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JEL Classification: H2, H3, H5, P16, D72

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

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

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February 25, 2020

Abstract

Homelessness and precarious living conditions are on the rise across much of the Western world. This paper exploits quasi-exogenous variation in the affordability of rents due to a cut to rent subsidies for low income benefit in the United Kingdom in April 2011. Using individual-level panel data as motivating evidence, we document that individuals exposed to the cut were significantly more likely to build up rent arrears and face evictions; further, they were more likely to endogenously attrit from the panel. Using comprehensive district-level administrative data, we show that the affordability shock caused a significant increase in: evictions; individual bankruptcies; property crimes; insecure temporary housing arrangements; statutory homelessness and actual rough sleeping with most notable rise in statutory homelessness among families with children. We also note political effects: the cut reduces electoral registration rates, and is associated with lower turnout and higher support for Leave in the 2016 EU referendum, likely capturing a change in composition of those that engage with democratic processes. Lastly, we estimate that the fiscal savings were much lower than anticipated: for every pound saved by the central government, council spending to meet statutory obligations for homelessness prevention increased by 53 pence, rendering the cost savings much smaller than expected.

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.[1] Those boom-and-bust cycles bring about drastic changes to the lives of millions and to urban landscapes alike: it is estimated that the subprime mortgage crisis of 2008 alone led to close to one million evictions in the United States (Desmond et al., 2018). Since then, an erosion of affordability contributed both to a rapid expansion of the private rental market as well as growing financial burden on renters. In the United States, the United Kingdom and the European Union the share of households living in the rented properties and facing market rents expanded by 5, 7 and 9.5 percentage points, respectively, since 2007. The share of households renting that spend more than 40% of the disposable income on rent in the European Union increased from 22.5% in 2005 to 28.0% in 2018. In the United Kingdom, 37.3% of tenants are housing-cost overburdened. Among them, 40% are estimated to be at risk of poverty.[2] These structural shifts in housing markets arguably coerced families into the duress of insecure living conditions that likely have significant long-run social and economic costs.

Yet, these developments also have significant fiscal implications too. Across the EU, expenditure on allowances to help low-income households cover the cost of rent increased from 54.5 to 80.8 billion Euros per year between 2009 and 2015 – while capital spending on new social housing has declined from 48.16 to 27.5 billion.[3] Much of housing assistance constitutes a simple transfer of wealth from taxpayers to property owners, which may further increase inequality. The growing fiscal shadow of such transfers, often directly related to underlying rental market conditions, increase political pressures to reform or reduce the generosity of housing allowances.[4]

[1]See Mian and Sufi (2009, 2014) on the subprime crisis, Knoll et al. (2017) on global house price cycles and Jordà et al. (2015) on the economic cost of such debt-fueled housing price cycles.

[2]Data from the US Census (https://www.census.gov/topics/housing.html), Eurostat (https://ec.europa.eu/eurostat/statistics-explained/index.php/Housing_statistics) and the UK's Office for National Statistics (https://www.ons.gov.uk/economy/inflationandpriceindices/articles/ukprivaterentedsector/2018).

[3]Data from Eurostat, General government expenditure by function, https://appsso.eurostat.ec.europa.eu/nui/show.do?dataset=gov_10a_exp&lang=en

[4]The 2020 budget in the US, for example, threatens to significantly cut the budget for the

This paper provides a careful analysis of both the intended and unintended effects of a housing assistance cut in the United Kingdom. We leverage on a deep cut to the UK's housing benefit which, like many similar assistance programs across advanced economies, aims to help low-income households pay for the cost of renting in the private rented sector.[5] From April 2011 onwards, two significant cuts to housing benefit became effective. First, the local housing allowance (LHA), which determines housing benefit payments, was drastically cut back: before April 2011, it was generous enough to cover up to the 50th percentile of rents within a local rental market and dwelling type. From April 2011 onwards, that rate was lowered such that only the 30th percentile of rents are affordable for housing benefit recipients. This cut affected all new claimants and many existing claimants immediately, and eventually affected all claimants the latest by 2012. The second cut was implemented simultaneously and immediately affected all existing housing benefit claimants that were paying a rent below their applicable LHA rate. Prior to April 2011, those claimants could keep the difference between the LHA rate and their actual rent up to at most £15 per week. These so-called *excess* payments were cut effective immediately. The two reforms together affected the near universe of housing benefit claimants in the private rented sector: an estimated 936,000 out of around 1 million claimants. This captures 5.1 per cent of all households in the UK or, around 25 per cent of all households renting in the private sector. The *average exposure* amounted to an annual housing benefit reduction of around £596, rising to significantly above £2000, on average, in some parts of London.

Using district and individual-level data, we use the different incidence of the cut across districts to trace out their causal effects. We study a broad set of outcomes that allows us to shed some light on the anticipated and unanticipated social and economic consequences. The cut, as we confirm with individual-level data, led to a significant increase in rent arrears among renters. In turn, evictions of private

Department of Housing and Urban Development (HUD), which is a main provider of housing assistance in the US.

[5]The rented sector in the UK is segmented between social rented housing and private rented housing. Social rented sector tenants have not been affected by the reform as we detail in the context section. Many advanced economies have rent assistance programs as part of their welfare setup, see for example the OECD's Affordable Housing database http://www.oecd.org/social/affordable-housing-database/.

sector tenants rose sharply, on average by 22.6 per cent. Individuals that find themselves at risk of becoming homeless due to an eviction can turn to their local councils for support. These, upon evaluating individual cases, may assess that an individual or household is (at risk of becoming) "unintentionally homeless", in which case councils owe a statutory obligation to provide housing to prevent homelessness. On average, the cut has caused a 18.9 per cent rise in councils providing temporary accommodation to vulnerable households. Statutory homelessness increased, on average, by at least 10.2 per cent due to the cut. Administrative data allow us to decompose this increase. We find that it is particularly driven by young families with children, single parents and/or households with a physical disability or a mental health condition who became homeless due to rent arrears and being evicted. Using, albeit imperfect street counts and estimates on prevalence of rough sleeping, we document that in the wake of the housing benefit cut, the prevalence of rough sleeping rose sharply in districts most exposed to the cuts.

A narrow economic accounting would suggest that the housing benefit cut was indeed successful in lowering the direct fiscal costs to the government: across districts, on average, spending on housing benefit declined by around £16 per resident households per year. Yet, these savings mask significant indirect financial costs and longer-term social costs. As councils have to provide emergency accommodation to meet their legal obligations to prevent homelessness this has led to many councils renting properties from the private rented sector – *at market rates* – to provide temporary accommodation. Not surprisingly, council spending on temporary accommodation and overnight shelters increased sharply, shooting up by, on average, 94.9 per cent owing to the cut. A significant driver of the cost increase for temporary accommodation (accounting for around 36 per cent of the total increase) is due to councils having to resort to costly overnight accommodation provided in hostels and bed-and-breakfasts. 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.

Throughout, our analysis, which mostly leverages various difference-in-difference strategies, we do not find diverging pre-trends and note sharp jumps in districts more affected in outcome measures immediately relevant and impacted by the cut in 2011 and particularly 2012. The nature of the reforms and the underlying identifying variation provide us with relaxed identifying assumptions allowing us to interpret the effects causally. We also document a number of additional results and point to some notable null results that help rule out a host of alternative mechanisms. First, we observe that in districts more exposed to the cut, there is a sizeable rise in individual insolvency and bankruptcies. Second, using data on crime, we document that in districts most exposed to the cut, property crimes and thefts increased timely but temporarily. Third, we do not find evidence of systematic divergence or jumps post-treatment in rents or property prices that would be consistent as providing an alternative explanation for our results. Fourth, we do not document systematic changes in both internal- and international migration, which could confound the results. Fifth, we do not find any evidence suggesting more exposed districts saw notable changes in economic activity rates or unemployment.

Lastly, we also provide some evidence that suggests that the cuts may have eroded democratic participation in the UK. Using annual data on electoral registrations measuring electorate sizes for both parliamentary and local elections, we observe that registration rates decreased pronouncedly in districts more exposed to the cut. The decline in electoral registration is closely linked with the increase in the share of households in temporary accommodation. 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 was significantly lower in districts more affected by the cut. Similarly, turnout is drastically lower. We estimate that a one standard deviation higher level of exposure to the cut in a district is associated with between 1-3 percentage point higher level of support for Leave. This effect is likely driven by the composition of those that did turn out. The findings suggest that there are further indirect margins through which welfare cuts, that increase housing insecurity, may erode democratic participation of particularly vulnerable demographic groups.

This paper, to the best of our knowledge, is the first to provide a comprehensive analysis of the intended and unintended socio-economic effects stemming from a nationwide drastic cut to a rent assistance program. Our paper documents that the housing benefit cuts have caused a notable increases in housing insecurity; naturally, the literature that studies the long-run social- and economic implications of housing insecurity provides an important backdrop to these findings. Among others, this literature has documented that evictions have pervasive negative impacts on consumption and access to credit (Humphries et al., 2019), mental and physical health (Burgard et al., 2012; Fowler et al., 2015), achievement of children (Chyn, 2018), and labor markets outcomes (Desmond et al., 2016). Desmond and Shollenberger (2015) document that households that were evicted tend to subsequently move to lower quality neighbourhoods. Desmond and Kimbro (2015), using a matching design, studies the impact of evictions on low income mothers in urban settings finding that mothers that had experienced being evicted are more likely to suffer from depression, have worse health outcomes and more stress.[6] Much less work has been done specifically regarding homelessness – a potential consequence of evictions or the most extreme form of housing insecurity. Honig and Filer (1993) explores what drives the cross-city variation in homelessness across the US. Phinney et al. (2007) suggests that drug use, mental and health problems are associated with homelessness, while Evans et al. (2016) suggests that homelessness prevention measures in Chicago may be quite cost effective. This literature suggests that the likely social cost due to the housing benefit cuts driven increase in housing insecurity may may be much larger than what we can currently account for.

Our paper also contributes to the literature studying the social- and economic effects that housing assistance programs may have. In that literature, Galiani et al. (2015) find that reducing housing subsidies increases exposure to poverty. Eriksen and Ross (2015), studying housing voucher programs, finds no evidence suggesting that expanding housing benefits affect overall rental prices, but suggest that recipients use more generous vouchers to move to more expensive properties. A separate strand of the literature showcases that housing allowance programs are unlikely to tackle the cause of the underlying symptom: the relative inelastic supply of affordable homes in many urban agglomerations. Alternative policies, such as rent control, may only provide temporary relief. Diamond et al. (2019) shows

[6] See Desmond and Gershenson (2017) for more work on who is getting evicted.

the rent control in San Francisco reduced evictions in the short term, but also led to a loss in housing supply undermining the short term effects of this policy (see also Choon-Geol Moon and Stotsky, 1993 on the effects of rent control). Brewer et al. (2019), which is closely related, highlight that the incidence of the cuts we study in this paper is ultimately on the side of the tenants, owing to the lack of effective renter protection and an overall regulatory environment favoring landlords. Whether public housing provision may undermine incentives to work is carefully studied in Van Dijk (2019), exploiting a national lottery for public housing units in Netherlands. They find that the average move into public housing negatively affects labor market outcomes and is associated to higher benefit receipt. Jacob and Ludwig (2012) find similar negative labor-supply effects in the US. The negative labor-supply effects that a generous system of housing assistance may have is likely an important factor that may be used to justify cuts to housing assistance. We do not find any strong evidence suggesting that the cut has contributed to lower unemployment or higher economic activity rates in the case of the UK.

Lastly, we contribute to the growing body of work on populism[7], with some new work studying the role that housing tenure may have (see Ansell, 2019 for a review of the literature on the political causes and consequences of homeownership). Adler and Ansell (2020) study the relationship between spatial sorting and populism, while Ansell (2014) finds that homeowners experiencing house price appreciation become less supportive of redistribution and welfare. Gyongyosi and Verner (2018), exploiting an exchange-rate shock in the wake of the financial crisis (see also Verner and Gyongyosi (2018)) and find that drastically higher debt burdens can account for up to 1/3 of the increase in populist party vote shares in Hungary. In this paper, we highlight that the rise in insecure and temporary housing conditions due to the housing benefit cut is associated with worsening electoral registration rates and lower turnout. This has affected the 2016 EU referendum, skewing the district level results in favor of Leave due to the composition of the eligible electorate and those that turn out. This is a distinct channel from Fetzer (2019), who documents that exposure to other welfare cuts from 2013 onwards, can explain an increase in protest voting and direct support for Leave.

[7]See Guriev and Papaioannou (2020) for an extensive review of the literature.

This paper proceeds as follows. In Section 2 we present the context and our data sources. Section 3 provides motivating individual-level evidence and shows the differential attrition in survey-based measures induced to the relocation associated with the effects of the housing allowance cut. Section 4 outlines our main empirical strategy at the district level and the sources of variation, followed by the main district-level results in Section 5. We present our back-of-the-envelope cost-benefit analysis in Section 6. Section 7 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 A1 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 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 with a mortgage has drastically declined from 37% in 2007 to 28% in 2017.

A predominant issue in the UK that is common across many countries is a lack of affordable housing. House prices have accelerated faster compared to incomes, resulting in a worsening affordability – despite record low interest rates. This dynamic, coupled with a decline in social housing, is pushing more households into the private rented sector. The increase in demand, with an overall inelastic supply, is also affecting the affordability of rents. 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 A2 displays the impact that housing benefit has on affordability across the main market segments. In the private rented sector, households spend, on average, 39% of their disposable income on housing costs prior to housing benefit. Housing benefit is reduces this to 35%.[8] In this paper, we focus on a reform that cut housing benefit in the private rented sector – but not the social rented sector – providing a natural placebo. We next describe how housing benefit is computed and discuss the cuts we study in this paper.

2.2 Housing Benefit and 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. It is the second biggest item in the Department for Work and Pensions' (DWP) 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. The generosity of housing benefit is determined by the so-called Local Housing Allowance (LHA), which was introduced in 2008. It provides 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. Tenants eligible for housing benefit can claim a benefit award up to the LHA.[9] Prior to April 2011, within a BMRA, the LHA for different sized properties was calculated with reference to an estimate of a BRMA's median rent. To estimate this median, 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 192 BRMA's and across five main property size categories across the United Kingdom. This data is used to

[8] In the social rented sector, the housing cost burden is lower to start with at around 35.7% of disposable income, with housing benefit being relatively more generous lowering the cost of housing to around 27.1%.

[9] The main types are a single room in shared accommodation, a 1, 2, 3 or 4 bedroom flat. BRMA's are defined as areas in 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.

estimate the BRMA and property-type specific median rent, defining an area's and property type's Local Housing Allowance.

For the purpose of the analysis we mostly rely on district-level data. Districts are the administrative areas responsible for much administration of benefits and for local housing policy. The BRMA's do not map into any existing administrative boundaries. Yet, most underlying measures are valid and accurate at the local authority district level, as they are constructed by the DWP using the confidential individual-level claimant database.

2.3 Housing benefit cuts

We focus on two cuts that were introduced simultaneously and affected the vast majority of housing benefit claimants. First, we exploit a reform that saw a change in how LHA rates are computed. 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. For around 76% of housing benefit claimants, this implied a significant cut to their financial support to pay rent. The cut was effective immediately for all new claimants and for all existing claimants whose circumstances may have changed triggering a reassessment. A change of circumstance may arise due to a change of the income, employment, disability status or an individual's family situation. For the other existing claimants whose circumstances did not change, the reform became effective gradually. The exact date depended on an individual claimant's last claim reassessment date or claim anniversary in the year prior to April 2011. By default, LHA awards are updated at least once a year, implying that the stock of existing claimants will have been affected the latest by December 2012. The bulk of claimants were treated earlier, though we do not know the exact date.[10]

The second cut that was simultaneously introduce affected nearly half of all housing benefit claimants immediately. Prior to April 2011, claimants whose rent was slightly lower than the housing benefit award, could keep the difference,

[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.

capped at £15 per week. Around 43% of all claimants were benefiting from this excess payment, amounting to, on average, £10 per week. They saw a notable and immediate cut of their housing benefit award from April 2011.[11] These two reforms mark the most distinct and sizable shocks that sharply affected the bulk of claimants. Yet, since 2011, there were a few additional notable reforms, which had a much more gradual impact. From April 2012, the LHA rates were computed only once a year rather than monthly. From April 2013, the decoupling of LHA rates from local rental markets was initiated by capping the LHA uprating to the national increase in the consumer price index. From April 2016 onwards, LHA rates were de-facto frozen for four years. As we will show, all results are essentially carried when restricting the sample to the period up to 2013. This implies that we need not worry about the subsequent reforms which may have amplified the reforms we focus on in this paper. Further, this way we steer clear of potential concerns about effects being compounded by subsequent welfare reforms from April 2013 onwards studied in Fetzer (2019).

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

2.4 Official impact estimates

The responsible Department for Works and Pension (DWP) has published in late 2010 an Economic Impact Assessment of the proposed reforms. For that purpose, the DWP constructed, using the detailed and confidential individual-level claimant count database, the expected economic effect of the cuts on claimants. To measure the projected impact of the cut in reference rent from covering the median to the 30th percentile, the DWP computed the affected number of claimants $C_{d,c,t}^{\text{percentile}}$ living in district d and property type c at time t. Further, they provide an estimate of the average financial loss, $L_{d,c,t}^{\text{percentile}}$ that is expected from the shift in LHA rates moving from the 30th or 50th percentile relative to the individual level

[11] There were two smaller reforms that became effective from April 2011 that affected only a relatively small number of households. Prior to April 2011, there were housing allowance rates computed also for five bedroom properties, essentially benefiting very large families. This five bedroom rate was removed with claimants being eligible at most to claim the four bedroom rate. Further, maximum housing allowance rates were introduced with rates for a shared room, 1-bedroom, 2-bedroom; 3-bedroom and 4-bedroom capped at £250, £250, £290, £340 and £400 per week respectively, from April 2011. These reforms only affected a very small share of claimants.

rent. Overall, it was estimated that 774,970 households would lose a part of their housing benefit – among a total case-load of nearly a million individual cases.

