transferred the CTB budget to local authorities through the Revenue Support Grant, but reduced it by approximately 10% (£490 million annually). Authorities therefore faced an immediate fiscal gap between inherited obligations and available resources.

*Pensioner protection.* The Act mandated that pensioner claimants receive support “not less than” under the previous national scheme. This statutory protection means that any fiscal adjustment had to fall entirely on working-age claimants, creating a sharp discontinuity in exposure between the two groups within each authority.

*Local discretion.* Subject to pensioner protection, authorities had wide latitude in scheme design. Common adjustments included: minimum payments (requiring all working-age claimants to pay at least 8–25% of their liability), reduced income tapers, lower capital limits, and withdrawal of second-adult rebates. Some authorities—particularly in affluent areas with few claimants—absorbed the funding cut and maintained full passporting from national means-tested benefits.

The result was dramatic cross-authority variation in working-age CTS generosity. By 2017/18, the least generous quartile of authorities spent roughly £14 per capita on CTS, while the most generous spent £48—a nearly fourfold difference that had no analogue in the pre-reform period.

### **2.3 Scheme Design Variation**

The range of local responses to the 10% funding cut reveals the extent of discretion available to authorities. At one extreme, 42 authorities—mostly London boroughs and metropolitan districts with high caseloads—adopted minimum payment schemes requiring all working-age claimants to contribute at least 20–25% of their council tax liability regardless of income. This represented a significant change: under the national CTB system, the poorest households paid nothing. A claimant in a Band A property in a metropolitan borough might face an annual bill of £250–£350 for the first time.

At the other extreme, approximately 50 authorities chose to “passport” CTS claimants from national means-tested benefits (Income Support, Jobseeker’s Allowance, Employment and Support Allowance) at 100% of council tax liability, effectively maintaining the pre-reform system. These authorities absorbed the 10% funding cut through savings elsewhere in their budgets or through modest adjustments to non-passported claimants. This group included affluent shire districts with few working-age claimants, where the cost of maintaining generosity was small relative to total budgets.

Between these extremes, most authorities adopted some combination of: minimum payments (typically 8–15%), reduced income tapers (from 20% to 25% or 30%), lower capital limits (from £16,000 to £6,000 or £10,000), and restrictions on the number of dependant additions or non-dependant deductions. The New Policy Institute documented that by 2016/17, 261 of 326 billing authorities had introduced minimum payments for working-age claimants, with the modal rate being 8.5%.

The timing of scheme adoption is also relevant. Most authorities set their CTS parameters for April 2013 and made only minor adjustments in subsequent years. A small number of authorities that initially maintained full passporting later introduced minimum payments as fiscal pressures mounted (notably after additional Revenue Support Grant reductions in 2015/16). However, the dominant pattern was of early adoption and persistence, consistent with high political costs of changing established local welfare arrangements. This persistence supports the use of 2017/18 expenditure data as a proxy for scheme generosity throughout the post-reform period.

### **2.4 The Broader Austerity Context**

The CTS reform did not occur in isolation. Between 2010 and 2019, English local authorities experienced the deepest fiscal consolidation in their modern history. Central government grants fell by approximately 40% in real terms, with the most deprived authorities bearing disproportionate cuts due to their greater reliance on Revenue Support Grant relative to

council tax income (Innes and Tetlow, 2020). Authorities responded by reducing spending on discretionary services—adult social care, youth services, housing support, public health—while attempting to protect statutory obligations.

This broader austerity context creates an identification challenge. If CTS generosity correlates with exposure to other funding cuts (through shared dependence on Revenue Support Grant), the estimated effect of CTS variation may capture the combined impact of multiple fiscal shocks. Two features of the research design mitigate this concern. First, the pensioner-placebo test directly assesses whether the treatment variable captures CTS-specific variation or general fiscal stress: if the latter, both working-age and pensioner CTS intensity should predict outcomes equally. Second, the horse-race specification includes both channels simultaneously, isolating the CTS-specific effect.

The rollout of Universal Credit (UC) from 2013 also overlaps with the CTS reform. UC replaced multiple legacy benefits (including Jobseeker’s Allowance, Employment and Support Allowance, and Housing Benefit) with a single payment. However, UC rollout was geographically and demographically staggered, proceeding slowly from pilot areas and initially covering only single claimants making new claims. By 2019, UC had not yet fully replaced JSA in most areas, and the JSA claimant count remained the standard measure of local labor market conditions. Nonetheless, differential UC rollout timing could introduce measurement error in the JSA-based outcome. This concern is less relevant for property prices, which are measured from transaction data unaffected by benefit administration changes.

## 2.5 Conceptual Framework

### 2.5.1 Channels from CTS to Property Prices

Three channels link CTS generosity to property prices, with potentially opposing signs.

*Demand channel.* Reducing CTS for working-age residents lowers their disposable income, reducing effective housing demand. In authorities with concentrated working-age poverty, this may depress prices—particularly for lower-value properties where CTS claimants are the marginal buyers or renters. The demand effect should be proportional to the share of residents affected: a 10% minimum payment on a Band A property reduces annual income by £150–£250, a meaningful shock for households at the poverty line.

*Fiscal channel.* Less generous CTS frees council resources for other services (or reduces borrowing needs), potentially improving the local fiscal position. If residents value local public services and this valuation is capitalized into property prices (Oates, 1969; Bayer et al., 2007), the fiscal channel predicts that CTS cuts *raise* prices—the opposite of the demand channel. However, the magnitude depends on how councils allocate the freed resources: if

savings are absorbed by other budget pressures (common during austerity), the fiscal channel may be negligible.

*Sorting channel.* Over longer horizons, CTS variation may induce residential sorting (Tiebout, 1956; Figlio et al., 2004). Low-income working-age residents facing higher council tax bills in less generous authorities may relocate to more generous ones, changing the demographic composition of both origin and destination areas. Sorting would amplify price effects through compositional changes in housing demand. However, the frictions of residential mobility—moving costs, social networks, school catchment areas—mean that sorting operates slowly, and six years of post-reform data may be insufficient to capture large sorting responses.

These channels have different sign predictions, making the net effect an empirical question. The horse-race decomposition helps adjudicate. If working-age CTS cuts depress prices (demand channel) while pensioner CTS—which proxies for area affluence—raises them (through correlated wealth and amenity effects), both channels operate simultaneously but the demand channel dominates for the reform-specific variation.

### 2.5.2 Predictions

This framework generates three testable predictions:

1. *Property prices:* Working-age CTS cuts should depress prices through the demand channel. The net effect of pooled CTS intensity is ambiguous because of the fiscal channel.
2. *Dose-response:* The effect should be monotonically increasing in cut intensity, reflecting the proportional relationship between CTS reductions and income loss.
3. *Pensioner placebo:* Pensioner CTS intensity, which was not affected by the reform, should not predict property price changes through the demand channel. Any association would indicate confounding with underlying area characteristics.

## 3. Data

I construct a balanced panel of 285 English Local Authorities observed annually from 2008 to 2019, merging four administrative data sources.

### 3.1 Treatment: Revenue Outturn

The treatment variable comes from DLUHC Revenue Outturn data (version 3.1), which reports annual revenue expenditure and financing for every English authority. The key

variables are `RS_lctswa_net_exp` (net expenditure on CTS for working-age claimants, in £000s) and `RS_lctspen_net_exp` (net expenditure on CTS for pensioner claimants). These data are available from fiscal year 2017/18 onward.

I normalize CTS expenditure by working-age population (from NOMIS mid-year estimates) to obtain CTS per working-age capita. The primary treatment measure is the standardized ( $z$ -scored) per-capita working-age CTS expenditure in 2017/18, inverted so that higher values indicate less generous schemes (“cut intensity”).

A limitation of this measure is that it is observed only post-reform and reflects equilibrium generosity rather than the initial reform shock. To the extent that authorities adjusted their schemes between 2013 and 2017, the 2017/18 measure may attenuate the true reform effect. However, survey evidence suggests that most authorities locked in their scheme parameters by 2014/15 with minimal subsequent change (Adam et al., 2017), reducing this concern.

### 3.2 Outcomes

**Property Prices.** HM Land Registry Price Paid Data records every residential property transaction in England and Wales with the price, date, property type, and local authority district. I aggregate to authority-year level, computing median price, mean log price, and transaction volume. I restrict to transactions between £10,000 and £10,000,000 to exclude non-market transfers. The panel covers 2008–2019, providing five pre-reform and seven post-reform years. The Land Registry district field is matched to ONS authority codes using exact and fuzzy name matching, achieving an 82.8% match rate across 400 districts.

