Dec 2019 No.444

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WORKING PAPER SERIES

Centre for Competitive Advantage in the Global Economy

Department of Economics

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December 7, 2019

Abstract

Homelessness and precarious living conditions are on the rise across much of the Western world. This paper exploits exogenous variation in the affordability of rents due to a cut that substantially lowered housing benefit – a welfare benefit aimed at helping low income households pay rent. Before April 2011, local housing allowance covered up to the median level of market rents; from April 2011 onwards, only rents lower than the 30th percentile were covered. We exploit that the extent of cuts significantly depend on statistical noise due to estimation of percentiles. We document that the affordability shock caused a significant increase in: evictions; individual bankruptcies; property crimes; share of households living in insecure temporary accommodation; statutory homelessness and actual rough sleeping. The fiscal savings of the cut are much smaller than anticipated. We estimate that for every pound saved by the central government, council spending to meet statutory obligations for homelessness prevention increases by 53 pence. We further document political effects: the housing benefit cut causes lower electoral registration rates and is associated with lower turnout and higher support for Leave in the 2016 EU referendum, most likely driven by its unequal impact on the composition of those that engage with democratic processes.

Keywords: HOUSING MARKETS, WELFARE CUTS, AUSTERITY, VOTING
JEL Classification: H2, H3, H5, P16, D72

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1 Introduction

In the past decades, housing markets in much of the Western world have seen dramatic swings, often bringing about drastic changes to the lives of millions and to urban landscapes alike. For example, the subprime mortgage crisis of 2008 led to close to one million evictions in the following year in the United States.[1] More recently, increasing property prices, making them unaffordable for the many, also implied substantial reallocations in space in places like London and San Francisco. These structural shifts in housing markets arguably coerced families into the duress of insecure living conditions. And, yet, those are known to have impacts on the long-run economic outcomes of the families: it has pervasive effects on the achievement of the children (Chyn, 2018), it is the harbinger of poor health (Johnson and Johnson, 2011; Fowler et al., 2015), and increases the chances of being laid off from work (Desmond and Gershenson, 2016).

In this paper, we investigate the consequences of an austerity policy in the United Kingdom that attempted to reduce the fiscal cost of providing housing assistance to low income households. The reform was implemented in March 2011, and affected between 0.7 to 1 million households in the private rented sector (between 3-4% of all households or between 17.5-25% of all households renting in the private sector). We explore quasi-exogenous variation in the extent of the cuts to housing benefits that is due to how the value of housing benefits is computed. From April 2011 onwards, the reference rates were cut to cover up to the median level of rents in local rental markets to only cover up to the 30th percentile of rents. along the the distribution of rents in a given area, and for a given dwelling type. The average exposure to the cut amounted to an annual housing benefit reduction equivalent to a £470, rising to as much as nearly £ 2,400 in many parts of London.

We explore intended and unintended shocks stemming from the policy implementation. The reference rents are empirically estimated by the Valuation Office Agency on the basis of a sample of rents submitted voluntarily by the landlords and real estate agencies. Thus, naturally, the 50th and 30th percentiles are noisy empirical estimates of the corresponding quantities along the *true* underlying dis-

[1] https://evictionlab.org/national-estimates/

tribution of rents. This creates the prospect that in some places the policy shock was stronger than in others, simply due to different random draws of the effective benefit cut which is driven by the difference between the 50th and 30th rent percentiles.

While in our main specifications we explore the spatial decomposition of the reference rents to identify the causal effects of the policy, we also leverage on the randomness of the the empirical estimates of the quantiles for robustness. To do so, we parse the explainable policy shock through an exercise whereby we attempt to predict the shift from the 50th to 30th percentile in the period in which the policy was implemented, using data from all time periods leading to that point in time. We show that, in most cases, the predicted policy component accounts for roughly half of the policy variation.

Overall, we show that the policy had substantial negative consequences. First, in the following years it led to an increase in 22.1 per cent on forced evictions and repossession orders in the private housing sector. This was accompanied by a rise in individual insolvency and bankruptcies, which increased by about 2.6 and 3.7 per cent. We next trace what happened to the displaced households. We find that the demand temporary housing offered by the councils increased by 17.8 as a consequence of the reduction in reference rents, with substantial cost increase by 94 per cent at that local administrative level. What is more, statutory homelessness and rough sleeping substantially increased in the years following the reform, by 13.2 and 36.7 per cent, respectively. It is essentially a foreseeable sequence of events: the reduction of the benefit had a material effect on the benefit claimants, some of whom went to arrears and were forcefully displaced from their homes, and setting the scene for unstable and insecure living conditions for many affected by the policy.

Importantly, we show that the policy was close to neutral from the fiscal standpoint. The direct fiscal savings to the central government for lower housing benefit payments were substantially offset by increase in the demand for temporary accommodation. Local authorities and councils have a legal responsibility to prevent homelessness and offer housing to qualifying households. We estimate that, on average, for each pound of implied fiscal savings accruing to the central government

due to the cuts, local government expenditures on homeless prevention increased by 53 pence. The distribution is substantially skewed: at the median council, while the fiscal savings amounted to £ 12.92 per 1000 households per district, it is offset by increases in homelessness prevention measures by councils of around £9.86 per 1000 households per district, resulting in a slim cost-saving of just £3.06 per 1000 households. Yet, it also implies substantial reallocation of resources, since the savings were accrued by the central government, and the costs are mostly born by the local councils. Taken together, this evidence suggest that policy substantially reduced the welfare of the benefit claimants, with little-to-none implied fiscal savings.

Lastly we also document that the cut likely has eroded the state of democracy and democratic participation in the UK. Using annual data on electoral registration, we observe that electoral registration rates have significantly and more substantially decreased in districts more affected, across both the parliamentary and the local electorates. Studying the 2016 EU referendum vote, we further document that the official 2016 EU referendum electorate as a share of the 2016 voting age population, is significantly lower in districts more affected by the cut. Similarly, turnout is drastically lower. Lastly, we also observe that a one standard deviation higher level of exposure to the cut in a district is associated with an between 1-3 percentage point higher level of support for Leave. This effect is likely driven by the composition of the electorate as studies since the 2016 EU referendum highlight that support for Remain among those that did not turn out in the 2016 EU referendum outnumbers support for Leave by 2:1. This highlights an indirect impact that welfare cuts can have on democratic participation of particularly vulnerable demographic groups, which may give rise to policies that further undermine the welfare of those demographic and social groups.

This paper relates to three strands of the literature. First, it relates to the growing literature on the effects of insecure housing and homelessness, and showcases how the interaction between public policies and private rental markets play a pivotal role in the provision of housing. Using data on foreclosures in the United States stemming from the Great Depression in the late 2000s, Humphries et al. (2019) show that eviction affects the consumption of durable goods years after

eviction. Johnson and Johnson (2011) and Fowler et al. (2015) highlight the consequences on health. The former argues that housing instability led to increases in levels of depression, anxiety, and poorer health more generally; the latter shows an increase in the number of suicides following forced evictions. The effect of a large public housing spending in the Netherlands is analysed by Van Dijk (2019), pointing to negative labour market outcomes after moving into public housing. Chyn (2018) uses the opposite variation – public housing demolition in Chicago, and consequentially low-income households relocated into less-disadvantaged neighbourhoods – and shows that moving to better areas during childhood increases employability in young adulthood.[2] In contrast, our paper shows that a public policy intended to generate fiscal savings forced the displacement of households and created conditions under which insecure living conditions arose, ultimately with little savings to the public treasury.

This paper also relates to the growin literature on the role of the welfare state in the provision of housing amenities, and political and social preferences. In the context of significantly rising property prices, the issue of affordability of rent becomes increasingly a social policy challenge.[3] Ansell (2014) argues that homeowners who experience house price appreciation become less supportive of redistributive and social insurance policies; this argument is expanded by Ansell (2019) who suggests that welfare state policies and property ownership are, in some sense, substitutes.[4]

[2]Desmond (2012) mentions that "in poor black neighbourhoods, eviction is to woman what incarceration is to men: a typical but severely consequential occurrence contributing to the reproduction of urban poverty." See Desmond and Shollenberger (2015), Desmond and Kimbro (2015), Desmond and Gershenson (2016) and Humphries et al. (2019) for economic effects of displacement on neighbourhood inequality, maternal health, labour market outcomes, financial distress, residential mobility and neighbourhood quality. Other papers discussed the causes of eviction. Desmond et al. (2013) shows that neighbourhoods with a percentage of children experience evictions more often. Desmond and Gershenson (2017) applies a hazard model and identifies that family size, job loss and neighbourhood crime levels are predictive of eviction. Diamond et al. (2019) shows the rent control in San Francisco prevented eviction in the short term, but on the loss of housing supply in the long run undermined the effects of this policy; Phinney et al. (2007) suggests that drug use, mental and health problems are associated with homelessness.

[3]See Ansell et al., 2018 for a discussion on about the global imbalances of surplus channeled to the real estate markets of deficit countries.

[4]See also Kingston et al. (1984) and Gilderbloom and Markham (1995) for earlier works on the connection between homeownership and voter preferences. Scheve and Stasavage (2017) reviews more generally how wealth may shape preferences towards redistributive policies.

It is thus possible that declining shares of homeownership in the UK is associated with increasing social proclivity of housing benefit expenditures over time prior to the implementation of the policy.[5]

Finally, we also contribute to work on Brexit. While much of the discourse is still driven by mostly descriptive and cross-sectional studies, a growing body of work explores the dynamics in the run up to the 2016 EU referendum. Specifically, Fetzer (2018) focuses on the impact of a set of welfare cuts on political preferences and, in particular, protest voting. The paper uses detailed micro data, that however, has a significant problem with sample attrition, especially among private-rented sector tenants, the object of this papers study. The relationship between populism and spatial sorting has been explored in related work in Adler and Ansell (2020).[6]

This paper proceeds as follows. In Section 2 we present the context and our data sources. 3 outlines our empirical strategy, followed by the results in Section 4. We present our back-of-the-envelope cost-benefit analysis in Section 5. Section 6 concludes.

2 Context and data

2.1 Housing in the UK

The UK's real estate market is fragmented into three main sectors: the private-rented sector, the social-rented sector and owner occupation. Appendix Figure A2 highlights the evolution of the three sectors over time since 2007 using data from the Office of National Statistics. The private rented sector has significantly expanded: in 2007, only 13% of households lived in the private rented sector. The share has since expanded to cover 20% of households by 2017. The social-rented sector has stayed fairly constant covering around 18% of households. On the other hand, owner occupation has declined from around 68% of households in 2007 to only cover 62% of households in 2017. The decline within this category is driven

[5]In the context of the United States, Mian et al. (2010) shows that subprime mortgage lenders and borrowers influenced government policy toward housing finance. See also Mian et al. (2014) and Mian et al. (2012).

[6]For other papers that connect the provision of urban amenities to voting behaviour at the local level, see Ahlfeldt (2011), Ahlfeldt and Kavetsos (2014), Ahlfeldt and Maennig (2015), Enos (2016) .

by a worsening access to home ownership: while the share of outright owners has increased from 31% of households in 2007 to 34% in 2017, the share of homebuyers has drastically declined from 37% in 2007 to 28% in 2017.

A predominant issue in the UK is a lack of housing supply and steadily eroding affordability. Home prices have accelerated at faster rates compared to incomes, resulting in a worsening affordability despite record low interest rates and various schemes to boost demand. This dynamic is pushing more households into the private rented sector. The increase in demand, with an overall inelastic housing supply, is increasing the cost of renting. In England, the median household spends more than 33% of their net disposable income on housing. In the lower tercile, this share increases to 41% across England; in the lowest income decile, English households spend 64% of their disposable income on housing.

Housing benefit, described in more detail in the next section, aims to relax household budgets. Appendix Figure A3 displays the impact that housing benefit across the three market segments. For home owners or mortgage owners, housing benefit is not impacting the affordability. This group of households spends around 18.2% of their household disposable income on housing. In the private rented sector, households spend, on average, 39% of their disposable income on housing costs prior to housing benefit. Housing benefit is reducing the cost to 35%. In the social rented sector, the housing cost burden is lower to begin with at around 35.7% of disposable income; housing benefit there lowers the cost of housing to around 27.1%. This highlights that tenants in the social rented sector and the private rented sector benefit differentially from housing benefit. Further, the two benefit systems are not integrated: not only is housing in the social rented sector cheaper, also the value of housing benefit is significantly higher.

In this paper, we focus on a reform that cut housing benefit in the private rented sector (but not the social rented sector). We next describe how housing benefit is computed and discuss the reform we study in this paper.

2.2 Local Housing Allowance

Housing Benefit is a means tested social security benefit in the United Kingdom that is intended to help meet housing costs for rented accommodation.[7] It is the second biggest item in the Department for Work and Pensions' budget after the state pension. In 2016-17 housing benefit cost around £23 billion, 11 per cent of total welfare spending and 1.2 per cent of GDP. From April 2008, a new policy for calculating Housing Benefit for private rented sector tenants (and not those in council or social housing) was introduced nationally, called the Local Housing Allowance (LHA). LHA introduced a method of calculating housing benefit based on the composition of the household and the median rent in a local *Broad Market Rental Area* (BMRA). The LHA is a flat rate allowance for different types of properties within a BMRA.[8]

Prior to April 2011, within a BMRA the LHA for different sizes of properties was calculated with reference to an estimate of a rental market's median rent. To estimate the median market rent, VOA Rent Officers rely on data submitted by private sector landlords and, in particular, letting agencies. While we do not have access to the full micro data, the VOA uses around half a million data points provided voluntarily to estimate the reference rents for each of the 152 BRMA's and across five main property size categories across England.[9] This data is used to estimate the BRMA and property-type specific median rent, defining an area's and property type's Local Housing Allowance. Up until March 2012, the reference rents and medians were computed every month, implying that each year, 9,120 empirical estimates of quantiles were estimated off just 500,000 genuine data points. From April 2012 onwards, the reference rents were only computed once a

[7] Similar schemes to support rent payments for low income households exist across many OECD countries and are quite common.

