Housing insecurity and homelessness: Evidence from the UK*

Thiemo Fetzer Srinjoy Sen Pedro CL Souza

May 27, 2022

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 in rent subsidies for low income households in the United Kingdom in April 2011. Using comprehensive district-level administrative data, we show that the affordability shock caused a significant increase in: financial distress, evictions, property crimes, insecure temporary housing arrangements, statutory homelessness, and actual rough sleeping. The most notable rise in statutory homelessness is driven by families with children, lone parents, individuals with existing health conditions, and as a result of having been evicted. We estimate that the fiscal savings were low and shifted towards the local administration: savings by the central government were partially offset by increase in council spending to meet statutory obligations for homelessness.

Keywords: HOUSING POLICY, RENTAL SUBSIDIES, FINANCIAL DISTRESS, EVICTIONS, WELFARE CUTS, AUSTERITY

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

*Fetzer and Sen are based in the Department of Economics at the University of Warwick. Souza is based in the School of Economics and Finance at Queen Mary University of London. We thank Tim Besley, Mirko Draca, Karsten Mueller, Yuriy Gorodnichenko and seminar audiences at Warwick University, The Tony Blair Institute for Global Change and the Ministry of Communities, Housing and Local Government for detailed comments.

1 Introduction

In the past decades, an erosion of affordability contributed both to a rapid expansion of the private rental market and to growing financial burden on renters. In the United States (US), the United Kingdom (UK) and the European Union (EU) the share of households living in 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 EU increased from 22.5% in 2005 to 28.0% in 2018. In the UK, 37.3% of tenants are overburdened with housing costs.[1] These developments also have significant fiscal implications: across the EU, the 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.

This paper analyses the effects of a housing assistance cut in the UK. We explore a cut to the UK's housing benefit in April 2011 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.[2] Two simultaneous significant cuts to housing benefit became effective after the reform. First, the local housing allowance (LHA), which determines housing benefit payments, was cut back from the 50th to the 30th percentile of rents within a local rental market and dwelling type. The second cut removed the so-called *excess* payments: prior to the reform, claimants could keep the difference between the LHA rate and their actual rent up to at most £15 per week. Both cuts affected all new claimants and many existing claimants immediately, eventually reaching all claimants the latest by 2012. The reforms affected the near universe of housing benefit claimants: 936,000 out

[1]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).

[2]The rented sector in the UK is segmented between social rented housing and private rented housing. Social rented sector tenants have not been directly 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/.

of around 1 million claimants, or, around 5.1 per cent of all households, representing 25 per cent of all households renting in the private sector. The *average loss* amounted to a housing benefit reduction of around £596 per year – around two and a half weeks of income for those earning the minimum-wage at the time. This rises to significantly above £2,000, on average, in some parts of London, and was even higher for some property types.

We use the differential incidence of the housing benefit cut across districts to trace out their causal effects. Our identification strategy contrasts places with higher exposure to the cuts relative to the places with lower exposure over time exploiting some distinct features of the cuts that make it quite unlikely that these could have been anticipated. This identifies the causal impact of the reforms under the assumption that the proportion of housing benefit claimants does not vary across districts in a way that is correlated with the unobservables of those districts and the timing of the 2011 reform.

We document that the cut led to a significant increase in rent arrears. Evictions of private sector tenants rose on average by 31.4 per cent for each unit of standard deviation of the cut. The cut caused a 15.5 per cent rise in the flow of vulnerable households seeking temporary accommodation from councils.[3] The flow of individual into statutory homelessness increased, on average, by 6.4 per cent due to the cut. Administrative data allow us to decompose this increase. We find that it is driven by young families with children, single parents, or households with a physical disability or a mental health condition who became homeless due to rent arrears and being evicted. Using street count estimates on prevalence of rough sleeping,[4] we document that the prevalence rose sharply in districts most exposed to the cuts. We interpret our results as the temporary effects that are followed by a permanent and long-term strategy of housing benefits reduction in the UK.[5]

[3]This occurs because councils owe a statutory obligation to provide housing to vulnerable households.

[4]Public Health England defines rough sleeping as "*people sleeping, about to bed down (...) or actually bedded down in the open air (...) or other places not designed for habitation.*" Source: https://www.gov.uk/government/publications/health-matters-rough-sleeping/health-matters-rough-sleeping

[5]Indeed a set of gradual and continuous further reforms were introduced after 2011 that undermined the real value of the benefits; the most recent of which was the freeze of the reference rents

We do not find diverging pre-trends and note sharp jumps in districts more affected in outcome measures immediately relevant and consistent with the timing of the cuts in 2011 and particularly 2012. We also document a number of additional results that help rule out a host of alternative mechanisms. First, we rule out that the results are confounded by a correlation between the policy shock and exposure to the Great Recession of 2009. We do so by flexibly controlling by pre-reform changes in economic conditions. Second, we do not find any evidence suggesting more exposed districts saw notable changes in economic activity rates or unemployment. 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, using data on crime, we document that in districts most exposed to the cut, property crimes and thefts increased timely but temporarily. Importantly, the effects predate any that could be attributed to the welfare reforms implemented through the Welfare Reform Act of 2013 two years later and that are studied in Fetzer (2019).

A narrow economic accounting suggests that the housing benefit cut was indeed successful in lowering the direct fiscal costs to the government. Yet, these savings mask significant indirect financial costs and longer-term social costs. Councils have to provide emergency accommodation to meet legal obligations to prevent homelessness, and is often sourced from the private rented sector *at market rates*. Not surprisingly, council spending on temporary accommodation and overnight shelters increased sharply, shooting up by, on average, 87.8 per cent owing to the cut. A significant driver of the cost increase for temporary accommodation (accounting for around 50.4 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

in 2016 for four years, while market rents continuously rose in that period. The primary impacts identified in the paper occur almost immediately and well before the subsequent reforms could have an effect.

on homeless prevention increased by at least 38 pence as a result of the cuts.

This paper, to the best of our knowledge, is the first to provide an analysis of the intended and unintended effects from a nationwide cut to a rent assistance program. The literature that studies the long-run social and economic implications of housing insecurity provides an important backdrop to these findings, which 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; Desmond and Kimbro, 2015; Desmond and Gershenson, 2017), achievement of children (Chyn, 2018), labor markets outcomes (Jacob and Ludwig, 2012; Desmond et al., 2016; Van Dijk, 2019), spatial sorting (Desmond and Shollenberger, 2015) and long-run housing instability (Collinson and Reed, 2018). Much less work has been done specifically on homelessness – a potential consequence of evictions or the most extreme form of housing insecurity. Existing literature[6] suggests that the likely social cost due to housing benefit cuts may be much larger than what we can currently account for.

Our paper also contributes to the literature studying the social and economic effects of housing assistance programs. Galiani et al. (2015) finds that reducing housing subsidies increases exposure to poverty. Eriksen and Ross (2015) 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. Diamond et al. (2019) shows that rent control reduced evictions in the short term, but led to a loss in housing supply undermining the short term effects of this policy.[7] Brewer et al. (2019) highlight that the incidence of the cuts is ultimately on the side of the tenants, due to the lack of effective renter protection and an overall regulatory environment favoring landlords. The effect sizes that we estimate are comparable in magnitude and direction to those in the aforementioned literature, which indicate a reduction in compliance with rents or mortgage payment, increase in evictions,

[6] Honig and Filer (1993) explores drivers of homelessness across the US. Phinney et al. (2007) suggests that drug use, mental and health problems are associated with homelessness. Evans et al. (2016) suggests that homelessness prevention measures in Chicago may be quite cost effective.

[7] See also Choon-Geol Moon and Stotsky, 1993 on the effects of rent control

and increase in measures of poverty due to benefit cuts that affect disposable income.

This paper proceeds as follows. In Section 2 we present the context and the data. Section 3 outlines the empirical strategy, followed by the results in Section 4. We present our back-of-the-envelope fiscal saving analysis in Section 5. Section 6 concludes.[8]

2 Context

2.1 Housing in the UK

The UK's real estate market is segmented 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.

A predominant issue in the UK, also common across many countries, is the lack of affordable housing. House prices have accelerated faster compared to incomes, resulting in worsening affordability – despite record low interest rates. This dynamic, coupled with a stagnant supply of social housing, pushed 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 subsection, aims to relax household budgets. Appendix Figure A2 displays the impact that housing benefit

[8] In the Online Appendix A we discuss further evidence from individual-level Understanding Society survey. Appendix B provides further institutional details. Appendix C describes the data sources.

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 reduces this to 35%. We next describe how housing benefit is computed and discuss the cuts we study in this paper. Further institutional details can be found in Online Appendix B.

2.2 Cuts to the Housing Benefit and Local Housing Allowance and the Identifying Variation

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 the housing benefit is determined by the so-called Local Housing Allowance (LHA), which limits the benefit amount that eligible household can claim. The LHA is calculated and updated by the Valuation Office Agency (VOA) for each *Broad Market Rental Area* (BMRA) and property type[9] based on a sample of rents voluntarily submitted by the landlords and real estate agencies.

We focus on two cuts that were introduced simultaneously from April 2011 and affected the vast majority of housing benefit claimants, the *percentile* and *excess* shocks. Both reforms implied a cut in household disposable income though the reduction of the value of housing benefit. We are primarily interested in the combined effects of the policies. However, we also review the individual policy cuts.

Percentile shock. 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 that point onwards, this reference rent was shifted to be the 30th percentile. This defines three groups in terms of the pre-reform exposure to the cuts: *i)* households with rents above the 50th percentile were fully exposed to

[9] The types are: single room in shared accommodation, a 1, 2, 3 or 4 bedroom flat. There are 193 BRMA's across the UK. BRMA's are defined as areas in which a person could live while having similar access to health, education, recreation, personal banking, shopping facilities and services.

the cuts; *ii)* households with rents between the 30th and 50th percentiles were partially hit by the benefit cut, and *iii)* rents below the 30th percentile prior to the reform were not affected. The left-hand side panel of Figure 1 provides a visual illustration of how the percentile shock affected these three groups. Around 76% of housing benefit claimants, corresponding to the first two groups, experienced a significant cut to their financial support to pay rent due to the reform. We denote the average loss per affected claimant, at a given rental market area d and property class c, which is reported in the official impact estimates, as $L_{d,c}^{\text{percentile}}$.[10]

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 would have been affected the latest by December 2012. The bulk of claimants were treated earlier, though we do not know the exact date. The reform did not change eligibility criteria for the housing assistance.

Of course, individuals might optimize and choose to live in premises paying rents that take the Local Housing Allowance reference rents into consideration. This type of bunching is quite unrealistic as the Local Housing Allowance rate itself is an empirical estimate that was updated monthly (up until the shift to annual uprating from April 2012). This renders it unlikely that households were able to optimize and maximize their excess payments prior to the reform. Importantly, the existence of any form of bunching is not a threat to identification in our difference-in-difference framework, which would only be violated if the the proportion of affected housing benefit claimants vary in a way that is correlated with district

[10]In Appendix Figure A3 we note that the size of the cut is not uniform across the size of the type of properties to which individuals are entitled to receive support. For example, on average, housing benefit claimants entitled to support for a one bedroom flat saw, on average, a cut in their benefit by £9.6 per week, while claimants entitled to support for a one bedroom property saw their benefit cut by £7.5 per week.

unobservables and the timing of the reform. We find no evidence suggesting that this is the case.