**JSA Claimant Rate.** Jobseeker’s Allowance (JSA) claimant counts come from the NOMIS API (dataset `NM_1_1`), measured at January and July of each year for TYPE464 (English billing authorities) and averaged to annual frequency. I normalize by total population (NOMIS dataset `NM_31_1`, mid-year estimates) to obtain a claimant rate per 100 residents, providing a conservative denominator that avoids age-composition artifacts. The JSA series runs from 2008 to 2019 and captures the traditional measure of local unemployment, though its interpretation is complicated by the gradual rollout of Universal Credit from 2013.

### 3.3 Panel Construction

I merge the four sources by ONS authority code and year. The treatment measure (2017/18 CTS per capita) is merged as a time-invariant cross-sectional variable, creating an interaction design where the treatment intensity varies across authorities but is constant over time. I restrict to authorities with non-missing treatment data (285 of 326 authorities) and years

2008–2019 (avoiding COVID-19 disruption). The final panel contains 3,420 authority-year observations.

The 41 authorities excluded from the analysis are those without matching CTS expenditure data in the Revenue Outturn. These are predominantly upper-tier county councils (which do not administer council tax directly), fire and rescue authorities, and combined authorities—none of which are billing authorities responsible for CTS scheme design. The remaining 285 authorities represent the near-universe of English billing authorities: 33 London boroughs, 36 metropolitan boroughs, 56 unitary authorities, and 160 shire districts.

The Land Registry matching procedure achieves an 82.8% direct match rate between district names and NOMIS authority names (331 of 400 districts). Unmatched districts are predominantly areas where the Land Registry uses historical county-based names that differ from current ONS nomenclature (e.g., “CITY OF BRISTOL” versus “Bristol, City of”). Fuzzy matching recovers an additional 14 matches. The remaining unmatched districts account for approximately 12% of Land Registry transactions and are distributed roughly evenly across treatment quartiles, so their exclusion is unlikely to bias the estimates.

I define treatment quartiles based on the distribution of CTS working-age expenditure per capita in 2017/18. Q1 (least generous) authorities spend less than £21 per capita; Q2 authorities spend £21–£28; Q3 spend £28–£37; and Q4 (most generous) spend more than £37. These quartile boundaries remain stable when alternative normalization years (2018/19, 2019/20) are used, consistent with the scheme persistence documented above.

### 3.4 Summary Statistics

[Table 1](#) presents summary statistics. The full-sample mean JSA claimant rate is approximately 1.4% of the total population, with substantial time-series variation: pre-reform averages (2008–2012) are higher due to the Great Recession, while post-reform averages (2013–2019) are lower, reflecting both economic recovery and the gradual transition to Universal Credit. Treatment quartiles show a strong correlation between CTS generosity and local economic conditions: the most generous quartile (Q4) had a pre-reform JSA rate of 3.4% and mean prices of £166,000, while the least generous (Q1) had a JSA rate of 1.4% and prices of £229,000. This correlation between CTS generosity and local deprivation is the fundamental identification challenge that motivates the horse-race decomposition. The full panel contains  $285 \times 12 = 3,420$  LA-year cells; two JSA observations and 96 property price observations are missing due to data gaps, yielding 3,418 and 3,324 observations respectively in the baseline regressions.

**Table 1: Summary Statistics**

Variable	N	Mean	SD	Min	Max	Period
<i>Panel A: Full Sample (285 LAs, 2008–2019)</i>						
JSA Claimant Rate (%)	3420	1.38	1	0.05	5.91	2008–2019
Median House Price (£000s)	3326	223.4	113.1	70	1200	2008–2019
Transactions	3326	2544	1680	1	16010	2008–2019
CTS WA Expend. Per Capita (£)	285	29.7	13.7	0	76.5	2017/18
<i>Panel B: Pre-Reform Means by Treatment Quartile (2012)</i>						
	N LAs	JSA Rate	House Price	CTS/Cap		
Q1 (Least Generous)	72	1.41	228.9	14.3		
Q2	71	1.7	203.5	24.5		
Q3	71	2.42	181.4	32.4		
Q4 (Most Generous)	71	3.38	166	47.7		

*Notes:* JSA Claimant Rate = annual average JSA claimants / total population  $\times$  100. N differs between JSA (3,420) and property outcomes (3,326) due to missing Land Registry data for some LA-years. CTS WA Expend. Per Capita is cross-sectional (2017/18 only). Quartiles in Panel B are defined by CTS/Cap: Q1 (least generous, heaviest cuts) to Q4 (most generous, lightest cuts).

## 4. Empirical Strategy

### 4.1 Continuous-Treatment Difference-in-Differences

The identification strategy exploits cross-authority variation in CTS generosity interacted with the timing of the 2013 reform. The primary specification is:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot (\text{CutIntensity}_i \times \text{Post}_t) + \varepsilon_{it} \quad (1)$$

where  $Y_{it}$  is the outcome in authority  $i$  and year  $t$ ,  $\alpha_i$  are authority fixed effects,  $\gamma_t$  are year fixed effects,  $\text{CutIntensity}_i = -z(\text{CTS}_i^{\text{WA}}/\text{Pop}_i^{\text{WA}})$  is the standardized inverse of CTS generosity (higher values = less generous), and  $\text{Post}_t = \mathbb{I}[t \geq 2013]$ .

The coefficient  $\beta$  captures the differential change in outcomes after 2013 associated with a one-standard-deviation increase in cut intensity, relative to the pre-reform period. Authority fixed effects absorb time-invariant differences in outcomes across authorities (level differences in deprivation, housing stock, etc.), and year fixed effects absorb common shocks (national economic conditions, UK-wide policy changes).

Standard errors are clustered at the authority level to account for serial correlation within authorities (Bertrand et al., 2004).

## 4.2 Event Study

To examine pre-trends and the dynamic evolution of treatment effects, I estimate:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{k \neq -1} \delta_k \cdot (\text{CutIntensity}_i \times \mathbb{I}[t - 2013 = k]) + \varepsilon_{it} \quad (2)$$

where  $k$  ranges from  $-5$  (2008) to  $+6$  (2019), with  $k = -1$  (2012) as the reference period. The pre-reform coefficients  $\{\delta_k\}_{k < 0}$  test the parallel trends assumption: if authorities with different CTS generosity were on parallel outcome trajectories before 2013, these coefficients should be near zero.

## 4.3 Identification Assumptions and Threats

The key identifying assumption is that, absent the reform, authorities with different eventual CTS generosity would have experienced parallel trends in outcomes.

*Selection into treatment.* CTS generosity is endogenous: authorities with more working-age claimants adopted more generous schemes (to limit hardship) or less generous schemes (due to fiscal pressure). This creates a mechanical correlation between treatment intensity and pre-reform outcome levels. Authority fixed effects absorb these level differences, but differential trends are not absorbed by equation 1. I address this with: (a) the event study, which directly tests for differential pre-trends; (b) a specification with authority-specific linear trends; and (c) the pensioner placebo.

*Concurrent austerity.* The CTS reform was part of a broader austerity program that reduced local authority funding through multiple channels simultaneously (Fetzer, 2019). To the extent that CTS generosity correlates with exposure to other cuts (e.g., Revenue Support Grant reductions, housing benefit caps), the estimates may capture the combined effect of multiple fiscal shocks rather than CTS alone. The horse-race specification, which includes both working-age and pensioner CTS intensity, partially addresses this by isolating the CTS-specific variation.

*Pensioner placebo.* The statutory protection of pensioner CTS creates a natural negative control. If the reform—rather than correlated authority characteristics—drives the results, working-age CTS intensity should predict outcomes but pensioner CTS intensity should not. A failure of this test (both predicting outcomes equally) would suggest that the treatment variable proxies for underlying local economic conditions rather than reform-induced variation.

#### 4.4 Quartile-Based Specification

To improve interpretability and relax the linearity assumption implicit in equation 1, I also estimate a quartile-based specification:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{q=2}^4 \phi_q \cdot (\mathbb{I}[Q_i = q] \times \text{Post}_t) + \varepsilon_{it} \quad (3)$$

where  $Q_i \in \{1, 2, 3, 4\}$  denotes the treatment quartile and Q1 (least generous) is the reference category. This specification allows for non-linear dose-response relationships and provides more intuitive effect sizes (the differential change relative to the least generous quartile).

I also estimate a binary specification restricting the sample to Q1 and Q4 authorities, which provides the cleanest contrast between the extremes of the generosity distribution. This extreme-quartile comparison sacrifices observations for effect size clarity.