[8] The main types are a single room in shared accommodation, a 1, 2 or 3 bedroom flat. Appendix Figure A1 provides the BRMA's as per present. The BRMA's do not map into any existing administrative boundaries but broadly compromise areas of residential accommodation, within which a person could reasonably be expected to live thereby having access to facilities and services for the purposes of health, education, recreation, personal banking and shopping. When determining BMRAs the Rent Officer takes account of the distance of travel, by public and private transport, to and from those facilities and services.

[9] Scotland and Wales have independent rent-officers that collate similar data to estimate percentiles for the 18 BRMA's in Scotland and the 22 BRMA's in Wales.

year using data points from the previous calendar year.

Cut to Local Housing Allowance The reform we exploit in this paper is a change in the reference rent that is covered. Up until April 2011, the reference rent that defined the LHA for a property class was the *median of the empirical distribution* of rents within a BRMA. From April 2011 onwards, this reference rent was shifted to be the *30th percentile*, rather than the median. Naturally, this implied that many housing benefit recipients will have experienced a significant cut to their financial support to pay for rent.

Individual reform timing The cut of housing benefit was rolled out to existing housing benefit claimants between April 2011 and December 2012. The dates at which a particular claimant becomes treated is linked to the date of their last claim reassessment or claim anniversary in the year prior to April 2011 (or the date on which the claim began, if it began in the year prior to April 2011 and there had been no reassessment since). By default, LHA awards are updated at least once a year, implying that the stock of existing claimants will have been affected by December 2012 the latest.[10]

We next describe how we measure exposure to the cut at the district level.

2.3 Official impact estimates

The responsible Department for Works and Pension has conducted, in late 2010, an Economic Impact Assessment of the proposed reform. For that purpose, the DWP constructed, using the detailed and confidential individual-level claimant count database the two main ingredients to compute the ex-ante expected impact of the cut: the claimant count $C_{d,c,t}$, which captures the number of claimants living in district d and property type c at time t that would be affected by the housing benefit cut. And, second, the respective individual level losses, which ultimately are due to the difference in LHA rates between the 30th or 50th percentile relative to the individual level rent. Overall, it was estimated that 774,970 households

[10]Individuals may be affected earlier if there was other changes to their eligibility, such as the number of bedroom entitlement due to a change in household composition etc.

would lose a part of their housing benefit – among a total case-load of 939,220 individual cases.

From the official impact assessment, we extracted both the baseline number of claimants that would be affected by the cut, $C_{d,c,baseline}$, along with the average loss per claimant $L_{d,c,t}$. The latter, ultimately, is directly a function of the estimated difference in the median versus 30th percentile rent. In Figure 1, we present the variation in the measure $L_{d,c,t}$ that this implied across the UK for three different types of properties: one-bedroom flats, two-bedroom flats and three bedroom flats. These types of properties make up the vast majority of housing benefit claims. The map highlights the spatial distribution in the amount per week that benefit claimants, on average, lost due to the change in the reference rents. In the top quintile, the loss per affected household consistently amounts to at least £500 per year, but is usually notably higher. In Camden, the average loss per household in a two bedroom property amounted to £1,924 per year. Given that median household disposable income across the UK in 2010 stood at £24,400, this implies a near 8% cut in benefit income. The map highlights that there is significant variation across the UK in the intensity of the cut across different property types. While London clearly stands out as being among the worst affected parts, there is clear and distinct and extensive variation across the UK.[11]

Figure 2 provides the spatial distribution of the share of households living in one of the UK's 380 local authority districts in the left Panel A. This highlights that the baseline distribution of benefit claimants that would be affected by the cut is far from homogenous across the UK. Rather, it suggest that the share of households affected appears distinctly high in Central London, but less so in the commuting areas around London. Further hotspots appear to be along the UK's coastal towns and the North East and North West. Panel B provides the total financial loss per affected households from the official impact estimates. This underscores again that the losses are quite heterogenous across the UK; financially, the cuts appear to be most severe in Greater London and around Oxford.

For a significant part of the analysis, we leverage the official impact estimates provided by the Department of Works and Pension. In the empirical strategy

[11] Our results are robust to dropping London throughout.

section we also develop another estimation strategy that ultimately decomposes the treatment intensity into a predictable and an unpredictable part. We next describe the main outcome data.

2.4 Measuring precarious living conditions and homelessness

We draw on a host of official data sources to shed a comprehensive light on the economic and social impact of the housing benefit cut shock.

Forced evictions and repossessions We use annual data on eviction and repossession procedures covering England and Wales from 2008 onwards. The data was obtained from the Ministry of Justice and is broken down by local authority. We focus on repossessions of properties by landlords. The data allow us to distinguish between evictions and repossessions at the various stages of the underlying legal proceedings with the responsible County Court. Further, we can distinguish between evictions and possession orders pertaining to individuals living in private rented accommodation (and hence possibly affected by the housing benefit cut) or those living in the social rented sector (which was unaffected by the housing benefit cuts, which we will later use as a placebo); similarly, we also observe evictions and repossession actions following mortgage default. This provides us with some placebo outcomes.

Individual insolvencies We further leverage annual data from the UK's Insolvency Service. This data provides us with the number of new *individual* insolvency cases. This data is available at the district level from 2008 to 2016. Rent arrears are the most common reason for evictions of tenants in the private rented sector, but they usually exacerbate already distressful financial situations. Individual insolvencies are a further outcome measure to measure financial distress, which may be exacerbated by the steep rise in the cost of renting that the housing benefit cut implied.

Temporary Housing & Statutory Homelessness We leverage data from the Ministry of Housing, Communities and Local Government (MHCLG) measuring the

share of households in a local authority that is living in temporary accommodation. Local housing authorities in England have a duty to secure accommodation for unintentionally homeless households in priority need under Part 7 of the Housing Act 1996 (as amended). Households might be placed in temporary accommodation pending the completion of inquiries into an application, or they might spend time waiting in temporary accommodation after an application is accepted until suitable secure accommodation becomes available. As such, being housed in temporary accommodation is a primary and first indicator capturing the distinct risk of homelessness.

The statutory homelessness count refers to the number of households over the course of a year which the local authority has agreed it has a duty to house under the 1996 Housing Act. Homeless households can apply to their local authority for housing assistance. Households are accepted if they are eligible, unintentionally homeless, and in a priority need group. Priority need groups include households with dependent children, pregnant women and vulnerable individuals. MHCLG provides annual statutory homelessness statistics which consists of the total households which the local authorities deem to be homeless. We also have data on the various components of the total which comprises of the number of households who are accepted as being homeless and require/not require priority need, households which are eligible but not homeless, households which are eligible and in priority need but intentionally homeless. All these statistics are based on decisions made in each financial year (from April to March) and the data runs from April 2008 to March 2018. The Homelessness Reduction Act 2017 came into force in England on 1st April 2018. The Act puts a new duty on local authorities to prevent or relieve homelessness for anyone eligible for public funds, not just those who are unintentionally homeless and in priority need.

Local government expenditure data To study financial outcomes at the district level, we further obtained data pertaining to Local Government Finances, which separately lists the cost of homelessness prevention, administration and the associated cost of housing homeless households. We compute the cost associated with homelessness prevention measures in the broadest sense at the level of the

local government area and use this as a main outcome measure when studying the cost- and benefits. Lastly, we also obtained data from the Department of Works and Pension, that administers Housing Benefit. Specifically, we measure the total amount of spending per household in a district, to explore the impact of the cut on spending.

Rough sleeping street counts We also leverage data capturing street counts of rough sleepers at the district level. This data is far from perfect as districts use different methodologies and timings to conduct street counts. The data is available from 2010 to 2018; some councils have conducted street counts already prior to 2010. Rough sleeping is defined as people sleeping, about to bed down or actually bedded down in the open air or in buildings and other places not designed for habitation. The definition does not include people in hostels or shelters, people in campsites or other sites used for recreational purposes or organised protest, squatters or travellers.

The numbers on rough sleepers is a result of street counts, evidence-based estimates and estimates informed by a spotlight street count of rough sleeping by local authorities. It is up to local authorities to decide whether to carry out a rough sleeping count in the light of rough sleeping problems in their area. Where local authorities have decided to count, a count is essentially a snapshot of the number of rough sleepers in any given area on a particular night and it will not therefore record everyone in the area with a history of rough sleeping. This is usually done post midnight by volunteers in the local authorities' own workforce or from the local voluntary sector and formally take place between 1 October and 30 November. Given that rough sleepers often move between local authority areas (particularly in urban areas) it is suggested that neighbouring authorities count on the same night whenever possible. This eliminates double counting and ensures that more mobile rough sleepers are not missed. If a local authority chooses not to conduct a formal rough sleeper count, it should provide an annual estimate of rough sleeping numbers to CLG each year, after consultation with local agencies (e. g. outreach workers, police, faith groups, voluntary sector organisations, etc) to help inform the national picture on rough sleeping.

Democratic participation, registration and the 2016 EU referendum We further obtained data on the electoral registration rates. In the UK, every resident individual (with and without abode) is regularly reminded to register on the electoral roll. Using data from the UK's Electoral Commission we construct the share of the electorate among the voting age population in a district that is registered. Technically, this share should be very close to one. One source of the discrepancy could be due to migration as electorate statistics are produced based on the eligibility to vote in respective elections. To allay some concerns about mis-measurement, we study both the parliamentary as well as the local electorates. The former includes all UK, EU and most Commonwealth nationals, while the latter only includes UK and many Commonwealth nationals. Lastly, we also study the 2016 EU referendum results and vote shares to document that housing benefit cuts appear to have had an impact on the 2016 EU referendum vote, in particular, through its impact on turnout and the electorate.

Auxiliary outcomes We further study a host of auxiliary outcomes. We gather data for England and Wales on crime in the UK, specifically, we focus on property crimes and thefts from person. As auxiliary outcomes, we further have collected data from the Annual Population Survey on unemployment rates and inactivity rates. These will highlight that our treatment measure are not confounding auxiliary effects or economic shocks to local labor markets.
We next discuss some qualitative evidence.

2.5 Qualitative evidence

The cut to housing benefit is said to have had a dramatic impact on the private rented market. Specifically, due to the increase in arrears and evictions, many private sector landlords flat out refuse to house or rent to housing benefit recipients. In some cases, there is reports that indicate that some landlords, in response to the cuts, terminated the common short-hold tenancies.

Representatives of private landlords, such as the Residential Landlords Association (RLA), suggest that "the vast majority [of our members] are reluctant to take benefit claimants, not just because of the benefit and welfare changes, but be-

cause of higher management costs involved in managing benefit claimants." The RLA reported that arrears have increased since the introduction of the LHA. It warned that, as LHA rates continue to lose value relative to private sector rents, landlords will have less motivation to rent to benefit claimants or invest in the affordable rental market and less incentive to keep homes properly repaired and maintained. Since the cuts to LHA from 2011 onwards, is not uncommon for rental advertisements to explicitly state "no children, no pets, no DSS [Department for Social Security]." The exclusion of families with children is particularly common as eviction procedures for families with children is more costly for landlords; the reference to the Department of Social Security explicitly refers to exclusion of tenants on benefits.

The National Landlords Association made a similar point:

> "in the last three years there has been a 50% drop in the number of landlords taking people who are on benefits. It is now down to only one fifth; 22% of our landlord members whom we surveyed say they have LHA tenants, and 52% of those surveyed said they would not look at taking on benefits tenants." (House of Commons, 2016)

Witnesses reported that the quality of PRS properties that remain available at LHA rates was relatively poor. The charity of St Mungo stated that the majority of housing available to housing benefit recipients was near the "lower limit" of minimal standards of accommodation. It was finding that, before placing people in available properties, "a lot of work has to be done around addressing issues that have a big impact on health, around damp and other issues." Homeless Link reported that it was being compelled to place people in poor quality PRS properties because there were no other available options.[12]

We next present the empirical strategy, followed by the results.

[12]See https://publications.parliament.uk/pa/cm201314/cmselect/cmworpen/720/72005.htm

3 Empirical strategy

3.1 Official Impact estimates

As described in the context section, we rely on the official impact estimates that were constructed just prior to the reform becoming effective. The documentation around the economic impact assessment provides us with both, the number of claimants affected, $C_{d,c,\text{baseline}}$, as well as the average loss per claimant and property type, $L_{d,c}$. The cross-sectional measure capturing the total predicted financial loss in a district d, combining all property types c, can thus be computed as

$$S_d^{\text{official}} = \sum_c L_{d,c} \times C_{d,c,\text{baseline}}.$$

Throughout, we normalize the shock by the (time-varying) population levels or by the number of households living in an area.[13]

Empirically, we put specific emphasis on the expected loss per household $L_{d,c}$. As indicated, this is directly a function of the difference between the median and the 30th percentile. Given the high degrees of sampling variation to be expected, this difference is likely estimated with significant noise. We next describe a framework to construct alternative exposure measures.

3.2 Decomposing the identifying variation

As indicated, when determining the LHA, the Valuation Office Agency (VOA) studies the empirical distribution of rents within the area under consideration. The VOA estimates, for each district d, each property type c, at each point in time t the percentiles of the private sector rents.[14] We label those estimates for the p-th quantile – either 30th or 50th – as $\hat{\tau}_{d,c,p,t}$. We fix this measure at March 2011, the last month prior to the implementation of the cut. We thus write the financial loss

[13] Results are robust to using baseline population levels. Normalizing by the time-varying population figures is conservative as the UK has seen notable population growth.