Excess shock The second cut that was simultaneously introduced 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, 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 very timely cut of their housing benefit award from April 2011 onwards.[11]

An individual claimant was affected by the cut of the excess payments if his or her rent 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}. Specifically, housing benefit claimants that were living in rental properties that were cheaper than what the LHA rate allowed, prior to April 2011 were allowed to keep the difference as long as that difference was less than £15 per week. These excess payments were cut in a very timely fashion. Similar to the percentile shock, we can classify the house benefit claimants into three groups just prior to the reform. The right-hand side panel of Figure 1 provides a visual illustration defining these three groups. We refer to the average loss per claimant as $L^{\text{excess}}_{d,c}$.

Again, individuals might chose to live in premises to maximize the amount of excess payments prior to the reform. We do not think this is a threat to our identifying assumptions for our difference-in-difference design for the same reasons argued above: the LHA was re-computed every month, making it unlikely that households could optimize.[12] Second, even the existence of bunching would not

[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.

[12]In fact the official impact assessments suggest that the average financial losses arising from

necessarily violate the identifying assumptions for the differences-in-differences framework.

From individual-level exposure to district-level treatment Since districts are the administrative areas responsible for most administration of benefits and for local housing policy, we mostly rely on district-level data. While BRMA's do not map into any existing administrative boundaries, the data used here is valid and accurate at the local authority district level, as the DWP produces official statistics from detailed individual micro data at that level.

For our empirical estimation at the district-level, we construct district-level shocks, which ultimately are just aggregated versions of the average exposures. The data comes from the official impact assessments, described in Subsection 2.3. They provide us with the measures of the average loss per affected claimant $L^{j}_{d,c}$, for $j \in \{\text{percentile \& excess}, \text{percentile}, \text{excess}\}$, along with the number of individuals (likely) affected by each of the reform independently, and, by both reforms combined, $C^{j}_{d,c,\text{baseline}}$, at the time that the impact assessment was conducted in late 2010.[13]

We leverage this information from the ex-ante impact assessments to construct a treatment exposure measure at the district level. This is ultimately just a weighted average of the average individual level financial losses due to the two cuts described in the previous paragraphs. Note that we also observe the financial losses per claimant that arises from the combination of the two measures together.

$$S^{j}_{d} = \sum_{c} L^{j}_{d,c} \times C^{j}_{d,c,\text{baseline}} \tag{1}$$

where c denotes the market area, and c is property class. For the empirical exercises, we normalize the above S^{j}_{d} by the number of resident households at baseline. We also normalize the dependent variables by the (time-varying) number of

the cut in the excess across the UK was around £10, which is considerably smaller compared to the maximum excess of £15 that could have been achieved.

[13] Appendix Figure A4 highlights the variation in numbers of households affected by the housing benefit cut due to their respective different entitlement.

households living in an area.[14,15]

We finally note that the percentile $S_d^{percentile}$ and excess shocks S_d^{excess} are correlated, which is unsurprising given the overlap in housing benefit claimants. The can be seen in Appendix Figure A6. Yet, as argued above, both reforms imply a cut to household disposable income, and for this reason we do not expect different responses to the reforms.

2.3 Official impact estimates

The responsible Department for Works and Pension (DWP) 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. Overall, it was estimated that 774,970 households would lose a part of their housing benefit due to the percentile shock alone; combined with the loss of the excess shock, DWP estimated that 936,960 households would be affected by either cut, implying that nearly 92% of all claimants of housing benefit in the private rented sector were expected to be affected.

Figure 2 provides a visualization of the expected financial losses from the two cuts at the district level drawn from the economic impact assessments.[16] 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 district, sum of $L_{d,c}^{\text{percentile\& excess}}$ across property types c. 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

[14] In Appendix Figure A5 we decomposed the variation in the S_d^j measure into the constituent pieces to give a sense of how much variation there is and what is the main driver of this variation, highlighting that not any one measure of $C_{d,c,\text{baseline}}^j$ or $L_{d,c}^j$ particularly dominates the variation in the overall district-level shock measure.

[15] All results are robust to alternative functional forms, alternative normalizations or estimating specifications in levels or using different forms of weighting. These are available upon request.

[16] Appendix Figures A7, A8, and A9 provide the maps with the two separate elements of the April 2011 housing benefit cuts broken out. Appendix Figure A3 highlights the expected difference in rents between the 30th and 50th percentile for three different property types.

the losses also strongly depend on the claimant's housing situation: across districts the expected financial loss 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. This is the shock measure $S_d^{\text{percentile\& excess}}$. On average, the ex-ante assessments suggest that housing benefit spending would decline by £28 per resident household and year.

2.4 Measuring precarious living conditions and homelessness

We draw on a host of official data sources to shed a comprehensive light on the economic and social impact of the housing benefit cut shock. For further information, see Online Appendix C for details regarding the data sources.

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 only indirectly affected by the housing benefit cut, to the extent that social housing may become relatively more valuable after the reform).

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 worsened 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 of 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 2006 to March 2017. From 2009 onwards, we also have detailed statistics on who and why households become 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. The detailed breakdown of local government spending is available since 2008.

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.[17] 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.

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

[17]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.

rates and inactivity rates. These will highlight that our treatment measure are not confounded by 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.

3 Empirical strategy and data

Throughout the empirical analysis at the district-level we use a difference-in-differences design contrasting places with higher exposure to the cuts relative to places with lower exposure over time.

We assume that the exposure to the cuts does not vary in a way that is correlated with district-level unobservables and with the timing of the reform. In what follows, we argue that this is unlikely to be the case, for the following two reasons. First, the exposure to the cut is not driven by the universe of claimants – since not all claimants were affected by either the percentile or the excess shock. Second, whether the household was affected by the cut ultimately depended on the reference rates, which were computed from empirical estimates at the local level, and that were updated on a monthly basis until April 2012. This made it difficult and unrealistic for benefit claimants to *ex-ante* choose their accommodation taking the housing benefit into consideration. For the same reason the share of households in that category is also unlikely to be correlated with district-level confounders.

Our baseline specification takes the following form:

$$y_{d,t} = \alpha_d + \gamma_t + \sum_{t \neq 2010} \eta_t^j \times \text{Year}_t \times S_d^{\text{j}} + \beta' X_{d,t} + \epsilon_{i,t} \quad (2)$$

We present the main results using the combined exposure of a district to both the percentile- and the excess shock, $j = \{\text{percentile \& excess}\}$, while also presenting the results pertaining to each of the two cuts individually in the appendix. 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 were implemented. 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. We use the data from the earliest year available, which is usually 2006, providing us with four or five data points prior to the introduction of the reform.[18]

We also present the results in tables, where we pool the post-treatment coefficients into a single estimate. We then conduct the following additional exercises. We disaggregate the combined shock into the percentile and excess shock individually. This will allow us to show that the effects are robust to the definition of the shock measure. For the main difference-in-difference 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 Reform Act studied in detail in Fetzer (2019) becomes effective from April 2013. Further, we also present results including and dropping London, which accounts for 13% of the UK population, from the analysis. We finally 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 claimants that are affected by the cuts. This ensures that we focus on the impact of the financial losses $L^j_{d,c}$ resulting from the cuts and are not spuriously attributing trends in the different levels of demand for housing benefit. Throughout the paper, standard errors are clustered at the district level.

[18] With the exception of few outcomes such as roughsleeping where data starts in 2010.

4 Main Results

4.1 Housing benefit spending

As a first step, we document the impact of the cuts on effective spending on housing benefit. Figure 3 suggests that, relative to 2010, there are most noticeable drops in housing benefit spending that are most concentrated in districts more exposed to the two cuts. While the drop in 2011 is already statistically significant, it is small in relative terms. This is a feature of the cuts as most individuals were only affected at the time of their individual claim anniversary. By 2012, the bulk of claimants will have been affected by the cuts, explaining the sizeable drops in housing benefit spending vis-a-vis 2010 in the places that were predicted to be most severely affected by the cuts. The average cut in housing benefit spending, relative to 2010, is around 1 and 3 per cent.

4.2 Evictions

We begin by presenting the results on evictions. Visually, these are presented in Figure 4, using the combined 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. On average, households affected by the reform were expected to lose £596 per year. In our case, the standard deviation of the shock is close to the mean benefit loss, so the coefficients are also economically interpretable in units of average benefit reduction. 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. In the last panel we study evictions in the social rented sector. Social rented sector tenants were not exposed to the reform. There is a modest downward trend in social-rented sector evictions prior to the implementation of the housing benefit cuts affecting private sector tenants. Yet, in contrast to the notable jumps in evictions of private-sector tenants subject to the cuts we do not see similar effects for social sector tenants that were not affected by the cuts.

The point estimates in Table 1 pool the individual post-treatment estimates. The estimates in Panel A indicate that one standard deviation in the exposure to the cuts causes an increase of 0.48 possession claims per one thousand inhabitants, or a 22.4 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 17.7 per cent increase due to the housing benefit cuts. We also find that all private sector evictions increased by 31.5 per cent in the districts affected by a shock of one unit of standard deviation.[19] All estimates are similar when studying the percentile shock or the excess shock in isolation and in the corresponding matched difference-in-difference estimation.[20]

4.3 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.[21] Hence, it is not inconceivable that some households and individuals started to accumulate debt that subsequently needed to be restructured. The results are presented in Table 2. The point estimate in column (1) in Panel A suggests that a 1 standard deviation increase in the exposure to the housing benefit cut causes a 2.8 per cent increase in total new individual bankruptcies cases. Panel B finds similar effect sizes on individual voluntary arrangements – an insolvency procedure that is typically used to restructure consumer loans – indicating a treatment effect of

[19] 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.

[20] See Appendix Figures A11 to A15 and Appendix Tables A1 to A4.

[21] 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.

around 3.7 per cent for a district with a 1 standard deviation higher exposure.[22]

4.4 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 5 we plot out the estimated effects 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-and-breakfast accommodations. Both figures have skyrocketed dramatically from 2011 onwards.

In Table 3 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, 15.5 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. In Panel B finds significant and marketed effects on the changes in temporary accommodation occupancy. What is more, we find in Panel C that the council spending on temporary housing increased sharply by around 81.9 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 D 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 and 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.

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

4.5 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 be "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 an individual sleeping, or bedded down, in open air or in buildings or other places not designed for habitation. For this later outcome, as explained in Section 2.4, we rely on rough sleeping street counts carried by the councils themselves.

In Figure 6 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 of rough sleeping in 2010 in districts more affected by the reform, but notice notable increases from 2012.