To compute the impact of the loss in the excess the official impact assessment computed the number of cases $C_{d,c}^{\text{Excess}}$ who received housing benefit in excess of the rent they were actually paying, along with the average amount of the excess amount, $L_{d,c,t}^{\text{Excess}}$. Again, both figures are provided in the official impact assessment for each district d and property type c. A total of 438,130 cases were estimated to be affected by the removal of the excess. The combined treatment was estimated to affect 936,960 households, or nearly 92% of all claimants of housing benefit in the private rented sector.

We leverage the information from the ex-ante impact assessments, to construct a treatment exposure measure at the district level:

$$S_d^j \;=\; \sum_c L_{d,c}^j \times C_{d,c,\text{baseline}}^j \quad \text{for} \quad j \in \{\text{Percentile}, \text{Excess}\}$$

For the empirical exercises, we normalize the above S_d^j by the number of resident households at baseline. We also normalize the dependent variables by the (time-varying) population levels or by the number of households living in an area.[12]

Figure 1 provides a visualization of the combined and normalized shock measures.[13] Panel A displays the share of households affected by either reform across the 366 districts for which data from the impact assessments is available. On average, around 5.1 percent of all households were impacted by the reform. Panel B presents the distribution of the average financial loss per loser across districts. On average, households affected by the reform were expected to lose £596 per year. In 14 districts, the average expected losses per affected household exceeds £1,000 per year. This still masks significant heterogeneity as the losses also strongly depend on the claimant's housing situation: across districts the expected financial loss

[12]Results are robust to alternative functional forms, alternative normalization or estimating the main regression with measures in levels. These are available upon request

[13]Appendix Figures A3, A4 and A5 provide the corresponding maps with the two separate elements of the April 2011housing benefit cuts broken out individually. Appendix Figure A6 highlights the expected difference in rents between the 30th and 50th percentile for three different property types across districts.

varies in the 1st and 99th percentile from £260 - £1,612 per year for claimants living in 1-bedroom flats to between £364 - £3,900 for claimants living in 3-bedroom flats. In Camden in North London, the average loss per affected household was estimated to be £ 2,258 per year. The cuts are economically sizable when comparing them with the median household disposable income across the UK, which in 2010 stood at £24,400. In Panel C, we present the variation that is implied in the housing benefit cut upon normalizing the estimated impact of the shock by the total number of households. On average, the ex-ante assessments suggest that housing benefit spending would decline by £28 per resident household and year.

The maps 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. As we will see, our results are quite robust to dropping London. The cuts we study afford us with very relaxed identifying assumption for our difference-in-difference exercise. In section 4, we provide more detail around the source of the identifying variation, the identifying assumptions and potential threats to identification. We next describe our main outcome measures.

2.5 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 cut providing us with a placebo).

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 to capturing 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 (henceforth, MHCLG) measuring the share of households in a local authority that is living in temporary accommodation. Local authorities have a duty to secure accommodation for unintentionally homeless households in priority need under the Housing Act 1996. 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 they have been classified as being unintentionally homeless until suitable 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. 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 2017. From 2009 onwards, we also have detailed statistics on who and why households became homeless.

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 housing 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, to measure the amount the central government – as opposed to local councils – spend on housing benefit. This will allow us to study the distribution of the fiscal burden and savings between the central- and local government actors.

Rough sleeping street counts We also leverage data capturing street counts or estimates of rough sleeping at the district level. The data is available from 2010 to 2018. 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 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 takes place between 1 October and 30 November.[14] If a local authority chooses not to conduct a formal rough sleeper count, it should provide an annual estimate of rough sleeping numbers each year, after consultation with local agencies (e. g. outreach workers, police, faith groups, 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 indi-

[14] 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.

vidual (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. To allay some concerns about mis-measurement, we study both the parliamentary as well as the local electorates. The former includes most immigrants as all UK, EU and most Commonwealth nationals have the right to vote; 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 and measures We draw in a host of auxiliary outcomes from a vast set or resources. We gather data for England and Wales on crime. 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 effects or economic shocks to local labor markets. We also use detailed district-level internal- and external migration. This includes measures such as new social security number registrations typically issued to new international migrants; registration of non-UK citizens with the National Health Service; in addition to estimates of the non-British resident population; inflows- and outflows from a council capturing domestic migration. We also incorporate data from the MHCLG measuring private sector average rents (this is a separate database from what the VOA uses); the number of households on waiting lists for council housing; and the structure, composition and changes in home tenancy within a district between the 2001 and 2011 census. Lastly, we also leverage property price data as further outcome of interest.

We next present some motivating evidence from individual-level panel survey data; this points to the issue of endogenous attrition which induces us to focus most of the work on comprehensive and detailed administrative data.

3 Motivating individual-level panel evidence

Ideally, we would be able to leverage a detailed individual-level panel data set to both measure an individual's social and economic outcomes. Yet, households that shift into insecure living arrangements may be particularly prone to drop out from such panel-survey studies. We showcase this studying patterns of attrition using an individual-level panel data set around the cuts studied above. This, while nevertheless providing some substantive results, it motivates why we leverage quite comprehensive district-level administrative data in the rest of the paper.

3.1 (Endogenous) Attrition

We draw data from the UK's largest household panel study – the Understanding Society Study (henceforth, USOC) to provide some motivating evidence.[15] We augment this data as an individual-level panel capturing the presence or absence of a respondent in each survey wave. On average, USOC respondents are interviewed once a year. Attrition is quite high but, not surprisingly, differs a lot depending on an individual's housing situation: among homeowners, constituting around 65% of respondents, year-on-year attrition is 30%. Among participants living in (furnished) rented accommodation attrition is significantly higher at around 40% (55%) – these groups represent around 8% (6%) of cases respectively.

To study attrition and (likely) exposure to the housing benefit cut, we identify all individuals that, at the most recent wave they were surveyed prior to April 2011 reported non-zero housing benefit income. This defines an indicator $T_{i,d}$ capturing whether an individual is likely to have been exposed to the housing benefit cut.

$$T_i = \begin{cases} 1 & \text{housing benefit recipient prior to April 2011} \\ 0 & \text{else} \end{cases}$$

We then estimate variants of the following difference-in-difference specification.

[15]This has been recently used to study the impact of UK welfare reforms, mostly after 2013, on populist support and support for Leaving the EU more broadly in Alabrese et al. (2019); Fetzer (2019).

$$A_{i,w,t} = \alpha_i + \beta_{d,t} + \gamma \times Post_{i,t} \times T_i + \epsilon_{i,d,t} \tag{1}$$

The dependent variable $A_{i,w,t}$ is a dummy variable indicating whether a respondent i participated in survey wave w in year t. In the most demanding version of the specification we control for individual-level fixed effects and local authority district specific non-linear time trends $\beta_{d,t}$. Note, this is the spatial unit at which we will conduct most of the substantive analysis in the main empirical exercises. The indicator $Post_{i,t}$ takes the value 1 for responses that are collected or expected to be collected after April 2011.

We focus on the original sample of respondents that participated in wave 1 and explore whether they are still present in the data in later waves and to what extent, having been a recipient of housing benefit in the wave just prior to the housing benefit reform, affects attrition differentially. We restrict the sample to the set of individuals that are reporting to live in any form of rental accommodation.

The results from estimating specification are presented in Table 1. Columns (1) and (2) exploit between-individual variation. We note that individuals likely exposed to the housing benefit cuts implemented from April 2011 onwards were 10% more likely to not be present in the future waves of the survey relative to the control group of individuals that also live in rented accommodation, but were not claiming housing benefit prior to April 2011. In columns (3) - (4) we see find similar results when solely exploiting within-individual variation. The point estimate is lower but still suggests that among the population likely affected by the reform, attrition is nearly 5% higher.

3.2 Rent arrears and attrition

To highlight the sequence of effects, we next study whether attrition is particularly pronounced among individuals that report an increase in rent-arrears (possibly) due to being exposed to the housing benefit cut after the reform took effect.

To do so, we estimate a two stage least-squares model:

$$R_{i,t} = \beta_{d,t} + \xi \times Post_{i,t} \times T_i + \nu \times T_i + \epsilon_{i,d,t} \tag{2}$$

where $R_{i,t}$ is equal to 1 in case an individual reports to be in arrears with rent. The coefficient ξ would capture the impact of exposure to the benefit cut on rent arrears. We can obtain fitted values $\widehat{R}_{i,t}$ and study whether attrition in the next wave $t+1$, $A_{i,t+1}$, is more pronounced among housing benefit recipients that report an increased propensity to be in rent arrears in period t.

The results are presented in Table 2. In column (1), we observe that individuals who received housing benefit just before the cut was implemented were more likely to report being in arrears with their rent after the reform. In column (2), we highlight that this set of individuals is also more likely to attrit from the panel in the subsequent wave. Column (3) combines the results from the first two exercises, highlighting that the underlying variation of (likely) exposure to the housing benefit cuts produces the empirical link between rent-arrears and attrition.

3.3 Individual benefit cut exposure, rent arrears and evictions

Lastly, we study similar outcomes among the set of individuals that do not attrit from the sample post-treatment. This serves as a prelude to the main analysis. For that sample, we can construct a direct exposure measure capturing the drop in self-reported housing benefit income at the two points in time closest to the reform becoming effective. Based on the set of individuals that report receiving housing benefit both before- and after April 2011, we construct a measure by how much their housing benefit income dropped, ΔB_i.[16] The empirical specification is, in its most demanding form, very similar to model (2):

$$y_{i,w,t} = \alpha_i + \beta_{d,t} + \gamma \times Post_{i,t} \times \Delta B_i + \epsilon_{i,d,t} \tag{3}$$

We focus on two main outcome measures: rent arrears and self-reported evictions. The latter is possible as a small subset of individuals that have physically moved their residence address and that have not dropped out from the study are asked *why they have moved*. Among this set of movers there are around 700-800 cases

[16] Among the set of individuals that saw a drop in the housing benefit value, the median drop was around GBP 60 per month consistent with the loss of the full excess. The mean was significantly higher at around GBP 120 per month. Nevertheless it is reassuring to see that the individual level housing benefit cut measure measure is throughout positively correlated with the measure of anticipated losses per household from the ex-ante impact assessments.

report that they moved because they were evicted. Naturally, as the measure ΔB_i may be confounding a lot of other factors, such as possibly improved individual economic circumstances resulting in a drop in housing benefit income, we study some further auxiliary outcomes, which allow us to rule this out.

The results are presented in Table 3. In Panel A, we observe that the housing-benefit cut induced drop in rent affordability is associated with an increase in individuals reporting to be in arrears with their rent. In the specifications presented in column (5) and (6) we solely exploit within-individual variation; the specifications in columns (1)-(4) exploit between-individual variation within districts. In Panel B, we observe that some individuals exposed to the cut report that they have been evicted in subsequent survey waves. Lastly, panel C highlights that the drop in housing benefit does not seem to be masking a general improvement in the economic situation of a household through higher non-benefit household income. This highlights that the housing benefit cut is not systematically masking an improvement of the financial position of households.

These findings highlight that attrition, especially if endogenous to economic shocks or specific reforms, may make it quite problematic to work with panel surveys. This necessitates a shift to administrative data, which we leverage in the remainder of the paper.

4 Empirical strategy

We next describe the main empirical specification we study. Throughout, we estimate variations of a difference-in-differences design.

4.1 Main difference-in-difference

The main baseline specification takes the following form:

$$y_{d,t} = \alpha_d + \gamma_t + \sum_{t \neq 2010} \eta_t^j \times Year_t \times S_d^{\mathrm{j}} + \beta' X_{d,t} + \epsilon_{i,t} \tag{4}$$

where $j \in \{\text{Percentile \& Excess}, \text{Percentile}, \text{Excess}\}$ indicates the three shock measures. The dependent variable $y_{d,t}$ denotes a district d level outcome, such as eviction rates, the share of households living in temporary accommodation or deemed homeless. The main model includes district level fixed effects α_d absorbing any time-invariant differences, while the year fixed effects γ_t remove common year-specific shocks.

The main coefficients are the estimates η_t^j on the interaction between the various cross-sectional exposure measures S_d^j before and after the cuts was implemented. We focus mostly on annual administrative data. 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 distribution of claimants across different property types c affected by the reform j, $C^j_{d,c,\text{baseline}}$. As such, this implies we flexibly control for trends specific to the baseline composition of claimant that are affected by the cuts. This puts further emphasis on the fact that what we are primarily interested in the effect of the cut to the generosity of housing benefit. Throughout the paper, standard errors are clustered at the district level.

4.2 Matched difference-in-difference

For each of the two shocks, we further implement a matched difference-in-difference design. To do so, we create an indicator capturing whether a district is in the upper quartile of the treatment intensity S_d^j. For each district in the upper quartile of the treatment intensity distribution, we then identify a district that is similar on pre-treatment observables and trends drawn from the set of districts that has experienced a treatment exposure in the lower 75th percentile.

We match on an extensive vector of both time-varying and time-invariant characteristics. 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, as well as the average rent levels in 2010 along with their average year-on-year changes between

2005 and 2010 to capture local distinct rental market dynamics.

To focus again on the component of the variation that is due to the financial losses entailed by the two reforms, we also match on the shares of residents that are affected by the reform j for each property type (shared room, 1 bedroom, 2 bedrooms, and so forth), $C^{j}_{d,c,\text{baseline}}$. This ensures that the resulting matched districts essentially differ only in the extent of the monetary losses and not in terms of the baseline benefit claimant distribution affected by the two elements of the cuts post 2011. We only retain matched pairs where the difference in propensity scores is less than 0.2. We then re-estimate a similar specification as Equation (4), with the difference that we also add highly demanding matched pair by year fixed effects, allowing for non-parametric time trends in the propensity scores or the quality of the match.

We next discuss the identifying variation and the implicit identifying assumptions.

4.3 Identifying variation and identifying assumptions

The shocks we exploit provide us with quite relaxed identifying assumptions for our difference-in-difference estimation strategy. We outline the identifying variation and the identifying assumption here.

Percentile shock Denote $\hat{\tau}_{d,c,p,\text{baseline}}$ as the percentile p of the distribution of all rents reported to the Valuation Office Agency, at district d and dwelling type c, at the time of the baseline in March 2011. The cut of housing allowance is equivalent to $\Delta_{d,c,\text{baseline}} = \hat{\tau}_{d,c,50,\text{baseline}} - \hat{\tau}_{d,c,30,\text{baseline}}$. Our identification strategy leverages on the intensity of the cut across districts. For example, take districts d and d' and a given dwelling type c. The identification assumption is that the size of the cut $\Delta_{d,c,\text{baseline}}$ is not correlated to unobservables of districts d and d' and the timing of the reform. This could be violated if, for example, housing market dynamics (which reflects itself on the distribution of rents, and thus potentially the size of the allowance cut) coincides specifically with the timing of the reform. We have no indication of this. To the contrary, as we discuss below, pre-reform trends seems to be entirely absent.

Excess shock An individual claimant i was affected by the cut of the excess if his or her rent $R_{i,d,c}$ for in a rental market area d and property class c was below the applicable local housing allowance rate valid at the time the individual claim was made, LHA_{d,c,t_i}. Therefore, the excess loss for individual i is

$$\text{Excess}_{i,c,d} = \begin{cases} \min\{LHA_{d,c,t_i} - R_{i,d,c}, 15\} & \text{if } LHA_{d,c,t_i} \geq R_{i,d,c} \\ 0 & \text{otherwise} \end{cases}$$

We can classify the house benefit claimants into three groups just prior to the reform. The first group of claimants, denoted as $\mathcal{S}_1$, is such that their rents are lower than $LHA_{d,c,t_i} - 15$ and thus were hit by the full loss of the excess of £15 per week. The second group, $\mathcal{S}_2$, pay a rent that lies within the interval $[LHA_{d,c} - 15, LHA_{d,c}]$, and thus lost an amount that is equivalent to the difference between their rents $R_{i,d,c}$ and $LHA_{d,c}$. Finally, the third group $\mathcal{S}_3$ is composed by individuals for which their rents is above $LHA_{d,c}$. This group was not impacted by the excess allowance cut.

We characterize the aggregate loss due to the excess shock at district d and property type c in the following manner. The expected loss per individuals is

$$\mathbb{E}(\text{Excess}_{i,d,c}|LHA_{d,c}) \quad = \quad 15 \times P[i \in \mathcal{S}_1] + \int_{i \in \mathcal{S}_2} [LHA_{d,c} - R_{i,d,c}] f(R_{d,c}) d(R_{d,c})$$

where $R_{d,c}$ is the distribution of rents among claimants in district d, and property type c. Of course, individuals might chose rents taking $LHA_{d,c}$ into consideration. This type of bunching may occur but is rather unrealistic, given that the actual $LHA_{d,c}$ rate prior to April 2011 was itself an empirical estimate that was frequently changing. In fact, the average excess that was cut was estimated to be around £10, sizably smaller than the maximum excess that could be attained. But more importantly, the existence of bunching is not a threat to identification in our difference-in-difference framework. The difference in the financial losses in the excess in different districts d and d' is driven by the number of individuals in the $\mathcal{S}_1$ and $\mathcal{S}_2$ categories, as well as the distribution of the rents within the latter group.

To illustrate, consider the number of individuals in the $\mathcal{S}_1$ group (similar point can be made regarding $\mathcal{S}_2$ and the distribution of rents $f(R_{d,c})$). Suppose that individuals choose rent such that they are able to claim the full excess prior to the introduction of the policy, i.e., $R_{i,d,c}$ is (just) below $LHA_{d,c} - 15$. Our identification assumption would require that the proportion of claimants in $\mathcal{S}_1$ does not vary across two districts d and d' in a way that is correlated with unobservables of those districts and the timing of the reform.