[14] Formally, the statistics are constructed at the level of the Broad Rental Market Area. We map these to the level of the district d using a cross-walk constructed on the ward-level housing benefit spend data from 2010.

to claimants with property type c in district d as

$$L_{d,c} \quad = \quad \hat{\tau}_{d,c,50,\text{baseline}} - \hat{\tau}_{d,c,30,\text{baseline}}$$

and the implied fiscal shock in district d can be represented as

$$S_d^{\text{official}} \quad = \quad \sum_c [\hat{\tau}_{d,c,50,\text{baseline}} - \hat{\tau}_{d,c,30,\text{baseline}}] \times C_{d,c,\text{baseline}}. \tag{1}$$

Naturally, the percentile of rents is an empirical estimate which is likely to feature both signal about the underlying true distribution of rents as well as statistical noise. The statistical noise can occur to a variety of reasons. The most direct are sampling errors due the estimation of over 2,400 moments of the distribution of rents. Furthermore, the VOA computes the 30th and 50th quantiles on the basis of the stock of rental agreements in a given period, and those underpin the reference rents for the following period. Thus, the reference rents $\hat{\tau}_{b,c,50,\text{baseline}}$ and $\hat{\tau}_{b,c,30,\text{baseline}}$ also embed what is essentially related to a forecasting error. We compound the two sources of noise into an error term $\epsilon_{b,c,p,\text{baseline}}$ and write the estimate of the p-th quantile as the combination of signal and noise:

$$\hat{\tau}_{b,c,p,\text{baseline}} \quad = \quad \tau_{b,c,p,\text{baseline}} + \epsilon_{b,c,p,\text{baseline}}.$$

In this expression, $\hat{\tau}_{b,c,p,\text{baseline}}$ is the observed p-th quantile of the rent distribution for on dwellings of type c, in rental area b, and applicable as reference rates at the baseline. Moreover, $\tau_{b,c,p,\text{baseline}}$ is the p-th quantile of the *true* distribution of rents, and $\epsilon_{b,c,p,\text{baseline}}$ is the unpredictable error in estimating this latter object.

As indicated above, in our main specifications use the shifts in the *observed* distribution on rents, as in Equation (1). However, this framework allows us to go one step further and explore separately the predictable and unpredictable policy shocks. More specifically, the predictable component arises from the shift from the 50th to the 30th quantile along the *true* distribution of rents; this is essentially the intended policy shock and is represented by $\tau_{b,c,50,\text{baseline}} - \tau_{b,c,30,\text{baseline}}$. The unintended and unpredictable element stems from the forecasting error and the statistical noise in estimating the quantiles themselves. Formally, we can decom-

pose the fiscal savings in Equation (1) as:

$$S_d^{\text{official}} = \underbrace{\sum_c [\tau_{b,c,50} - \tau_{b,c,30}] \times C_{d,c}}_{\text{intended shock} \equiv S_d^{\text{predicted}}} + \underbrace{\sum_c [\epsilon_{b,c,50} - \epsilon_{b,c,30}] \times C_{d,c}}_{\text{unintended shock} \equiv S_d^{\text{residual}}}$$

where all measures are computed at the baseline (the "baseline" subscript was removed for conciseness). The first and second components are the predictable and unpredictable components of the policy variation, respectively.

To parse out these two elements, we proceed as follows. For each period prior to and including March 2011, we compute the observed interquantile difference $\hat{\tau}_{b,c,50,t} - \hat{\tau}_{b,c,30,t}$. We can do so for the ten months from June 2010 to March 2011, as we obtained data from the VOA for the those quantiles during this time window.

We then draw on the data up to February 2011 to predict the out-of-the-sample interquantile difference for the next month (our baseline measure of the policy shock). We associate the model forecast to the predictable policy shock $\tau_{b,c,50} - \tau_{b,c,30}$, from which we compute $S_d^{\text{predicted}}$ after applying to the measure of affected claimants $C_{d,c}$. The forecast error, or unexplained portion, gives rise to our measure of $\epsilon_{b,c,50} - \epsilon_{b,c,30}$, and thus S_d^{residual}. This allows us to decompose the overall policy shock into intended and unintended components, the latter of which is driven by statistical fluctuations in the estimates of the quantiles.

We use out-of-the-sample prediction as this is effectively the policy dynamics in which the reference rentals are set in anticipation of the prices effective in the next period. We use a number of models to forecast the difference between the 50th and 30th quantile in March 2011. More specifically, we introduce lagged dependent variables, district fixed effects and linear trends, as well as the combination between these features. We ultimately select the final forecasting model based on the out-of-the-sample forecasting performance, measured through the mean-squared prediction error in the baseline month. Intuitively, this broad search for the best-performing forecasting model ensures that most of the predictable variation in the policy shock is being accounted for.[15]

[15]The mean-squared prediction error per dwelling type can be seen in Table A1 in the Appendix. The model with local linear trends out-performs the competing models for 1, 2 and 3 bedrooms.

In Figure 3, we plot the predicted and residualised shock measures relative to the standard deviation of the official impact estimates. This highlights that there is substantive variation in both the predictable- as well as the unpredictable component. Strictly speaking, the former is exogenous by virtue of being predetermined, while the latter is exogenous, by virtue of the variation mostly and likely capturing sampling variation. In the empirical design where we exploit this decomposition, we focus on both the projected- as well as the residualized treatment measure.

We next describe the main empirical specification we study.

3.3 Empirical specification

Throughout, we estimate variations of a difference-in-differences design.

Main difference-in-difference The main baseline specification takes the following form:

$$y_{d,t} = \alpha_d + \gamma_t + \sum_{t \neq 2010} \eta_t \times Year_t \times S_d^j + \beta' X_{d,t} + \epsilon_{i,t} \tag{2}$$

where $y_{d,t}$ denotes a district d level outcome, such as eviction rates, the share of households living in temporary accommodation or deemed homeless. The district level fixed effect α_d absorbs any time-invariant differences, while the year fixed effects γ_t remove common idiosyncratic trends.

The main coefficients of interest are the estimated coefficients η_t on the interaction between the various cross-sectional exposure measures S_d^j before and after the reform was implemented, for $j \in \{\text{official}, \text{predicted}, \text{residual}\}$. The above specification estimates a separate coefficient for each year, allowing the results to be presented visually in graphical form, providing evidence in support of the underlying implicit common trends assumption. In the tables, we pool the post-treatment coefficients into a single estimate. In some specifications, we also include a vector of additional controls. In particular, we interact a set of year fixed effects with the baseline claimant distribution across different property types c per capita, $C_{d,c,\text{baseline}}$. As such, this implies we flexibly control for trends that could

The policy shock for shared properties is best forecasted with autoregressive lags 1 and 6 and BRMA fixed effects. Finally the policy shock is best predicted for 4-bedroom properties with a model containing lag 6 and BRMA fixed effects.

drive variation in the S_d^j measure due to changes in *demand for housing benefit* that may be independent from the change in the LHA rates. This puts further emphasis on the fact that what we aim to causally identify in this paper is the impact of the shock to affordability.

We present results including- and excluding London; similarly, we also present results where the sample is restricted to include data up until 2013. From 2013 onwards, Fetzer (2018) highlights that numerous other welfare reforms were implemented, which may be correlated with the treatment we study in this paper.

Exogenous treatment We also explore specifications where we replace S_d^{official}, with the decomposed S_d^{residual} and $S_d^{\text{predicted}}$. In the table presentation, these exercises are labelled as exogenous treatment. Econometrically speaking, the former is exogenous by virtue of being predetermined leveraging only pre-treatment data. The latter is exogenous by virtue of the residual variation most likely capturing sampling variation in the empirical estimates of the quantiles. For the exercise where we exploit variation in S_d^{residual}, we also flexibly control for non-linear time trends in the across the quintiles of the $S_d^{\text{predicted}}$ component. This essentially implies that we allow for non-linear time trends in the expected component of the shock, further zooming in on the unexplained exogenous variation, much of which is driven by noise in the empirical estimation of the quantiles.

Matching design As a further robustness check, we also implement a matching design. We dummify the treatment for districts in the upper quartile of exposure and then constructed matched pairs. We match on a vector of characteristics capturing local housing markets and supply. Specifically, we match on: the levels as well as changes in the shares of households living in owner occupied properties, in the social rented sector and the private rented sector between the 2001 and 2011 census. Similarly, we match on the share of residents commuting to London for work as of the 2011 census, the share of resident households on waiting lists for social housing and the average rent levels in 2010. To focus again on the component of the variation that is due to the losses entailed by the estimated difference in quantiles, we also match on the official DWP impact estimates capturing the share

of resident households affected by the reform, $C_{d,c,\text{baseline}}$. The result from the matching design is a set of matched pairs. For each district in the upper treatment quartile, we find a matched observation in the lower quartile that is similar on observables. We only retain matched pairs where the difference in propensity scores is less than 0.2. We then re-estimate a similar specification as 2, with the difference that we also add matched pair by year fixed effects, allowing for non-parametric time trends in the propensity scores or the quality of the match.

Throughout the paper, standard errors are clustered at the district level (constituency level for the Westminster election analysis). [16]

4 Main Results

4.1 Housing benefit spending

As a first step, we document the impact of the change in reference rents on the effective spending on housing benefits. Figure 4 indicates that the policy reduced the actual spending between 1 and 3 per cent, and become especially pronounced in 2012 and subsequent years. This is a feature of the sequential rollout of the policy, as the reference rates for individual claimants are updated in their claim anniversary. At latest, the stock of individuals would have updated the new reference rates at December 2012, and the period between March 2011 and December 2012 can be regarded as transitional periods.

4.2 Evictions

We begin by presenting the results on evictions. Visually, these are presented in Figure 5, using the main and official district level impact estimate, S_d^{official}, as treatment variable. he independent treatment variable has been normalised to have unit standard deviation. The figure suggests a sharp increase in eviction action following the implementation of the cuts. Further, there is no evidence that suggests significant pre-treatment trends.

The point estimates in Table 1 pool the individual estimates. The estimates in Panel A indicate that 1 standard deviation in the exposure to the cut in LHA is

[16] Districts are the main meaningful subnational administrative unit in the UK. Results are robust to computing spatial HAC errors or clustering at a higher level statistical areas.

associated to an increase of .121 possession claims per one thousand inhabitants, or a 22.1 per cent increase relative to the mean of the dependent variable. Results are robust but notably higher in London, which is not surprising. The impact on actual repossessions carried out by county court bailiffs, in Panel B, in relative terms suggests a 19.8 per cent increase due to the drop in LHA. Again, the effect is stronger in London, which, however, also sees a higher level of evictions and repossessions to begin with.

Panel C and Panel D can be seen as a form of placebo test. The cut to LHA did not affect the social-rented sector, but only the private rented sector. There is no discernible impact on eviction actions issued to the social rented sector; the impact is fully carried by eviction and repossession actions, usually due to rent arrears, concentrated in the private rented sector in which housing benefit claimants were directly impacted by the cut to LHA.

4.3 Individual Insolvencies

We begin by presenting the results on individual bankruptcies. Typically, mortgage and rent arrears can not be included in common insolvency routines as they are classified as priority debt to be recovered via the court proceedings around evictions. Nevertheless, the data provide a window into financial grievances that households may face. Anecdotally, many households have accommodated the losses to their housing benefit by drawing down savings or by starting to finance consumption through consumer loans, while still paying rent. Hence it is not inconceivable that some households and individuals started to accumulate problematic consumer debt that subsequently needed to be restructured. There is evidence that this is indeed the case. In Figure 6, we again focus on the main and official district level impact estimate, S_d^{official}, as treatment variable. The figure suggests a sharp increase in overall new individual bankruptcy cases as well as a notable increase in individual voluntary agreements as a debt restructuring method. Notably, the latter appears to have been on a declining trend relative to 2010, making a sharp turn and dramatically increasing in districts more affected by the cut to LHA.

The point estimates in Table 2 pool the individual post-treatment estimates.

The point estimate in column (1) in Panel A suggests that a 1 standard deviation increase in the exposure to the cut in LHA, is associated with 2.58 per cent increase in total new individual bankruptcies cases. The results are fairly stable across specifications and are precisely estimated. Panel B finds slightly higher effect sizes on individual voluntary arrangements – an insolvency procedure that is typically used to restructure consumer loans – indicating a treatment effect of around 3.7 per cent for a district with a 1 standard deviation higher exposure.[17]

4.4 Temporary accommodation and council homelessness spending

As indicated, councils have a legal obligation to provide housing for households that are deemed priority – typically families with children, pregnant, or sick and disabled households – and at risk of homelessness. Councils bear the cost of providing this temporary accommodation. In Figure 7 we present evidence on the demand for temporary accommodation by councils in Panel A, along with the councils' spending on hosting homeless in hostels and bread-and-breakfast accommodations. Both figures have skyrocketed dramatically from 2011 onwards.

In Table 3 we present the corresponding impact estimates. Using the official treatment estimates, in Column (1) we find that the demand for temporary accommodation grew by 17.8 per cent as a consequence of the reduction in reference rents, although the results are driven mostly by the London metropolitan area, the point estimates excluding London are nevertheless positive and just at the border of being statistically significant. What is more, we find that the council spending on temporary housing increased sharply by around 94 per cent as a consequence of the policy. This is possibly explained by the relative high costs of harbouring individuals in temporary housing, as opposed to more permanent arrangements. Panel B focuses on council spending on overnight temporary accommodation, such as hostels and bed and breakfasts; Panel C includes more broadly, spending on temporary housing. As a result of the increase in demand for temporary accommodation due to the sharp rise in evictions, many councils had to dramatically

[17]Rent arrears can be included under the insolvency procedures but require the permission of the landlord, who typically prefer to use court action.

expand their homeless prevention spending, often, this involved renting properties from the private-rented sector at market rates, ultimately, eliminating much of the fiscal savings that were projected to be generated by decoupling housing benefit cost from local rental markets.