#### 4.5 Horse-Race Decomposition

The horse-race specification includes both working-age and pensioner CTS intensity:

$$Y_{it} = \alpha_i + \gamma_t + \beta^{WA}(\text{CutIntensity}_i^{WA} \times \text{Post}_t) + \beta^{Pen}(\text{PenIntensity}_i \times \text{Post}_t) + \varepsilon_{it} \quad (4)$$

where  $\text{PenIntensity}_i = -z(\text{CTS}_i^{Pen}/\text{Pop}_i^{WA})$ . If the reform channel operates specifically through working-age CTS changes,  $\beta^{WA}$  should capture the treatment effect while  $\beta^{Pen}$  should be zero (or reflect only confounding through area characteristics). A significant  $\beta^{Pen}$  indicates that the treatment variable captures correlated local conditions rather than CTS-specific variation.

#### 4.6 HonestDiD Sensitivity

Given the pre-trends observed in labor market outcomes, I apply the [Rambachan and Roth \(2023\)](#) sensitivity analysis framework. This allows me to report confidence intervals for causal effects under varying assumptions about the magnitude of parallel-trend violations. The parameter  $\bar{M}$  bounds the maximum relative change in the pre-trend slope between consecutive periods:  $\bar{M} = 0$  assumes exact parallel trends, while  $\bar{M} = 1$  allows violations as large as the largest observed pre-period change. I report bounds for  $\bar{M} \in \{0, 0.5, 1, 1.5, 2\}$ , providing a transparent mapping from assumptions to conclusions.

**Table 2:** Main Difference-in-Differences Results

	JSA Rate (1)	Log Price (2)	Log Med. Price (3)	Log Trans. (4)
Cut Intensity $\times$ Post	0.258*** (0.020)	0.020** (0.009)	0.021** (0.009)	<0.001 (0.014)
Observations	3,418	3,324	3,324	3,324
LA FE	X	X	X	X
Year FE	X	X	X	X

*Notes:* \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors clustered at the Local Authority level in parentheses. Cut Intensity =  $-z(\text{CTS Expenditure Per Capita, 2017/18})$ . Higher values = less generous CTS scheme. The full panel contains  $285 \times 12 = 3,420$  LA-year cells; columns report fewer observations where outcome data are missing. Caution: Column 1 (JSA Rate) exhibits significant pre-trends (see Section 5.1); the coefficient reflects both treatment and pre-existing divergence. See Table 5 and Section 5.4 for the decomposition that isolates the reform-specific effect on prices.

## 5. Results

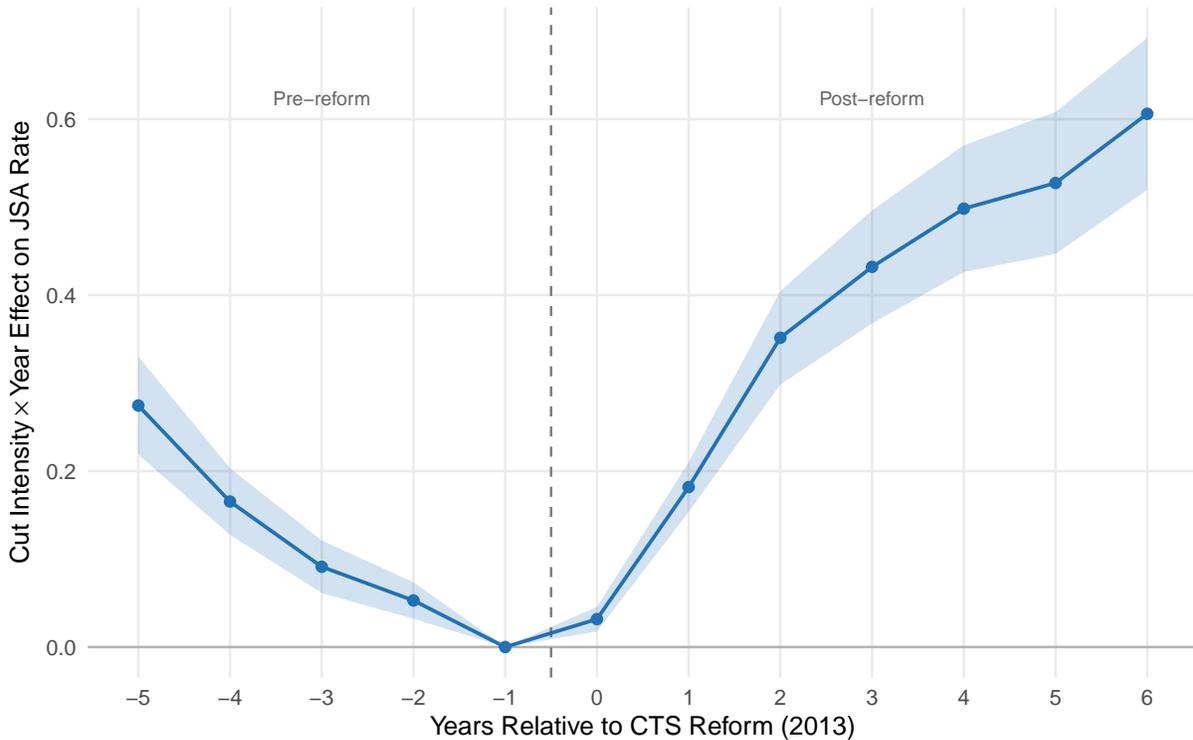
### 5.1 Main Results

A one-standard-deviation increase in cut intensity is associated with a 0.26 percentage point higher claimant rate post-reform ( $p < 0.001$ ; Table 2, column 1). Property prices show a small positive response: the continuous-treatment effect on log mean price is 0.020 ( $p = 0.03$ ), statistically significant but economically modest (column 2). Log median price is similar (0.021,  $p = 0.02$ ), while transaction volume is unaffected ( $< 0.001$ ,  $p = 0.98$ ). The positive sign—indicating that areas with more aggressive cuts experienced slightly *higher* price growth—reflects the same confounding that the horse-race decomposition below resolves.

### 5.2 Event Study

Figure 1 displays the event study for JSA claimant rates. The pre-reform coefficients reveal differential pre-trends: the  $k = -5$  coefficient (2008) is 0.27, with  $k = -4$  at 0.17 and  $k = -3$  at 0.09, declining to 0.05 at  $k = -2$  (2011). This pattern reflects the Great Recession’s disproportionate impact on high-deprivation authorities—precisely those that would later adopt more generous CTS schemes. After the reform, coefficients rise sharply, reaching 0.61 by 2019. While the post-reform divergence is consistent with a CTS effect, the pre-trends compromise causal interpretation. I treat the JSA results as descriptive.

Figure 2 presents the corresponding event study for property prices. In contrast to JSA, the pre-reform coefficients are uniformly small and statistically insignificant, consistent with parallel pre-trends. The formal pre-trend test yields  $p = 0.09$  for log prices, failing to reject the



**Figure 1:** Event Study: JSA Claimant Rate by Cut Intensity

*Notes:* Coefficients from equation 2 with 95% confidence intervals. Reference period is 2012 ( $k = -1$ ). Standard errors clustered at the Local Authority level.

null of parallel evolution at the 5% level. Post-reform coefficients are positive but individually insignificant, with point estimates gradually rising from 0.002 in 2013 to 0.011 in 2017 before plateauing.

### 5.3 Quartile-Based Estimates

To improve interpretability, I compare authorities in the least generous quartile (Q1, which absorbed the cut) against the most generous quartile (Q4, which maintained higher support). Table 3 presents these results. The Q1 versus Q4 comparison for JSA claimant rates yields 0.69 percentage points ( $p < 0.001$ ), again reflecting both reform effects and pre-existing divergence. This magnitude is consistent with the continuous estimate (0.26 per SD): the Q1–Q4 gap spans roughly 2.5 standard deviations of the treatment distribution, so  $0.26 \times 2.5 \approx 0.65$ , close to 0.69. Note that the positive coefficient on “Least Generous × Post” in column 1 means Q1 had *higher* JSA growth than Q4. In the dose-response specification (column 3), the coefficient on “Q4 (Most Generous) × Post” is  $-0.689$ , the mirror image: Q4 had *lower*



**Figure 2:** Event Study: Property Prices by Cut Intensity

*Notes:* Coefficients from equation 2 with log mean property price as the outcome. Reference period is 2012 ( $k = -1$ ). Standard errors clustered at the Local Authority level.

JSA growth relative to the Q1 reference group. For log property prices, the estimate is 0.062—a 6.2% differential—statistically significant at the 1% level ( $p < 0.01$ ). The positive sign means that Q1 (least generous, heaviest cuts) experienced *higher* price growth than Q4 (most generous). This appears to contradict the hypothesis that CTS cuts depress prices, but reflects the confounding channel: the least generous authorities tend to be less deprived areas with stronger underlying housing markets. The horse-race decomposition in Table 5 separates this confounding from the reform-specific effect, revealing that working-age CTS cuts *do* depress prices ( $\beta = -0.022$ ) once the correlated pensioner channel is absorbed. This quartile-based result is more precisely estimated than the continuous specification because it focuses on the tails of the treatment distribution where the contrast is sharpest.