4.4 Robustness checks

Throughout the presentation of the results, we estimate the three difference-in-difference estimates. We present the main results using the combined exposure of a district to both the percentile- and the excess shock, while also presenting the results pertaining to each of the individually. For the main difference-in-difference that explores the main treatment exposure measures from the two cuts, we present the results focusing data on the period up to 2013. This will highlight that the bulk of the results are carried before the welfare reforms studied in detail in Fetzer (2019) become implemented from April 2013. Further, we also present results including and dropping London, which accounts for 13% of the UK population, from the analysis. Lastly, we replicate each of the main tables focusing on the two shocks separately in the appendix highlighting that results are carried throughout when studying the two simultaneously introduced cuts in isolation.

5 Main Results

5.1 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 2 indicates that the policy reduced the actual spending, on average, between 1 and 3 per cent. The drop in spending becomes most notable in 2012 and continues in subsequent years.[17] 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

[17] Appendix Table A1 presents results in tabular format pooling the post 2010 coefficients.

between April 2011 and December 2012 can be regarded as transitional periods.

5.2 Evictions

We begin by presenting the results on evictions. Visually, these are presented in Figure 3, using the district level impact estimate, $S_d^{\text{percentile \& excess}}$ per household as measure of treatment intensity. The treatment variable intensity has been normalised to have unit standard deviation. The figure suggests a sharp increase in eviction actions between 2011 and 2012, consistent with the timing of the cuts. There is no evidence that suggests significant pre-treatment trends.

The point estimates in Table 4 pool the individual post-treatment estimates. The estimates in Panel A indicate that 1 standard deviation in the exposure to the cuts is associated to an increase of 0.359 possession claims per one thousand inhabitants, or a 16.5 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 12.7 per cent increase due to the housing benefit cuts. 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 cuts.[18] All estimates are similar when studying the percentile shock or the excess shock in isolation and in the corresponding matched difference-in-difference estimation.[19]

[18] There is good case study evidence suggesting that the housing benefit cuts increased rent arrears, with Department for Work and Pensions (2014) reporting on a survey of landlords, suggesting that "45 per cent of landlords stated that the number of tenants in rent arrears had increased, compared with only 19 per cent of non-LHA landlords" with landlords attributing the rise to the cuts to housing benefit.

[19] Appendix Table A2 presents the results obtained in columns (1) - (4) focusing on each shock separately.

5.3 Insolvencies

We next turn to studying individual bankruptcies. Typically, mortgage and rent arrears can not be included in common insolvency procedures as they are classified priority debt. Nevertheless, the data provide a window into financial grievances that households may face that may be exacerbated by the housing benefit cuts (see also Humphries et al., 2019). Anecdotal evidence suggests that 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.[20] Hence, it is not inconceivable that some households and individuals started to accumulate problematic consumer debt that subsequently needed to be restructured. The results are presented in Table 5.[21] The point estimate in column (1) in Panel A suggests that a 1 standard deviation increase in the exposure to the housing benefit cut, is associated with 1.9 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 2.1 per cent for a district with a 1 standard deviation higher exposure.[22]

5.4 Temporary housing and council homelessness spending

As indicated, councils have a legal obligation to provide housing for households that are at risk of becoming homeless and particular, if they are considered priority – typically families with children, pregnant, or sick and disabled households. Councils bear the cost of providing this temporary accommodation. In Figure 4 we plot out the estimated capturing the change in the demand for temporary accommodation in councils more exposed to the housing benefit cut in Panel A, along with the councils' spending on hosting homeless in hostels and bread-

[20]Department for Work and Pensions (2014) present case study evidence suggesting "a quarter of [housing benefit] claimants said they would borrow money from family and friends; and one in ten thought that they would take out a loan or borrow from a credit card" to deal with the cuts.

[21]The event study estimates indicating a notable jump in insolvencies from 2011/2012 are presented in Appendix Figure A7.

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

and-breakfast accommodations. Both figures have skyrocketed dramatically from 2011 onwards.

In Table 6 we present the corresponding tabular estimates pooling the post 2010 point estimates. Using the official estimates of treatment intensity, in Column (1) we find that the demand for temporary accommodation grew by, on average, 18.9 per cent as a consequence of the cut to housing benefits. 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 at conventional levels.[23] What is more, we find that the council spending on temporary housing increased sharply by around 94.9 per cent as a consequence of the cut. 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 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.

5.5 Statutory homelessness and rough sleeping

We next turn our attention to the effects of the housing benefit cuts on statutory homelessness and actual rough sleeping. 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. 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

[23] Appendix Table A4 focuses on each of the two shocks separately.

in Section 2.5, we rely on rough sleeping street counts carried by the councils themselves.

In Figure 5 we show evidence of a strong increase in both statutory homeless and rough sleeping in the years following the reform. Statutory homelessness was effectively weakly trending downwards up to 2010, and the trend reverts in the post-reform years jumping markedly in 2011 and particularly, in 2012. The rough sleeping data is only available from 2010, but we do not observe systematically different levels in 2010 levels of rough sleeping in districts more affected by the reform, but notice notable increases from 2012.

Table 7 presents the point estimates for the full post-reform effects. It indicates a sizable increase in statutory homelessness of, at least, 10.3 per cent. We find similar effect sizes across the different specifications. We also observe a notable increase in rough sleeping, increasing by, on average, 49.6 per cent in the post-2011 years. Appendix Table A5 presents the results of estimating the difference-in-difference specifications in columns (1) - (4) separately for each of the two elements of the shock measure.

Who becomes homeless, and why? From 2009 onwards, we have detailed administrative data on individual statutory homelessness cases. These data provide insights into *who* and *why* individuals became unintentionally homeless. We observe notable shifts in the patterns underlying this data after 2010 in places most affected by the cuts. In Table 8, we document how the structure of who is becoming statutory homeless has changed. Consistent with the previous patterns, there is a notable jump in statutory homelessness levels increasing by, on average, around 14%. This increase is significantly carried by households with dependent children and, to a significant extent, also single parents seeking relief from their councils. In columns (5) - (8) we study the distribution of statutory homelessness across different age groups, finding most pronounced increase in homelessness concentrated among the working age adult population older than 25. In columns (9) - (12) we study the different priority need categories that councils use. This again highlights that the bulk of the increase in the districts most affected by the housing benefit cuts is due to households with dependent children becoming homeless, and, to a lesser extent also households with existing mental- or physical health conditions. Columns (11) and (12) highlight that the increases are not driven due to higher levels of substance abuse or changing patterns in domestic violence.

In Table 9 we explore why individuals are becoming homeless. While the administrative records are not providing individual case narratives, they are crudely categorizing individual cases. The sharp increase in statutory homelessness in districts most exposed to the housing benefit cuts is driven by three factors: rent arrears (column 4) and evictions (column 7), but not due to increased rent arrears among tenants in the social rented sector or in the local authority rented sector (columns 5-6). This pattern is very consistent with the data presented on evictions in the previous section and highlights that evictions did indeed sharply increase in districts most exposed to the housing benefit cuts, directly impacting councils through increased numbers of households applying for statutory homeless protection.

We next discuss the implications for electoral registration rates.

5.6 Electoral registration and EU referendum vote

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 is a superset of the parliamentary electors, which also includes most immigrants and in particular includes all European nationals living 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 housing situation.

We construct a measure of the electoral registration rates as the share of the registered electorates in the total 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 A8. 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.

The notable jump is not surprising as 2010 was thee first year for which elections were held on the new set of constituency boundaries, which typically triggers special registration efforts. 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 A6 provides the corresponding point estimates. The results suggest that a 1 SD higher exposure to the housing benefit cut is associated with a reduction in 0.2 per cent 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 constituting on average, a 3 per cent decrease in electoral registration coverage rates. We next study the 2016 EU referendum.

2016 EU referendum In Table 10, 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

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

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 10 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 1 SD higher exposure to the housing benefit cut is associated with 0.759 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 1.4 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 support 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.

5.7 Null effects and robustness

We next present a set of additional and notable null-results and robustness checks.

Robustness As indicated, our estimation approach normalizing the dependent variables with the time-varying number of households or the population is quite conservative as the UK has seen population growth and a growth in the number of households over the sample period. We can re-estimate the main empirical specification in levels or by normalizing with 2010 baseline numbers of households. Throughout, we find very similar if not stronger results in a statistical sense. These are available upon request.

As indicated, column (4) of each of the main Tables 4 presents results obtained from estimating a specification where we interact the baseline claimant counts S_d^j

affected by a respective reform with a set of year fixed effects. The purpose of this exercise is to essentially focus on the part of the treatment exposure measure that is due to the financial losses $L_{d,c}$ as opposed to capturing different composition of claimants across different property types. Throughout, we find very similar whereby our main treatment effects.

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 (7) we repeat the exercise with the individual percentile and excess cut shocks, replicating the specifications of Equation (1). Finally, columns (6) and (8) present the results of the matching estimator, which further corroborate the main findings under even more relaxed identifying assumptions. As indicated, the matched pairs are constructed so that districts have both a similar composition of households affected by the reform, $C^j_{d,c}$, have seen similar developments in real estate markets and property ownership between the 2001 and 2011 census, have seen similar levels of private sector rents as well as trends, have similar commuting exposure to the London metropolitan area and lastly, are also similar in terms of their social housing availability as proxied by the length of waiting lists. Further, we also remove a very demanding set of time effects that is specific to each matched pair.

Finally, Appendix Tables A2 to A5 present the main analysis in columns (1) - (4) broken out by the two different shocks. Throughout, we find very similar results. We finish this section by pointing to some notable null-results that can help ruling out alternative mechanisms.

No impact on unemployment or economic activity To allay some concerns that the results may be confounding shocks to local labour markets or changing patterns of economic activity rates, we study these explicitly as outcome measures in Appendix Table A7. Throughout there is no consistent discernible pattern suggesting that places more- or less exposed were subject to differential labour market shocks after the cuts were implemented. Further, we also do not observe a change in economic activity rates that could confound results.

Housing market impacts In Table A8 we study the impact of the reforms on property prices, while studies the effect on average private- and social rental prices. Turning to property prices, we find diverging evidence. While the shock is positively associated to an increase in property prices throughout the UK, we find that the result reverses when London is dropped from the sample, despite most outcomes on eviction, temporary accommodation and homelessness being broadly carried when excluding London. Table A9 presents results studying rents in the private- and social rented sector. The results suggest that districts, on average, saw modest growth in rents. A 1 SD higher exposure to the housing benefit cuts is associated with an average rent that is 30 pence higher per week when constraining the analysis to the period up to 2013. As we observe notable jumps in evictions, statutory homelessness, temporary accommodation and rough sleeping, we do not think that this pattern can account for these notable jumps. Appendix Figure A9 presents the difference-in-difference plots studying average private rents and points to a weak but steady upward trend. When zooming in on the excess cut in Panel C, this trend is absent, despite all main results being carried by this shock alone. This renders us confident that our results are not confounded by capturing rapidly changing property- or rental prices post 2010.

Temporary increase in property crimes In Appendix Figure A10 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 A10 we present the corresponding point estimates which suggest a large positive impact on thefts from persons.

No impact on (internal) migration We further study both internal- and international migration indicators at the local authority level. In Appendix Table A11 we focus on indicators of international migration. Panel A studies short-term international migration inflows. This is particularly relevant as it captures international migration for students at universities, many of which are located in the UK's urban centers. There is no discernible effect of housing benefit cut exposure of a district on this measure of migration that may increase pressures on the housing market. Panel B focuses on long-term international migration – there is no discernible difference. Panel C and D focuses on administrative data that may be particularly suitable at detecting new inflows of legal migration. Panel C highlights that there is no discernible increase in new registrations with general practitioner in the to access the UK's healthcare system. Panel D highlights there is not impact on new National Insurance registrations required of migrants entering the UK to work.

Appendix Table A12 studies internal migration indicators. In Panel A, we find no discernible effect of exposure to the housing benefit cut on the resident share that is non-British, indicating that the housing benefit cut is not associated with a local population shift towards more non British nationals. Panel B focuses on internal migration inflow estimates. Here we observe that councils most exposed to housing benefit cuts see significantly lower internal migration inflows. Panel C, on the other hand, highlights that internal migration outflows from councils most affected by the housing benefit cut is not systematically higher. This highlights that the stock of the population remains fairly constant.

6 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 A1, 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 6.

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 housing benefit.

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 6. 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 £7.96 per household and year. This is mostly offset with higher costs due to homelessness prevention efforts, increasing local council costs by £6.32 per resident household and year for the median council. Hence, for the median district, the net fiscal savings are merely For the median district, the net fiscal savings are merely £ 1.64 per resident household and year.

Across the whole of the UK, the projected ex-ante fiscal savings from the two elements of the housing benefit cut was estimated to be around around £618 million per year. Our estimates imply that the actual savings were closer to £311 million. This is offset with an overall increase in spending on council anti homelessness measures of £167 million, implying a dramatic shifting of burden from the central government to local governments.

7 Conclusion

In this paper we explore the effects of a severe cut to housing benefit in the UK. The cut to housing assistance was severe, affecting nearly 5.1 percent of households in the UK with average losses of around £600 per year. Using individual- and detailed district level administrative data, we carefully trace out the economic and social effects of this cut, finding evidence that the cut directly contributed to increased housing insecurity through increased evictions, a higher prevalence of temporary accommodation, statutory homelessness and actual rough sleeping. We exploit cuts that afford us with very relaxed identifying assumptions allowing us to interpret the effects in a causal fashion, noticing distinct and sharp jumps in most immediately relevant outcome measures by 2011 or 2012, immediately following the cut.

We document that the increased prevalence of (statutory) homelessness is broadly due to more families with children, single parents and people with health- and disabilities becoming homeless due to rent arrears and due to being evicted, highlighting that the cuts were particularly severely affecting already vulnerable population strata. We also show that the policy, intending to save significant financial resources, ended up primarily shifting the costs, rather than substantially lowering the financial cost of housing assistance. We find that for every for each pound saved by the central government in form of lower housing benefit payments, local councils saw an increase in spending of around 53 cents to meet statutory duties to provide housing for households at risk of becoming unintentionally homeless. In aggregate, the actual savings for the central government are estimated to be £311 million per year; these are offset with increased council spending of £167 million per year, implying a dramatic shifting of burden from the central government to local governments, which may in turn have had implications for other service provision of councils.

Moreover, we document the cuts also eroded the state of democracy 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 or could not vote for failure to be on the electoral roll.

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 homeless and sleeping rough at the extreme of this distribution. In the context of spiralling public spending on housing assistance programs, calls to reform benefit systems are growing not only in the UK, but elsewhere. This paper highlights that simple cuts to housing allowance may produce large indirect costs, ultimately not providing significant relief to the public purse. The focus hence needs to shift to reform benefit systems, while at the same time tackling the underlying reasons for worsening rent affordability to be found in tight and inelastic supply of housing.

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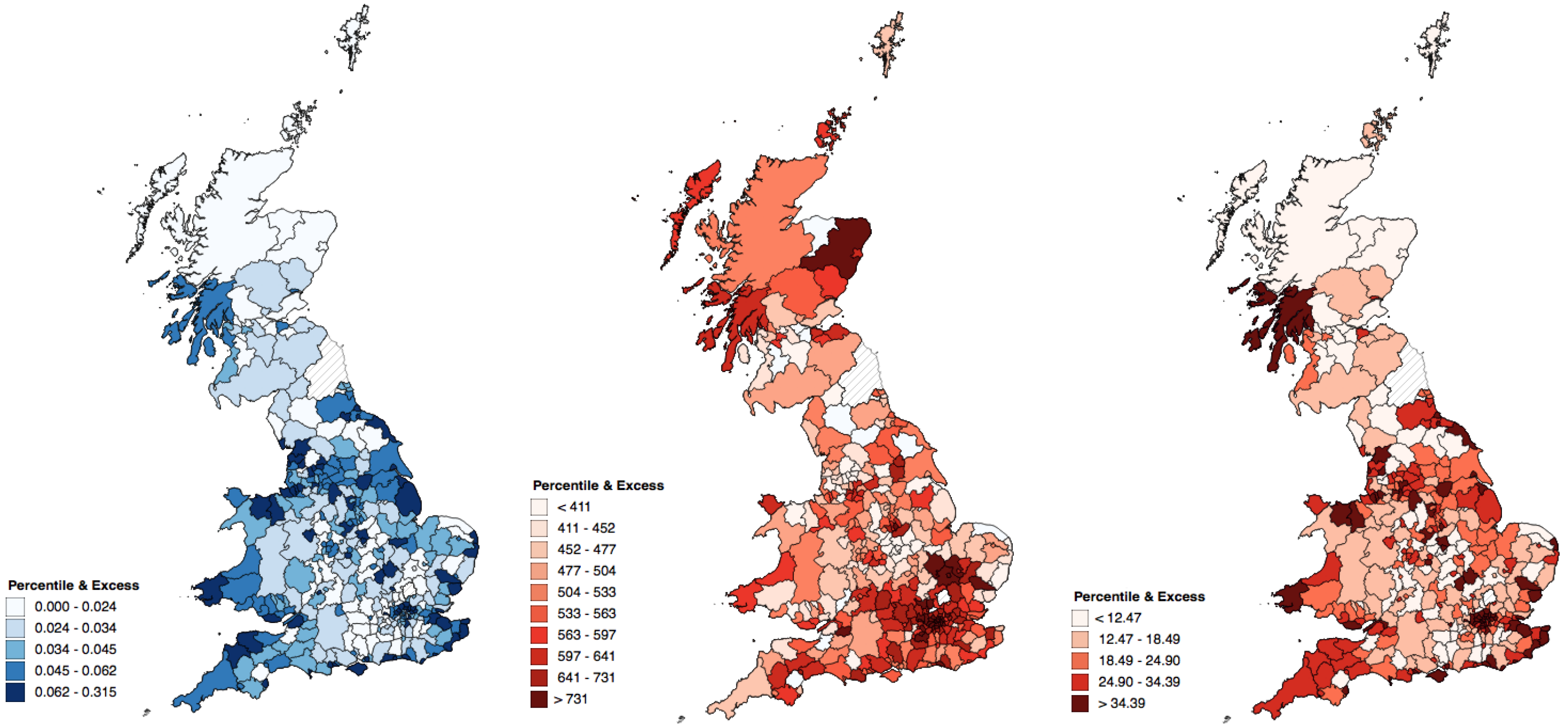
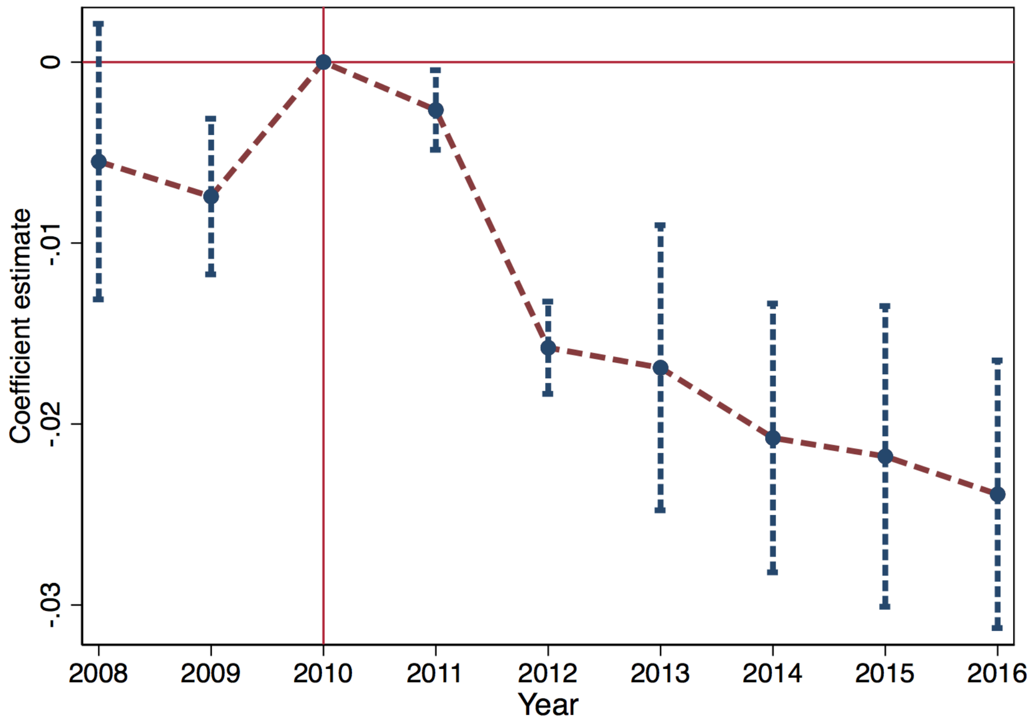
Figure 1: Ex-ante estimated impact of change in housing benefit reference rent: moving from median to 30th percentile of rents as maximum allowable rent