In Section 5 we compare the cost savings from the reduction of reference rents with the increase in spending in temporary housing; we find that approximately 53 per cent of the savings were offset by these measures undertaken by at the councils level. Given the relative lack of independence for councils to raise revenues or issue debt, for example, for home construction, this naturally had further implications for public service provision at the local level, as many councils simply had to finance this extra cost by cutting spending elsewhere.

4.5 Statutory homelessness and rough sleeping

We next turn our attention to the effects of the reduction of reference rents on homelessness. Households are considered to "statutory homeless" if the local authorities consider that they do not have a right to occupy a property, or are at imminent risk of becoming homeless according to the 1996 Housing Act 1996, 2002 Homelessness Act, and 2017 Homelessness Reduction Act. The several housing acts also specify eligibility status, which in broad terms refer to immigration status and exclude intentional homelessness. Satisfying those criteria, the councils have a statutory responsibility to provide for housing and services, free of charge. Rough sleeping is defined as sleeping, or bedded down, in open air or in buildings or other places not designed for habitation. For this later outcome, as explained in Section 2.4, we rely on rough sleeping street counts carried by the councils themselves.

In Figure 8 we show evidence of a strong increase in both statutory homeless and rough sleeping in the years following the reform. Statutory homelessness was effectively trending downwards up to 2010, and the trend reverts in the post-reform years. Pre-trends are largely absent from rough sleeping impact estimates.

Table 4 presents the point estimates for the full post-reform effects. It indicates a 13.2 per cent increase in statutory homelessness, a result that is robust to the exclusion of the London metropolitan area. Furthermore, suggests that the reduction

in the reference rents increased rough sleeping by a substantial 36.7 per cent in the post-2011 years; the numbers are slightly lower and still statistically significant by excluding London from the sample.

4.6 Electoral registration and EU referendum vote

Lastly, we also study the impact on democratic participation and the 2016 EU referendum.

Electoral registration We first study the impact on the electoral registration rates using two measures of the electoral registration coverage. The first studies the parliamentary electors, which includes all UK nationals resident in the UK, most Commonwealth citizens legally resident in the UK as well as Irish nationals. The second data considers local government electors. This set is a superset of the parliamentary electors, by also including all European nationals that are legally resident in the UK. Individuals in the UK need to register to vote and councils regularly update electoral rolls. Yet, it is known that coverage is particularly low among individuals living in less stable temporary or private rented accommodation.

We construct a measure of the electoral registration rates measuring the share of the respective electorates as a share of the voting age population. The latter data is provided by the Office of National Statistics and is updated annually. Visually, the results are presented in Figure 9. Panel A focuses on the electoral registration coverage of parliamentary electors, while Panel B focuses on local electors. There is a notable increase in 2010, which coincides with a parliamentary election year and with the first year for which elections were held on the new set of constituency boundaries. This typically triggers special registration effort. Relative to 2010, there is a steady and increasingly sharp drop in the electoral registration coverage rate both for parliamentary and local electors across the UK, concentrated in areas most exposed to the housing benefit cut.

Appendix Table A4 provides the corresponding point estimates. The results suggest that a 1SD higher exposure to the housing benefit cut is associated with a reduction in 0.3% lower electoral registration rates. This may seem small, but in

relation to the average electoral coverage gap of just 7 percent, this is not negligible.

2016 EU referendum In Table 5 , we present results pertaining to the 2016 EU referendum vote. The official counting areas for the 2016 EU referendum were local authority districts, the unit of analysis for this study.

We estimate the following cross-sectional regression

$$y_d = \alpha_{r(d)} + \gamma' S_d^j + \xi' X_d + \epsilon_d$$

where y_d measures three different outcomes: the official electorate that was eligible to vote by virtue of being registered in the 2016 EU referendum as a share of a districts voting age population; the actual turnout, measured as the share of votes cast as a proportion of the electorate; the vote share for Leave. The regression further includes region controls $\alpha_{r(d)}$, in particular, a set shifters capturing geographic heterogeneity across the 39 different NUTS2 level regions across England, Scotland and Wales (Northern Ireland is dropped). Similarly, we also include a set of district controls X_d taken from Becker et al. (2017) measuring both the levels as well as changes in immigration between 2001 and 2011 stemming from EU countries that were members of the EU already 2001; accession countries that joined the EU in 2004; and the rest of the world.

The result in Table 5 follow a similar layout as the previous exercises. The results in Panel A confirm our previous results suggesting that electoral registration rates appear distinctly lower. On average, a 1SD higher exposure to the housing benefit cut is associated with a near 1 percentage point lower electoral registration coverage rate for the 2016 EU referendum electorate. Panel B further and in addition highlights, that turnout also appears distinctly lower. A 1 SD higher exposure to the housing benefit cut is associated with a 1.8 percentage point lower turnout in the 2016 EU referendum. Lastly, Panel C highlights that the changes in the composition of the electorate or turnout, may have affected the EU referendum result at the district level. Support for Leave appears between 1-3 percentage points higher across all specifications. Part of this effect may be driven by systematically lower turnout and electoral participation that may, on average, have higher sup-

port for Remain. This is confirmed in analysis of opinion polling conducted after the 2016 EU referendum: support for Remain among the group of non-voters in 2016 outnumbers support for Leave by around 2:1 (see Alabrese and Fetzer, 2018).

While we do not want to interpret the effects causally, they do suggest that systematically lower levels of turnout, in particular in urban agglomerations, where the impact of housing benefit cuts were particularly severely felt, may have undermined support for Remain in the 2016 EU referendum, likely affecting the aggregate result.

4.7 Auxiliary results, robustness and null effects

Column (4) of Tables 1 implements the specification where we introduce year interacted with the distribution of claimants across property types. This controls for changes in the composition of the demands for the benefits. In all cases, we observe that the point estimates are either relatively unchanged or increase as compared to the baseline specifications in Columns (1) to (3). In Columns (5) and (6) we repeated the impact assessment exercise with the exogenous component of policy shock, i.e., the intended and unintended parts of the policy variation that are driven by the projected and the noise in the empirical estimates of the quantiles, $S_d^{\text{predicted}}$ and S_d^{residual} respectively. In most cases, we replicate the main policy effects obtained through the official policy variation S_d^{official}. Finally, in Column (6) we present the results of the matching estimator, which in further corroborate the main findings.

We finish this section by presenting some notable null-results that can help ruling out alternative mechanisms.

No impact on unemployment or economic activity In particular, an area's exposure to the cut is not associated with a change in local unemployment nor a change in the economic activity rate, suggesting that the shock is not confounding other structural changes to local labor markets.

Temporary increase in property crimes In Appendix Figure A5 we present results pertaining to crime data for England and Wales. These data suggest that,

in particular property crimes saw a sharp increase in 2011/2012 in locations more severely affected by the housing benefit cut, relative to the pre-treatment period. This sharp increase was of temporary nature however. In Appendix Table A3 we present the corresponding point estimates which suggest a persistent and large positive impact on thefts from persons.

5 Cost-benefit comparison

As indicated, the net fiscal savings that the cut to housing benefit spending brought about, may be mostly or partially be offset with increased cost to local councils for housing households that satisfy the legal definition of being threatened by unintentional homelessness and are deemed a priority need.

We can conduct a cost-benefit computation, ignoring the associated indirect human and economic costs that are associated with evictions. To do so, we compute the full distribution of treatment effects that are implied by the results in Table A2, along with the impacts documented on increased cost to councils to pay for temporary accommodation to homeless households (along with the associated administrative cost), that we documented in Table 3.

Rationale Since many councils were forced to sell a significant share of their housing stock at below-market prices to tenants under the UK's system of Right to Buy scheme introduced by Margaret Thatchers Conservative government in the 1980s, many councils do not have vacancies in their retained social housing stock. As a result, they need to resort to the private rented sector, in order to meet their legal obligations to house homeless households or households at risk of homelessness.

This sets up the possibility that the lower costs due to lower housing-benefit payments may indirectly just inflate the cost to councils to acquire capacity in the private rented sector in order to meet the legal obligations, partly neutralizing the fiscal savings that may have been generated due to lowering the LHA.

Results We simulate the full distribution of cost savings due to lower housing benefit spending that we empirically can attribute to the cut in reference rents.

Similarly, we simulate the full distribution of local council cost increases that we can attribute to the cut in housing benefits. We obtain two empirical distributions of point estimates and can compare these. We present the results graphically in Figure 10. The results suggest that much of the savings due to lower costs in housing benefit were immediately absorbed through higher council spending.

On average, across local authority districts, for every pound saved in lower housing benefit, the costs to councils for homelessness prevention increased by 53 pence. The distribution is quite skewed: for the median council, the fiscal savings due to lower housing benefit costs amount to a mere £12.92 per thousand household. This is mostly offset with higher costs due to homelessness prevention efforts, increasing local council costs by £9.86 per thousand households. For the median district, the net fiscal savings amount £ 3.06 per thousand household.

Across the whole of the UK, the projected ex-ante fiscal savings from the cut to local housing allowance were estimated to be around around £400 million. Our estimates imply that the actual savings were closer to £542 million. This is offset with an overall increase in spending on council anti homelessness measures of £259 million, implying a dramatic shifting of burden from the central government to local governments.

This is exacerbated by very strict limits to local governments to raise revenues or to borrow, implying that many councils in turn, had to dramatically cut other services in order to cover the increasing cost due to homelessness.

6 Conclusion

In this paper we explore the effects of a policy: the reduction of reference rents that underpin the housing benefit allowance in the UK, in the wake of the Conservative-led austerity cuts. We show that the policy had high social and human costs, as it substantially increased evictions, individual insolvencies, temporary accommodation, statutory homelessness and rough sleeping. It is, in a sense, a tale in the making, since the scale of the cut was severe (in the top quintile, at least £500 per household per year) and hit a financially-vulnerable strata of the population.

We also show how the policy to a large extent gave to one pocket by taking from another. More specifically, once the increase in local spending by local councils is taken into account, we estimate the cost-saving elasticity to be approximately 47 per cent for each pound of housing benefit reduction. In other words, for each pound saved, 53 cents were shifted onto temporary housing costs, which are substantially more expensive than the permanent kind, and catered for at the local level. For the median council, on aggregate the average savings per household was a mere £3.06 per thousand households. On aggregate, the projected fiscal savings were close to £542 million, and were substantially offset by an increase in temporary housing expenditure by £259 million. This highlights the potential for unintended policy consequences, with a social as well as economic consequences: it has been shown.

Moreover, we document the effects that the policy had on representative democratic participation in the elections in the UK: in the most affected districts, electoral registration rates dropped markedly. This finding is reproduced during the 2016 EU referendum vote, where we found evidence that the turnout is substantially lower. We also find that the support for Leave was higher in those places, which is possibly driven by a composition effect on the electorate since the proclivity to vote Remain was substantially higher among those who did not turn out to vote in that occasion.

This paper brings together a few strands of the literature concerning the causes and consequences of household displacement, and the role that policymaking exerts in preventing and mitigating insecure and precarious living conditions, being homelessness and rough sleeping at the extreme of this distribution. This paper also connects the effects of displacement effects onto the democratic representation and social choices, specifically when the electoral registration process is designed in such a way that it connected to the place of domicile; and particularly so when a vulnerable strata of the population is less likely to be represented.