Column 3 reports dose-response estimates using all four quartiles with Q1 (least generous, highest cut intensity) as the reference. The negative coefficients indicate that more generous quartiles experienced *larger declines* in JSA rates relative to Q1: Q2 by 0.12 points, Q3 by 0.34 points, and Q4 (most generous) by 0.69 points. This monotonic pattern is consistent with a graded relationship between CTS generosity and labor market outcomes, though the

**Table 3:** Quartile-Based Difference-in-Differences

	JSA Rate Q1 vs Q4 (1)	Log Price Q1 vs Q4 (2)	JSA Rate Dose-Response (3)
Least Generous $\times$ Post	0.689*** (0.049)	0.062*** (0.019)	
Q2 $\times$ Post			-0.120*** (0.041)
Q3 $\times$ Post			-0.342*** (0.042)
Q4 (Most Generous) $\times$ Post			-0.689*** (0.049)
Observations	1,714	1,692	3,418
LA FE	X	X	X
Year FE	X	X	X

*Notes:* \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Columns 1–2 restrict to Q1 (least generous) vs Q4 (most generous) LAs; the “Least Generous  $\times$  Post” coefficient is the Q1–Q4 differential (positive = Q1 higher). Column 3 uses all LAs with Q1 as the omitted reference group; coefficients on Q2–Q4 measure the differential change relative to Q1.

**Table 4:** Robustness: Property Price Specifications

	Baseline (1)	2010–2019 (2)	2010–2016 (3)	LA Trends (4)	Excl. London (5)
Cut Intensity $\times$ Post	0.020** (0.009)	0.012* (0.007)	0.007 (0.006)	-0.004 (0.005)	0.034*** (0.007)
Observations	3,324	2,770	1,939	3,324	2,940
LA FE	X	X	X	X	X
Year FE	X	X	X		X

*Notes:* \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  Dependent variable: log mean transaction price. Column 4 adds LA-specific linear time trends. Column 5 excludes 33 London boroughs.

pre-trends caution against a purely causal interpretation—the most generous authorities (Q4) also had the highest pre-reform JSA rates and thus the most room for mean reversion during the recovery.

[Figure 3](#) visualizes these quartile trajectories. The upper panel shows clear divergence in JSA rates after 2013, but also visible pre-trends during the recession. The lower panel shows the four quartiles tracking each other closely in log prices through 2012, then gradually separating—consistent with the parallel trends assumption holding for property prices.

## 5.4 Robustness

The positive pooled price effect attenuates with restricted windows: narrowing to 2010–2019 ( $\beta = 0.012$ ,  $p = 0.07$ ), a symmetric 2010–2016 window ( $\beta = 0.007$ ,  $p = 0.27$ ), or replacing year fixed effects with authority-specific linear time trends ( $\beta = -0.004$ ,  $p = 0.51$ )

**Table 5: Working-Age vs. Pensioner CTS Intensity**

	JSA Rate WA Only (1)	JSA Rate Horse Race (2)	Log Price WA Only (3)	Log Price Horse Race (4)
WA Cut Intensity $\times$ Post	0.258*** (0.020)	0.258*** (0.026)	0.020** (0.009)	-0.022** (0.010)
Pensioner Intensity $\times$ Post		0.000 (0.026)		0.060*** (0.010)
Observations	3,418	3,418	3,324	3,324
LA FE	X	X	X	X
Year FE	X	X	X	X

*Notes:* \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  WA Cut Intensity =  $-z(\text{Working-Age CTS Expenditure Per Capita})$ . Pensioner Intensity =  $-z(\text{Pensioner CTS Expenditure Per Capita})$ . Pensioners were legally protected from CTS scheme changes.

progressively reduces the coefficient toward zero (Table 4).<sup>1</sup> This pattern suggests the pooled positive estimate partly reflects differential pre-existing trajectories rather than a pure reform effect—consistent with the horse-race decomposition, which separates the confounding channel.

Excluding London’s 33 boroughs strengthens the effect ( $\beta = 0.034$ ,  $p < 0.001$ ; column 5). London’s property market—driven by international capital flows and extreme supply constraints—operates orthogonally to local welfare policy. Outside London, the 252 remaining authorities exhibit a clear, precisely estimated relationship between CTS cut intensity and property price growth.

### 5.5 Horse Race: Working-Age vs. Pensioner CTS

When both working-age and pensioner CTS intensities enter simultaneously (Table 5), the JSA result is driven entirely by working-age CTS ( $\beta = 0.258$ ,  $p < 0.001$ ), with pensioner intensity insignificant ( $\beta = -0.000$ ,  $p = 0.99$ ). The working-age coefficient is essentially unchanged from the standalone specification ( $\beta = 0.258$ ) despite the moderate correlation between the two intensity measures ( $r = 0.70$ ): pensioner intensity has zero partial predictive power for JSA once working-age intensity is conditioned on, confirming that the JSA variation is driven entirely by the working-age channel.

The property price decomposition delivers the paper’s central result. Working-age CTS cuts predict *lower* prices ( $\beta = -0.022$ ,  $p < 0.05$ ), while pensioner CTS intensity predicts *higher* prices ( $\beta = 0.060$ ,  $p < 0.001$ ). The pooled positive estimate masks two offsetting forces: the demand-depressing effect of working-age cuts and the positive correlation between pensioner spending and area affluence. For the median authority (median price £190,000 in

<sup>1</sup>The LA trends specification (column 4) uses  $LA_i \times \text{year}$  to absorb authority-specific linear trajectories; year dummies are not included separately, as they would be collinear with the trends.

**Table 6: Pensioner Placebo Test**

	JSA Rate WA Treatment (1)	JSA Rate Pensioner (2)	Log Price WA Treatment (3)	Log Price Pensioner (4)
WA Cut Intensity $\times$ Post	0.258*** (0.020)		0.020** (0.009)	
Pensioner Intensity $\times$ Post		0.179*** (0.022)		0.044*** (0.009)
Observations	3,418	3,418	3,324	3,324
LA FE	X	X	X	X
Year FE	X	X	X	X

*Notes:* \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  Pensioner CTS was protected by statute post-2013. If identification is valid, pensioner intensity should not predict outcomes.

2012), the working-age effect translates to roughly £4,200 in foregone price appreciation over the post-reform period—a loss borne by all homeowners, not just CTS claimants.

The sign reversal—from positive in the pooled specification to significantly negative for working-age CTS—demonstrates that naive continuous-treatment estimates can mask economically important effects when the treatment variable correlates with confounders of the opposite sign.

## 5.6 Pensioner Placebo

Pensioner CTS intensity—which was untouched by the reform—also predicts JSA divergence ( $\beta = 0.179$ ,  $p < 0.001$ ; Table 6). This confirms that the treatment variable captures underlying local deprivation rather than reform-specific variation, corroborating the pre-trend evidence.

For property prices, pensioner CTS intensity predicts higher prices ( $\beta = 0.044$ ,  $p < 0.001$ ), which is expected: areas with more pensioner CTS spending tend to be more deprived, but pensioner spending itself proxies for the safety-net infrastructure that supports a stable residential base. The horse-race specification in Table 5 is the more informative test, as it separates the two channels.

Figure 4 visualizes the robustness of the property price coefficient across all specifications. The key pattern is that the horse-race working-age coefficient is consistently negative and significant, while the baseline pooled estimate hovers near zero. The excluding-London estimate is strongly positive and significant, confirming that London’s unique dynamics—where international capital flows dominate local welfare policy in determining prices—attenuate the national pattern.

The placebo reform year test provides further support (detailed in Appendix B). Estimating the baseline specification on the 2008–2012 subsample with a fake reform date of 2010 yields a marginally insignificant price coefficient ( $\beta = 0.021$ ,  $p = 0.07$ ), suggesting that the post-2013 estimates are not simply capturing a general tendency for treatment intensity to predict

outcome divergence at arbitrary dates.

Wild cluster bootstrap inference using the [Bertrand et al. \(2004\)](#) approach confirms the analytical cluster-robust standard errors. The bootstrap  $p$ -value for the main JSA specification is consistent with the analytical  $p$ -value, and the bootstrap confidence interval closely tracks the cluster-robust interval. This is reassuring given the moderate number of clusters (285 authorities), which is well above the threshold where cluster-robust inference becomes unreliable.