Panel A: % of households affected *Panel B*: Loss per affected household *Panel C*: Loss per household



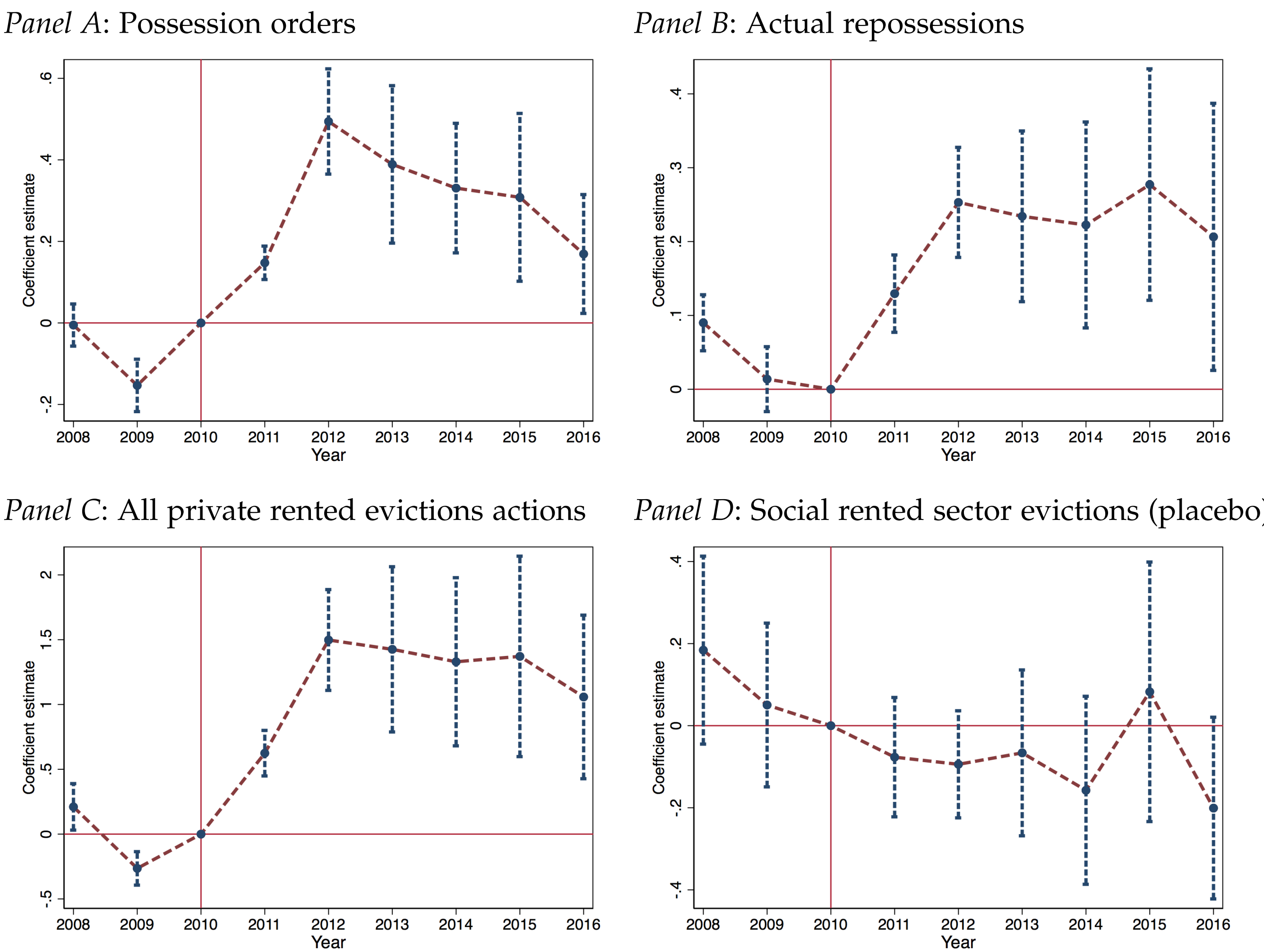
Notes: Map plots out the exposure to the cut to local housing allowance across districts using data from the Department for Works and Pension's Official Economic Impact Assessment. Panel A presents data on the number of households affected expressed as a share of all resident households. Panel B presents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

Figure 2: Impact of cuts in reference rents on housing benefit spending



Notes: Figure plot the results of the regression of the log of the housing benefit spending on the exposure measure 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. All regressions control for local authority district fixed effects and year effects. 90% confidence bands obtained from clustering standard errors at the district level are indicated.

Figure 3: Impact of cut to housing benefit on forced evictions of people living in rental accommodation

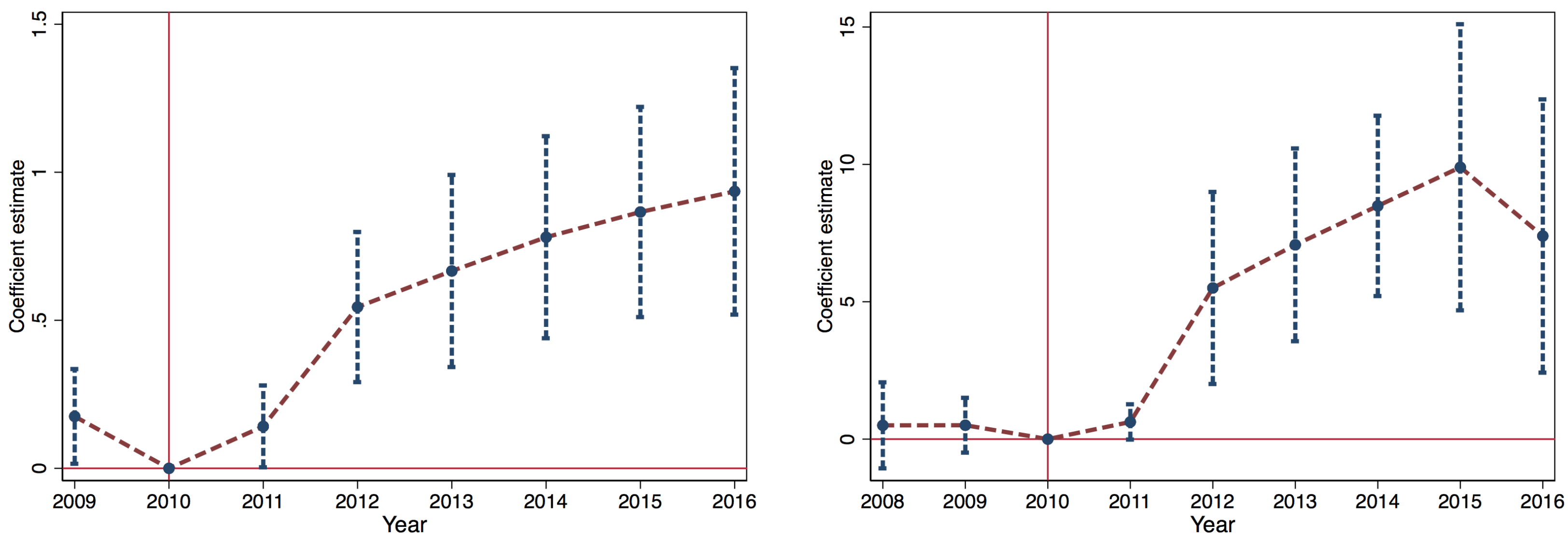


Notes: All dependent variables are measured as rates relative to the number of resident households in a district. Figure plots results from studying the impact of the cut to local housing allowance to cover the median rent to only cover the 30th percentile of rents from April 2011 onwards. The dependent 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 4: Impact of cut to housing benefit on rate of residence in temporary accommodation and statutory homelessness

Panel A: Households in temporary accommodation

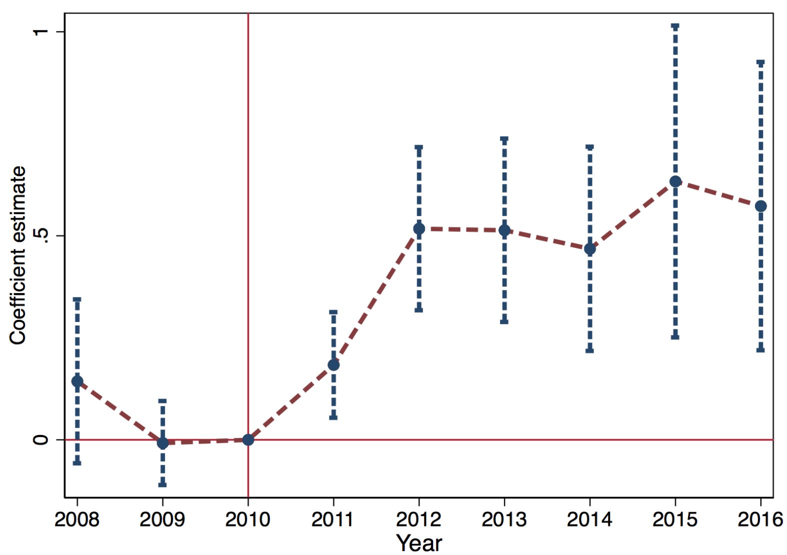
Panel B: Spending on homeless hostels & BnB's



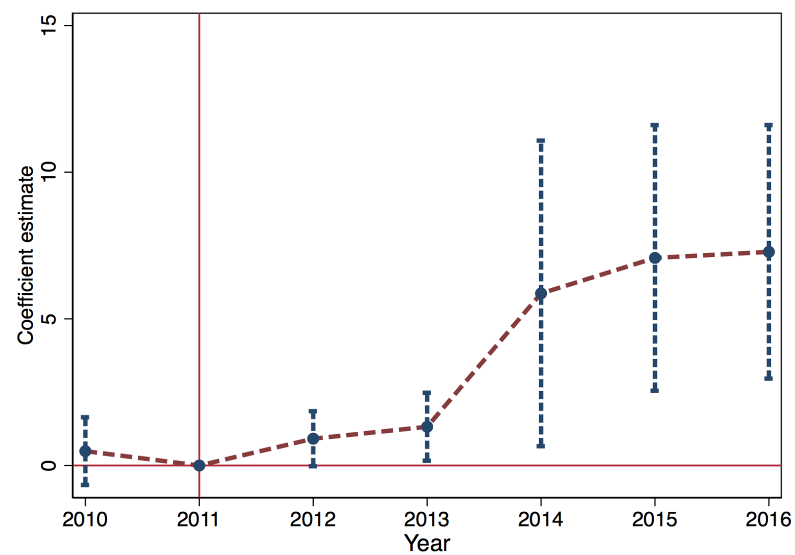
Notes: Figure plots from the regression studying the impact of the cut to local housing allowance to cover the median rent to only cover the 30th percentile of rents from April 2011 onwards. All dependent variables are measured as rates relative to the number of resident households in a district. The dependent variable in Panel A measures the number of residents in temporary accommodation. Panel B is the spending on hosting homeless in hostels and bread-and-breakfast. 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 5: Impact of cut to housing benefit on measures of statutory homelessness

Panel A: Statutory homeless

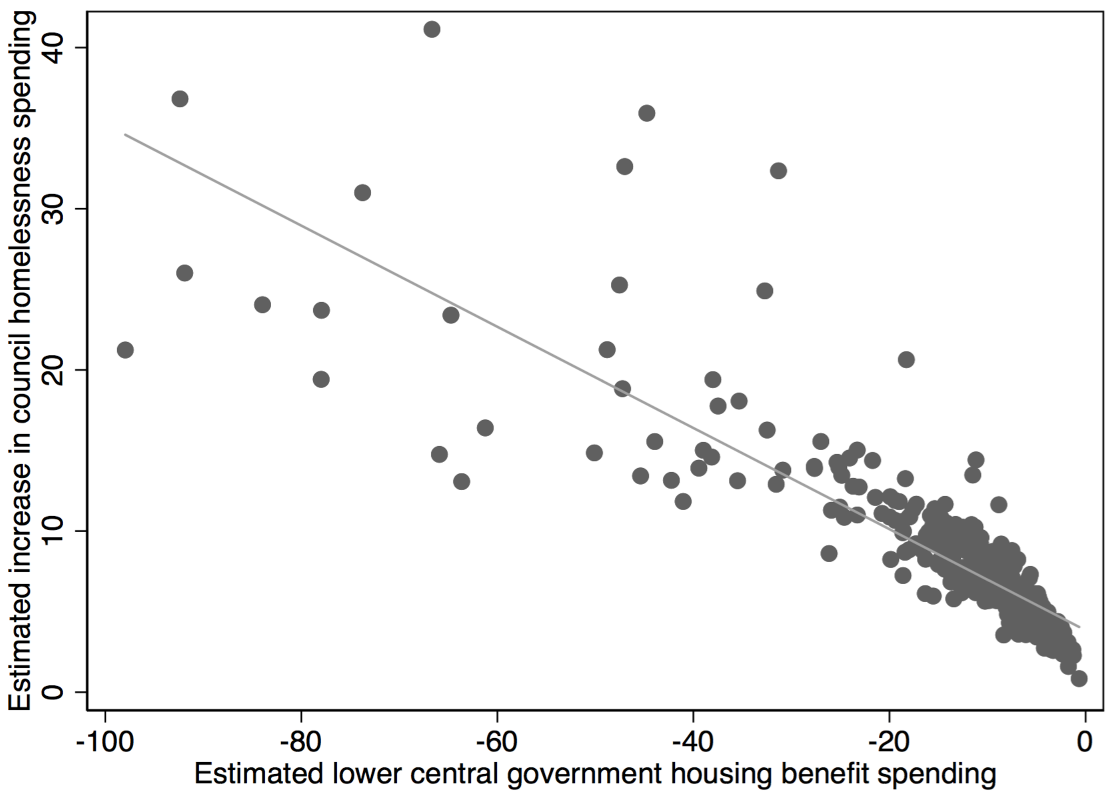


Panel B: Roughsleeping



Notes: Figure plots from the regression 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. 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 number of statutory homeless individuals. Panel B is the street count of rough sleepers. 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.