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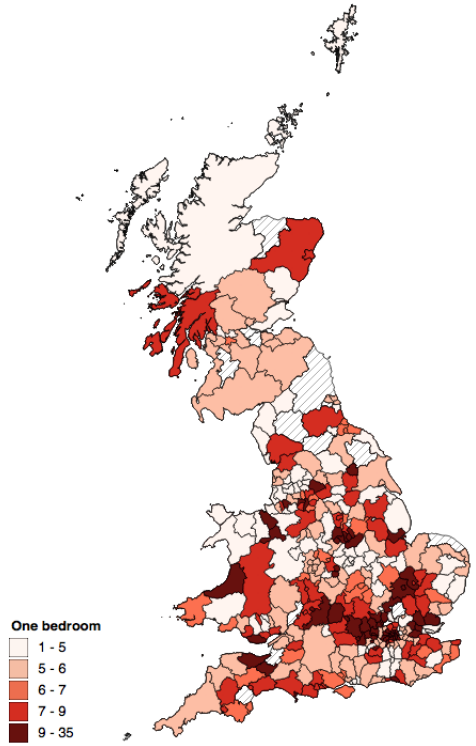
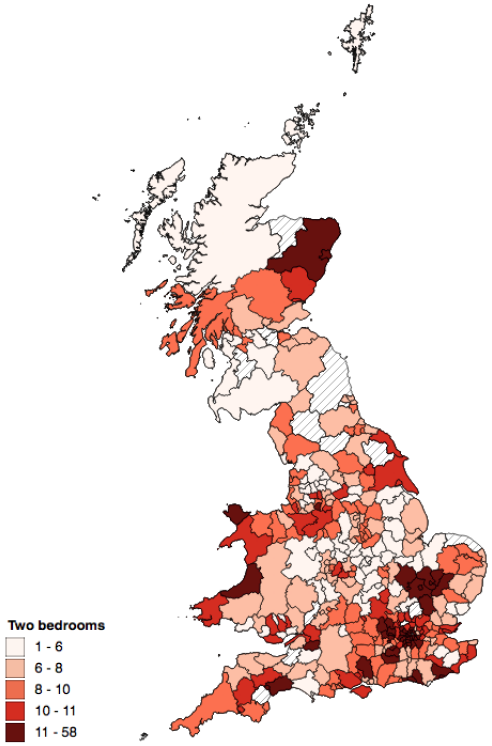
Figure 1: Estimated impact of reducing Local Housing Allowance from covering median to 30th percentile of rents at the district level for different types of properties

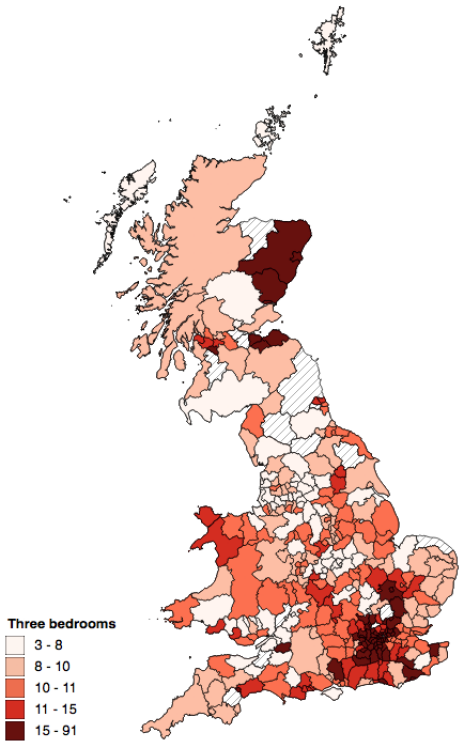
Panel A: One Bedroom flats



Panel B: Two Bedroom flats



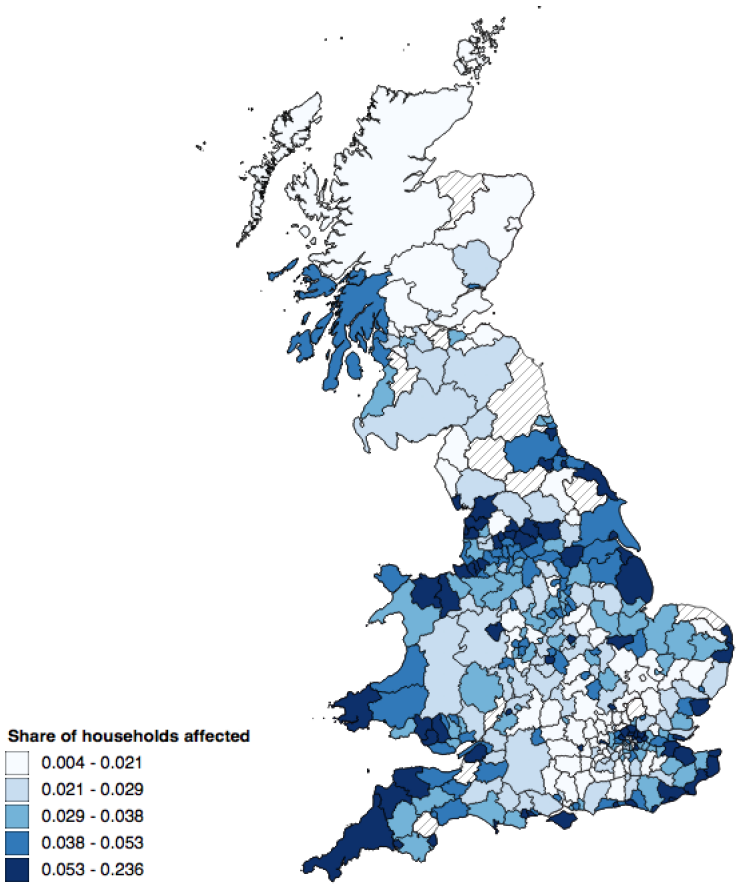
Panel C: Three Bedroom flats



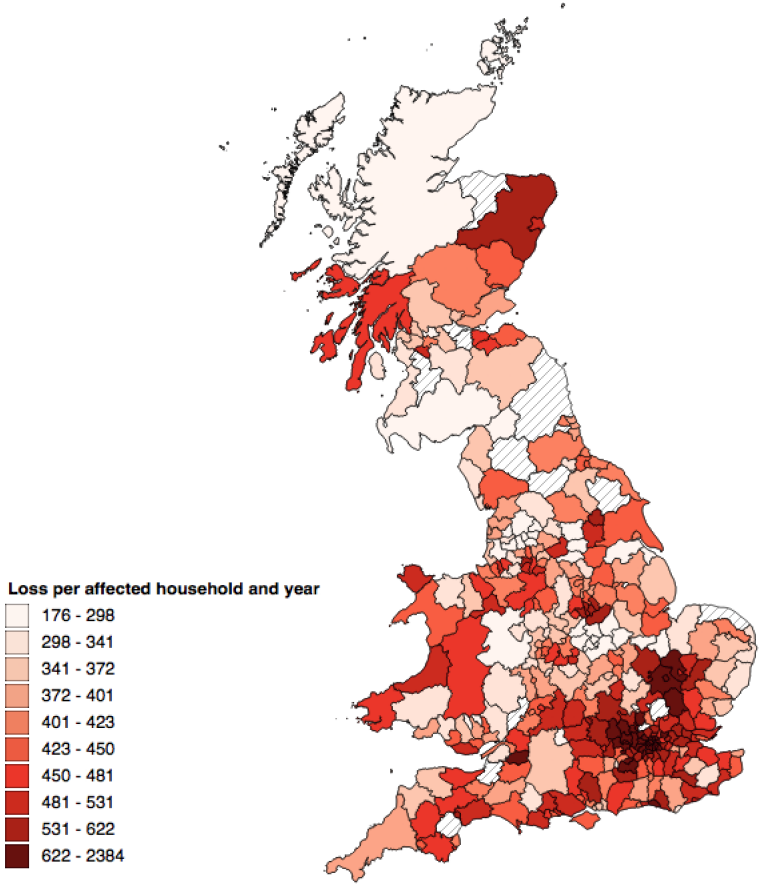
Notes: Figure plots the amount lost in pounds per week in housing benefit per household due to the reduction in the local housing allowance rate covering the 50th percentile of private sector rents to only cover up to the 30th percentile of private sector rents. The figure highlights significant spatial variation of the incidence of the shock.

Figure 2: Ex-ante estimated impact of change in housing benefit reference rent: moving from median to 30th percentile of rents as maximum allowable rent

Panel A: Households affected

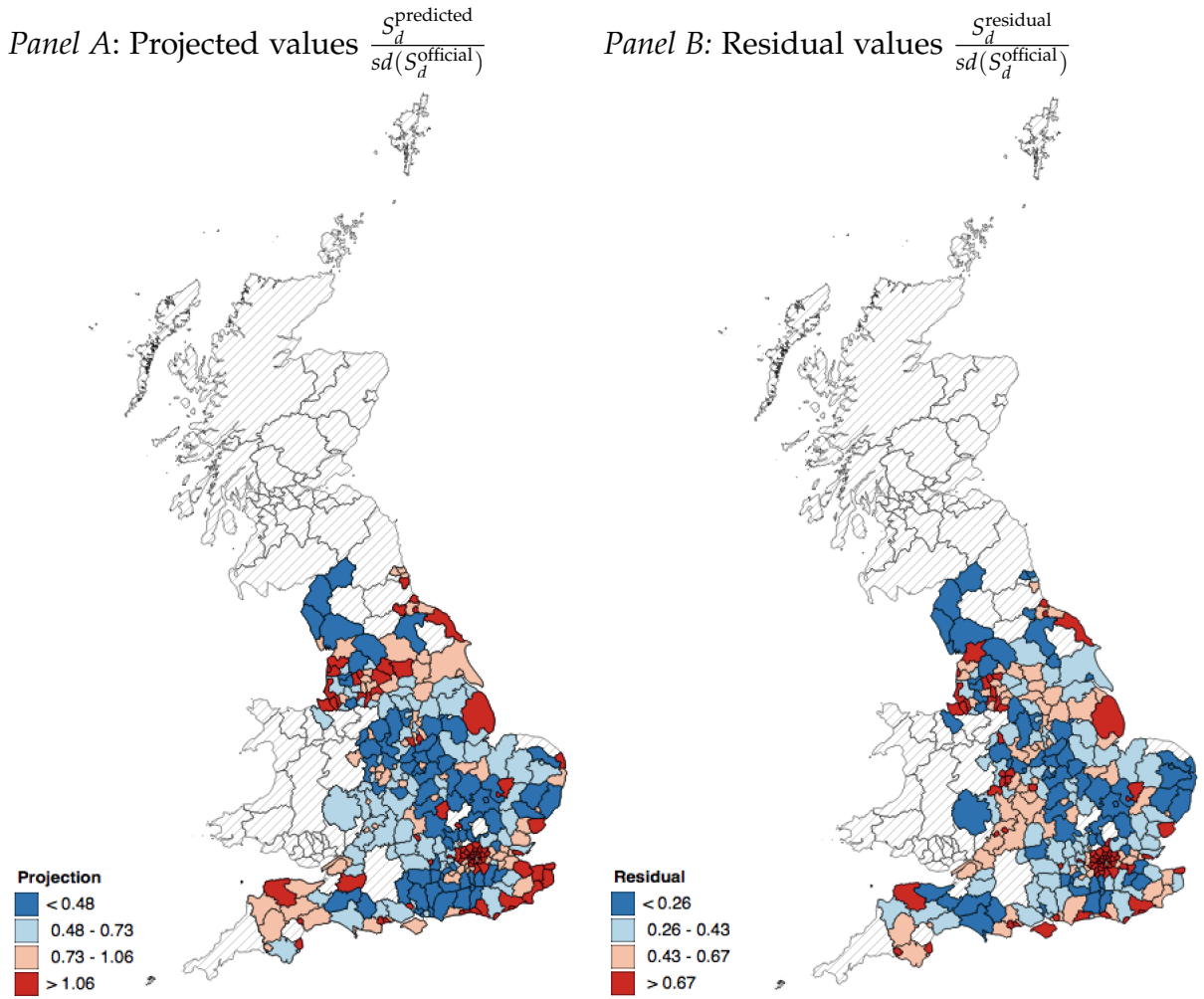


Panel B: Avg loss per affected household per year



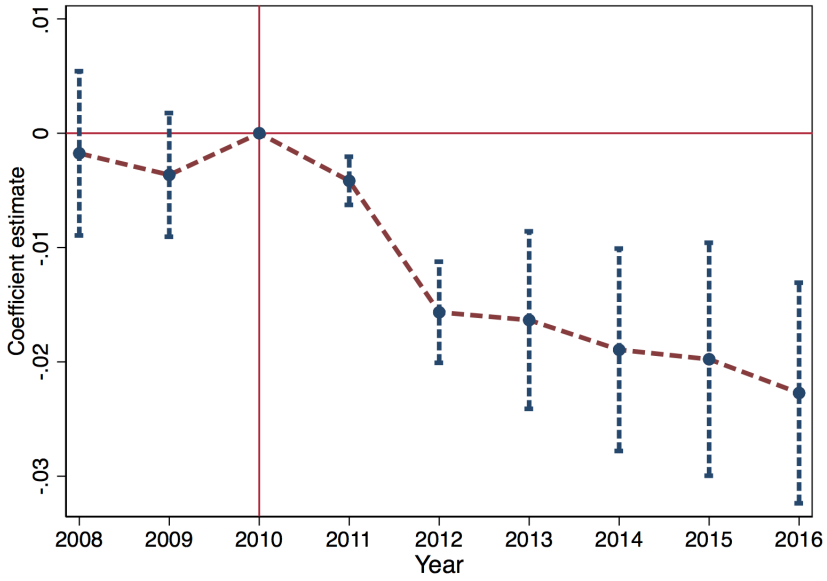
Notes: Map plots out the exposure to the cut to local housing allowance across districts using data from the Department for Works and Pension's Official Economic Impact Assessment. Panel A presents data on the number of households affected expressed as a share of all resident households. Panel B presents the distribution of the average loss per affected household at the district level.

Figure 3: Variance decomposition of projected and residualized impact estimates relative to official impact estimates across England



Notes: Map plots out the exposure to the cut to local housing allowance across districts using data from the Department for Works and Pension's Official Economic Impact Assessment. Panel A presents data on the number of households affected expressed as a share of all resident households. Panel B presents the distribution of the average loss per affected household at the district level.