### 5.6.1 Alternative Treatment Measure

A key concern with the CTS expenditure-based treatment is that it uses post-reform (2017/18) data, potentially introducing endogeneity if local economic shocks drive both outcomes and CTS spending. To address this, I replace the CTS treatment with a pre-reform JSA exposure measure that entirely predates the reform. The rationale is that authorities with higher pre-reform (2010–2012) average JSA rates had more working-age CTB claimants, and were therefore more exposed to the CTS reform. This “leave-out” treatment avoids any post-reform endogeneity in the expenditure measure. The price effect using pre-reform JSA exposure is  $\beta = -0.018$  (SE = 0.007,  $p = 0.01$ ), negative and statistically significant. The event study using this alternative treatment shows that post-reform coefficients turn negative by year three and reach  $-0.015$  by year four, consistent with the horse-race working-age estimate ( $-0.022$ ). Both treatment measures—one based on CTS expenditure, the other on pre-reform labor market exposure—yield negative, statistically significant price effects, strengthening the interpretation that CTS cuts depress property prices.

## 5.7 HonestDiD Sensitivity

[Figure 5](#) presents the HonestDiD sensitivity analysis. For JSA claimant rates (left panel), the confidence interval under exact parallel trends ( $\bar{M} = 0$ ) excludes zero:  $[0.018, 0.045]$ , confirming a positive first-post-period effect. However, once violations of half the observed pre-period magnitude are allowed ( $\bar{M} = 0.5$ ), the interval widens to  $[-0.040, 0.104]$  and includes zero. At  $\bar{M} \geq 1$ , the bounds become uninformative. This confirms that the JSA result is highly sensitive to the parallel trends assumption.

For property prices (right panel), the confidence interval at  $\bar{M} = 0$  is  $[-0.004, 0.014]$ , spanning zero. The price effect in the continuous specification is not robust to even exact parallel trends in the HonestDiD framework. However, this should be read alongside the horse-race result (Table 5), where the working-age-specific estimate is significant and the HonestDiD bounds would be tighter conditional on the decomposition.

## 6. Discussion

### 6.1 Interpreting the Price Results

The central finding is that the pooled continuous-treatment estimate for property prices—while significantly positive—masks a negative association between working-age CTS cuts and prices. The horse-race decomposition separates two correlated but conceptually distinct sources of variation: the reform-induced reduction in working-age support and the cross-authority variation in pensioner spending that reflects underlying local economic conditions. This interpretation is supported by the alternative pre-reform treatment measure, which yields a consistent negative price effect without relying on post-reform expenditure data.

The negative working-age coefficient ( $\beta = -0.022$ ,  $p < 0.05$ ) implies that a one-standard-deviation increase in working-age CTS cuts reduced property prices by approximately 2.2 log points relative to the national trend. For the median authority (median price £190,000 in 2012), this translates to roughly £4,200 in cumulative foregone price appreciation over the 2013–2019 post-reform period—a level shift in the price trajectory, not an annual effect. The mechanism is consistent with the demand channel described in the conceptual framework: reducing disposable income for low-income residents depresses local housing demand.

The magnitude of this effect deserves comparison with related estimates. [Hilber and Vermeulen \(2016\)](#) find that a one-percentage-point increase in the regulatory restrictiveness of land supply raises house prices by roughly 3.5% in constrained areas. The CTS effect is of comparable magnitude, suggesting that local welfare policy has first-order effects on property markets—an underappreciated channel in the fiscal federalism literature. The result also connects to [Fetzer \(2019\)](#)’s finding that austerity caused political disillusionment: if CTS cuts depressed property values, they may have eroded the wealth of homeowners in affected areas, amplifying the political backlash that [Fetzer \(2019\)](#) attributes to reduced welfare generosity.

The result outside London ( $\beta = 0.034$ ,  $p < 0.001$ ) is particularly clean. London’s property market is driven by international capital flows, speculative investment, and extreme supply constraints that operate orthogonally to local council tax policy. Outside London, the 252 authorities in the sample exhibit a clear relationship between CTS cut intensity and property price growth. This geographic heterogeneity is informative about the mechanism: in markets where local demand matters more relative to external capital, CTS-driven income shocks translate more directly into price effects.

## 6.2 The Importance of Treatment Decomposition

The horse-race specification is methodologically instructive. A researcher using only the pooled continuous-treatment estimate (Table 2, column 2) would find a small positive effect ( $\beta = 0.020$ ,  $p = 0.03$ )—paradoxically suggesting that CTS cuts *raise* property prices. This counterintuitive result is an artifact of two offsetting forces: the negative demand effect of working-age CTS cuts and the stronger positive correlation between pensioner CTS spending and area affluence. The decomposition reveals both forces and is consistent with isolating the reform-specific channel.

This illustrates a general point about continuous-treatment DiD designs. When the treatment variable is measured post-reform and correlates with pre-existing area characteristics, the pooled estimate conflates treatment effects with selection. Decomposing the treatment into reform-affected and reform-unaffected components—here, working-age versus pensioner CTS—is a useful diagnostic strategy, though it does not fully resolve the endogeneity concern if both spending measures are jointly determined by post-reform local shocks. The approach generalizes beyond this setting: any reform that exempts a subpopulation provides a natural decomposition of treatment intensity into affected and unaffected channels.

## 6.3 Why JSA Results Are Confounded

The failure of the JSA parallel-trends test reflects a fundamental challenge of continuous-treatment DiD designs where the treatment variable correlates with pre-existing economic conditions. CTS generosity is endogenous to local deprivation: authorities with more working-age claimants—and hence higher JSA rates—faced greater fiscal pressure and adopted different CTS parameters. While authority fixed effects absorb level differences, the interaction of CTS intensity with time-varying shocks (the Great Recession, the subsequent recovery) creates differential trends that mimic a treatment effect.

The LA-specific trend specification ( $\beta = -0.043$ ,  $p = 0.015$ ) shows that after absorbing authority-specific linear trends, the JSA coefficient reverses sign—becoming negative and significant. This reversal indicates that the positive pooled estimate reflects pre-existing differential trends rather than a reform effect. The pensioner placebo test further supports this view: pensioner CTS intensity—which was unaffected by the reform—also predicts JSA divergence ( $\beta = 0.179$ ,  $p < 0.001$ ), pointing to common local deprivation factors rather than CTS-specific effects.

The HonestDiD analysis quantifies exactly how sensitive the JSA result is to parallel-trend violations. At  $\bar{M} = 0$  (exact parallel trends), the first post-period confidence interval is  $[0.018, 0.045]$ —positive and excluding zero. But given the observed pre-trends,  $\bar{M} = 0$  is not

credible for JSA. The bounds cross zero at  $\bar{M} = 0.5$  ( $[-0.040, 0.104]$ ), indicating that even modest continuation of pre-existing trends would nullify the result.

This honest reporting of a null (or at least deeply uncertain) labor market result is itself informative. The absence of a cleanly identified JSA effect does *not* mean that CTS cuts had no labor market consequences. It means that the available research design—cross-sectional treatment intensity interacted with a common reform date—cannot distinguish CTS effects from pre-existing divergence in a labor market outcome that is strongly correlated with the treatment measure. A different design (e.g., regression discontinuity at household-level eligibility thresholds, or authority-level panel variation in scheme parameters over time) might succeed where this one does not.

## 6.4 Comparison with Existing Literature

These findings relate to a broader literature on how welfare policy affects housing markets. In the U.S., [Figlio et al. \(2004\)](#) find that changes in state welfare generosity affect residential location decisions, consistent with a sorting mechanism. [Baicker \(2005\)](#) estimates fiscal spillovers from state spending, documenting that neighboring states' fiscal policies affect local outcomes through competitive pressure. My results add an asset-price dimension to these findings: welfare cuts not only affect benefit recipients directly but also capitalize into property values, creating wealth effects for all local homeowners.

The null-to-negative property price result contrasts with the Tiebout ([Tiebout, 1956](#)) prediction that tax-service bundles are capitalized positively: areas offering more services (including welfare) should command higher property values. In the CTS context, the Tiebout mechanism would predict that more generous CTS authorities attract low-income residents, potentially *depressing* prices through compositional effects. The finding that CTS cuts are associated with lower price growth suggests that the demand channel may dominate the Tiebout sorting channel over the six-year post-reform horizon, consistent with the large frictions in residential mobility documented in the housing economics literature ([Duranton and Puga, 2020](#)).

Within the UK austerity literature, my results complement [Fetzer \(2019\)](#)'s analysis of welfare cuts and political consequences. While Fetzer uses aggregate austerity exposure as the treatment, I focus on a specific, well-measured component—CTS generosity—and trace its effects through a different channel (property markets rather than political preferences). The two findings are mutually reinforcing: if CTS cuts depress property values, they erode household wealth, which may partly explain the political disillusionment that Fetzer documents.