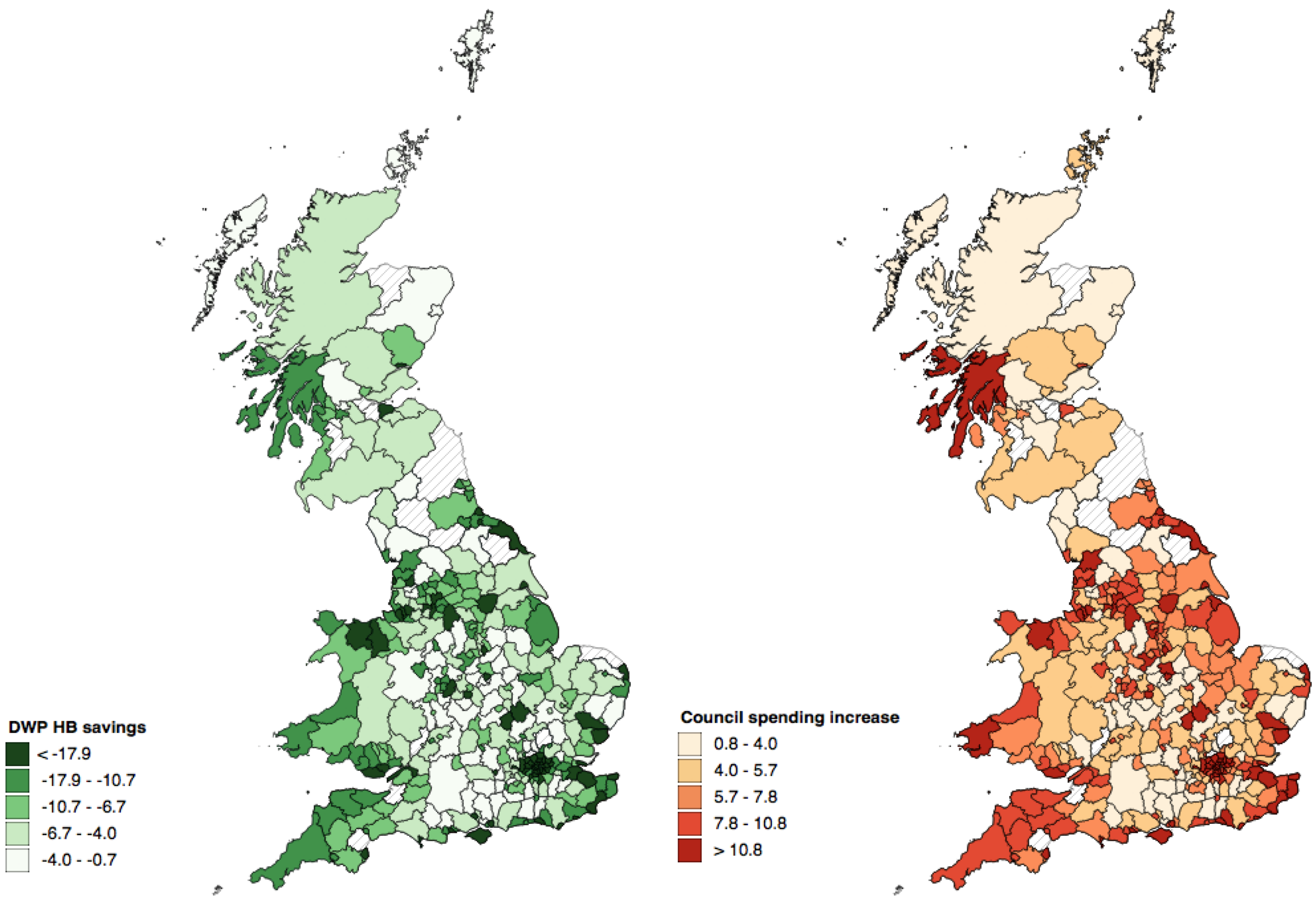
Figure 6: Cost-benefit analysis: Implied fiscal savings to central government due to lower housing benefit costs versus higher council spending for homelessness



Notes: Figure plots out the full empirical distribution of the projected fiscal savings per household in a district due to lower housing benefit payments as a result of the cuts to housing benefit since April 2011. The vertical axis displays the corresponding estimated impact on increased overall council spending on homelessness and homelessness prevention per household in a district since the cuts were implemented.

Figure 7: 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: DWP housing benefit savings *Panel B*: Council homelessness prevention cost increase



Notes: Panel A plots out the full empirical distribution of the projected fiscal savings per household in a district due to lower housing benefit payments as a result of the cuts to housing benefit since April 2011. Panel B plots out the estimated impact on increased overall council spending on homelessness and homelessness prevention per household in a district since the cuts were implemented.

Table 1: Impact of housing benefit cut on attrition from survey

	(1)	(2)	(3)	(4)
DV: Attrition				
Post 2011 × Pre 2011 Housing benefit recipient	0.045***	0.046***	0.028***	0.029***
	(0.008)	(0.008)	(0.008)	(0.008)
Pre 2011 Housing benefit recipient	-0.079***	-0.081***		
	(0.012)	(0.012)		
Mean of DV	.492	.492	.494	.494
Local Authority Districts	378	377	378	377
Observations	86438	86427	86010	85994
District FE	x			
Time FE	x		x	
District x Time FE		x		x
Individual FE			x	x

Notes: The dependent variable is an indicator capturing whether an individual did unexpectedly not participate in the panel study in a given year. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 2: Impact of housing benefit cut on rent-arrears, attrition and rent-arrear induced attrition from survey

	(1)	(2)	(3)
	Rent arrears$_t$	Attrition$_{t+1}$	Attrition$_{t+1}$
Post 2011 × Pre 2011 Housing benefit recipient	0.031***	0.053***	
	(0.009)	(0.009)	
Rent arrears			1.722***
			(0.567)
Mean of DV			
Local Authority Districts	378	378	
Observations	71079	71079	71079
Weak IV			11.2
District x Time FE	x	x	x

Notes: All regressions include district by time fixed effects. The dependent variable in column (1) is a dummy indicating whether a respondent stated they are behind with their rent payments. In column (2) the dependent variable is an indicator capturing wether a respondent would drop out in the subsequent wave of the panel study. Column (3) estimates an IV regression to highlight that individuals reporting increased rent arrears due to (likely exposure to) the housing benefit cut in time t are more likely to attrit in $t+1$. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 3: Impact of housing benefit cut on rent-arrears and evictions

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Rent arrears						
Post April 2011 × Cut in Housing benefit	0.021**	0.024**	0.027***	0.032***	0.022**	0.025***
	(0.009)	(0.009)	(0.009)	(0.011)	(0.010)	(0.010)
Mean of DV	.178	.178	.179	.168	.178	.179
Local Authority Districts	378	378	378	346	378	378
Observations	94713	93785	60694	47481	85248	84118
Panel B: Evictions						
Post April 2011 × Cut in Housing benefit	0.003**	0.003*	0.006**	0.002	0.004*	0.004
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)
Mean of DV	.00648	.00646	.00541	.00539	.007	.00696
Local Authority Districts	378	378	378	346	378	378
Observations	98080	97179	62876	49305	88395	87300
Panel C: Non-benefit household income						
Post April 2011 × Cut in Housing benefit	16.495	40.141	53.358	13.652	48.849*	34.091
	(29.920)	(35.893)	(35.915)	(42.572)	(26.078)	(28.680)
Mean of DV	1787	1790	1668	1565	1743	1746
Local Authority Districts	378	378	378	346	378	378
Observations	97872	96968	62872	49301	88154	87058
District & Time FE	x				x	
District x Time FE		x	x	x		x
Individual FE					x	x
Drop post 2013			x			
Drop London				x		

Notes: The dependent variable in Panel A measures the share of housing benefit income as a share of rent. Panel B studies rent arrears, while Panel C focuses on evictions. The dependent variable in Panel D is non-benefit household income. The sample includes all individuals that live in rental accommodation. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 4: Impact of cut to housing benefit on eviction measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: Possession claims due to rent arrears								
post × $S^{\text{percentile \& excess}}$	0.359***	0.396***	0.125**	0.423***				
	(0.079)	(0.064)	(0.058)	(0.136)				
post × $S^{\text{percentile}}$					0.483***	0.333		
					(0.082)	(0.239)		
post × S^{excess}							0.361***	0.392***
							(0.101)	(0.144)
Mean of DV	2.18	2.07	1.66	2.18	2.18	2.06	2.18	2.55
Panel B: Repossessions								
post × $S^{\text{percentile \& excess}}$	0.186***	0.171***	0.052	0.186**				
	(0.058)	(0.034)	(0.037)	(0.079)				
post × $S^{\text{percentile}}$					0.290***	0.158		
					(0.060)	(0.149)		
post × S^{excess}							0.232***	0.186**
							(0.071)	(0.089)
Mean of DV	1.47	1.34	1.19	1.47	1.47	1.31	1.47	1.65
Panel C: All private rented-sector eviction actions								
post × $S^{\text{percentile \& excess}}$	1.236***	1.200***	0.386**	1.447***				
	(0.289)	(0.201)	(0.176)	(0.496)				
post × $S^{\text{percentile}}$					1.701***	1.138		
					(0.290)	(0.792)		
post × S^{excess}							1.281***	1.299**
							(0.362)	(0.515)
Mean of DV	5.46	4.99	3.99	5.46	5.46	5.08	5.46	6.44
Panel C: (Placebo) social-rented rented-sector evictions								
post × $S^{\text{percentile \& excess}}$	-0.164	-0.158	-0.092	-0.256*				
	(0.109)	(0.098)	(0.191)	(0.147)				
post × $S^{\text{percentile}}$					-0.137	-0.110		
					(0.107)	(0.620)		
post × S^{excess}							-0.173	-0.876**
							(0.124)	(0.436)
Mean of DV	10.5	10.7	9.83	10.5	10.5	10.1	10.5	11.6
Local authority districts	366	366	333	366	366	74	365	94
Observations	3293	2195	2997	3293	3293	666	3284	846
London included?	X	X		X	X	X	X	X
Include data after 2013	X		X	X	X	X	X	X
$C^{j}_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		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 5: Impact of cut to housing benefit on bankruptcies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: Total individual bankruptcies								
post $\times$ $S^{\text{percentile \& excess}}$	0.117***	0.092**	0.122	0.088**				
	(0.044)	(0.045)	(0.074)	(0.039)				
post $\times$ $S^{\text{percentile}}$					0.155***	0.149		
					(0.037)	(0.159)		
post $\times$ S^{excess}							0.141***	0.189**
							(0.050)	(0.088)
Mean of DV	6.01	6.54	6.27	6.01	6.01	5.61	6.01	5.35
Local authority districts	338	338	305	338	338	74	337	94
Observations	3041	2027	2745	3041	3041	666	3032	846
Panel B: Individual voluntary arrangements								
post $\times$ $S^{\text{percentile \& excess}}$	0.055**	0.016	0.117***	-0.003				
	(0.024)	(0.014)	(0.039)	(0.007)				
post $\times$ $S^{\text{percentile}}$					0.097***	0.250***		
					(0.019)	(0.056)		
post $\times$ S^{excess}							0.096***	0.141***
							(0.019)	(0.033)
Mean of DV	2.62	2.63	2.73	2.62	2.62	2.46	2.61	2.36
Local authority districts	338	338	305	338	338	74	337	94
Observations	3041	2027	2745	3041	3041	666	3032	846
London included?	X	X		X	X	X	X	X
Include data after 2013	X		X	X	X	X	X	X
$C^{j}_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		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 6: Impact of cut to housing benefit on council spending on temporary housing and statutory homeless duties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: Temporary accommodation								
post × $S^{\text{percentile \& excess}}$	0.565***	0.359**	0.072	0.938**				
	(0.203)	(0.173)	(0.180)	(0.392)				
post × $S^{\text{percentile}}$					0.532***	1.176*		
					(0.177)	(0.620)		
post × S^{excess}							0.337*	0.147
							(0.186)	(0.167)
Mean of DV	2.99	2.69	1.72	2.99	2.99	1.88	2.99	2.62
Local authority districts	366	366	333	366	366	74	365	94
Observations	2807	1800	2545	2807	2807	554	2799	722
Panel B: Council spending on hostels and BnB's								
post × $S^{\text{percentile \& excess}}$	6.148***	4.062***	0.915*	8.304***				
	(1.466)	(1.258)	(0.545)	(2.477)				
post × $S^{\text{percentile}}$					8.295***	-0.794		
					(1.968)	(1.306)		
post × S^{excess}							4.200**	6.948*
							(1.884)	(3.820)
Mean of DV	9.83	7.58	4.11	9.83	9.83	6.81	9.85	10.9
Local authority districts	366	366	333	366	366	74	365	94
Observations	3243	2195	2947	3243	3243	666	3234	846
Panel C: Total council spending on temporary housing								
post × $S^{\text{percentile \& excess}}$	17.279***	10.172***	1.751	26.614***				
	(3.841)	(2.651)	(1.311)	(7.708)				
post × $S^{\text{percentile}}$					17.536***	9.774		
					(3.700)	(6.027)		
post × S^{excess}							8.978***	7.890
							(2.421)	(4.999)
Mean of DV	18.2	14.6	5.15	18.2	18.2	12.4	18.2	18
Local authority districts	366	366	333	366	366	74	365	94
Observations	3293	2195	2997	3293	3293	666	3284	846
London included?	X	X		X	X	X	X	X
Include data after 2013	X		X	X	X	X	X	X
$C^j_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		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 7: Impact of cut to housing benefit on homelessness and rough sleeping

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: Statutory homelessness								
post $\times$ $S^{\text{percentile \& excess}}$	0.436***	0.361***	0.259	0.518**				
	(0.139)	(0.121)	(0.197)	(0.218)				
post $\times$ $S^{\text{percentile}}$					0.563***	0.243		
					(0.121)	(0.283)		
post $\times$ S^{excess}							0.396***	0.221
							(0.123)	(0.180)
Mean of DV	4.27	4.39	4.17	4.27	4.27	2.46	4.27	2.53
Local authority districts	366	366	333	366	366	74	365	94
Observations	3265	2179	2971	3265	3265	658	3256	832
Panel B: Rough sleeping street counts								
post $\times$ $S^{\text{percentile \& excess}}$	4.248**	0.873**	2.361**	5.845***				
	(1.657)	(0.378)	(1.182)	(1.887)				
post $\times$ $S^{\text{percentile}}$					3.310**	4.025*		
					(1.539)	(2.158)		
post $\times$ S^{excess}							2.667***	2.669***
							(0.764)	(0.729)
Mean of DV	8.56	6.79	7.23	8.56	8.56	8.4	8.56	8.37
Local authority districts	316	316	283	316	316	74	315	94
Observations	2212	1264	1981	2212	2212	518	2205	658
London included?	X	X		X	X	X	X	X
Include data after 2013	X		X	X	X	X	X	X
$C^{j}_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		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 8: Who becomes homeless?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A:												
Who becomes homeless?		Household type			Age group				Priority need category			
	All	Couple with children	Lone parents	Singles	16-24	25-44	45-59	60 older	HH's with children	Health	Substance abuse	Violence
Panel A: Percentile & excess												
Post 2011 × $S^{\text{percentile \& excess}}$	22.448***	4.754**	6.152	0.981	-0.613	15.633***	6.563***	1.463***	19.478***	4.063***	-0.005	0.104
	(6.715)	(2.328)	(4.038)	(1.821)	(1.113)	(4.126)	(1.251)	(0.355)	(5.559)	(1.336)	(0.014)	(0.157)
Mean of DV	154	27.4	69.5	28.4	40.9	78.6	15.6	1.39	97.5	16.7	.13	4.1
Panel B: Percentile												
Post 2011 × $S^{\text{percentile}}$	29.701***	7.286***	10.223***	0.710	-1.241	20.800***	8.168***	1.464***	26.392***	3.681***	-0.022	0.236
	(7.025)	(2.076)	(3.757)	(1.942)	(1.742)	(4.470)	(1.402)	(0.304)	(5.897)	(1.301)	(0.025)	(0.214)
Mean of DV	154	27.4	69.5	28.4	40.9	78.6	15.6	1.39	97.5	16.7	.13	4.1
Panel C: Excess												
Post 2011 × S^{excess}	23.340***	6.480***	9.729**	-0.769	-4.031	16.476***	5.997***	0.893***	21.435***	2.668***	0.017	-0.142
	(7.700)	(2.124)	(3.977)	(1.648)	(2.542)	(4.809)	(1.668)	(0.237)	(6.554)	(0.988)	(0.026)	(0.531)
Mean of DV	154	27.4	69.6	28.4	40.9	78.7	15.6	1.39	97.6	16.7	.13	4.12

Notes: All regressions include district- and year fixed effects. The dependent variable measures the count of the number of cases per year belonging to each category or classification. distinguishing who becomes homeless by household type, age and priority need category. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 9: Why do they become homeless?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Why became homeless?		... not willing to house		Rent arrears & Evictions					Relationship breakdown		Other reasons	
	All	Parents	Friends & Relatives	Private	SRS	LAD	Evictions	Other	Non-violent	Violent	Left care	Other
Panel A: Percentile & excess												
Post 2011 × $S^{\text{percentile \& excess}}$	22.448***	1.792***	0.482	0.349**	0.038	0.041	14.533**	2.663***	-0.060	0.582	-0.030	3.694**
	(6.715)	(0.682)	(0.786)	(0.173)	(0.041)	(0.031)	(7.123)	(0.986)	(0.177)	(0.487)	(0.165)	(1.770)
Mean of DV	154	23.3	16.6	1.65	.137	.153	33.5	6.6	4.48	15.5	1.67	7.4
Panel B: Percentile												
Post 2011 × $S^{\text{percentile}}$	29.701***	1.387	0.795	0.559**	0.003	0.035	22.775***	2.756**	-0.118	0.836	0.059	2.437
	(7.025)	(1.085)	(1.191)	(0.235)	(0.053)	(0.043)	(5.998)	(1.100)	(0.237)	(0.508)	(0.295)	(1.699)
Mean of DV	154	23.3	16.6	1.65	.137	.153	33.5	6.6	4.48	15.5	1.67	7.4
Panel C: Excess												
Post 2011 × S^{excess}	23.340***	-0.178	0.664	0.197	0.004	0.053	22.530***	1.428	-0.292	0.512	-0.003	0.651
	(7.700)	(1.203)	(1.154)	(0.387)	(0.057)	(0.072)	(6.183)	(1.393)	(0.295)	(0.800)	(0.220)	(0.958)
Mean of DV	154	23.4	16.6	1.65	.137	.154	33.6	6.62	4.47	15.5	1.68	7.42