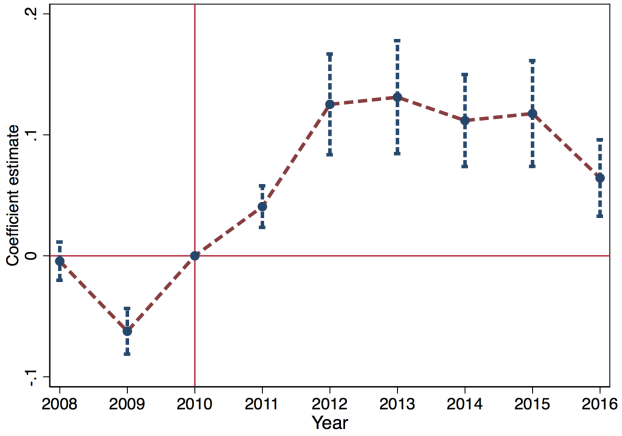
Figure 4: Impact of cuts in reference rents on housing benefit spending



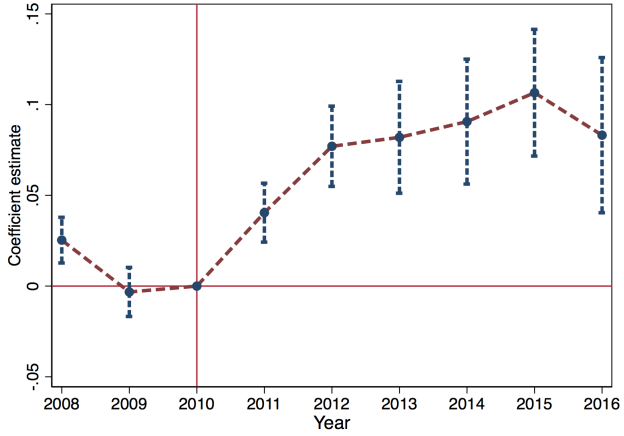
Notes: Figure plots results from regressions studying the impact of shifting the reference benefit sanctions arrivals in different quarters on support for Leave in the 2016 EU referendum at the ward level. All regressions control for local authority district fixed effects. 90% confidence bands obtained from clustering standard errors at the district level are indicated.

Figure 5: Impact of change in reference rent on forced evictions of people living in rental accommodation

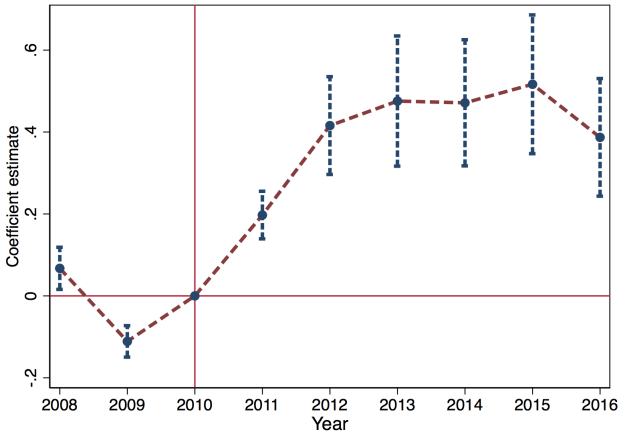
Panel A: Possession orders



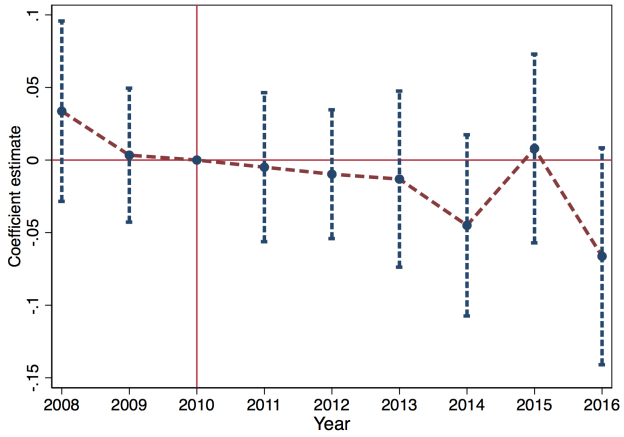
Panel B: Actual repossessions



Panel C: Private sector evictions



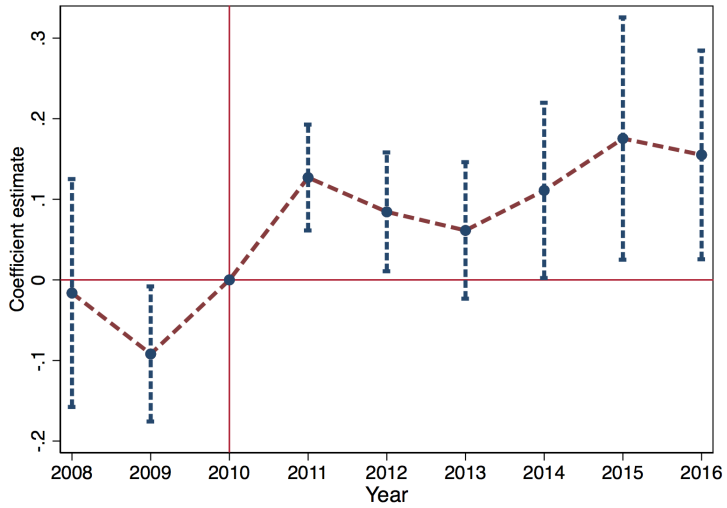
Panel D: Social rented sector evictions (placebo)



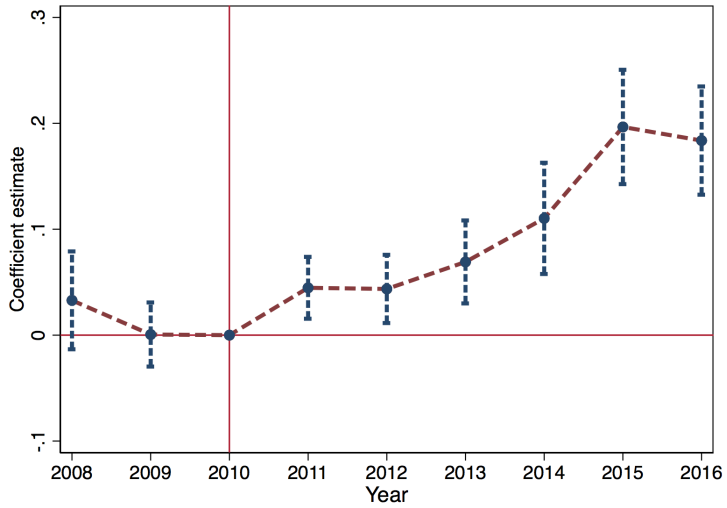
Notes: All dependent variables are measured as rates relative to the number of resident households in a district. Figure plots results from studying the impact of the cut to local housing allowance to cover the median rent to only cover the 30th percentile of rents from April 2011 onwards. The dependent variable in Panel A measures all Landlord possession claims raised. Panel B studies actual repossessions carried out by county court bailiffs. Panel C studies all private rented sector related eviction actions (including claims being launched, eviction notices being issued and actual repossessions). Panel D contrasts all social rented sector related eviction actions as a placebo outcome. All regressions control for local authority district fixed effects and year fixed effects. 90% confidence bands obtained from clustering standard errors at the district level are indicated.

Figure 6: Impact of change in reference rent on individual insolvency cases and bankruptcies

Panel A: All individual insolvency cases



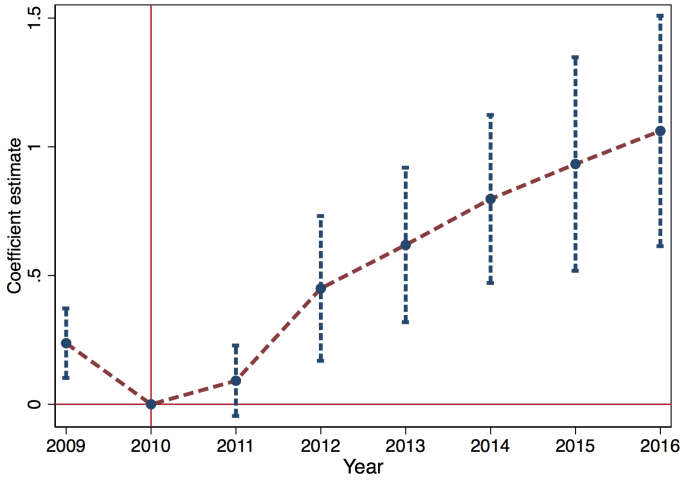
Panel B: Individual voluntary arrangements



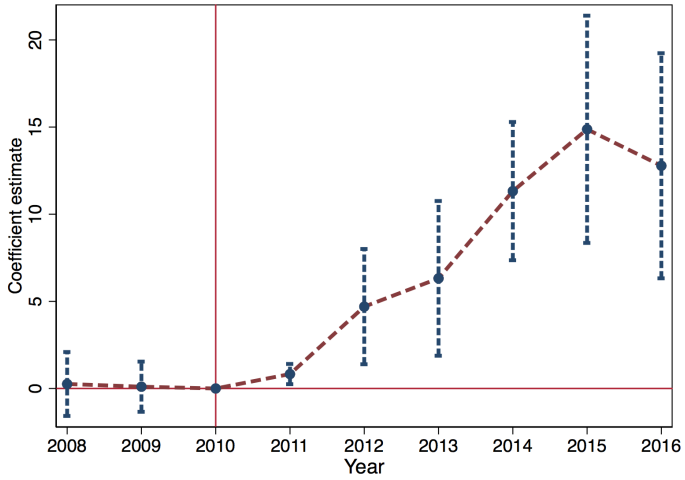
Notes: All dependent variables are measured as rates relative to the number of resident households in a district. The dependent variable in Panel A measures all individual new (not corporate) bankruptcy cases issued in a calendar year. Panel B focuses on all new so-called individual voluntary arrangements as an insolvency procedure that is typically used to restructure consumer loans; rent arrears can be included by require the permission of the landlord, which typically prefer to use court action.

Figure 7: Impact of change in reference rent on rate of residence in temporary accommodation and statutory homelessness

Panel A: Temporary accommodation



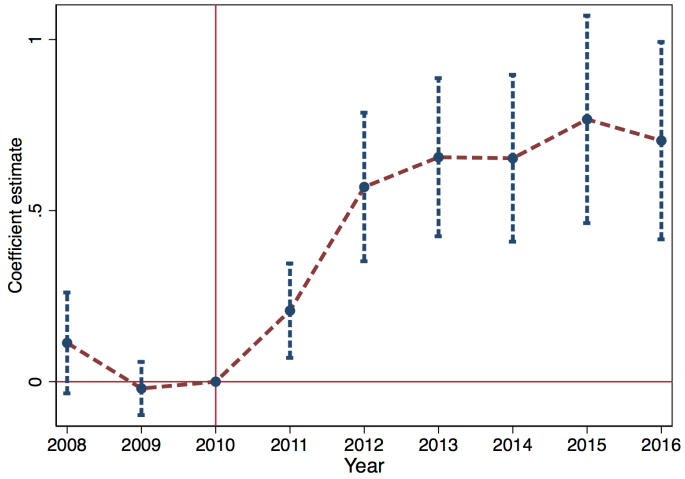
Panel B: Spending on hosting homeless in Hostels & BnB's



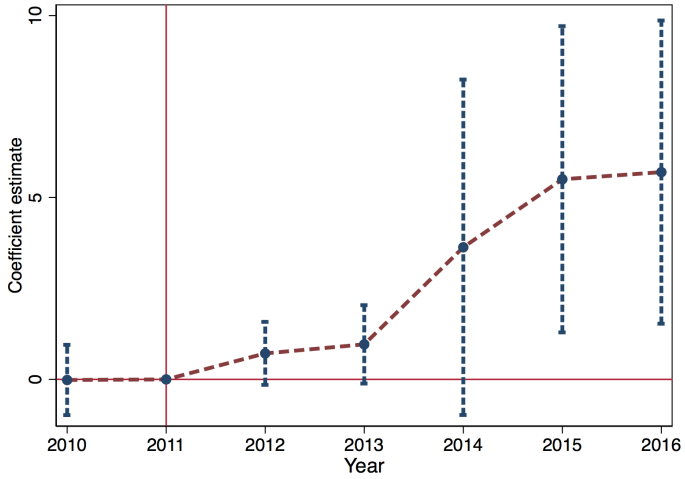
Notes: Figure plots results from regressions studying benefit sanctions arrivals in different quarters on support for Leave in the 2016 EU referendum at the ward level. All regressions control for local authority district fixed effects. 90% confidence bands obtained from clustering standard errors at the district level are indicated.

Figure 8: Impact of change in reference rent on measures of statutory homelessness

Panel A: Statutory homeless



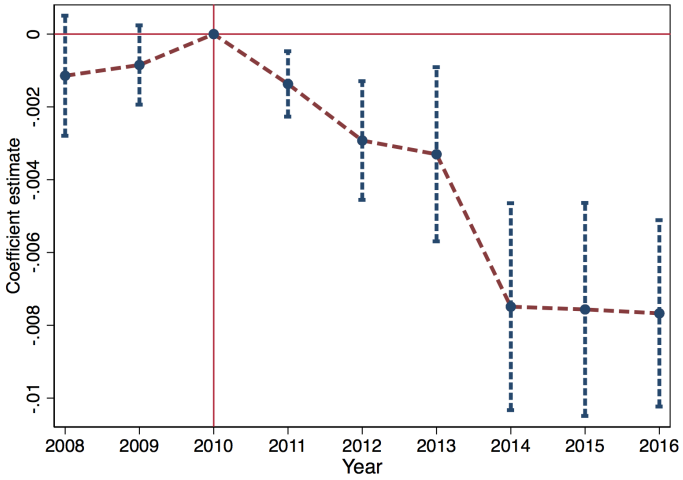
Panel B: Roughsleeping



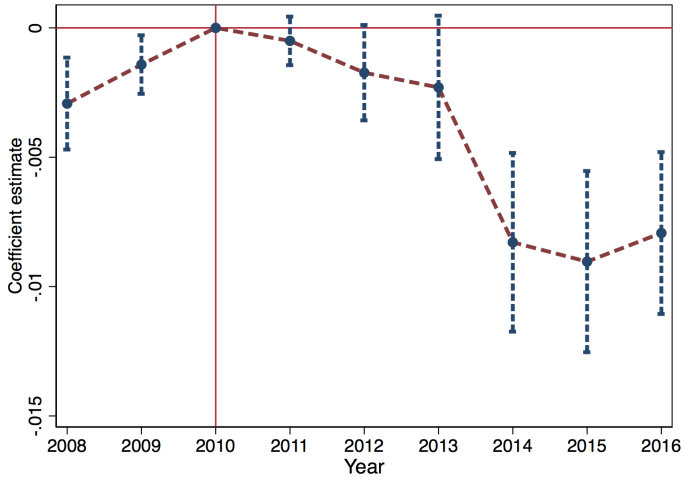
Notes: Figure plots results from regressions studying benefit sanctions arrivals in different quarters on support for Leave in the 2016 EU referendum at the ward level. All regressions control for local authority district fixed effects. 90% confidence bands obtained from clustering standard errors at the district level are indicated.