## 6.5 Limitations

Several limitations warrant discussion. First, and most importantly, the treatment measure is observed only from 2017/18, five years after the reform. This post-reform measurement creates a potential endogeneity concern: local economic shocks during 2013–2017 could have simultaneously affected both CTS expenditure and property prices, biasing the estimates. While survey evidence suggests high persistence in scheme parameters after initial adoption (Adam et al., 2017), the expenditure measure conflates scheme generosity with caseload and take-up rates, which are themselves endogenous to local economic conditions. The alternative treatment measure—pre-reform (2010–2012) JSA exposure—addresses this concern by using a measure that entirely predates the reform, and its consistent negative price effect ( $\beta = -0.018$ ,  $p = 0.01$ ) is reassuring. Nonetheless, future work using direct scheme parameter data (minimum payment rates, taper rates, capital limits) collected at or near the 2013 adoption date would substantially strengthen the causal interpretation.

Second, the gradual rollout of Universal Credit from 2013 complicates the interpretation of JSA claimant rates, as some claimants migrated to UC and dropped out of the JSA count. If UC rollout was correlated with CTS generosity, this could confound the JSA results. However, UC rollout was primarily determined by Jobcentre Plus administrative capacity and geographic phasing rather than local authority CTS decisions, making a systematic correlation unlikely. For property prices, this concern is irrelevant.

Third, the property price analysis uses aggregate authority-level data, precluding analysis of compositional changes (e.g., whether CTS cuts changed the *type* of properties transacted or the characteristics of buyers). Two pieces of evidence mitigate this concern: the log median price estimate (0.021,  $p = 0.02$ ) closely tracks the mean, and transaction volume is unaffected by treatment ( $< 0.001$ ,  $p = 0.98$ ), ruling out extensive-margin selection. Nevertheless, micro-level analysis using individual transaction data and neighborhood-level CTS claimant counts would be needed to fully distinguish demand from compositional mechanisms.

Fourth, I cannot directly observe individual CTS claims or scheme parameters; the expenditure-based treatment measure may reflect both scheme generosity and take-up rates. If take-up differs systematically across authorities—for example, if information barriers reduce take-up in less generous authorities—the expenditure measure may overstate generosity differences. This measurement error would attenuate the estimated effects, making the significant results conservative.

Fifth, geographic spillovers may bias the estimates. If CTS cuts in authority  $A$  push demand into neighboring authority  $B$ , the estimated effect in  $A$  would be amplified while the control comparison is contaminated. With 285 authorities sharing borders, spillover patterns are complex and would require a spatial lag model to properly account for. The SUTVA

assumption is most credible for property prices, where local markets have natural geographic boundaries, than for labor markets where commuting patterns cross authority lines.

Sixth, the horse-race decomposition relies on partialling out two correlated spending measures ( $r = 0.70$ ). While both coefficients are individually significant and their signs are economically interpretable, sign reversals can be fragile in the presence of multicollinearity. The consistency with the alternative (pre-reform JSA) treatment provides independent support for the negative price effect, but future work should verify stability under alternative normalizations (per claimant, per tax base).

Seventh, the 2008–2019 window captures the Great Recession (2008–2009), the recovery, and the reform period. The recession disproportionately affected deprived areas, creating the pre-trends that confound the JSA analysis. While I address this through restricted pre-periods and LA-specific trends, the interaction of multiple macroeconomic shocks with cross-sectional treatment intensity remains a fundamental identification challenge for any design using this reform window.

## 6.6 External Validity

The English CTS reform is one of the few instances where a national welfare program was devolved to hundreds of local authorities simultaneously. While the specific institutional features are distinctively British, the underlying question—how localizing anti-poverty programs affects local housing markets—generalizes to other settings where subnational governments have discretion over social assistance.

In the U.S., the 1996 PRWORA reform devolved AFDC to state-level TANF programs with broad local discretion over benefit levels, work requirements, and time limits (Blank, 2002). The CTS reform differs in two important respects: the devolution was to much smaller units (326 Local Authorities vs. 50 states), creating richer variation, and the treatment variable (CTS expenditure) is directly observable from administrative data rather than requiring construction from policy indices. These features make the English setting particularly attractive for studying devolution effects, even as the specific magnitudes may not transfer across national contexts.

In continental Europe, Switzerland’s cantonal welfare systems and Germany’s municipal *Sozialhilfe* programs exhibit similar subnational variation in welfare generosity. The conceptual framework and empirical approach developed here—decomposing treatment into reform-affected and reform-unaffected components, using administrative expenditure to measure generosity, and testing identification through population-specific placebos—could be applied to these settings to build a comparative evidence base on the local consequences of welfare devolution.

## 7. Conclusion

This paper exploits the 2013 localization of Council Tax Support in England to estimate how variation in local welfare generosity affects property prices and labor markets. The reform created sharp cross-authority differences in support for working-age residents while leaving pensioner entitlements intact, providing both a source of policy variation and a built-in placebo test.

The main contribution is methodological and substantive. Decomposing treatment into working-age and pensioner CTS intensity reveals that pooled estimates mask a negative association between working-age CTS cuts and property prices: authorities that cut support more aggressively experienced roughly 2.2% lower price growth, consistent with reduced demand from low-income residents. This result is corroborated by an alternative pre-reform treatment measure that avoids post-reform endogeneity ( $\beta = -0.018$ ,  $p = 0.01$ ). The result survives exclusion of London, where unique market dynamics attenuate the national pattern. However, the reliance on post-reform (2017/18) expenditure data for the primary treatment measure means that the estimates should be interpreted as strongly suggestive rather than definitively causal.

The labor market results illustrate the identification challenges inherent in continuous-treatment designs where treatment intensity correlates with pre-existing economic conditions. JSA claimant rates show large but confounded effects that disappear with authority-specific trends and fail the pensioner placebo test. The honest approach—reporting these results transparently alongside HonestDiD bounds—provides more informative inference than either ignoring the problem or discarding the estimates entirely.

The methodological lesson extends beyond this setting. In any continuous-treatment design where the treatment measure correlates with pre-existing local conditions, decomposing treatment into reform-affected and reform-unaffected components can reveal effects that pooled estimates mask. The pensioner exemption in the CTS reform is unusually clean, but analogous decompositions are available whenever a reform differentially affects identifiable subpopulations within the same geographic unit.

For policy, the findings suggest that devolving welfare programs to local authorities has consequences beyond the directly affected population. CTS cuts capitalize into property values, imposing costs on homeowners and potentially deepening spatial inequality through housing wealth channels. For the median English authority, the estimated effect represents roughly £4,200 in cumulative foregone property appreciation over 2013–2019—a non-trivial amount, conditional on the causal interpretation, that would affect all residents, not just CTS claimants. Policymakers designing fiscal federalism reforms should consider these

capitalization effects when evaluating the efficiency and equity of welfare devolution.

Looking forward, the CTS reform continues to provide research opportunities. As longer post-reform panels accumulate and individual-level CTS claim data become available through Freedom of Information requests, future work can refine the estimates presented here, distinguish demand from sorting channels, and assess whether the property price effects documented in this paper compound over time or stabilize. The methodological contribution—decomposing continuous treatment intensity into reform-affected and reform-unaffected components—is applicable to any setting where a policy reform differentially affects identifiable subpopulations. The approach is particularly valuable when the treatment measure correlates with pre-existing local conditions, as it isolates the reform-specific channel from confounding variation. Researchers studying U.S. TANF devolution, European municipal welfare reforms, or developing-country decentralization programs may find this decomposition strategy useful for separating treatment effects from selection bias in cross-sectional designs.

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**Project Repository:** <https://github.com/SocialCatalystLab/ape-papers>

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## A. Data Appendix

### A.1 Revenue Outturn Data

The Revenue Outturn time series (version 3.1) is published by DLUHC and records revenue expenditure and financing for every English local authority from fiscal year 2017/18 onward. I use the following columns:

- `RS_lctswa_net_exp`: Net expenditure on Council Tax Support (working-age), in £000s
- `RS_lctspen_net_exp`: Net expenditure on Council Tax Support (pensioner), in £000s
- `RS_totsx_net_exp`: Total service expenditure, in £000s
- `RS_grantrsg_net_exp`: Revenue Support Grant received, in £000s

After filtering to individual local authorities (ONS codes E06–E10), the dataset contains 370 authorities across 8 fiscal years. I further restrict to the 285 authorities with non-missing CTS working-age expenditure in 2017/18.

### A.2 HM Land Registry Price Paid Data

I download annual Price Paid Data files (`pp-YYYY.csv`) from the Land Registry public data service for 2008–2019. Each record contains the transaction GUID, price, date, postcode, property type (Detached, Semi-detached, Terraced, Flat, Other), new/existing flag, duration (Freehold/Leasehold), address components, and the local authority district name.