Notes: All regressions include district- and year fixed effects. The dependent variable measures the count of the number of cases per year belonging to each category or classification. Panel A distinguishes who becomes homeless by household type, age and priority need category. Panel B studies why individuals become homeless. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 10: 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)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel B: EU referendum electorate								
$S^{\text{percentile \& excess}}$	-0.759**	-0.392	-0.759**	-0.759**				
	(0.322)	(0.303)	(0.322)	(0.322)				
$S^{\text{percentile}}$					-0.992***	-1.383**		
					(0.309)	(0.626)		
S^{excess}							-0.764***	-1.232**
							(0.225)	(0.589)
R2	.786	.676	.786	.786	.792	.801	.788	.858
Mean of DV	91.3	92.4	91.3	91.3	91.3	91.6	91.3	91.6
Observations	365	332	365	365	365	71	364	89
Panel B: Turnout								
$S^{\text{percentile \& excess}}$	-1.380***	-1.779***	-1.380***	-1.380***				
	(0.387)	(0.343)	(0.387)	(0.387)				
$S^{\text{percentile}}$					-1.824***	-2.450***		
					(0.247)	(0.579)		
S^{excess}							-1.457***	-2.142***
							(0.241)	(0.335)
R2	.718	.724	.718	.718	.746	.799	.733	.912
Mean of DV	73.8	74.2	73.8	73.8	73.8	76.3	73.8	75.5
Observations	365	332	365	365	365	71	364	89
Panel C: % support for Leave								
$S^{\text{percentile \& excess}}$	1.995***	0.884	1.995***	1.995***				
	(0.515)	(0.552)	(0.515)	(0.515)				
$S^{\text{percentile}}$					2.191***	1.090		
					(0.508)	(1.986)		
S^{excess}							0.503	0.959
							(0.593)	(0.756)
R2	.771	.757	.771	.771	.774	.715	.752	.79
Mean of DV	53.2	54.6	53.2	53.2	53.2	53	53.1	52.4
Observations	365	332	365	365	365	71	364	89
London included?	X		X	X	X	X	X	X
$C_{d,c,2010}$			X					
Matched sample						X		X

Notes: All regressions include NUTS2 level shifters and also include 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. 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. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Online Appendix

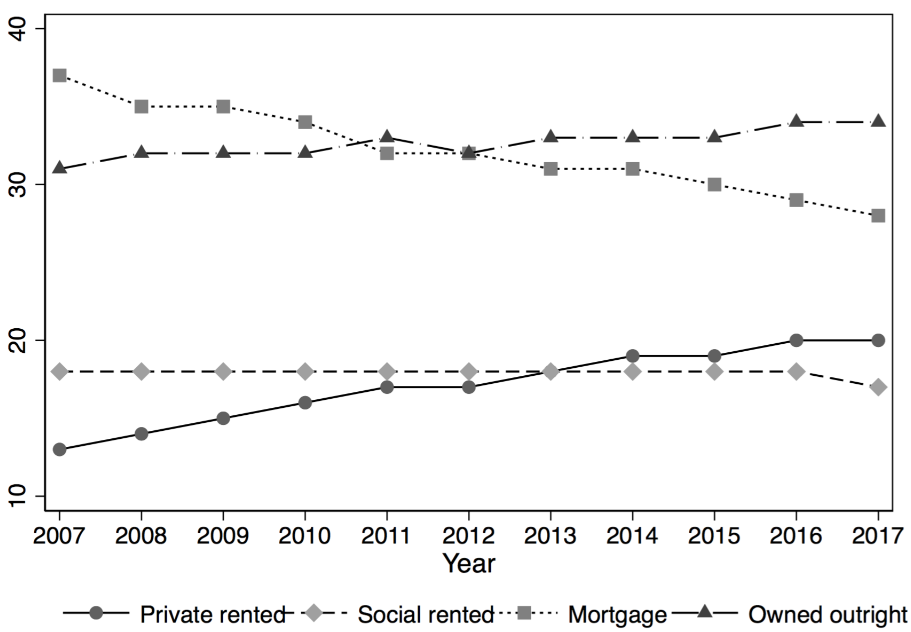
"Housing insecurity, homelessness and populism: Evidence from the UK"

For Online Publication

Thiemo Fetzer, Srinjoy Sen and Pedro CL Souza

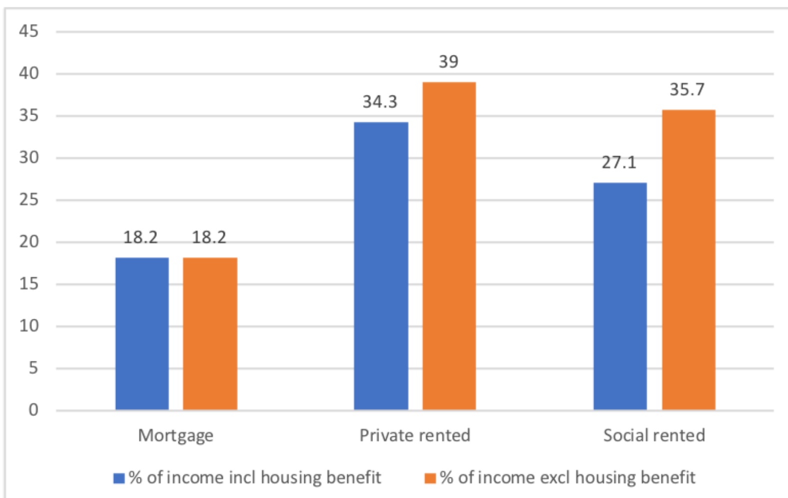
February 25, 2020

Figure A1: 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).

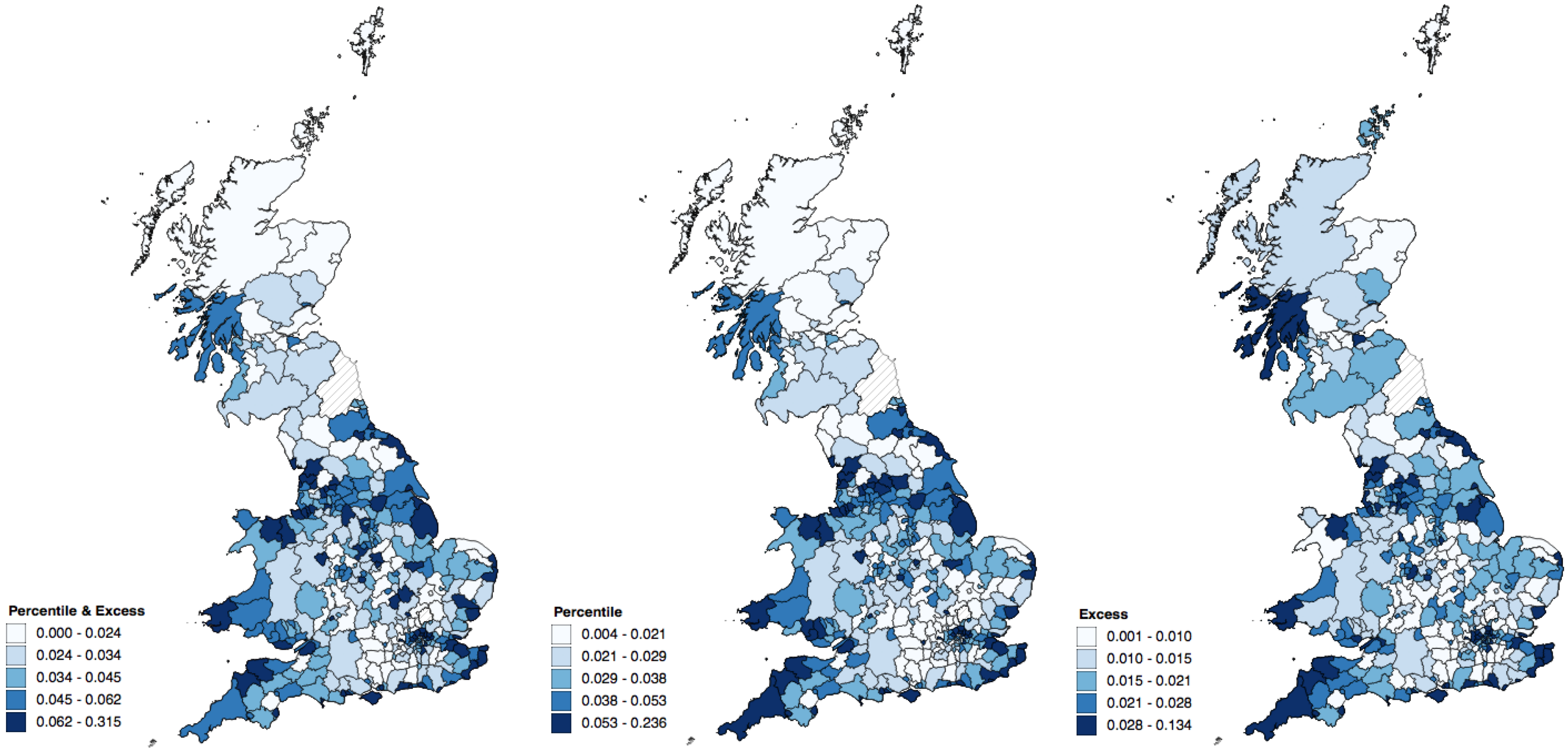
Figure A2: Affordability and the impact of housing benefit across the market segments



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 A3: Ex-ante estimated impact of cuts to housing benefit: spatial distribution of share of resident households affected

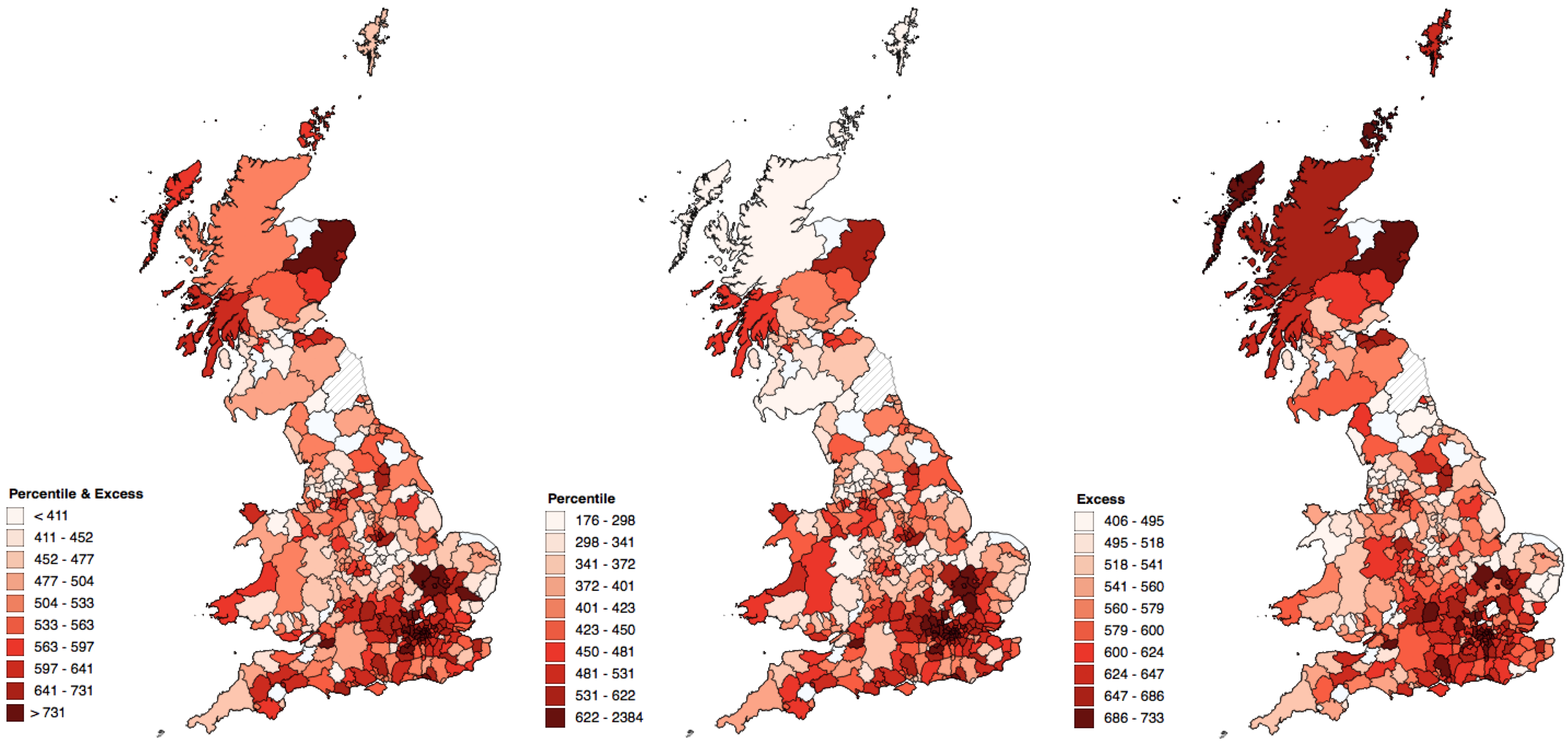
Panel A: Percentile & excess *Panel B*: Percentile *Panel C*: Excess



Notes: Map plots out the share of households affected by the housing benefit reforms implemented from April 2011. Panel A presents the combined measure, Panel B focuses on the share of households affected by the cut to local housing allowance, while Panel C presents the share of households affected by the removal of the excess.

Figure A4: Ex-ante estimated impact of cuts to housing benefit: spatial distribution of financial losses per resident households

Panel A: Percentile & excess *Panel B*: Percentile *Panel C*: Excess



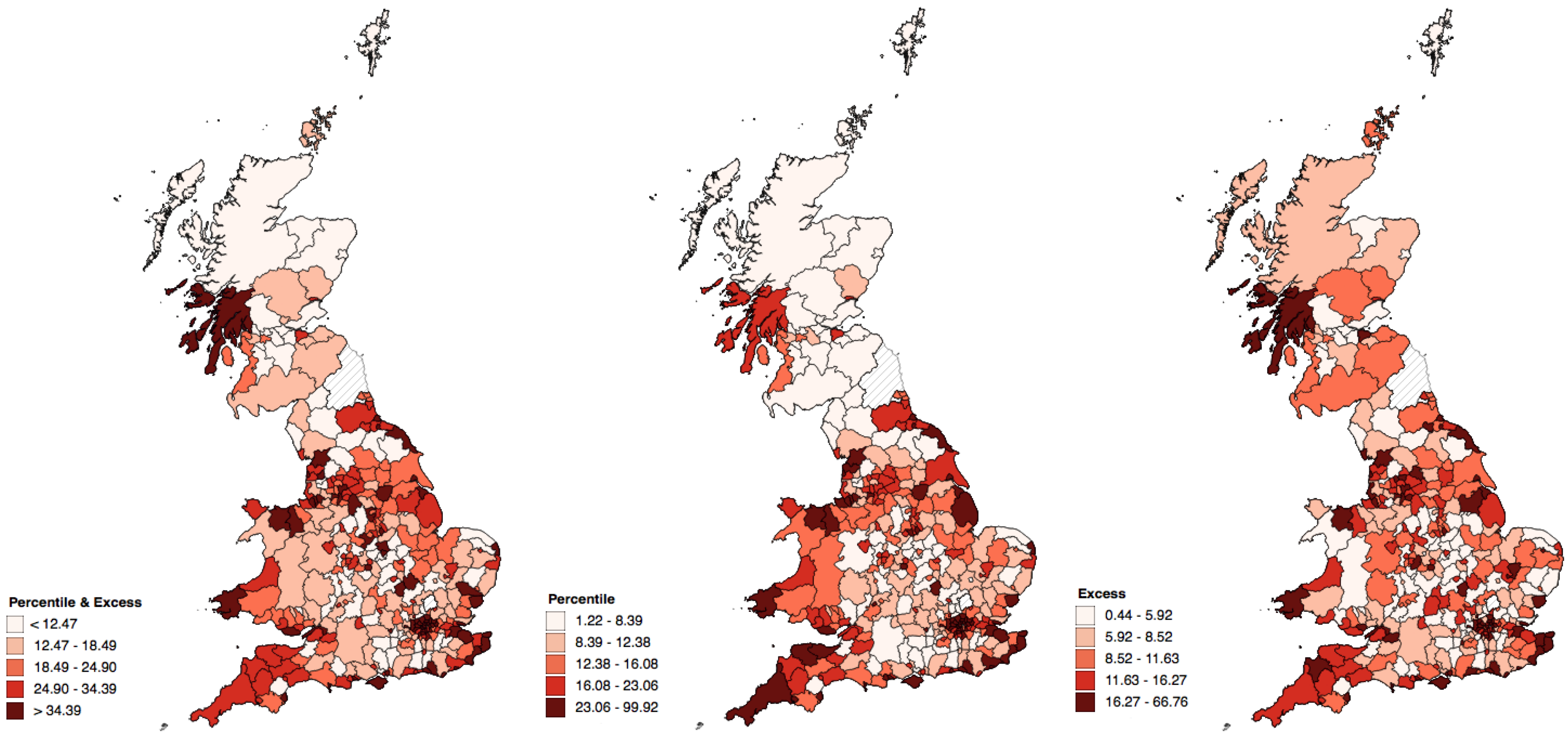
Notes: Map plots out the share of households affected by the housing benefit reforms implemented from April 2011. Panel A presents the combined measure, Panel B focuses on the share of households affected by the cut to local housing allowance, while Panel C presents the share of households affected by the removal of the excess.