Table 4 presents the point estimates for the full post-reform effects. It indicates a sizeable increase in statutory homelessness of 6.4 per cent. We find similar effect sizes across the different specifications. We also observe a notable increase in rough sleeping, increasing by, on average, 41.3 per cent in the post-2011 years.

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 5, we document how the structure of who is becoming statutory homeless has changed. Consistent with the previous patterns, in Panel A, there is a notable jump in statutory homelessness levels increasing by, on average, around 21.7 per cent. 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 6 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 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.

4.6 Robustness and null effects

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

Exposure to the Great Recession. We now show that the results are robust to inclusion of controls that capture the correlation between the shock explore and the Great Recession of 2009. To do so, we constructed the following additional control variables: Jobseeker Allowance Claimant (JSA) rate, Unemployment Rate estimates, and Economic inactivity rate, all measures in April 2010 and in changes between April 2008 and April 2010. We plot the spatial distribution of the shocks in Appendix Figure A10. These measures capture unemployment rates and inactivity rates that may have been impacted by the Great Recession across three different margins. The unemployment rate captures a broad measure of the share of working age adults that are (involuntarily) unemployed. The inactivity rate captures further people that are in the working age population but may have, as a result of the Great Recession, dropped out of the labor force. Lastly, the Job Seeker Allowance claimants capture those that are currently receiving unemployment benefits. Job seeker allowance is only paid for six months after which individuals who may still be unemployed may not appear in the claimant count statistics anymore. These three measures thus capture broadly how the labor market may have been affected by the Great Recession at a local level across a broad range of margins, which may be most immediately relevant to the ability of individuals to cover their cost of housing. Each of these six measures is subsequently interacted with a set of year fixed effects which are further interacted with a dummy indicating London. This allows the impact of the Great Recession, as captured either through broad measure of unemployment at the start of 2010 or through its increase on unemployment measures between 2008 and 2010, to impact outcomes differentially over time and in London. In total, this adds 12 different time trends to the specifications with results being presented in Appendix Table A5.

Overall, we do not see any effect that controlling for an areas' exposure to the Great Recession as measured through its labor market signature has on the estimated coefficients of interest. In addition, Appendix Table A6 presents specifications for the main that include time-varying unemployment, inactivity and job seeker allowance claimant rates. The results are qualitatively very similar.

Alternative shock definitions. As indicated, our estimation approach for the main difference-in-differences normalizes the dependent variables with the time-varying number of households or the population. This 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. Results are also robust to different functional forms and to using different weighting schemes. These are available upon request.

Further, Columns (2) and (3) each of the main tables presents results replacing the combined percentile and excess shocks by their individual measures. In most cases we find remarkably similar results, highlighting that the effect is not driven by any particular aspect of the benefit entitlement reform. Notable exceptions are the stocks in temporary accommodation and statutory homelessness. We note that, in both cases, the point estimates although reduced are still sizeable. Estimates in Column (5) show that most of the effects are not driven by the London housing market specifically. Column (6) presents results obtained from estimating a specification where we interact the baseline claimant counts $C_{d,c,\text{baseline}}$ 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 treatment effects.

Finally, Appendix Figure A11-A14 presents the estimates of η_t^j in Equation (2) replacing the shock broken down by the *excess* and *percentile* shocks separately. Equivalently, Appendix Tables A1 to A4 present Columns (1), (4), (5) and (6) in the main tables broken out by the individual shock measures. Throughout, we find very similar results.

Child Benefit cut and the 2013 Welfare reforms. Fetzer (2019) focuses on welfare reforms that came into force from April 2013. It is not inconceivable that the welfare reforms implemented interact with the Housing Benefit cuts. For this reason we show across our main results that these are robust to dropping data after 2013 in column (4) of the main tables. The estimates show that the effects of the housing benefit reduction are not driven by the welfare reforms. These specifications are likely conservative since they will estimate only the transitional effects up to two years after the housing benefit reform.

Other cuts were implemented from 2011-12 and, as we now show, also do not confound the estimates of the effects of Housing Benefit cut. The cuts were the freeze of child benefit rates for three years and the introduction of a means test that withdrew child benefit from households including a higher earner (threshold at £50,000 and taper to £60,000), which, however was only implemented from January 2013. The second cut were changes in Child Tax Credits paid to lower and middle income households. This was not a single sharp reform but involved many different tweaks to eligibility, minimum working hours along with mild changes to withdrawal rates. Lastly, there was a cap in the annual uprating of most benefits to 1%, which led to a gradual erosion of welfare benefits. In Appendix Table A7 we added a set of control variables with the reforms' differential impact across districts in the UK. The results are throughout very similar with effect sizes being quite comparable across. This is not surprising because other benefit cuts were not sharply place-specific and not a function of developments in Broad Rental Market Areas; or were introduced gradually.

Matched difference-in-difference We replicate each of the main tables and figures focusing on the two shocks separately in the appendix highlighting that results are carried throughout when studying the two simultaneously introduced cuts in isolation. We further provide for each of the two shocks results from 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 (2), 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.

Spillover effects and migration. We next show that spillover effects are unlikely to confound the estimates of the main effects of the housing benefit cut. To do so, we first construct a measure $W_{d,i}$ to capture the extent to which each district d is subject to spillover from every other district $i = \{1, \ldots, N\}$. As we expand below, we compute a broad set of spillover measures. For each, we implement the following differences-in-differences regression controlled by the treatment status of other districts,

$$y_{d,t} = \alpha_d + \gamma_t + \eta^j \times \text{Post}_t \times S^{\text{j}}_d + \beta \times \text{Post}_t \times \left[\sum_{i=1}^{n} W_{d,i} \times S^{\text{j}}_i \right] + \epsilon_{d,t}. \tag{3}$$

where the shock measure is the combined $j = \{\text{percentile \& excess}\}$ for simplicity, and Post_t is equal to one if, and only if, $t \geq 2011$. This is the version of the main specification in Equation (2) with pooled post-treatment effects and the spillover control given by the interaction of $W_{d,i}$ and S^{j}_d. Intuitively, this terms accounts for the fact the shock status of other districts may confound the treatment effects estimates of interest η^j. The potential for confoundedness arises because both the shock measure and the dependent variable may be spatially correlated across districts. This specification then flexibly controls for the shock measure of other districts (see Miguel and Kremer, 2004).

We use the following five alternative specifications of $W_{d,i}$ to account for spillovers. Columns (2) and (3) in Table A8 models spillovers across districts that are part of the same Broad Rental Market Area; Column (4) measurers intra-UK migration from the 2011 Census, the last pre-treatment year when the data is available; and Columns (5) and (6) are related to commuting flows across districts also inferred from the 2011 UK Census based on usual places of work and place of residence. Commuting flows measure the extent to which households in one district offer labour supply in other districts; this essentially reflects a range of implicit costs of commuting that households would bear by moving houses but not jobs to different districts.

We find that, throughout the analysis, most outcomes are unaffected by the spillover controls, with the notable exception of statutory homelessness. This is not all too surprising, due to the increasing use of out-of-borough placements – especially due to London councils. If councils can not identify suitable accommodation for a statutory homeless household, they can refer that family into accommodation outside the area. This mechanically creates spillovers whereby new statutory homeless households are placed outside the area. In early 2019, around 26% of households that are statutory homeless are placed in accommodation outside their district of usual residence – London councils account for 90% of all out of area placements across England and Wales.[23]

We further study both internal and international migration indicators at the local authority level. This could be important if households are relocating to districts with more generous allowances. Appendix Table A9 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

[23]See https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/831246/Statutory_Homelessness_Statistical_Release_Jan_to_March_2019.pdf, accessed 18.06.2020.

systematically higher. This highlights that the stock of the population remains fairly constant.

In Appendix Table A10 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 order 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.

No impact on unemployment or economic activity To allay 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 A11. This is particularly relevant to tackle concerns that the impact measures could be affected by differential exposure to the Great Recession. 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 A12 we study the impact of the reforms on property price; Table A13 presents corresponding estimates for rental prices. The results suggest that districts, on average, saw modest growth in property prices, and rents. A 1 SD higher exposure to the housing benefit cuts causes an average increase in property prices by up to 3 per cent, although the results are not robust to exclusion of London or post-2013 data. The benefit cut is also causes a rent increase of 59 pence higher per week, less than 1 per cent increase. The effects for the period up to 2013 are much weaker, which is inconsistent with the marked jumps we observe in most dependent variables. Similarly, we do not observe effects on rents outside London, while our main effects are broadly carried on the subsample that excludes London. Given this observation we are not concerned about the effects confounding distinct jumps in real estate or property markets.

Temporary increase in property crimes In Appendix Figure A15 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 and 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 A14 we present the corresponding point estimates which suggest a large positive impact on thefts from persons.

5 Central and local government combined fiscal effect

As indicated, the fiscal savings that the cut to housing benefit spending brought about accrue primarily to the central government and the Department of Works and Pension that manages housing benefit. Local government councils, on the other hand, may find themselves with higher costs due to housing households that satisfy the legal definition of being threatened by unintentional homelessness and are deemed a priority need. We can conduct a simple analysis of the extent to which central government savings resulting from the cuts are offset by higher costs to councils to provide shelter (naturally, this completely ignores the associated indirect human costs that are associated with housing insecurity).

Background. Many local government 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. As a result, today, many have limited housing stock left to house vulnerable households. As a consequence of the cuts-induced increase in housing insecurity, many councils had to resort to the private-rented sector to rent accommodation *at market rents* to meet their legal obligations to house households at risk of homelessness. This sets up the possibility that the lower costs due to lower housing-benefit payments for the central government may indirectly just inflate the cost to local governments, ultimately neutralizing the policy objective hidden in the cuts to shelter government budgets from spiralling private sector rents.

Method. To conduct this analysis, we compute the full distribution of treatment effects that are implied by the results. Specifically, we take the estimate in Column (1) of Table A15 and the estimate in Column (1), Panel C from Table 3. The former captures the impact of the cuts on housing benefit outlays born by the central government, while the latter captures the increased costs to local government councils to meet their legal obligations to house statutory homeless households. We compute the full distribution of the treatment effect estimates from the respective table, multiplying the point estimate $\hat{\eta}$ with the district-specific shock S_d^{j}, for $j = \{\text{percentile \& excess}\}$:

$$\Delta\hat{y}_d = \hat{\eta} \times S_d^{\mathrm{j}}$$

The $\Delta\hat{y}_d$ capture the full distribution of the changes in the dependent variable as a function of the district-specific shock.

Results. We visually present the results in Figure 7. The figure plots the $\Delta\hat{y}_d$'s capturing the changes in central government funded housing benefit savings on the horizontal axis and the estimated increases in local government spending to house vulnerable households on the vertical axis. The results suggest that much of the savings due to lower costs in housing benefit were immediately absorbed through higher council spending. This can also be seen in Figure 8.

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. The actual savings to the central government estimated in this paper suggest total savings of £557 million per year. Our estimates imply that council spending for such activities increased between £216-324 million per year. The cost accounting will depends on assumptions about the permanence of the flow of individuals into insecure housing conditions and homelessness, and the intertemporal dimension of the savings and spending cuts.