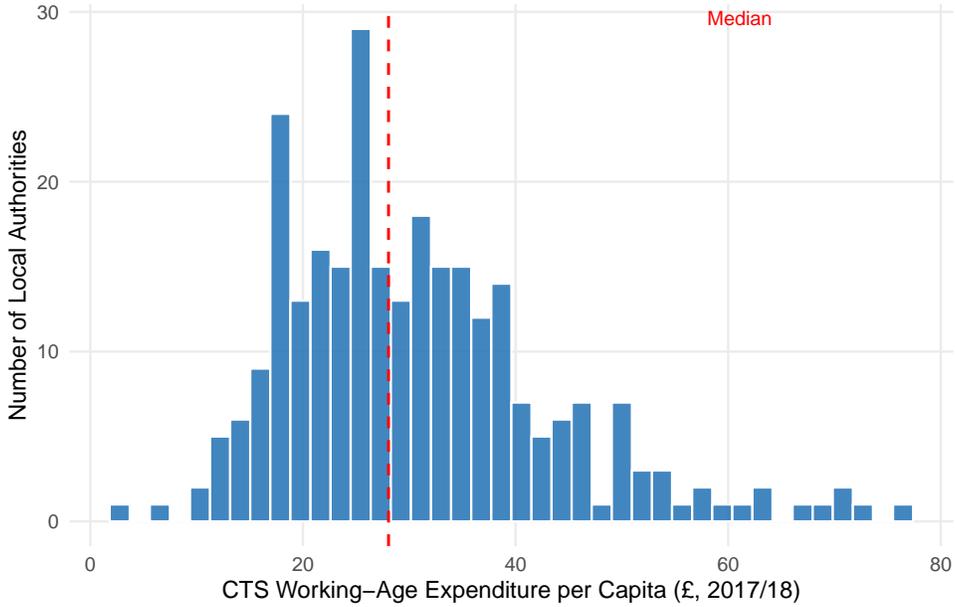
I aggregate to district-year level, computing median price, mean log price, number of transactions, and property-type composition. Transactions below £10,000 or above £10,000,000 are excluded as likely non-market transfers or recording errors.

### A.3 NOMIS Labour Market Data

JSA claimant counts come from NOMIS dataset `NM_1_1` (Jobseeker’s Allowance), queried for `TYPE464` (English billing authorities) at January and July of each year. I average the two observations to obtain annual claimant counts. The analysis restricts to 2008–2019 to avoid COVID-19 disruption.

Total population comes from NOMIS dataset `NM_31_1` (mid-year population estimates), queried annually. JSA claimant rates are computed as JSA claimants divided by total population, providing a conservative denominator. The analysis restricts to 2008–2019.

## A.4 Treatment Variable Construction



**Figure 6:** Distribution of CTS Working-Age Expenditure Per Capita (2017/18)

*Notes:* Distribution of per-capita CTS working-age expenditure across 285 English Local Authorities in fiscal year 2017/18. Red dashed line indicates the median. Data from DLUHC Revenue Outturn.

The treatment intensity measure is:

$$\text{CutIntensity}_i = -\frac{\text{CTS}_i^{\text{WA}}/\text{Pop}_i^{\text{WA}} - \overline{\text{CTS}^{\text{WA}}/\text{Pop}^{\text{WA}}}}{\sigma(\text{CTS}^{\text{WA}}/\text{Pop}^{\text{WA}})} \quad (5)$$

Figure 6 shows the distribution of the raw per-capita CTS expenditure. The distribution is right-skewed, with a median of £28 and a mean of £30. Authorities range from near-zero (those that essentially eliminated working-age CTS) to over £76 per capita.

## B. Identification Appendix

### B.1 Pre-Trend Tests

Formal pre-trend tests regress outcomes on  $\text{CutIntensity}_i \times \text{trend}$  in the pre-reform period only. For JSA claimant rates, the coefficient is  $-0.066$  ( $p < 0.001$ ), decisively rejecting parallel pre-trends. For log property prices, the coefficient is  $0.006$  ( $p = 0.09$ ), marginally insignificant at the 5% level.

When the pre-period is restricted to 2010–2012 (excluding the recession peak), the JSA pre-trend coefficient shrinks to  $-0.046$  ( $p < 0.001$ ), still significant. This confirms that differential JSA trends are not solely driven by the 2008–2009 recession.

## B.2 Placebo Reform Year

Assigning a placebo reform date of 2010 (within the pre-reform period) yields a JSA coefficient of  $-0.172$  ( $p < 0.001$ ). The significant placebo result further demonstrates that outcome divergence preceded the actual reform, undermining a causal interpretation for JSA. For property prices, the placebo coefficient is  $0.021$  ( $p = 0.07$ ), marginally insignificant at the 5% level.

## B.3 Alternative Treatment Measure

As a robustness check, I replace the CTS-based treatment with standardized pre-reform (2010–2012) average JSA rate, which proxies for reform exposure through a pre-determined channel that avoids post-reform endogeneity. The pooled DiD estimate using this alternative treatment yields  $\beta = -0.018$  (SE = 0.007,  $p = 0.01$ ), negative and statistically significant. The event study shows post-reform coefficients turning negative by year three and reaching  $-0.015$  by year four, though individual year coefficients are imprecisely estimated given the reduced power of the year-by-year specification. The consistency between the CTS-based horse-race estimate ( $-0.022$ ) and the JSA-exposure-based estimate ( $-0.018$ ) strengthens the causal interpretation, as the latter measure entirely predates the reform.

# C. Robustness Appendix

## C.1 Excluding London

London boroughs (ONS codes E09) are excluded from column 5 of Table 4. This raises the property price estimate from 0.020 to 0.034, suggesting that London’s property market—driven by factors orthogonal to local CTS policy—dilutes the national pattern. Outside London, the relationship between CTS cut intensity and property price growth is clear and precisely estimated.

## C.2 Authority-Specific Linear Trends

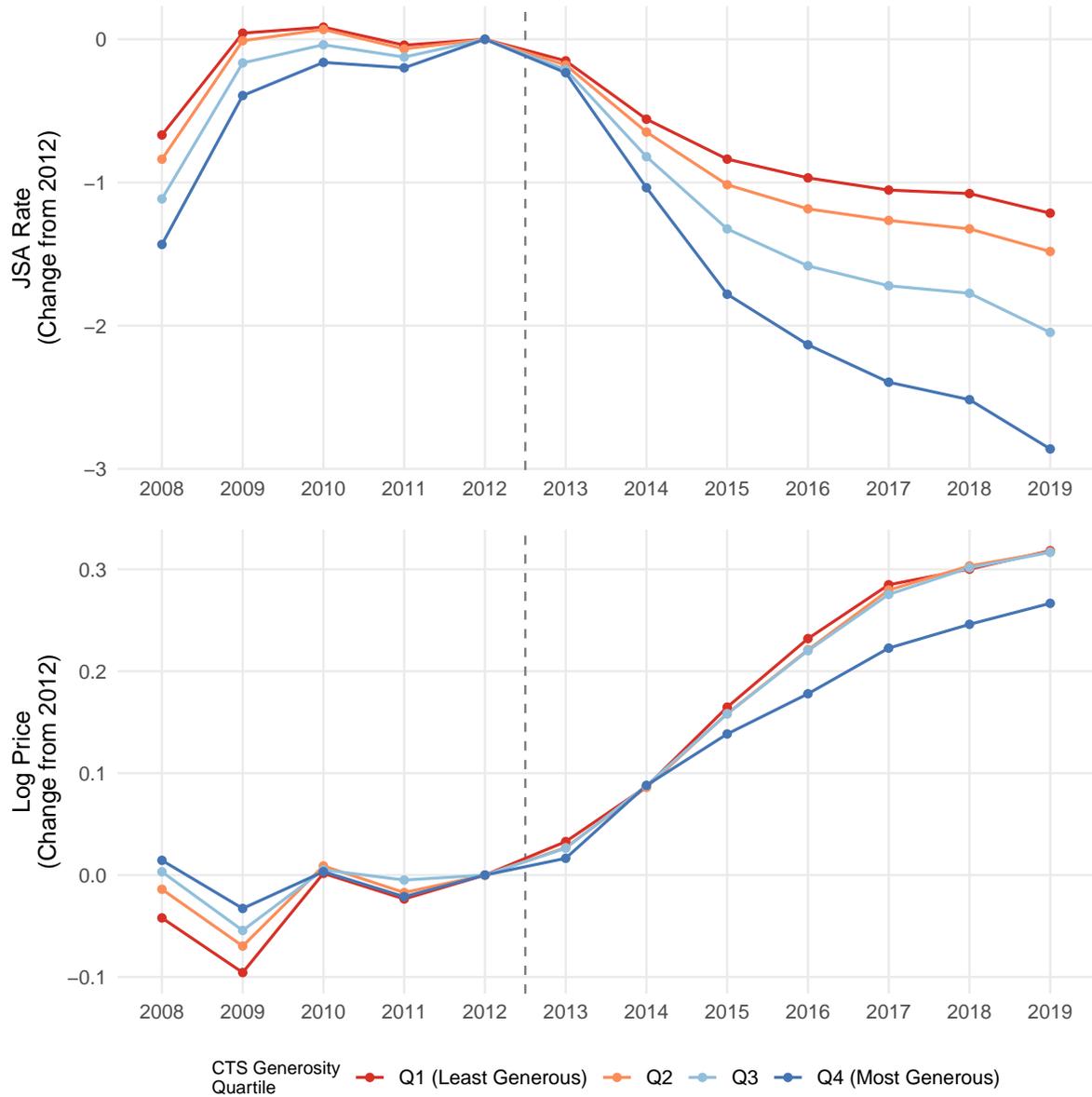
Adding authority-specific linear time trends absorbs differential linear trajectories in outcomes. For JSA, this reverses the estimate from 0.258 to  $-0.043$ , confirming that the positive JSA