Figure A5: Ex-ante estimated impact of cuts to housing benefit: spatial distribution of financial losses per resident households

Panel A: Percentile & excess

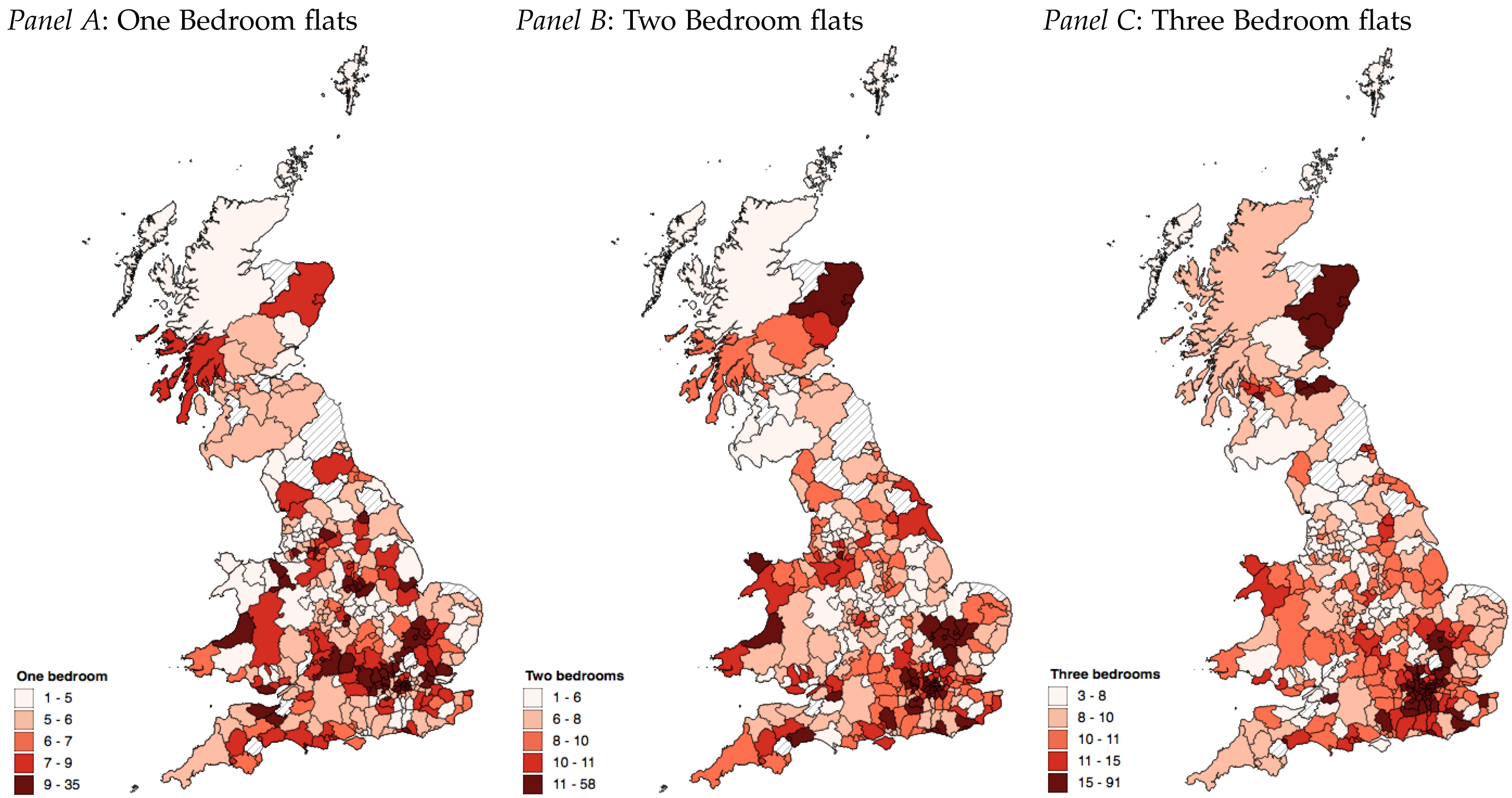
Panel B: Percentile

Panel C: Excess



Notes: Map plots out the share of households affected by the housing benefit reforms implemented from April 2011. Panel A presents the combined measure, Panel B focuses on the share of households affected by the cut to local housing allowance, while Panel C presents the share of households affected by the removal of the excess.

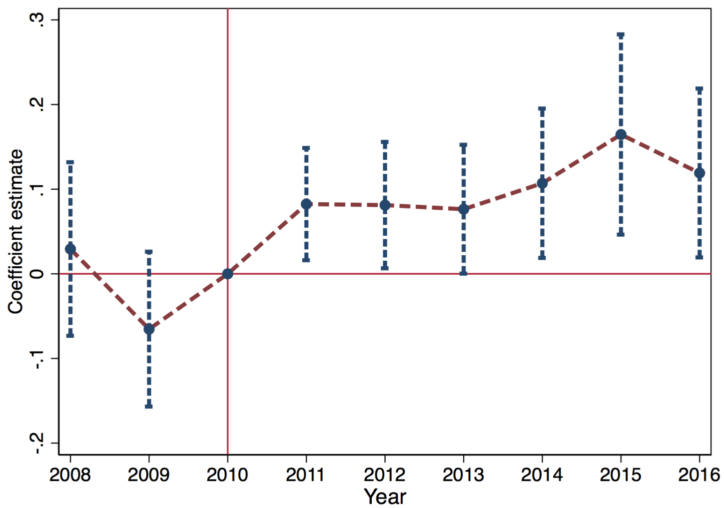
Figure A6: Estimated impact of reducing Local Housing Allowance from covering median to 30th percentile of rents at the district level for different types of properties



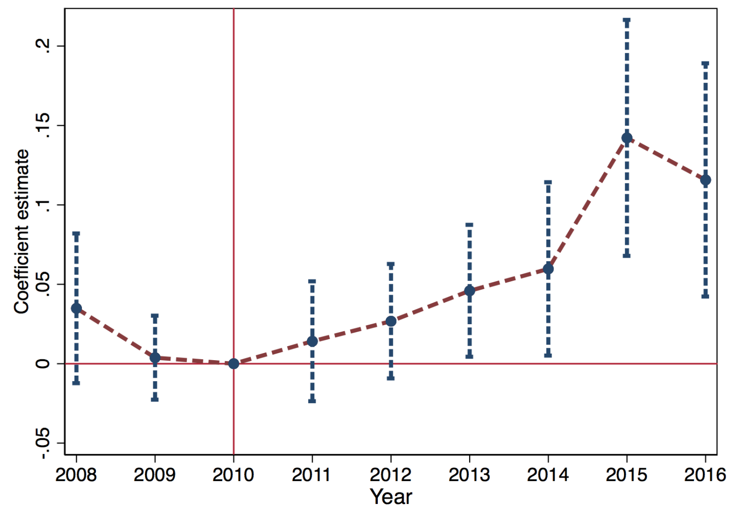
Notes: Figure plots the amount lost in pounds per week in housing benefit per household due to the reduction in the local housing allowance rate covering the 50th percentile of private sector rents to only cover up to the 30th percentile of private sector rents. The figure highlights significant spatial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

Figure A7: Impact of cut to housing benefit on individual insolvency cases and bankruptcies

Panel A: All individual insolvency cases



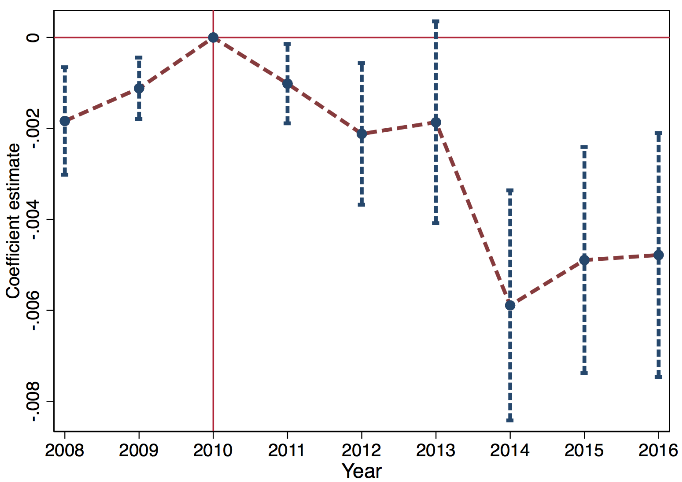
Panel B: Individual voluntary arrangements



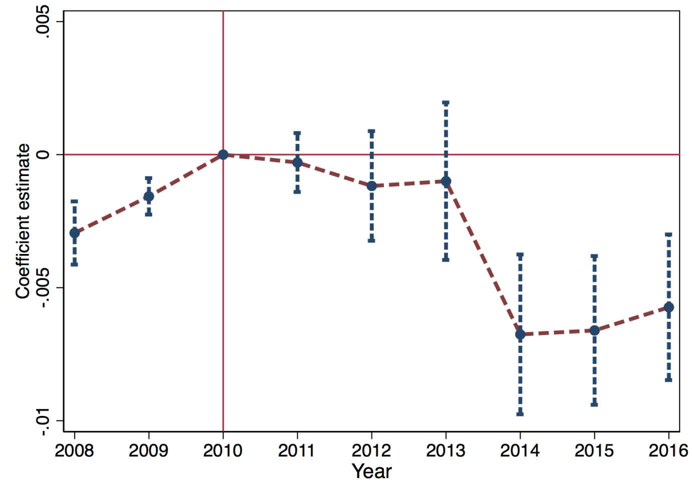
Notes: All dependent variables are measured as rates relative to the number of resident households in a district. The dependent variable in Panel A measures all individual new (not corporate) bankruptcy cases issued in a calendar year. Panel B focuses on all new so-called individual voluntary arrangements 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. 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 A8: Impact of cut to housing benefit on measures of electoral registration rates - parliamentary electorate / voting age population

Panel A: % parliamentary electorate/ voting age population

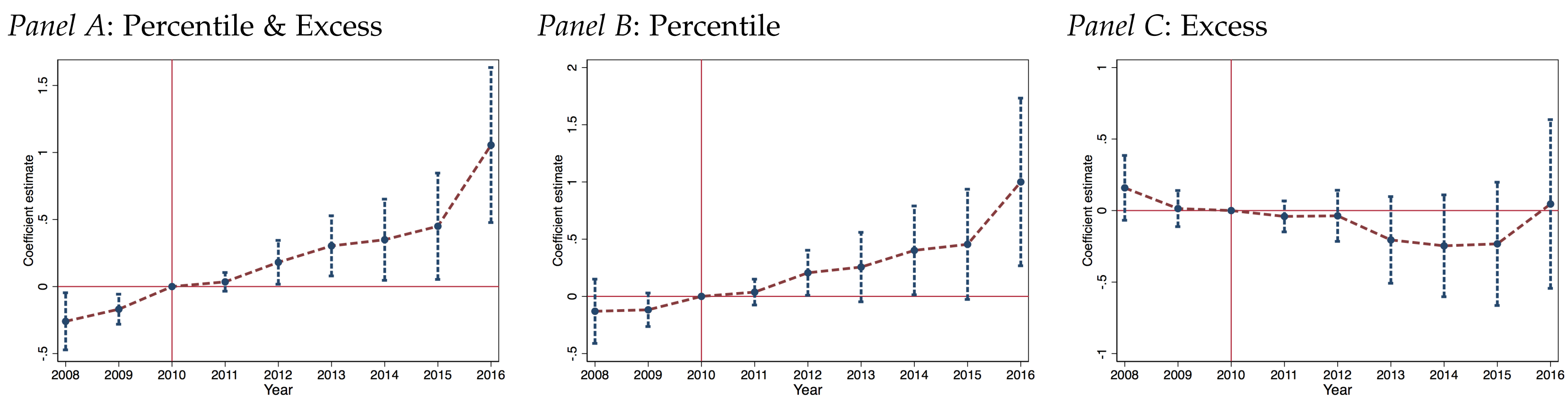


Panel B: % local election electorate/ voting age population



Notes: Figure plots from the regression 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 is the parliamentary electorate as a share of the voting age population. Panel B is the share of the local election electorate with respect to the voting age population. 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.

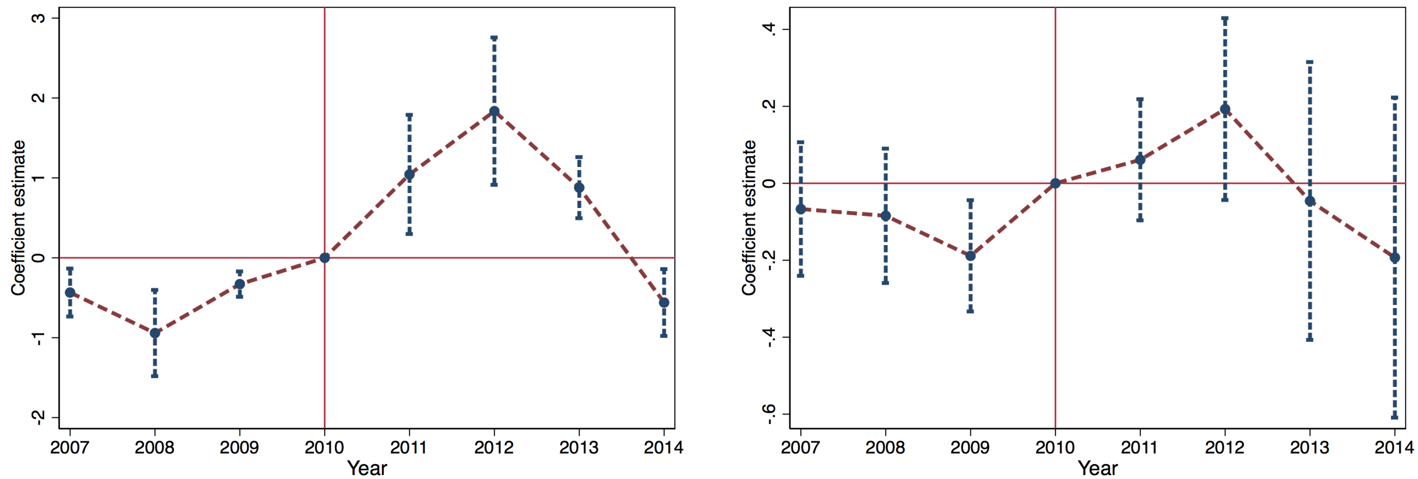
Figure A9: Impact of cut to housing benefit on average private sector rents



Notes: Figure plots from the regression 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 is the parliamentary electorate as a share of the voting age population. Panel B is the share of the local election electorate with respect to the voting age population. 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 A10: 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: Impact of cut to housing benefit on housing benefit spending per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: log(Housing benefit per capita)								
post $\times$ $S^{\text{percentile \& excess}}$	-0.013***	-0.007***	-0.011**	-0.013***				
	(0.003)	(0.002)	(0.005)	(0.003)				
post $\times$ $S^{\text{percentile}}$					-0.014***	-0.018*		
					(0.004)	(0.009)		
post $\times$ S^{excess}							-0.012***	-0.008
							(0.003)	(0.007)
Mean of DV	6.6	6.57	6.52	6.6	6.6	6.46	6.6	6.57
Local authority districts	366	366	333	366	366	74	365	94
Observations	3294	2196	2997	3294	3294	666	3285	846
Include data after 2013	X		X	X	X	X	X	X
London included?	X	X		X	X	X	X	X
$C^{j}_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		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 A2: Impact of cut to housing benefit on eviction measures: focusing on percentile- and excess-shock separately

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile shock				*excess shock*			
Panel A: Possession claims due to rent arrears								
post × $S^{\text{percentile}}$	0.483***	0.485***	0.154**	0.594***				
	(0.082)	(0.078)	(0.060)	(0.084)				
post × S^{excess}					0.361***	0.323***	0.087**	1.913***
					(0.101)	(0.093)	(0.041)	(0.233)
Mean of DV	2.18	2.07	1.66	2.18	2.18	2.07	1.66	2.18
Local authority districts	366	366	333	366	365	365	332	365
Observations	3293	2195	2997	3293	3284	2189	2988	3284
Panel B: Repossessions								
post × $S^{\text{percentile}}$	0.290***	0.236***	0.095**	0.288***				
	(0.060)	(0.043)	(0.046)	(0.053)				
post × S^{excess}					0.232***	0.143***	0.044	0.963***
					(0.071)	(0.048)	(0.029)	(0.158)
Mean of DV	1.47	1.34	1.19	1.47	1.47	1.34	1.19	1.47
Local authority districts	366	366	333	366	365	365	332	365
Observations	3293	2195	2997	3293	3284	2189	2988	3284
Panel C: All private rented-sector eviction actions								
post × $S^{\text{percentile}}$	1.701***	1.509***	0.492**	2.100***				
	(0.290)	(0.239)	(0.190)	(0.301)				
post × S^{excess}					1.281***	1.003***	0.265**	6.439***
					(0.362)	(0.287)	(0.127)	(0.800)
Mean of DV	5.46	4.99	3.99	5.46	5.46	4.99	3.99	5.46
Local authority districts	366	366	333	366	365	365	332	365
Observations	3293	2195	2997	3293	3284	2189	2988	3284
Panel D: All social-rented rented-sector eviction actions								
post × $S^{\text{percentile}}$	-0.137	-0.088	0.026	-0.339*				
	(0.107)	(0.101)	(0.134)	(0.187)				
post × S^{excess}					-0.173	-0.233**	-0.033	-1.170**
					(0.124)	(0.118)	(0.126)	(0.592)
Mean of DV	10.5	10.7	9.83	10.5	10.5	10.7	9.83	10.5
Local authority districts	366	366	333	366	365	365	332	365
Observations	3293	2195	2997	3293	3284	2189	2988	3284
London included?	X	X		X	X	X		X
Include data after 2013	X		X	X	X		X	X
$C^{j}_{d,c,2010}$ trends				X				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 A3: Impact of cut to housing benefit on bankruptcies: focusing on percentile- and excess-shock separately

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile shock				*excess shock*			
Panel A: Total individual bankruptcies								
post × $S^{\text{percentile}}$	0.155***	0.127***	0.119*	0.132***				
	(0.037)	(0.045)	(0.062)	(0.049)				
post × S^{excess}					0.141***	0.129**	0.091	0.044
					(0.050)	(0.054)	(0.057)	(0.154)
Mean of DV	6.01	6.54	6.27	6.01	6.01	6.53	6.26	6.01
Local authority districts	338	338	305	338	337	337	304	337
Observations	3041	2027	2745	3041	3032	2021	2736	3032
Panel B: Individual voluntary arrangements								
post × $S^{\text{percentile}}$	0.097***	0.041***	0.159***	0.011				
	(0.019)	(0.015)	(0.028)	(0.016)				
post × S^{excess}					0.096***	0.043**	0.113***	-0.148
					(0.019)	(0.017)	(0.026)	(0.091)
Mean of DV	2.62	2.63	2.73	2.62	2.61	2.63	2.73	2.61
Local authority districts	338	338	305	338	337	337	304	337
Observations	3041	2027	2745	3041	3032	2021	2736	3032
London included?	X	X		X	X	X		X
Include data after 2013	X		X	X	X		X	X
$C^j_{d,c,2010}$ trends				X				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 A4: Impact of cut to housing benefit on council spending on temporary housing and statutory homeless duties: focusing on percentile- and excess-shock separately