We obtain that the present value of fiscal savings by the central government are close to £5.3 billion over the following ten-year period.[24] Under the strict assumption that the April 2011 entitlement cut is permanent but only generated a *temporary increase* of one year in homelessness and temporary accommodation, thus affecting only the 2012 claimant cohort, the excess spending on housing offsets between 4.0-6.1% of the original fiscal savings.

We also believe that these are under-estimates given that the empirical evidence in Figure 5 and Table 3 suggests that the April 2011 cut, which was permanent, led to persistent increased flows of households into homelessness and temporary accommodation. This is consistent with the estimate in in Column (1) of Panel C from Table 3 which captures the effect over a longer period of time. Even if individuals do not find themselves permanently in each of this state of being "statutory homeless", but what we see is a permanently increased level of flows into this state as future cohorts also got affected and the entire system became less generous in the long-run. In this case, the fiscal savings and excess costs which were computed on an annual basis should be applied at for each of the subsequent years, resulting in an excess housing cost in the £2.0-3.1 billion range, offsetting between 38.8% and 58.1% of the fiscal savings. This leaves aggregate net-savings of between £246-354 million per year. This implies that, on average, across local authority districts, for every pound saved in lower housing benefit, the costs to councils for homelessness prevention increased by between 38 and 58 pence. Overall, the net fiscal savings were much lower than anticipated due to this indirect impact on councils' budget.

6 Conclusion

In this paper we explore the effects of a sizable 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

[24] Assuming a 3% interest rates on public sector debt at the time, see https://researchbriefings.files.parliament.uk/documents/SN05745/SN05745.pdf.

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 an empirical strategy that allows 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 between 38-58 pence to meet statutory duties to provide housing for households at risk of becoming unintentionally homeless as a result of the cuts. The actual savings to the central government estimated in this paper suggest total savings of £ 557 million per year. Yet, the higher spending of councils imply that, in aggregate, the net-savings amount to just between £ 246- 354 million per year – this is substantially less than the £618 million per year that policy makers were expecting to save through the cuts.

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, homeless and sleeping rough being 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.

References

Alabrese, E., S. O. Becker, T. Fetzer, and D. Novy (2019). Who voted for Brexit? Individual and regional data combined. *European Journal of Political Economy 56*(March), 132–150.

Brewer, M., J. Browne, C. Emmerson, A. Hood, and R. Joyce (2019). The curious incidence of rent subsidies: Evidence of heterogeneity from administrative data. *Journal of Urban Economics 114*(October), 103198.

Burgard, S. A., K. S. Seefeldt, and S. Zelner (2012). Housing instability and health: Findings from the Michigan recession and recovery study. *Social Science & Medicine 75*(12), 2215–2224.

Choon-Geol Moon and J. G. Stotsky (1993). The effect of rent control on housing quality change: a longitudinal analysis. *Journal of Political Economy 101*(6), 1114–1148.

Chyn, E. (2018). Moved to opportunity: The long-run effects of public housing demolition on children. *American Economic Review 108*(10), 3028–3056.

Collinson, R. and D. Reed (2018). The effects of evictions on low-income households. *Unpublished Manuscript*.

Department for Work and Pensions (2014). Monitoring the Impact of Changes to the Local Housing Allowance System of Housing Benefit: final reports. Technical report.

Desmond, M. and C. Gershenson (2017). Who gets evicted? Assessing individual, neighborhood, and network factors. *Social Science Research 62*, 362–377.

Desmond, M. and R. T. Kimbro (2015). Eviction's fallout: Housing, hardship, and health. *Social Forces 94*(1), 295–324.

Desmond, M., A. V. Papachristos, and D. S. Kirk (2016). Police Violence and Citizen Crime Reporting in the Black Community. *American Sociological Review 81*(5), 857–876.

Desmond, M. and T. Shollenberger (2015). Forced Displacement From Rental Housing: Prevalence and Neighborhood Consequences. *Demography 52*(5), 1751–1772.

Diamond, R., T. McQuade, and F. Qian (2019). The effects of rent control expansion on tenants, landlords, and inequality: Evidence from San Francisco. *American Economic Review 109*(9), 3365–3394.

Eriksen, M. D. and A. Ross (2015). Housing vouchers and the price of rental housing. *American Economic Journal: Economic Policy 7*(3), 154–176.

Evans, W. N., J. X. Sullivan, and M. Wallskog (2016). The impact of homelessness prevention programs on homelessness. *Science 353*(6300), 694–699.

Fetzer, T. (2019). Did Austerity Cause Brexit? *American Economic Review 109*(11), 3849–3886.

Fowler, K. A., R. M. Gladden, K. J. Vagi, J. Barnes, and L. Frazier (2015). Increase in suicides associated with home eviction and foreclosure during the US housing crisis: Findings from 16 national violent death reporting system states, 2005-2010. *American Journal of Public Health 105*(2), 311–316.

Galiani, S., A. Murphy, and J. Pantano (2015). Estimating neighborhood choice models: Lessons from a housing assistance experiment. *American Economic Review 105*(11), 3385–3415.

Honig, B. M. and R. K. Filer (1993). Causes of Intercity Variation in Homelessness. *American Economic Review 83*(1), 248–255.

Humphries, J. E., N. S. Mader, D. I. Tannenbaum, and W. L. van Dijk (2019). Does Eviction Cause Poverty? Quasi-Experimental Evidence from Cook County, IL. *NBER Working Paper*.

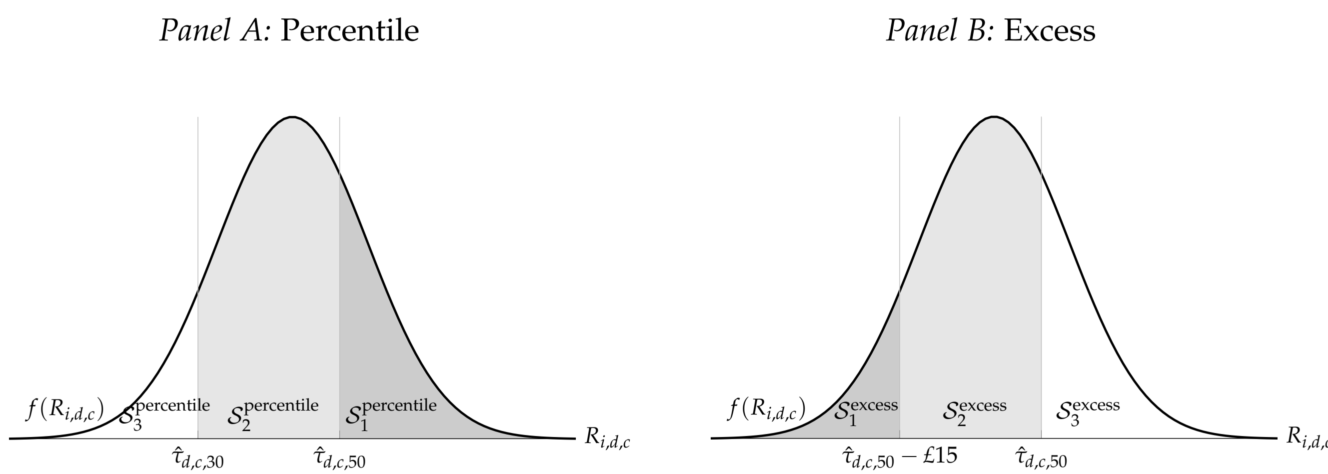
Jacob, B. A. and J. Ludwig (2012). The effects of housing assistance on labor supply: Evidence from a voucher lottery. *American Economic Review 102*(1), 272–304.

Miguel, E. and M. Kremer (2004). Worms: identifying impacts on education and health in the presence of treatment externalities. *Econometrica 72*(1), 159–217.

Phinney, R., S. Danziger, H. A. Pollack, and K. Seefeldt (2007). Housing instability among current and former welfare recipients. *American Journal of Public Health 97*(5), 832–837.

Van Dijk, W. (2019). The socio-economic consequences of housing assistance. *mimeo*, 0–46 i–xi.

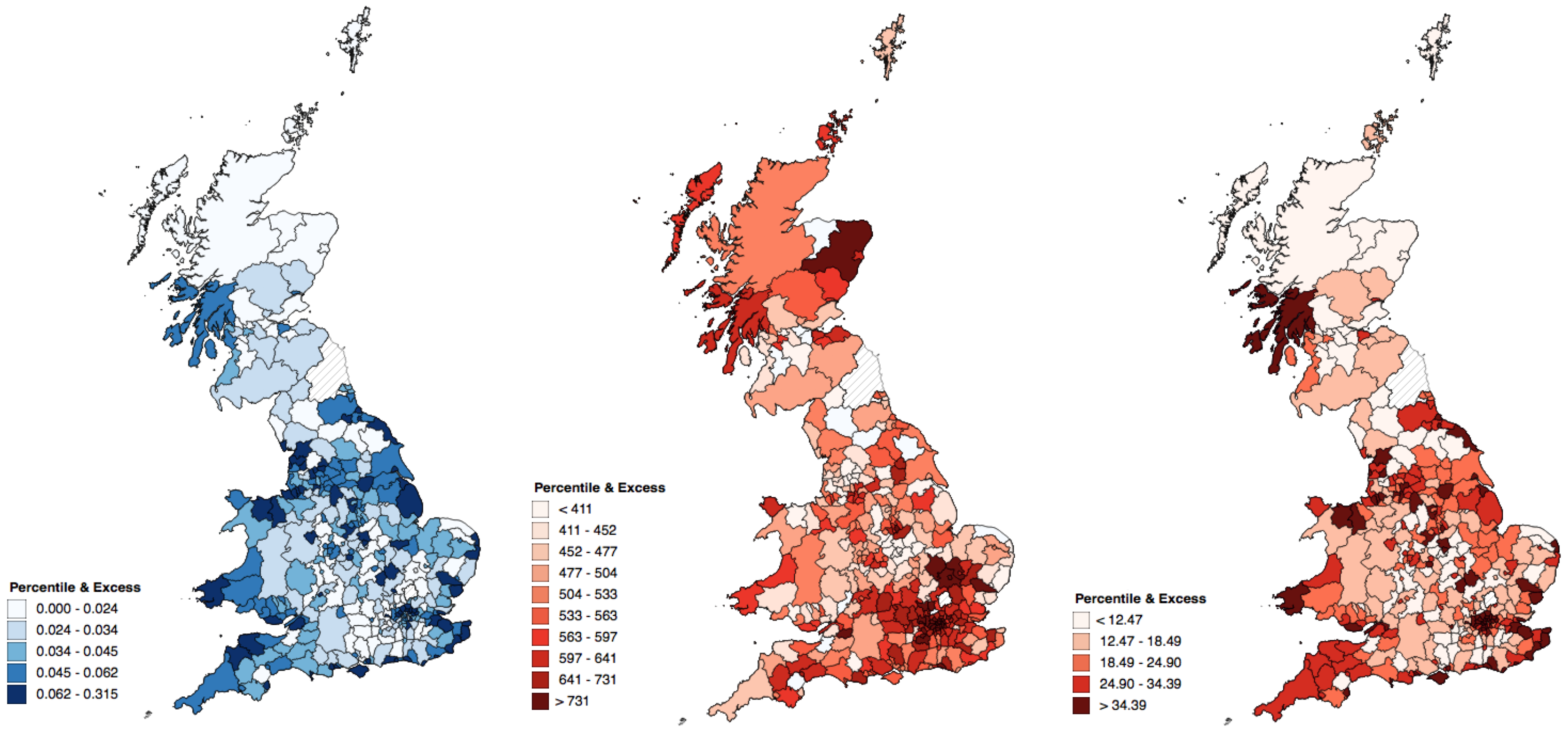
Figure 1: Illustration of Percentile and Excess Shocks



Notes: Illustration of percentile and excess shocks. Darker areas represent individuals that were hardest hit by the corresponding reform. $f(R_{i,d,c})$ represents the distribution of rents paid by housing benefit claimants. The percentiles $\hat{\tau}_{d,c,30}$ and $\hat{\tau}_{d,c,50}$ are computed across the distribution of all rents in the private sector.