Figure 9: Impact of change in reference rent on measures of electoral registration rates - parliamentary electorate / voting age population

Panel A: % parliamentary electorate/ voting age population

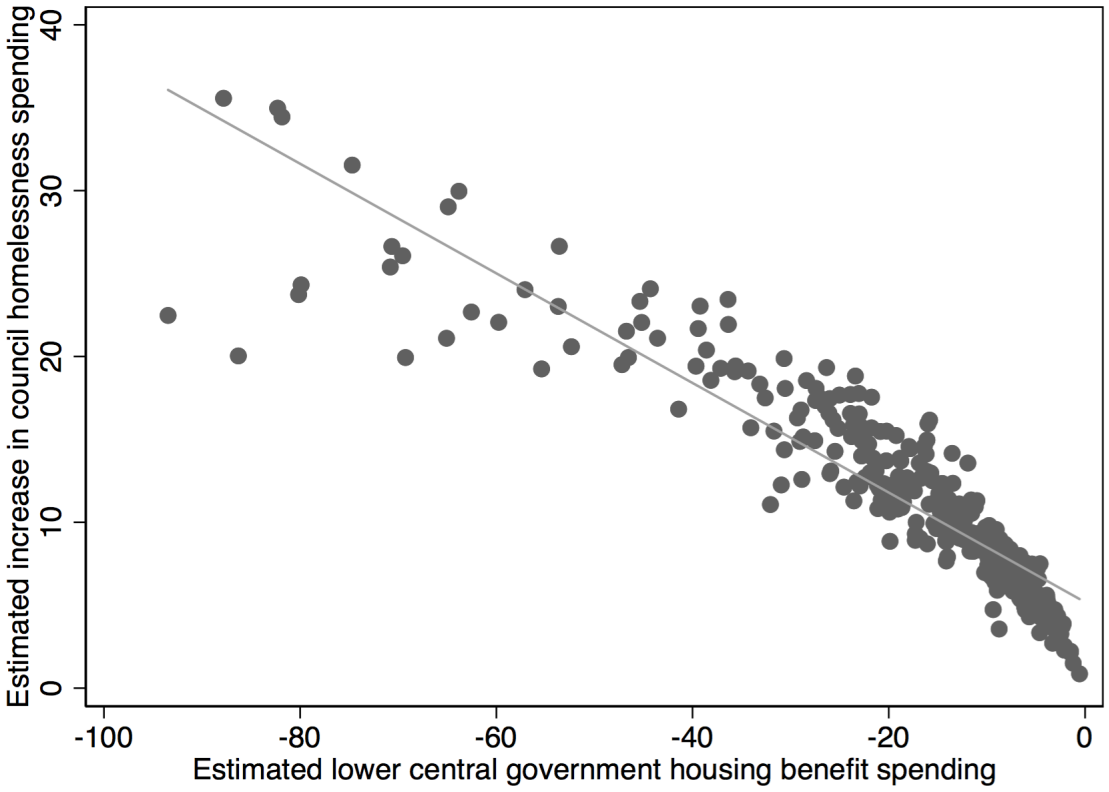


Panel B: % electorate/ voting age population



Notes: Figure plots results from regressions studying benefit sanctions arrivals in different quarters on support for Leave in the 2016 EU referendum at the ward level. All regressions control for local authority district fixed effects. 90% confidence bands obtained from clustering standard errors at the district level are indicated.

Figure 10: Cost- benefit analysis: Implied fiscal savings to central government due to lower housing benefit costs versus higher council spending for homelessness



Notes: Figure plots out the full empirical distribution of the projected fiscal savings per household due to lower housing benefit payments due to the cut to LHA on the horizontal axis. The vertical axis displays the corresponding estimated impact on overall council spending on homelessness and homelessness prevention per household.

Table 1: Impact of change in reference rent on eviction measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	main difference-in-difference				*exgenous treatment*		*matched*
Panel A: Possession claims due to rent arrears							
post × Official treatment measure	0.121***	0.038**	0.121***	0.148***			0.143*
	(0.020)	(0.015)	(0.020)	(0.021)			(0.075)
post × Projected treatment					0.131***		
					(0.029)		
post × Residualized treatment						0.071***	
						(0.017)	
Mean of DV	.546	.416	.517	.546	.617	.617	.661
Panel B: Actual repossessions							
post × Official treatment measure	0.073***	0.024**	0.059***	0.072***			0.083
	(0.015)	(0.011)	(0.011)	(0.013)			(0.061)
post × Projected treatment					0.072***		
					(0.022)		
post × Residualized treatment						0.035***	
						(0.012)	
Mean of DV	.368	.297	.336	.368	.409	.409	.44
Panel C: All private rented-sector eviction actions							
post × Official treatment measure	0.425***	0.123**	0.377***	0.525***			0.509*
	(0.073)	(0.048)	(0.060)	(0.075)			(0.281)
post × Projected treatment					0.465***		
					(0.108)		
post × Residualized treatment						0.244***	
						(0.062)	
Mean of DV	1.36	.999	1.25	1.36	1.55	1.55	1.69
Panel D: All social-rented rented-sector eviction actions							
post × Official treatment measure	-0.034	0.007	-0.022	-0.085*			-0.213
	(0.027)	(0.034)	(0.025)	(0.047)			(0.157)
post × Projected treatment					-0.069***		
					(0.027)		
post × Residualized treatment						0.035	
						(0.043)	
Mean of DV	2.64	2.46	2.67	2.64	2.85	2.85	3.01
Local authority districts	366	333	366	366	306	306	88
Observations	3293	2997	2195	3293	2753	2753	792
Include data after 2013	X	X		X	X	X	X
London included?	X		X	X	X	X	X
$C_{d,c,2010}$ trends				X			
Matched pair x Year FE							X
Quintile $S_d^{\text{predicted}}$ x Year FE						X	

Notes: All regressions include district- and year fixed effects. All dependent variables are measured as rates relative to the number of resident households in a district. The dependent variable in Panel A measures all Landlord possession claims raised. Panel B studies actual repossessions carried out by county court bailiffs. Panel C studies all private rented sector related eviction actions (including claims being launched, eviction notices being issued and actual repossessions). Panel D contrasts all social rented sector related eviction actions as a placebo outcome. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 2: Impact of change in reference rent on bankruptcies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	main difference-in-difference				*exgenous treatment*		*matched*
Panel A: Total individual bankruptcies							
post × Official treatment measure	0.155***	0.119*	0.127***	0.132***			-0.010
	(0.037)	(0.062)	(0.045)	(0.049)			(0.119)
post × Projected treatment					0.140***		
					(0.042)		
post × Residualized treatment						0.097*	
						(0.050)	
Mean of DV	6.01	6.27	6.54	6.01	5.95	5.95	5.67
Panel B: Individual voluntary arrangements							
post × Official treatment measure	0.097***	0.159***	0.041***	0.011			0.111**
	(0.019)	(0.028)	(0.015)	(0.016)			(0.054)
post × Projected treatment					0.061***		
					(0.022)		
post × Residualized treatment						0.003	
						(0.012)	
Mean of DV	2.62	2.73	2.63	2.62	2.59	2.59	2.56
Local authority districts	338	305	338	338	306	306	88
Observations	3041	2745	2027	3041	2753	2753	792
Include data after 2013	X	X		X	X	X	X
London included?	X		X	X	X	X	X
$C_{d,c,2010}$ trends				X			
Matched pair x Year FE							X
Quintile $S_d^{\text{predicted}}$ x Year FE						X	

Notes: All regressions include district- and year fixed effects. All dependent variables are measured as rates relative to the number of resident households in a district. The dependent variable in Panel A measures all individual new (not corporate) bankruptcy cases issued in a calendar year. Panel B focuses on all new so-called individual voluntary arrangements as an insolvency procedure that is typically used to restructure consumer loans; rent arrears can be included by require the permission of the landlord, which typically prefer to use court action. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 3: Impact of change in reference rent on council spending on temporary housing and homeless prevention

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	main difference-in-difference				*exgenous treatment*		*matched*
Panel A: Temporary accommodation							
post × Official treatment measure	0.532***	0.010	0.262*	1.152***			1.364**
	(0.177)	(0.166)	(0.153)	(0.306)			(0.576)
post × Projected treatment					0.698***		
					(0.238)		
post × Residualized treatment						0.652**	
						(0.276)	
Mean of DV	2.99	1.72	2.69	2.99	2.74	2.74	2.94
Panel B: Council spending on hostels and BnB's							
post × Official treatment measure	8.295***	0.781	3.824***	12.457***			6.345**
	(1.968)	(0.524)	(1.384)	(2.119)			(2.809)
post × Projected treatment					8.478***		
					(2.317)		
post × Residualized treatment						4.851***	
						(1.668)	
Mean of DV	9.83	4.11	7.58	9.83	11.5	11.5	13.9
Panel C: Total council spending on temporary housing							
post × Official treatment measure	17.536***	1.601	8.651***	31.967***			14.742***
	(3.700)	(1.151)	(2.699)	(4.699)			(4.917)
post × Projected treatment					21.509***		
					(4.005)		
post × Residualized treatment						19.791***	
						(4.658)	
Mean of DV	18.2	5.15	14.6	18.2	21.6	21.6	25.2
Local authority districts	366	333	366	366	306	306	88
Observations	3243	2947	2195	3243	2752	2752	792
Include data after 2013	X	X		X	X	X	X
London included?	X		X	X	X	X	X
$C_{d,c,2010}$ trends				X			
Matched pair x Year FE							X
Quintile $S_d^{\text{predicted}}$ x Year FE						X	

Notes: All regressions include district- and year fixed effects. All dependent variables are measured as rates relative to the number of resident households in a district. The dependent variable in Panel A measures the share of households housed in temporary accommodation by councils to prevent homelessness. Panel B focuses on council spending on overnight bed- and breakfast and hostel accommodation; Panel C focuses on total council spending for temporary accommodation. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 4: Impact of change in reference rent on homelessness and rough sleeping

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	main difference-in-difference				*exgenous treatment*		*matched*
Panel A: Statutory homelessness							
post × Official treatment measure	0.563***	0.336**	0.450***	0.761***			0.772**
	(0.121)	(0.162)	(0.113)	(0.173)			(0.316)
post × Projected treatment					0.381***		
					(0.104)		
post × Residualized treatment						0.330***	
						(0.124)	
Mean of DV	4.27	4.17	4.39	4.27	2.56	2.56	2.94
Panel B: Rough sleeping street counts							
post × Official treatment measure	3.310**	1.918**	0.847**	5.585**			3.065
	(1.539)	(0.877)	(0.385)	(2.644)			(3.040)
post × Projected treatment					4.264**		
					(1.778)		
post × Residualized treatment						4.079*	
						(2.408)	
Mean of DV	8.56	7.23	6.79	8.56	8.42	8.42	8.66
Include data after 2013	X	X		X	X	X	X
London included?	X		X	X	X	X	X
$C_{d,c,2010}$ trends				X			
Matched pair x Year FE							X
Quintile $S_d^{\text{predicted}}$ x Year FE						X	

Notes: All regressions include district- and year fixed effects. The dependent variable in Panel A measures the share of households that are classified as homeless and in priority need by councils. The dependent variable in Panel B is the total number of rough sleepers estimated or physically verified through street counts by councils. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 5: Impact of cut to local housing allowance on electoral registration, turnout and support for Leave in the 2016 EU referendum

	(1)	(2)	(3)	(4)	(5)	(6)
	main difference-in-difference			*exgenous treatment*		*matched*
Panel B: EU referendum electorate						
Official treatment measure	-0.992***	-0.757***	-0.992***			-1.916***
	(0.309)	(0.282)	(0.309)			(0.649)
Projected treatment				-1.431***		
				(0.406)		
Residualized treatment					-1.090***	
					(0.400)	
R2	.792	.685	.792	.827	.832	.813
Mean of DV	91.3	92.4	91.3	91.1	91.1	89.9
Observations	365	332	365	304	304	79
Panel B: Turnout						
Official treatment measure	-1.824***	-2.057***	-1.824***			-1.928***
	(0.247)	(0.281)	(0.247)			(0.517)
Projected treatment				-1.871***		
				(0.500)		
Residualized treatment					-0.743**	
					(0.348)	
R2	.746	.749	.746	.753	.805	.837
Mean of DV	73.8	74.2	73.8	74.3	74.3	74.5
Observations	365	332	365	304	304	79
Panel C: % support for Leave						
Official treatment measure	2.191***	1.784***	2.191***			1.261
	(0.508)	(0.496)	(0.508)			(1.251)
Projected treatment				2.773***		
				(0.537)		
Residualized treatment					3.134***	
					(0.526)	
R2	.774	.769	.774	.742	.777	.797
Mean of DV	53.2	54.6	53.2	54.5	54.5	53.4
Observations	365	332	365	304	304	79
NUTS2 FE	X	X	X	X	X	X
Immigration controls ?	X	X	X	X	X	X
London included?	X		X	X	X	X
$C_{d,c,2010}$			X			
Quintile $S_d^{\text{predicted}}$					X	