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile shock				*excess shock*			
Panel A: Temporary accommodation								
post × $S^{\text{percentile}}$	0.532***	0.262*	0.010	1.152***				
	(0.177)	(0.153)	(0.166)	(0.306)				
post × S^{excess}					0.337*	0.073	0.221	2.713***
					(0.186)	(0.161)	(0.211)	(0.548)
Mean of DV	2.99	2.69	1.72	2.99	2.99	2.69	1.72	2.99
Local authority districts	366	366	333	366	365	365	332	365
Observations	2807	1800	2545	2807	2799	1795	2537	2799
Panel B: Council spending on hostels and BnB's								
post × $S^{\text{percentile}}$	8.295***	3.824***	0.781	12.457***				
	(1.968)	(1.384)	(0.524)	(2.119)				
post × S^{excess}					4.200**	1.007	0.116	32.101***
					(1.884)	(1.171)	(0.383)	(6.383)
Mean of DV	9.83	7.58	4.11	9.83	9.85	7.6	4.12	9.85
Local authority districts	366	366	333	366	365	365	332	365
Observations	3243	2195	2947	3243	3234	2189	2938	3234
Panel C: Total council spending on temporary housing								
post × $S^{\text{percentile}}$	17.536***	8.651***	1.601	31.967***				
	(3.700)	(2.699)	(1.151)	(4.699)				
post × S^{excess}					8.978***	2.918**	1.859	57.223***
					(2.421)	(1.383)	(1.511)	(10.067)
Mean of DV	18.2	14.6	5.15	18.2	18.2	14.6	5.17	18.2
Local authority districts	366	366	333	366	365	365	332	365
Observations	3293	2195	2997	3293	3284	2189	2988	3284
London included?	X	X		X	X	X		X
Include data after 2013	X		X	X	X		X	X
$C^j_{d,c,2010}$ trends				X				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 A5: Impact of cut to housing benefit on homelessness and rough sleeping: focusing on percentile- and excess-shock separately

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile shock				*excess shock*			
Panel A: Statutory homelessness								
post × $S^{\text{percentile}}$	0.563***	0.450***	0.336**	0.761***				
	(0.121)	(0.113)	(0.162)	(0.173)				
post × S^{excess}					0.396***	0.247**	0.234*	2.262***
					(0.123)	(0.105)	(0.126)	(0.465)
Mean of DV	4.27	4.39	4.17	4.27	4.27	4.4	4.17	4.27
Local authority districts	366	366	333	366	365	365	332	365
Observations	3265	2179	2971	3265	3256	2173	2962	3256
Panel B: Rough sleeping street counts								
post × $S^{\text{percentile}}$	3.310**	0.847**	1.918**	5.585**				
	(1.539)	(0.385)	(0.877)	(2.644)				
post × S^{excess}					2.667***	0.852**	2.612***	10.116***
					(0.764)	(0.416)	(0.914)	(2.982)
Mean of DV	8.56	6.79	7.23	8.56	8.56	6.79	7.22	8.56
Local authority districts	316	316	283	316	315	315	282	315
Observations	2212	1264	1981	2212	2205	1260	1974	2205
London included?	X	X		X	X	X		X
Include data after 2013	X		X	X	X		X	X
$C^{j}_{d,c,2010}$ trends				X				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 A6: Impact of housing benefit cut on electoral registration coverage rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: Parliamentary electors								
post × $S^{\text{percentile \& excess}}$	-0.002**	-0.001	-0.001	-0.002**				
	(0.001)	(0.001)	(0.002)	(0.001)				
post × $S^{\text{percentile}}$					-0.004***	-0.005		
					(0.001)	(0.003)		
post × S^{excess}							-0.005***	-0.004**
							(0.001)	(0.002)
Mean of DV	.924	.938	.936	.924	.924	.926	.924	.922
Local authority districts	338	338	305	338	338	74	337	94
Observations	3042	2028	2745	3042	3042	666	3033	846
Panel B: Local government electors								
post × $S^{\text{percentile \& excess}}$	-0.002*	0.001	-0.001	-0.002				
	(0.001)	(0.001)	(0.002)	(0.002)				
post × $S^{\text{percentile}}$					-0.004***	-0.002		
					(0.001)	(0.003)		
post × S^{excess}							-0.003***	-0.002
							(0.001)	(0.002)
Mean of DV	.948	.961	.954	.948	.948	.948	.948	.948
Local authority districts	366	366	333	366	366	74	365	94
Observations	3257	2159	2960	3257	3257	664	3248	844
Include data after 2013	X		X	X	X	X	X	X
London included?	X	X		X	X	X	X	X
$C^j_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		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.

Table A7: Impact of cut to housing benefit on unemployment and economic inactivity rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: Unemployment rate								
post $\times$ $S^{\text{percentile \& excess}}$	-0.033	0.016	0.114	-0.148***				
	(0.052)	(0.094)	(0.071)	(0.041)				
post $\times$ $S^{\text{percentile}}$					-0.051	-0.264		
					(0.068)	(0.258)		
post $\times$ S^{excess}							-0.014	-0.076
							(0.059)	(0.173)
Mean of DV	6.8	7.13	6.65	6.8	6.8	5.98	6.79	6.52
Local authority districts	364	364	332	364	364	74	363	92
Observations	2778	2114	2522	2778	2778	510	2770	662
Panel B: Inactive but wants job								
post $\times$ $S^{\text{percentile \& excess}}$	-0.193	-0.198	-0.566*	-0.007				
	(0.220)	(0.285)	(0.337)	(0.195)				
post $\times$ $S^{\text{percentile}}$					-0.390	-0.759		
					(0.256)	(1.073)		
post $\times$ S^{excess}							-0.451*	-0.794
							(0.238)	(0.505)
Mean of DV	24.6	24.4	24.5	24.6	24.6	24.8	24.6	24
Local authority districts	365	365	333	365	365	74	364	94
Observations	2843	2137	2587	2843	2843	544	2835	710
London included?	X	X		X	X	X	X	X
Include data after 2013	X		X	X	X	X	X	X
$C^{j}_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		X

Notes: All regressions include district- and year fixed effects. The dependent variable in Panel A measures the district-level unemployment rate, while Panel B focuses on the share of inactive working age adults that want a job but are not actively searching. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A8: Impact of housing benefit cut on property prices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: All property types								
post × $S^{\text{percentile \& excess}}$	0.019***	0.011**	-0.019***	0.049***				
	(0.006)	(0.005)	(0.005)	(0.015)				
post × $S^{\text{percentile}}$					0.016**	-0.027***		
					(0.008)	(0.008)		
post × S^{excess}							0.004	-0.006
							(0.006)	(0.012)
Mean of DV	12.1	12.1	12.1	12.1	12.1	12.2	12.1	12.3
Local authority districts	338	338	305	338	338	74	337	94
Observations	3042	2028	2745	3042	3042	666	3033	846
Panel B: Flats								
post × $S^{\text{percentile \& excess}}$	0.025**	0.015**	-0.032***	0.072***				
	(0.010)	(0.007)	(0.010)	(0.024)				
post × $S^{\text{percentile}}$					0.026**	-0.029*		
					(0.011)	(0.016)		
post × S^{excess}							0.008	-0.004
							(0.010)	(0.019)
Mean of DV	11.8	11.7	11.7	11.8	11.8	11.8	11.8	11.9
Local authority districts	338	338	305	338	338	74	337	94
Observations	3042	2028	2745	3042	3042	666	3033	846
Panel C: Terraced houses								
post × $S^{\text{percentile \& excess}}$	0.023***	0.014***	-0.022***	0.060***				
	(0.007)	(0.005)	(0.007)	(0.022)				
post × $S^{\text{percentile}}$					0.023**	-0.024*		
					(0.009)	(0.013)		
post × S^{excess}							0.011	-0.008
							(0.008)	(0.017)
Mean of DV	12	11.9	11.9	12	12	12.1	12	12.1
Local authority districts	337	337	305	337	337	74	336	94
Observations	3033	2022	2745	3033	3033	666	3024	846
Panel D: Semi-detached houses								
post × $S^{\text{percentile \& excess}}$	0.031***	0.022***	-0.016***	0.070***				
	(0.008)	(0.008)	(0.006)	(0.017)				
post × $S^{\text{percentile}}$					0.028***	-0.023**		
					(0.010)	(0.010)		
post × S^{excess}							0.013*	-0.005
							(0.008)	(0.017)
Mean of DV	12.2	12.1	12.1	12.2	12.2	12.3	12.2	12.3
Local authority districts	337	337	305	337	337	74	336	94
Observations	3033	2022	2745	3033	3033	666	3024	846
Include data after 2013	X		X	X	X	X	X	X
London included?	X	X		X	X	X	X	X
$C^{j}_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		X

Notes: All regressions include district- and year fixed effects. All dependent variable capture the log of average property sales prices per district and year by property type indicated in the Panel heading. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A9: Impact of housing benefit cut on broader rental market developments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: Average private sector rent								
post × $S^{\text{percentile \& excess}}$	0.539**	0.316**	-1.211***	1.710***				
	(0.212)	(0.134)	(0.334)	(0.613)				
post × $S^{\text{percentile}}$					0.475*	-1.760***		
					(0.271)	(0.521)		
post × S^{excess}							-0.177	-1.254*
							(0.235)	(0.649)
Mean of DV	86.5	80.9	84.1	86.5	86.5	89.8	86.5	92.2
Local authority districts	316	316	283	316	316	74	315	94
Observations	2844	1896	2547	2844	2844	666	2835	846
Panel B: Social rent								
post × $S^{\text{percentile \& excess}}$	0.441*	0.295*	-1.364***	1.058***				
	(0.228)	(0.160)	(0.330)	(0.308)				
post × $S^{\text{percentile}}$					0.347	-3.411***		
					(0.325)	(1.106)		
post × S^{excess}							-0.149	0.544
							(0.254)	(0.546)
Mean of DV	78	73	74.2	78	78	77	78	78.9
Local authority districts	171	170	142	171	171	16	171	32
Observations	1479	992	1219	1479	1479	118	1479	260
Panel C: log(private sector rent)								
post × $S^{\text{percentile \& excess}}$	0.001	0.001	-0.009***	0.008***				
	(0.002)	(0.001)	(0.004)	(0.003)				
post × $S^{\text{percentile}}$					-0.000	-0.014***		
					(0.002)	(0.005)		
post × S^{excess}							-0.004**	-0.009*
							(0.002)	(0.005)
Mean of DV	4.44	4.38	4.42	4.44	4.44	4.49	4.44	4.51
Local authority districts	316	316	283	316	316	74	315	94
Observations	2844	1896	2547	2844	2844	666	2835	846
Panel D: log(social rent)								
post × $S^{\text{percentile \& excess}}$	-0.003	-0.003*	-0.012***	-0.000				
	(0.002)	(0.002)	(0.004)	(0.001)				
post × $S^{\text{percentile}}$					-0.005**	-0.025		
					(0.002)	(0.022)		
post × S^{excess}							-0.006***	0.005
							(0.002)	(0.009)
Mean of DV	4.34	4.27	4.29	4.34	4.34	4.33	4.34	4.35
Local authority districts	171	170	142	171	171	16	171	32
Observations	1479	992	1219	1479	1479	118	1479	260
Include data after 2013	X		X	X	X	X	X	X
London included?	X	X		X	X	X	X	X
$C^{j}_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		X

Notes: All regressions include district- and year fixed effects. The dependent variable in Panel A measures the average private sector rent per district and week. Panel B uses the average social rent per district and week. Panel C and D study the underlying rent in logs. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A10: Impact of cut to housing benefit on crimes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: Theft from person								
post × $S^{\text{percentile \& excess}}$	1.230***	1.681***	0.113	2.032***				
	(0.358)	(0.505)	(0.166)	(0.288)				
post × $S^{\text{percentile}}$					0.975**	0.610		
					(0.431)	(0.516)		
post × S^{excess}							0.371**	0.444
							(0.173)	(0.333)
Mean of DV	4.17	4.24	2.66	4.17	4.17	2.84	4.18	3.67
Local authority districts	325	325	292	325	325	66	324	86
Observations	2438	2146	2175	2438	2438	452	2430	634
Panel B: Burglaries								
post × $S^{\text{percentile \& excess}}$	0.090	0.154	-0.420*	0.320***				
	(0.160)	(0.141)	(0.224)	(0.107)				
post × $S^{\text{percentile}}$					0.113	-0.512		
					(0.150)	(0.529)		
post × S^{excess}							-0.187	-0.686
							(0.176)	(0.535)
Mean of DV	11.8	12.1	10.7	11.8	11.8	10.3	11.9	12.2
Local authority districts	325	325	292	325	325	66	324	86
Observations	2438	2146	2175	2438	2438	452	2430	634
Panel C: Bodily harm								
post × $S^{\text{percentile \& excess}}$	0.044	0.005	-0.274	0.312*				
	(0.176)	(0.175)	(0.464)	(0.174)				
post × $S^{\text{percentile}}$					0.057	-1.219*		
					(0.168)	(0.649)		
post × S^{excess}							-0.124	-0.498
							(0.185)	(0.532)
Mean of DV	19.6	19.5	18.7	19.6	19.6	16.6	19.6	17.2
Local authority districts	325	325	292	325	325	66	324	86
Observations	2438	2146	2175	2438	2438	452	2430	634
Include data after 2013	X		X	X	X	X	X	X
London included?	X	X		X	X	X	X	X
$C^{j}_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		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 A11: Impact of housing benefit cut on international migration indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: Short-term international migration								
post × $S^{\text{percentile \& excess}}$	0.632	0.070	1.492	0.142				
	(0.462)	(0.371)	(1.008)	(0.526)				
post × $S^{\text{percentile}}$					0.463	2.484		
					(0.483)	(2.197)		
post × S^{excess}							1.136*	1.212
							(0.663)	(1.370)
Mean of DV	19.7	17.6	15.8	19.7	19.7	19.4	19.7	21.3
Local authority districts	337	337	305	337	337	74	336	94
Observations	2696	2022	2440	2696	2696	592	2688	752
Panel B: Long-term international migration								
post × $S^{\text{percentile \& excess}}$	59.363	6.298	61.937*	68.321				
	(55.092)	(57.945)	(33.073)	(88.752)				
post × $S^{\text{percentile}}$					30.671	103.487*		
					(45.629)	(56.466)		
post × S^{excess}							4.885	17.119
							(34.030)	(54.304)
Mean of DV	1546	1498	1108	1546	1546	1346	1548	1856
Local authority districts	366	366	333	366	366	74	365	94
Observations	3294	2196	2997	3294	3294	666	3285	846
Panel C: New migrant GP registrations								
post × $S^{\text{percentile \& excess}}$	2.273	0.455	0.068	4.437*				
	(1.882)	(1.709)	(1.421)	(2.482)				
post × $S^{\text{percentile}}$					-0.403	-0.037		
					(1.948)	(1.641)		
post × S^{excess}							-2.193*	-5.101
							(1.262)	(3.257)
Mean of DV	95.5	92.1	76.6	95.5	95.5	93.5	95.6	108
Local authority districts	337	337	305	337	337	74	336	94
Observations	3033	2022	2745	3033	3033	666	3024	846
Panel D: New National Insurance (NINO) issue								
post × $S^{\text{percentile \& excess}}$	0.939	-1.532	1.688	1.281				
	(1.095)	(2.074)	(1.182)	(1.336)				
post × $S^{\text{percentile}}$					-0.874	4.363**		
					(1.306)	(1.701)		
post × S^{excess}							-0.784	-2.273
							(1.312)	(2.501)
Mean of DV	89.2	82.3	65.9	89.2	89.2	77.8	89.2	99.2
Local authority districts	365	365	333	365	365	74	364	94
Observations	3285	2190	2997	3285	3285	666	3276	846
Include data after 2013	X		X	X	X	X	X	X
London included?	X	X		X	X	X	X	X
$C^{j}_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		X

Notes: All regressions include district- and year fixed effects. The dependent variable in Panel A measures short term international migration inflows (typically students or seasonal workers); Panel B studies long term international migrant inflows. Panel C explores new migrant registration with general healthcare practitioners, while Panel D explores new issuance of national insurance numbers. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A12: Impact of housing benefit cut on internal migration indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	percentile & excess shock				*percentile shock*		*excess shock*	
Panel A: Non-British resident share								
post × $S^{\text{percentile \& excess}}$	3.126	1.879	10.462	0.448				
	(11.670)	(9.728)	(10.658)	(17.489)				
post × $S^{\text{percentile}}$					13.097	6.002		
					(11.306)	(33.792)		
post × S^{excess}							12.679	-10.320
							(8.813)	(23.366)
Mean of DV	707	668	549	707	707	691	707	826
Local authority districts	361	355	329	361	361	74	360	92
Observations	3058	2024	2770	3058	3058	614	3049	770
Panel B: Internal migration inflow rate								
post × $S^{\text{percentile \& excess}}$	-11.116***	-9.000***	-2.452	-16.970***				
	(2.782)	(2.329)	(2.725)	(2.529)				
post × $S^{\text{percentile}}$					-11.369***	-5.433		
					(2.834)	(5.434)		
post × S^{excess}							-7.481***	-3.143
							(1.928)	(4.447)
Mean of DV	497	485	477	497	497	575	497	547
Local authority districts	365	365	333	365	365	74	364	94
Observations	3285	2190	2997	3285	3285	666	3276	846
Panel C: Internal migration outflow rate								
post × $S^{\text{percentile \& excess}}$	-1.770	-1.324	-1.937	-1.183				
	(1.256)	(0.950)	(1.595)	(2.111)				
post × $S^{\text{percentile}}$					-1.044	2.339		
					(1.378)	(3.623)		
post × S^{excess}							-0.275	-3.046
							(1.090)	(2.782)
Mean of DV	485	475	457	485	485	545	485	536
Local authority districts	365	365	333	365	365	74	364	94
Observations	3285	2190	2997	3285	3285	666	3276	846
Include data after 2013	X		X	X	X	X	X	X
London included?	X	X		X	X	X	X	X
$C^{j}_{d,c,2010}$ trends				X				
Matched pair x Year FE						X		X

Notes: All regressions include district- and year fixed effects. Panel A measures the share of non-British residents as dependent variable; Panel B studies internal migration inflow rates, while Panel C studies internal migration outflow rates. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.