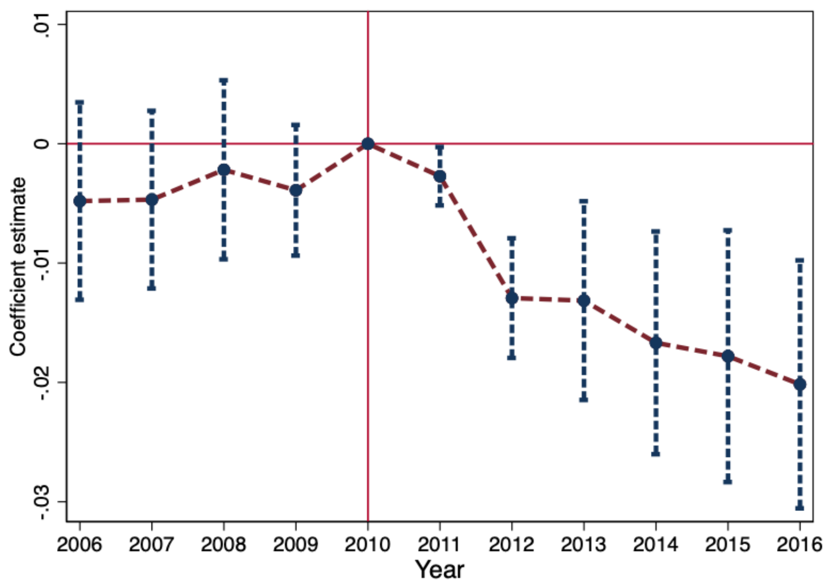
Figure 2: Ex-ante estimated impact of cuts to housing benefit

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



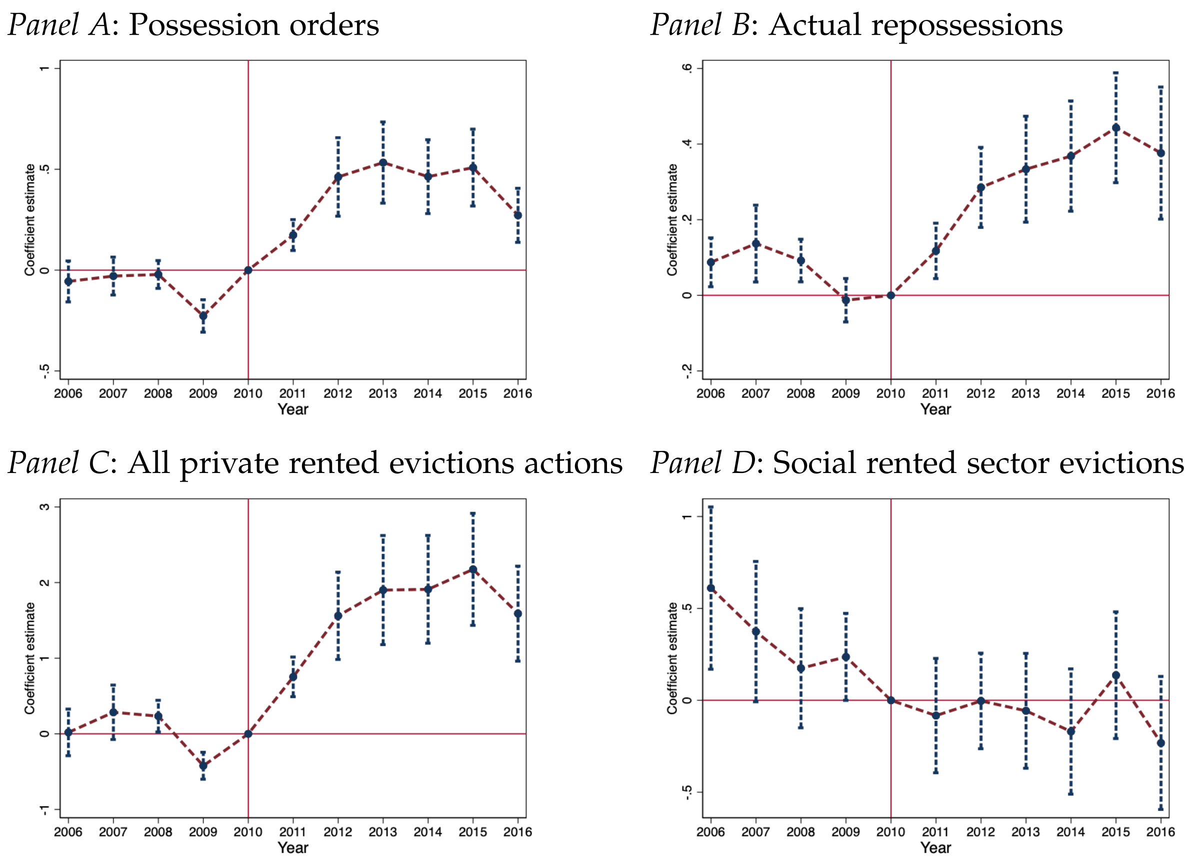
Notes: Map plots out the exposure to the cuts to local housing allowance across districts using data from the Department for Works and Pension's Official Economic Impact Assessment. Panel A presents data on the number of households affected expressed as a share of all resident households, Panel B presents the distribution of the average loss per affected household (equivalent to the measure of $L^{j}_{c,d}$ aggregated over property types d). Panel C measures the loss per household in district d and corresponds to the shock measure S^{j}_{d}. In all cases, the figure plots the combined effects of the cuts to housing benefit, i.e. $j = \{percentile\&excess\}$.

Figure 3: Impact of cuts to housing benefit on housing benefit spending



Notes: Figure plots regression coefficients obtained from estimating specification in Equation (2). The dependent variable is the log value of housing benefit spending. 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.

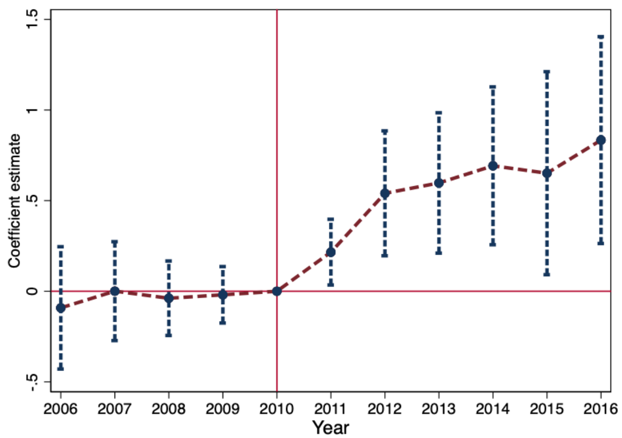
Figure 4: Impact of cut to housing benefit on forced evictions of people living in rental accommodation



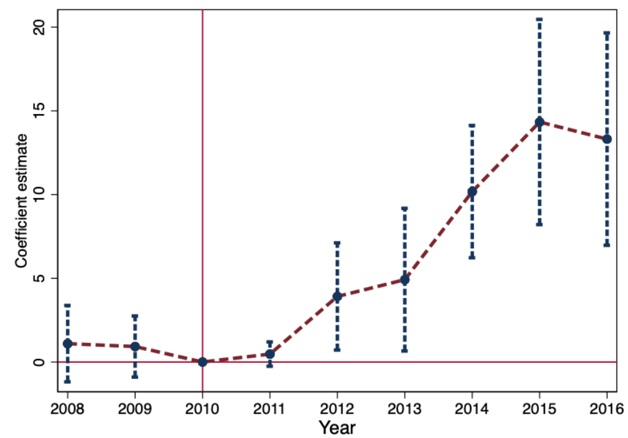
Notes: All dependent variables are measured as rates relative to the number of resident households in a district. Figure plots regression coefficients obtained from estimating specification in Equation (2). The dependent variable in Panel A measures all Landlord possession claims raised. Panel B studies 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. 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 rate of residence and spending in temporary accommodation

Panel A: Households in temporary accommodation



Panel B: Spending on homeless hostels & BnB's

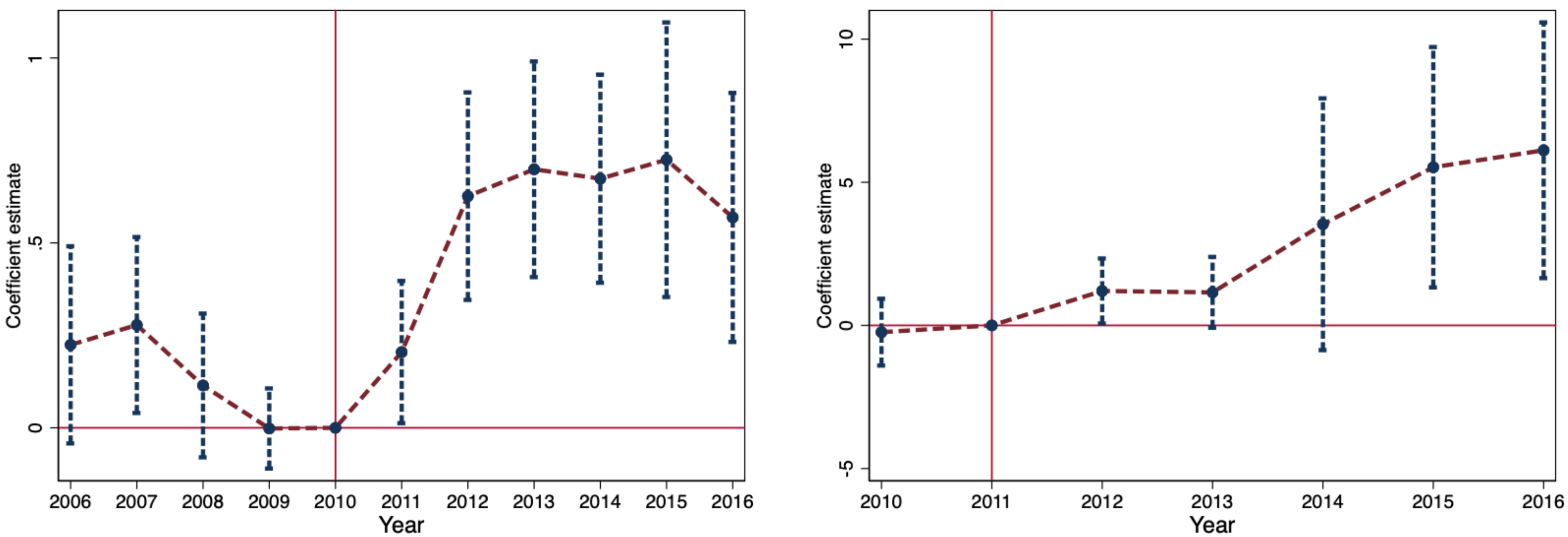


Notes: Figure plots regression coefficients obtained from estimating specification in Equation (2). All dependent variables are measured as rates relative to the number of resident households in a district. The dependent variable in Panel A measures the number of residents in temporary accommodation. 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 6: Impact of cut to housing benefit on measures of statutory homelessness