Notes: All regressions include NUTS2 level shifters. The dependent variable in Panel A is the official electorate in the 2016 EU referendum divided by the voting age population in 2016; in Panel B, the dependent variable is official turnout relative to the official electorate in the EU referendum; the dependent variable in Panel C is the % support for Leave among those that turned out. All regressions include also controls for the level and changes in migration measured as the share or the change in the share of the resident population between 2001 and 2011 census relative to 2001 coming from EU member countries as of 2001, the newly joined Accession EU member countries that the EU from 2004 onwards and from the rest of the world. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Online Appendix

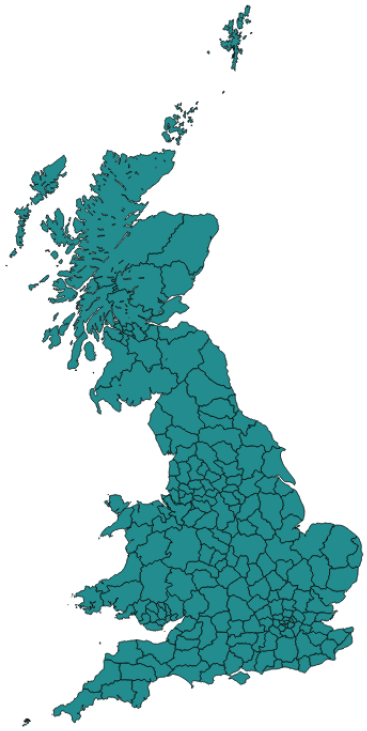
"Housing insecurity, homelessness and populism: Evidence from the UK"

For Online Publication

Thiemo Fetzer, Srinjoy Sen and Pedro CL Souza

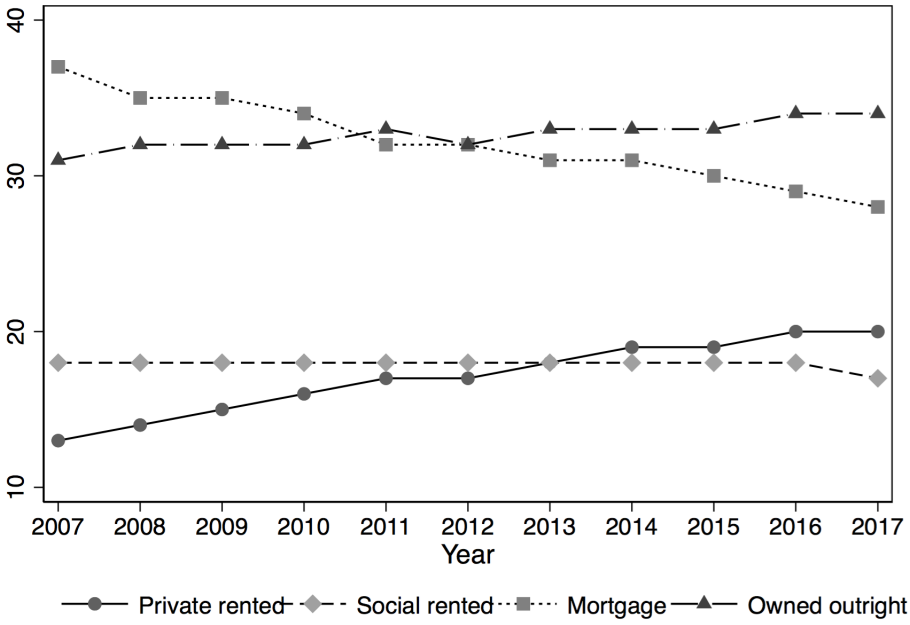
December 7, 2019

Figure A1: Broad Rental Market Areas across the United Kingdom



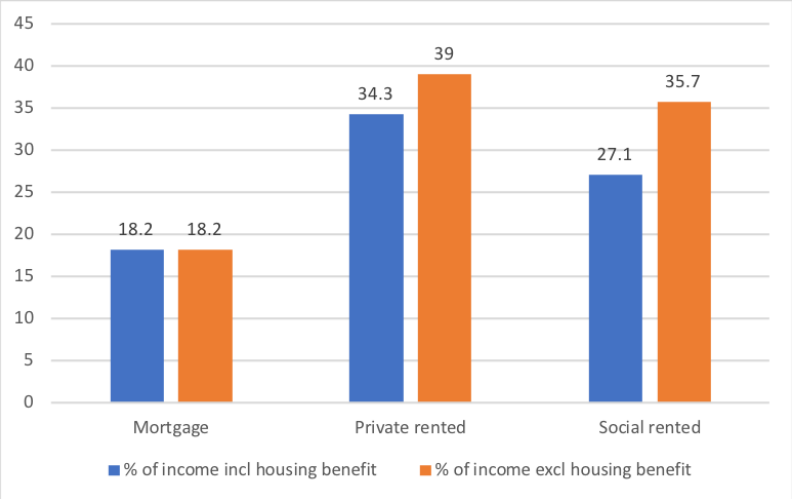
Notes: This figure presents the geographic shapes representing the UK's Broad Rental Market Area's at which level local housing allowance rates are determined based on rental market surveys by the UK governments Valuation Office Agency. This serves as key determinant of the value of housing benefits.

Figure A2: Private rental market development and home ownership in the UK over time



Notes: This figure presents data from the Office of National Statistics measuring the share of households living in the private rented sector versus the share of households living in owner occupied housing (owned outright or with mortgage).

Figure A3: Affordability and the impact of housing benefit across the market segments

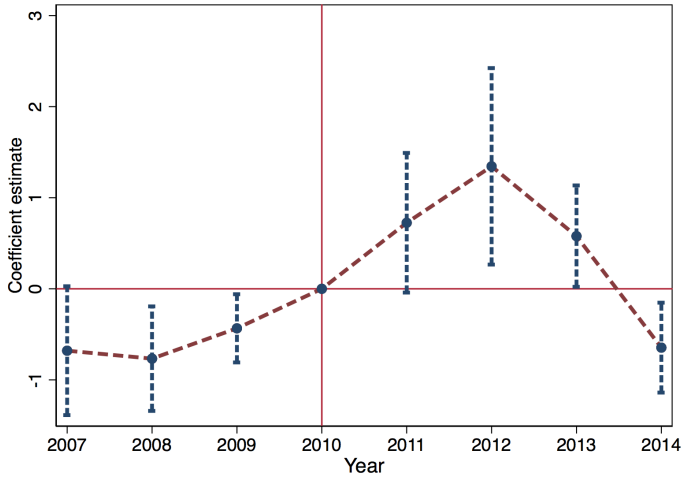


Notes: This figure presents data from the Office of National Statistics measuring the share of households living in the private rented sector versus the share of households living in owner occupied housing (owned outright or with mortgage).

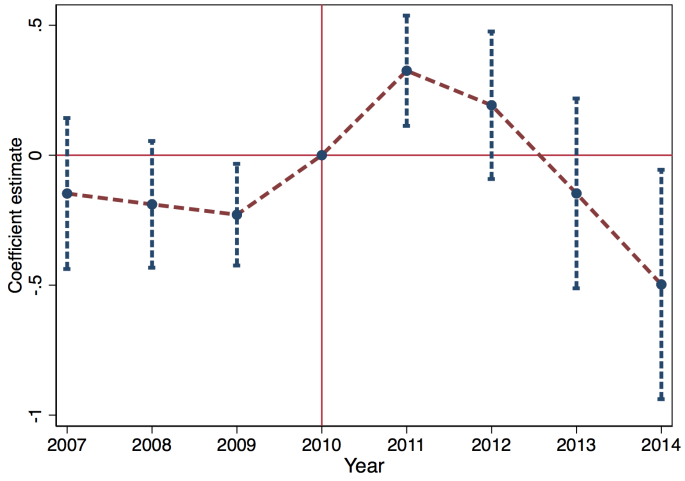
Figure A4: Sample images of estate agents explicitly excluding benefit claimants

Figure A5: Impact of housing benefit cut on crime

Panel A: Theft from person homeless



Panel B: Burglaries



Notes: All dependent variables are measured as rates relative to the number of resident households in a district. The dependent variable in Panel A measures the reported cases of theft from individuals; Panel B focuses on burglaries. All regressions control for local authority and year fixed effects. 90% confidence bands obtained from clustering standard errors at the district level are indicated.

Table A1: Mean-squared prediction error of reference rents in March 2011

Model	Shared	1 Bedroom	2 Bedrooms	3 Bedrooms	4 Bedrooms
Lag 1	36.89	177.48	649.02	1,934.47	837.85
Lag 6	10.16	88.42	307.70	909.56	354.64
Lag 1 and 6	36.06	220.22	801.37	2,433.36	2,019.63
FE	5.50	51.41	208.37	659.78	37,794.43
Lag 1 + FE	15.42	82.59	350.64	1,100.74	62,437.64
Lag 6 + FE	7.30	58.36	193.50	985.32	**277.47**
Lag 1 and 6 + FE	**4.43**	294.49	1,161.64	2,293.09	3,328.65
Linear trends	10.49	**48.73**	**187.31**	**544.33**	38,184.91
Lag 1 + Linear trends	18.63	136.15	554.68	1,746.28	29,149.53
Lag 6 + Linear Trends	65.70	381.08	1,274.73	4,287.36	2,633.92
Lag 1 and 6 + Linear Trends	9.34	270.75	1,136.40	3,884.34	2,596.28

Note: out-of-the-sample mean-square error (MSE) of the interquantile difference (50th to 30th quantile) in reference rents in March 2011, with data between June 2010 and February 2011. "Model" column contains the elements of the forecasting model that were used in the forecasting regression. The model with lowest MSE is highlighted in bold.

Table A2: Impact of change in reference rent on housing benefit spending per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	main difference-in-difference				*exgenous treatment*		*matched*
Panel A: log(Housing benefit per capita)							
post × Official treatment measure	-0.014***	-0.014***	-0.010***	-0.017***			-0.014*
	(0.004)	(0.003)	(0.003)	(0.005)			(0.008)
post × Projected treatment					-0.014***		
					(0.003)		
post × Residualized treatment						-0.009**	
						(0.004)	
Mean of DV	6.6	6.52	6.57	6.6	6.58	6.58	6.62
Include data after 2013	X	X		X	X	X	X
London included?	X		X	X	X	X	X
$C_{d,c,2010}$ trends				X			
Matched pair x Year FE							X
Quintile $S_d^{\text{predicted}}$ x Year FE						X	

Notes: All regressions include district- and year fixed effects. The dependent variable in measures the log value of housing benefit spending per household in a district and year. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A3: Impact of change in reference rent on crimes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	main difference-in-difference				*exgenous treatment*		*matched*
Panel A: Theft from person							
post × Official treatment measure	0.975**	0.119	1.357**	2.083***			0.447
	(0.431)	(0.146)	(0.578)	(0.513)			(0.727)
post × Projected treatment					1.589***		
					(0.398)		
post × Residualized treatment						1.663***	
						(0.303)	
Mean of DV	4.17	2.66	4.24	4.17	4.41	4.41	3.94
Panel B: Burglaries							
post × Official treatment measure	0.113	-0.227	0.267**	0.219			-0.709
	(0.150)	(0.272)	(0.135)	(0.262)			(0.933)
post × Projected treatment					0.183		
					(0.148)		
post × Residualized treatment						0.382**	
						(0.182)	
Mean of DV	11.8	10.7	12.1	11.8	12.2	12.2	12.7
Panel C: Bodily harm							
post × Official treatment measure	0.057	-0.150	-0.010	0.221			-0.210
	(0.168)	(0.314)	(0.175)	(0.222)			(1.303)
post × Projected treatment					0.193		
					(0.135)		
post × Residualized treatment						0.135	
						(0.202)	
Mean of DV	19.6	18.7	19.5	19.6	19.6	19.6	18.6
Include data after 2013	X	X		X	X	X	X
London included?	X		X	X	X	X	X
$C_{d,c,2010}$ trends				X			
Matched pair x Year FE							X
Quintile $S_d^{\text{predicted}}$ x Year FE						X	

Notes: All regressions include district- and year fixed effects. All dependent variables are measured as rates relative to the number of resident households in a district. The dependent variable in Panel A measures the reported cases of theft from individuals; Panel B focuses on burglaries while Panel C studies cases of bodily harm. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A4: Impact of housing benefit cut on electoral registration coverage rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	main difference-in-difference				*exgenous treatment*		*matched*
Panel A: Parliamentary electors							
post × Official treatment measure	-0.004***	-0.003**	-0.002**	-0.004**			-0.002
	(0.001)	(0.001)	(0.001)	(0.002)			(0.004)
post × Projected treatment					-0.004***		
					(0.001)		
post × Residualized treatment						-0.002	
						(0.001)	
Mean of DV	.924	.936	.938	.924	.923	.923	.917
Panel B: Local government electors							
post × Official treatment measure	-0.004***	-0.003**	-0.000	-0.003			0.002
	(0.001)	(0.001)	(0.001)	(0.002)			(0.004)
post × Projected treatment					-0.003***		
					(0.001)		
post × Residualized treatment						-0.001	
						(0.002)	
Mean of DV	.948	.954	.961	.948	.949	.949	.943
Include data after 2013	X	X		X	X	X	X
London included?	X		X	X	X	X	X
$C_{d,c,2010}$ trends				X			
Matched pair x Year FE							X
Quintile $S_d^{\text{predicted}}$ x Year FE						X	

Notes: All regressions include district- and year fixed effects. The dependent variable in Panel A measures annually the share of the registered voters eligible to vote in Westminster elections divided by the voting age population in a district and year. Panel B focuses on local government electors as a share of the voting age population as a broader measure of the electorate. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.