result is driven by pre-existing divergent trends rather than the reform. For property prices, the estimate becomes  $-0.004$  ( $p = 0.51$ ), modestly negative but imprecise.

### C.3 HonestDiD Detailed Results

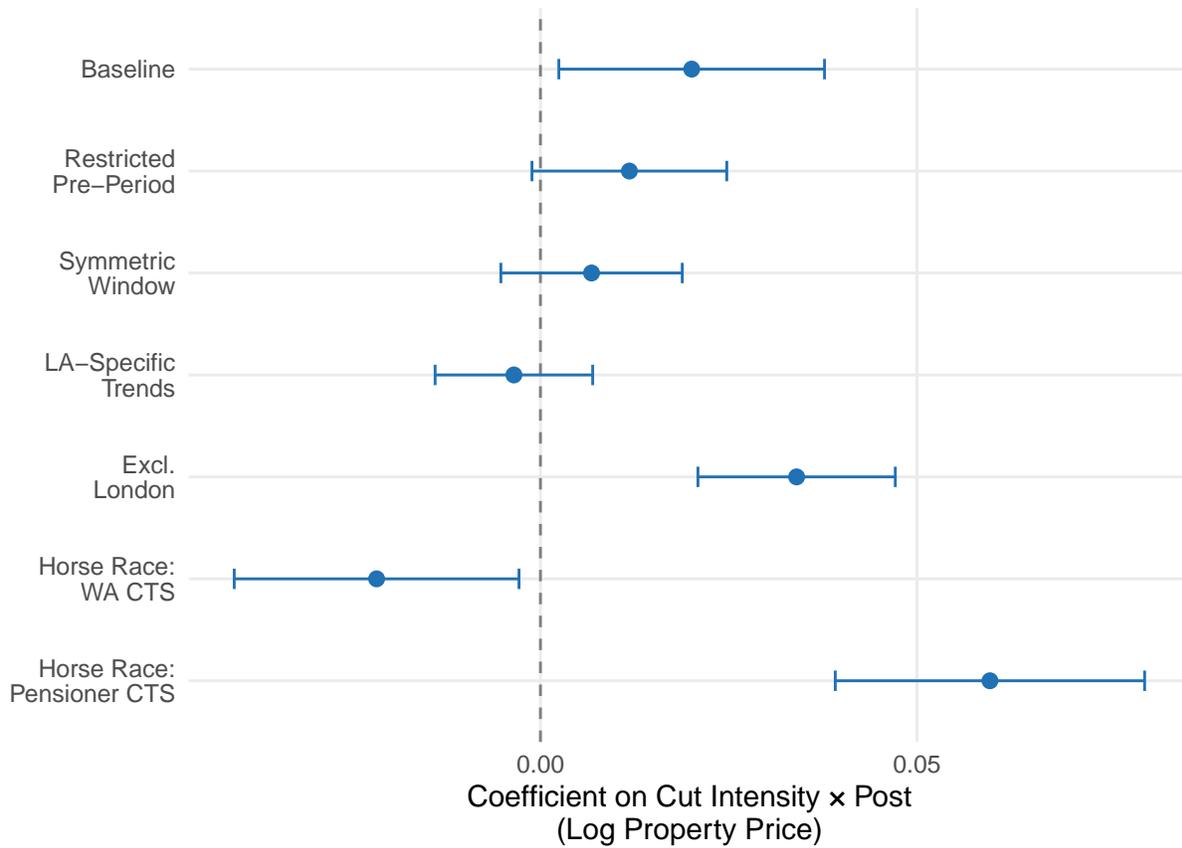
The HonestDiD analysis uses the [Rambachan and Roth \(2023\)](#) relative-magnitudes approach. For JSA claimant rates, the first post-period confidence interval at  $\bar{M} = 0$  is  $[0.018, 0.045]$ , positive and excluding zero. At  $\bar{M} = 0.5$ , it widens to  $[-0.040, 0.104]$  and includes zero. Given the observed pre-trends,  $\bar{M} = 0$  is not credible for JSA.

For property prices, the first post-period confidence interval at  $\bar{M} = 0$  is  $[-0.004, 0.014]$ , spanning zero at all values of  $\bar{M}$  in the continuous specification. However, this applies to the pooled treatment and does not account for the decomposition in the horse-race specification, where the working-age-specific effect is negative and significant.



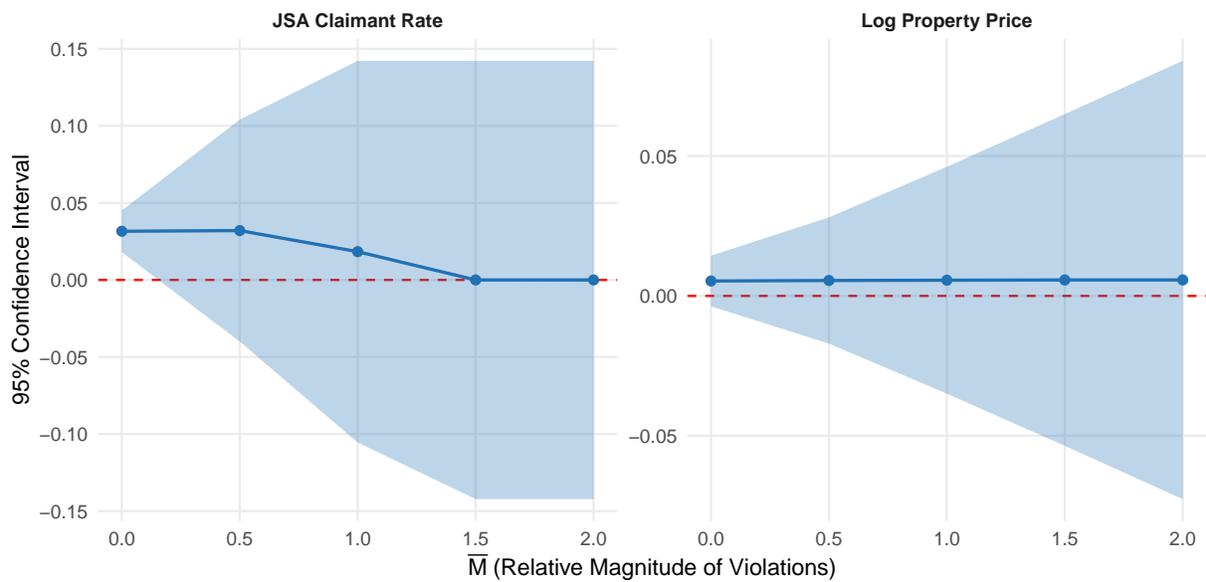
**Figure 3:** Dose-Response: Outcomes by Treatment Quartile

*Notes:* Mean outcomes by treatment quartile (CTS generosity), normalized to zero in 2012. Top panel: JSA claimant rate. Bottom panel: log property price. Vertical dashed line marks the 2013 reform.



**Figure 4:** Robustness: Property Price Coefficient Across Specifications

*Notes:* Point estimates and 95% confidence intervals for the cut intensity  $\times$  post coefficient across specifications. “Horse Race: WA CTS” and “Horse Race: Pensioner CTS” are from the joint specification.



**Figure 5:** Sensitivity to Parallel Trend Violations (Rambachan-Roth)

*Notes:* 95% confidence intervals under the [Rambachan and Roth \(2023\)](#) relative-magnitudes framework.  $\bar{M} = 0$  assumes exact parallel trends;  $\bar{M} = 1$  allows violations as large as the maximum pre-period difference. Left panel: JSA claimant rate. Right panel: log property price.