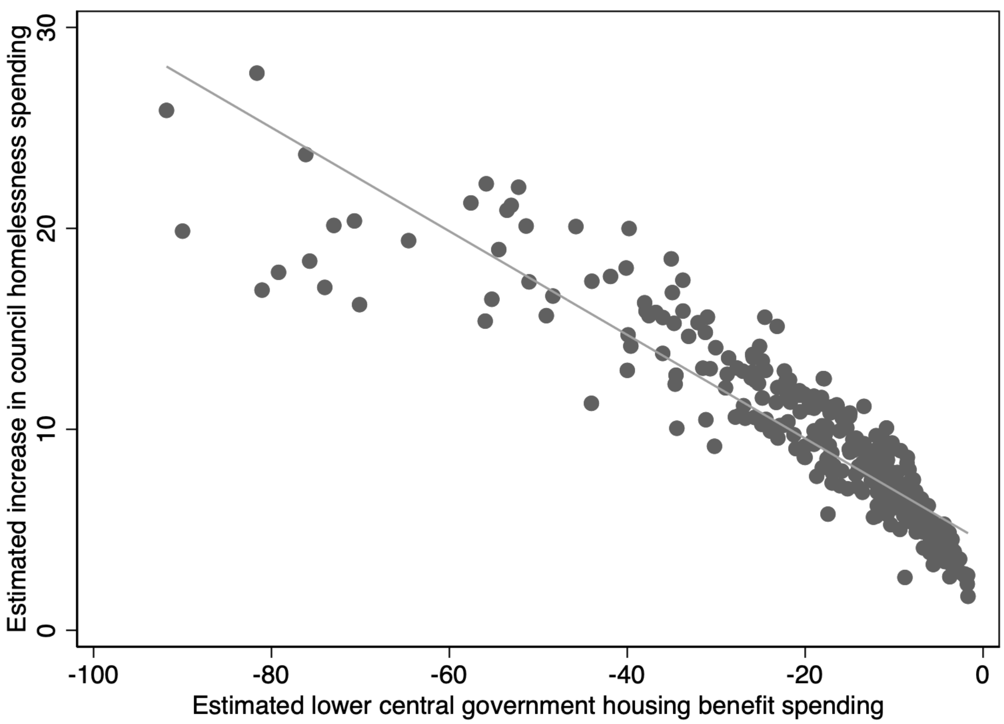
Panel A: Statutory homeless

Panel B: Roughsleeping



Notes: Figure plots regression coefficients obtained from estimating specification in Equation (2). 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.

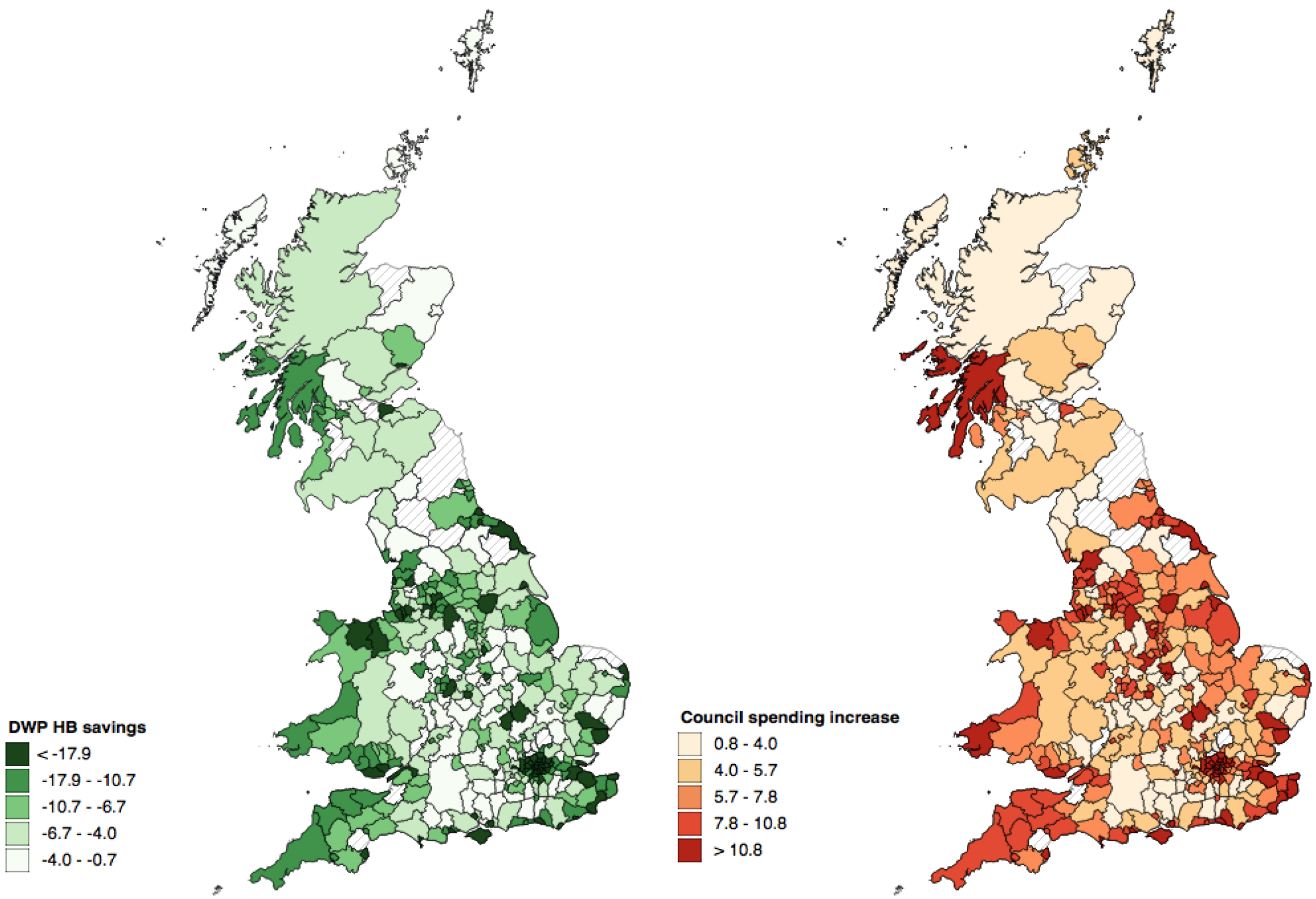
Figure 7: Fiscal saving: Implied fiscal savings to central government due to lower housing benefit costs versus higher council spending for homelessness



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 8: Estimated impact of cuts to housing benefit: DWP housing benefit savings and homelessness cost prevention increases

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 eviction measures

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Possession claims due to rent arrears						
post × $S^{\text{percentile \& excess}}$	0.478***			0.464***	0.425***	0.524***
	(0.092)			(0.089)	(0.109)	(0.101)
post × $S^{\text{percentile}}$		0.500***				
		(0.079)				
post × S^{excess}			0.363***			
			(0.099)			
Mean of DV	2.13	2.13	2.13	2.02	2	2.13
Local authority districts	365	366	365	365	353	364
Observations	4014	4025	4014	2919	3882	4003
Panel B: Repossessions						
post × $S^{\text{percentile \& excess}}$	0.259***			0.185***	0.264***	0.288***
	(0.060)			(0.043)	(0.076)	(0.062)
post × $S^{\text{percentile}}$		0.264***				
		(0.055)				
post × S^{excess}			0.207***			
			(0.064)			
Mean of DV	1.46	1.46	1.46	1.35	1.37	1.46
Local authority districts	365	366	365	365	353	364
Observations	4014	4025	4014	2919	3882	4003
Panel C: All private rented-sector eviction actions						
post × $S^{\text{percentile \& excess}}$	1.662***			1.412***	1.474***	1.804***
	(0.313)			(0.262)	(0.372)	(0.339)
post × $S^{\text{percentile}}$		1.725***				
		(0.271)				
post × S^{excess}			1.282***			
			(0.344)			
Mean of DV	5.28	5.28	5.28	4.86	4.93	5.29
Local authority districts	365	366	365	365	353	364
Observations	4014	4025	4014	2919	3882	4003
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^{j}_{d,c,2010}$ trends						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). Columns (1), (2) and (3) study the effect of the reform on the combined, percentile and excess shocks separately. Column (4) drops data post-2013 when welfare reforms were implemented. Estimates in Column (5) exclude London. Column (6) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. 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 bankruptcies

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total individual bankruptcies						
post × $S^{\text{percentile \& excess}}$	0.166***			0.131**	0.213***	0.131**
	(0.049)			(0.066)	(0.062)	(0.057)
post × $S^{\text{percentile}}$		0.166***				
		(0.048)				
post × S^{excess}			0.135**			
			(0.060)			
Mean of DV	6	6	6	6.39	6.09	6
Local authority districts	337	338	337	337	325	336
Observations	3706	3717	3706	2695	3574	3695
Panel B: Individual voluntary arrangements						
post × $S^{\text{percentile \& excess}}$	0.385***			0.125	0.559***	0.283**
	(0.107)			(0.102)	(0.133)	(0.124)
post × $S^{\text{percentile}}$		0.356***				
		(0.116)				
post × S^{excess}			0.359***			
			(0.113)			
Mean of DV	10.5	10.5	10.5	10.6	10.7	10.5
Local authority districts	337	338	337	337	325	336
Observations	3707	3718	3707	2696	3575	3696
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^{j}_{d,c,2010}$ trends						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 but require the permission of the landlord, which typically prefer to use court action. Columns (1), (2) and (3) study the effect of the reform on the combined, percentile and excess shocks separately. Column (4) drops data post-2013 when welfare reforms were implemented. Estimates in Column (5) exclude London. Column (6) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. 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 council spending on temporary housing and accommodation

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Temporary accommodation						
post × $S^{\text{percentile \& excess}}$	0.494**			0.382*	0.291	0.627**
	(0.237)			(0.219)	(0.229)	(0.265)
post × $S^{\text{percentile}}$		0.584**				
		(0.259)				
post × S^{excess}			0.249			
			(0.220)			
Mean of DV	3.18	3.17	3.18	3.05	2.75	3.19
Local authority districts	362	363	362	362	350	361
Observations	3854	3865	3854	2858	3724	3843
Panel B: Changes in temporary accommodation occupancy						
post × $S^{\text{percentile \& excess}}$	0.531***			0.516***	0.404***	0.586***
	(0.097)			(0.091)	(0.101)	(0.106)
post × $S^{\text{percentile}}$		0.530***				
		(0.086)				
post × S^{excess}			0.438***			
			(0.094)			
Mean of DV	-.0875	-.0871	-.0875	-.196	-.0808	-.0876
Local authority districts	365	366	365	365	353	364
Observations	3842	3853	3842	2863	3714	3831
Panel C: Council spending on hostels and BnB's						
post × $S^{\text{percentile \& excess}}$	8.066***			3.201**	5.117**	8.221***
	(2.124)			(1.409)	(2.056)	(2.094)
post × $S^{\text{percentile}}$		9.165***				
		(1.931)				
post × S^{excess}			5.035**			
			(2.024)			
Mean of DV	9.85	9.83	9.85	7.6	7.99	9.88
Local authority districts	365	366	365	365	353	364
Observations	3234	3243	3234	2189	3126	3225
Panel D: Total council spending on temporary housing						
post × $S^{\text{percentile \& excess}}$	15.988***			6.985***	9.471***	16.931***
	(3.361)			(2.332)	(2.927)	(3.611)
post × $S^{\text{percentile}}$		18.264***				
		(3.329)				
post × S^{excess}			9.816***			
			(2.443)			
Mean of DV	18.2	18.2	18.2	14.6	13.4	18.3
Local authority districts	365	366	365	365	353	364
Observations	3284	3293	3284	2189	3176	3275
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^{j}_{d,c,2010}$ trends						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 measure the change in temporary accommodation occupancy. Panel C focuses on council spending on overnight bed and breakfast and hostel accommodation; Panel D focuses on total council spending for temporary accommodation. Columns (1), (2) and (3) study the effect of the reform on the combined, percentile and excess shocks separately. Column (4) drops data post-2013 when welfare reforms were implemented. Estimates in Column (5) exclude London. Column (6) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 4: Impact of housing benefit cut on homelessness and rough sleeping

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Statutory homelessness						
post $\times$ $S^{\text{percentile \& excess}}$	0.287**			0.252**	0.238*	0.461***
	(0.117)			(0.110)	(0.140)	(0.142)
post $\times$ $S^{\text{percentile}}$		0.321***				
		(0.113)				
post $\times$ S^{excess}			0.188			
			(0.124)			
Mean of DV	4.5	4.5	4.5	4.68	4.46	4.51
Local authority districts	365	366	365	365	353	364
Observations	3957	3968	3957	2874	3830	3946
Panel B: Rough sleepers						
post $\times$ $S^{\text{percentile \& excess}}$	3.532***			1.061**	2.866***	3.687***
	(1.298)			(0.415)	(0.785)	(1.398)
post $\times$ $S^{\text{percentile}}$		3.560**				
		(1.530)				
post $\times$ S^{excess}			2.919***			
			(0.753)			
Mean of DV	8.56	8.56	8.56	6.79	7.77	8.57
Local authority districts	315	316	315	315	303	314
Observations	2205	2212	2205	1260	2121	2198
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^{j}_{d,c,2010}$ trends						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. Columns (1), (2) and (3) study the effect of the reform on the combined, percentile and excess shocks separately. Column (4) drops data post-2013 when welfare reforms were implemented. Estimates in Column (5) exclude London. Column (6) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table 5: Who becomes homeless?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
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 × $S^{\text{percentile \& excess}}$	33.443***	8.803***	12.165***	-0.236	-3.048	23.341***	8.834***	1.418***	30.054***	3.707***	-0.005	0.051
	(8.056)	(2.262)	(4.021)	(1.972)	(2.181)	(5.058)	(1.634)	(0.285)	(6.777)	(1.277)	(0.024)	(0.332)
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
Panel B: Percentile												
Post 2011 × $S^{\text{percentile}}$	33.728***	8.535***	11.535***	0.315	-1.721	23.526***	9.124***	1.552***	30.063***	3.826***	-0.020	0.209
	(7.665)	(2.350)	(4.110)	(2.140)	(1.928)	(4.711)	(1.407)	(0.281)	(6.361)	(1.390)	(0.027)	(0.217)
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}	27.657***	7.825***	11.159***	-1.064	-4.613	19.340***	6.998***	0.989***	25.270***	2.930***	0.018	-0.196
	(8.560)	(2.371)	(4.212)	(1.870)	(2.889)	(5.256)	(1.793)	(0.251)	(7.138)	(1.080)	(0.028)	(0.610)
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 6: 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 × $S^{\text{percentile \& excess}}$	33.443***	0.822	1.159	0.554*	0.001	0.047	27.924***	2.767**	-0.189	0.755	0.049	1.858
	(8.056)	(1.304)	(1.504)	(0.318)	(0.061)	(0.059)	(6.025)	(1.314)	(0.277)	(0.611)	(0.311)	(1.504)
Mean of DV	154	23.4	16.6	1.65	.137	.154	33.6	6.62	4.47	15.5	1.68	7.42
Panel B: Percentile												
Post 2011 × $S^{\text{percentile}}$	33.728***	1.380	1.127	0.660**	0.000	0.036	26.169***	3.109***	-0.121	0.831	0.069	2.453
	(7.665)	(1.241)	(1.470)	(0.260)	(0.058)	(0.048)	(6.267)	(1.136)	(0.251)	(0.536)	(0.343)	(1.766)
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}	27.657***	-0.175	1.018	0.306	0.003	0.056	26.157***	1.814	-0.289	0.508	0.014	0.665
	(8.560)	(1.385)	(1.425)	(0.412)	(0.064)	(0.076)	(6.457)	(1.493)	(0.319)	(0.899)	(0.255)	(1.022)
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 capturing the official classifications of reasons why individuals became homeless. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Online Appendix
"Housing insecurity and homelessness: Evidence from the UK"

For Online Publication

Thiemo Fetzer, Srinjoy Sen and Pedro CL Souza

May 27, 2022

A Individual-level panel evidence

We leverage data from the UK's largest household panel study – the Understanding Society Study (henceforth, USOC) to provide some additional evidence at the individual level.[1] As we will highlight, this data has some severe limitations due to selective and endogenous attrition. To study this, we augment the individual-level panel data to capturing the presence or absence of a respondent in each survey wave. We now show that attrition is endogenous with respect to the housing benefit cut. More specifically, households interviewed in the first wave of the survey and likely exposed to the benefit cuts were 10 per cent more likely to attrit in future waves, relative to the control group of individuals who also lived in rented accommodation but were not claiming housing benefit. In Appendix Subsection A.2, we confirm that the endogenous attrition is particularly pronounced among housing benefit claimants that report increase in rent arrears after the reform is likely to have impacted them. Finally, in Appendix Subsection A.3 we show that, among the sample that does not attrit, households more exposed to the cut are more likely to report being in rent arrears and having been evicted in subsequent waves. Taken together, these results suggest that households that shift into insecure living arrangements may be particularly prone to drop out from such panel-survey

[1] This data 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).

studies. While nevertheless providing some substantive results, it highlights that shocks that increase housing insecurity may be particularly difficult to study with otherwise high quality panel survey data.

A.1 (Endogenous) Attrition

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.

$$A_{i,w,t} = \alpha_i + \beta_{d,t} + \gamma \times Post_{i,t} \times T_i + \epsilon_{i,d,t} \tag{4}$$

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 A16. 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.

A.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:

$$R_{i,t} = \beta_{d,t} + \xi \times Post_{i,t} \times T_i + \nu \times T_i + \epsilon_{i,d,t} \tag{5}$$

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 A17. 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.

A.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.[2] The empirical specification is, in its most demanding form, very similar to model (5):

$$y_{i,w,t} = \alpha_i + \beta_{d,t} + \gamma \times Post_{i,t} \times \Delta B_i + \epsilon_{i,d,t} \tag{6}$$

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 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 Appendix Table A18. In Panel A, we observe that the housing-benefit cut induced drop in rent affordability causes 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

[2]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.

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.

In addition, we can implement an event-study version of Equation 6. The results can be seen in Appendix Figure A16. While the estimation is noisy, we see a similar pattern with rent arrears building up among individuals that received housing benefit both before and after April 2011 and experienced a decline in their housing benefit – possibly as a result of the cuts. The analysis suggests that those individuals saw a build up of rent arrears in 2011 and 2012 and experienced an increased change of being evicted. It is important to flag up that the coding of an eviction in this data is far from perfect as it is only available for individual households who *moved house* and for which the USOC study was able to maintain contact – which, as we document in the attrition analysis, is only a small subsample. The timing of the rent arrears buildup and the evictions map however, closely with the timing that we would expect to see. Albeit, as becomes clear, the effects are much more noisily estimated.

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.

B Institutional details about the housing benefits

Local Housing Allowance. In April 2008, the Local Housing Allowance (LHA) was introduced nationally for calculating Housing Benefit for private rented sector tenants (and not those in council or social housing). It replaced the previous Housing Benefit (HB) scheme. LHA introduced a method of calculating Housing Benefit based on the composition of the household and the median rent in a local Broad Market Rental Area (BMRA). The LHA is the maximum entitlement one can claim for different sizes of properties within a BMRA. The Rent Service (now part of the Valuation Office Agency) is responsible for determining BMRAs in England. By its own definition, "a BMRA comprises two or more distinct but adjoining areas of residential accommodation, within which a person could reasonably be expected to live thereby having access to facilities and services for the purposes of health, education, recreation, personal banking and shopping. When determining BMRAs the Rent Officer takes account of the distance of travel, by public and private transport, to and from those facilities and services."[3] Prior to April 2011, within a BMRA the LHA for different sizes of properties was calculated with reference to the median rent for properties of the same size in a BMRA.

The data from which LHA rates are calculated do not include all rents that might exist in each BRMA. In accordance with DWP legislation, Rent Officers are tasked with collecting a sufficient sample from the 12 month period ending 30 September prior to DWP publishing LHA rates on 1 April. The Rent Officers collect these rental information from letting agents, landlords and tenants. Rent Officers determine 5 different LHA rates for the 5 categories of property: shared accommodation (room in a shared property), 1 bedroom, 2 bedrooms, 3 bedrooms and 4 bedrooms within each Broad Rental Market Area (BRMA). The Local Authorities (LA) are responsible for revealing information about the BRMAs which fall wholly or partly in the LA area and the LHA rates for the different categories of dwelling that apply within them.

It is the LA's duty to make LHA payments to the customers/tenants account in order to encourage personal responsibility and financial inclusion rather than to the landlord when a claim is assessed according to Local Housing Allowance (LHA) rules. According to the LHA guidance manual, "payment is usually on the 14th day after the claim is made in LHA cases. Payment to the landlord is made only if the tenant has built up rent arrears of eight weeks or more or is having deductions from their income-related benefits such as Income Support (IS), income-based Jobseeker's Allowance (JSA(IB)), income-related Employment and Support Allowance (ESA(IR)) or Pension Credit to pay off rent arrears."[4]

[3] See https://commonslibrary.parliament.uk/research-briefings/sn04957/.

[4] See https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/324708/lha-guidance-manual.pdf.

Eligibility. Housing Benefit is usually given to people who generally need it to pay their rent such as those who are unemployed, on a lower income or claiming benefits. People who have permanent residency in the UK can make a new claim for LHA to their LA if they have reached State Pension Age or they're in supported, sheltered or temporary housing. As mentioned earlier, the LHA is the maximum amount that people renting from a private landlord can claim in Housing Benefit. This maximum rent is based on where they live, the number of bedrooms one needs (determined by the number of people who live with the claimant as members of his/her family), the rent the claimants need to pay and their household income including benefits, pension and savings. There is also a cap on the amount of benefits that a working-age claimant and their household can receive. A claimant's housing benefit will be reduced to ensure that the total amount of benefits they receive is not more than the benefits cap level. By default, LHA awards are simply updated to reflect the current applicable LHA rate on each annual anniversary of the claim start date. However, any change in the maximum eligible rent (caused by changes to the applicable LHA rate as compared to the previous month or changes to the number of bedrooms to which a claimant is entitled to or a change in address) triggers a reassessment and resets the claimant's claim anniversary month to the month in which that change took place. As LHA is intended for people who are unemployed or on low incomes, once a claimant starts working or changes to a higher paying job, the LHA benefit usually goes down since their overall income increases. The specific rules are complex and depend on other benefit income[5], but it has been approximated as a loss of 55 pence of benefits for each pound of employment income, raising to 65 pence if receiving housing benefits.[6]

Local Housing Allowance Reforms. As part of the June 2010 Budget the Government announced an intention to reduce expenditure on Housing Benefit. This has involved changing the basis on which LHA rates are calculated. Since April

[5]See https://www.gov.uk/housing-benefit/what-youll-get

[6]See https://england.shelter.org.uk/housing_advice/benefits/local_housing_allowance_lha_for_private_renters

2011 LHA rates within BMRAs have been based on the 30th percentile of local rents (rather than the median). Up to April 2012, LHA rates within BMRAs had been subject to monthly review by Rent Officers. But then, LHA rates were frozen between 2012 and 2013. However, from April 2013, LHA rates were up rated annually instead of monthly. In April 2013 LHA rates were uprated by the Consumer Price Index (CPI) and was uprated by a maximum of 1% in 2014 and 2015 (there was an exemption from the 1% cap for areas with the highest rent increases). However, there has been a four-year freeze on rates from April 2016. As a consequence, LHA rates have failed to keep pace with rents at the bottom 30th percentile of local markets. Families have seen the shortfall between their eligible LHA rate and local rent levels to grow year-on-year.

In the absence of any changes in circumstances triggering a new claim reassessment, claimants had their benefits cut (which they were allowed to retain before the reforms) 12 months after April 2011 at the point of their first annual reassessment after April 2011 (i.e. at some point between April 2011 and March 2012). After their first annual reassessment post April 2011, the claimants were transitionally protected in cash terms from the other LHA reductions (such as setting of LHA rates at the 30th percentile of local rents, capping of LHA rates for different property sizes) for 9 months, before being rolled fully onto the new system (i.e. at some point between January 2012 and December 2012). More specifically, "claimants belonging to the April cohort (those with a claim anniversary in April) lost the 15 GBP excess in April 2011 and were rolled fully onto the new system of LHA reductions 9 months later in January 2012. The March cohort on the other hand, would not lose any excess until March 2012 and would not be rolled fully onto the reformed system until December 2012. Now, suppose if any change in circumstances triggered a reassessment between April 2011 and the next annual claim anniversary, or during the period of 9 months of transitional protection, the claimant was fully rolled onto the new system at that point."[7]

[7] See https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/445618/rr871-lha-econometric-analysis-of-the-impacts-of-reforms-on-existing-cl pdf

C Data and Measures of precarious living conditions and homelessness

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.[8] The data allow us to distinguish between evictions and repossessions at the various stages of the underlying legal proceedings with the responsible County Court. We focus on repossessions of properties by landlords, which are broken down by the type of landlord (social landlord, private landlord) and the procedure used to repossess the property (claims, orders, warrants or repossession by a county court bailiff). Orders, in turn, are broken down by whether they were outright orders or suspended orders. If the number of claims that were issued in a local authority during a particular quarter is lower than 5, it is suppressed to protect the confidentiality of those involved in the claim, order, warrant or repossession. The data thus allows us to 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 only indirectly affected by the housing benefit cut, to the extent that social housing may become relatively more valuable after the reform).

Individual insolvencies. We further leverage annual data from the UK's Insolvency Service.[9] 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

[8] Data can be obtained from https://www.gov.uk/government/collections/mortgage-and-landlord-possession-statistics for England and Wales and https://www.gov.scot/publications/housing-statistics-management-of-local-authority-housing/ for Scotland.

[9] Data available from https://www.gov.uk/government/collections/insolvency-service-official-statistics.

worsened 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. MHCLG provides annual statutory homelessness statistics which consists of the total households which the local authorities deem to be homeless.[10] All these statistics are based on decisions made in each financial year (from April to March) and the data runs from April 2006 to March 2017. From 2009 onwards, we also have the breakdown by age group, households/single parents with dependent children, victims of substance abuse and violence, and the reason for why households become homeless (one of which is rent arrears and evictions among others).

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 obtained from the Ministry of Housing, Communities and Local Government(MHCLG).[11] Rough sleeping is defined as "people sleeping, about to bed down or actually bedded down in the open air or in buildings and other places not designed for habitation. The definition does not include people in hostels or shelters, people in campsites or other sites used for recreational purposes or organised protest, squatters or travellers."[12] 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

[10] Data sources are available at https://www.gov.uk/government/collections/homelessness-statistics of England, https://www.gov.scot/collections/homelessness-statistics/ for Scotland and https://statswales.gov.wales/Catalogue/Housing/Homelessness for Wales.

[11] Available at https://www.gov.uk/government/collections/homelessness-statistics#rough-sleeping

[12] See https://www.gov.uk/guidance/homelessness-data-notes-and-definitions.

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. Given that rough sleepers often move between local authority areas (particularly in urban areas) it is suggested that neighbouring authorities count on the same night whenever possible. This eliminates double counting and ensures that more mobile rough sleepers are not missed. If a local authority chooses not to conduct a formal rough sleeper count, it should provide an annual estimate of rough sleeping numbers to MHCLG each year, after consultation with local agencies (e.g. outreach workers, police, faith groups, etc) to help inform the national picture on rough sleeping.

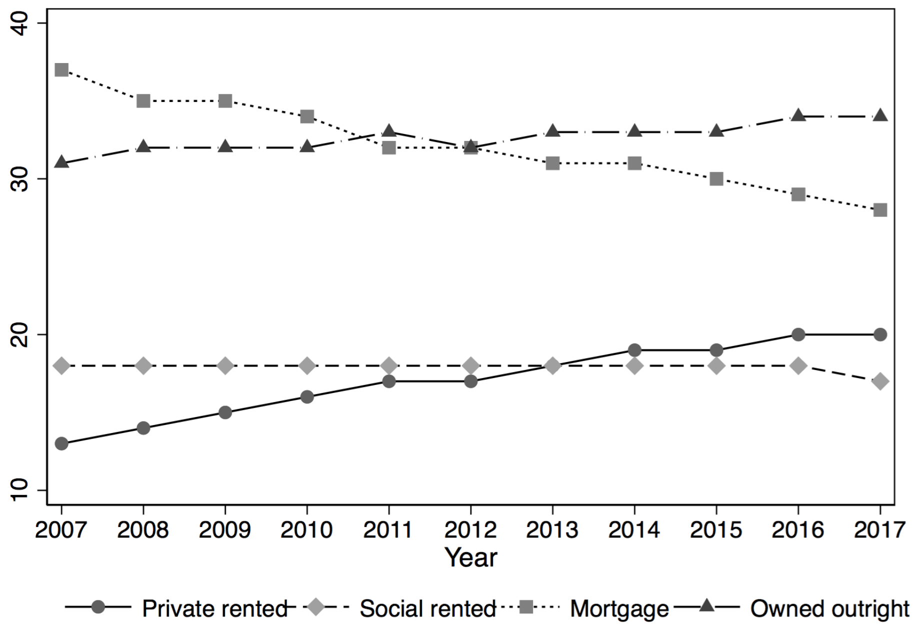
Local government expenditure data To study financial outcomes at the district level, we further obtained data pertaining to Local Government Finances from the Local Government Finance - Data Collection Analysis and Accountancy division of Department for Communities and Local Government.[13] The data covers the expenditure in British pounds incurred by the local authorities to provide housing services which range from private sector housing renewal, housing welfare services, housing benefits and costs to mitigate homelessness. We focus on the cost of homelessness prevention, homeless support, administration and the associated cost of housing homeless households either in private-rented sector, hostels, bed and breakfasts and other temporary housing. 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,[14] 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

[13] Available at `https://www.gov.uk/government/collections/local-authority-revenue-expenditure-and-financing`.

[14] Available at `https://stat-xplore.dwp.gov.uk/webapi/jsf/login.xhtml`.

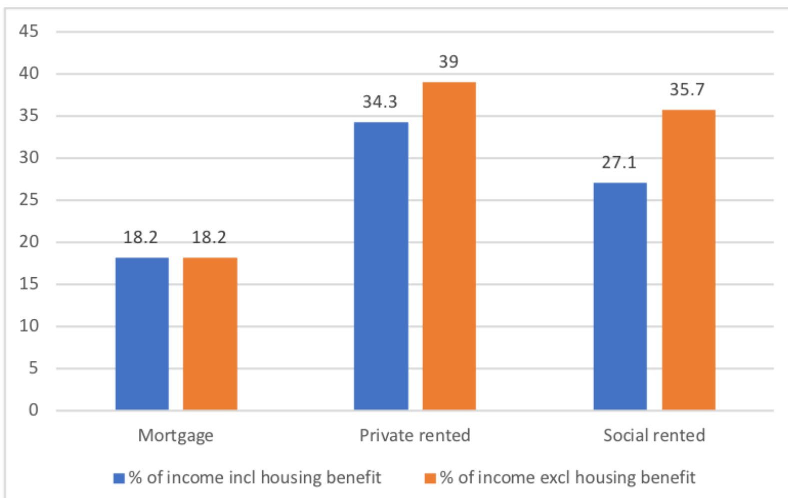
between the central and local government actors. The detailed breakdown of local government spending is available since 2008.

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).

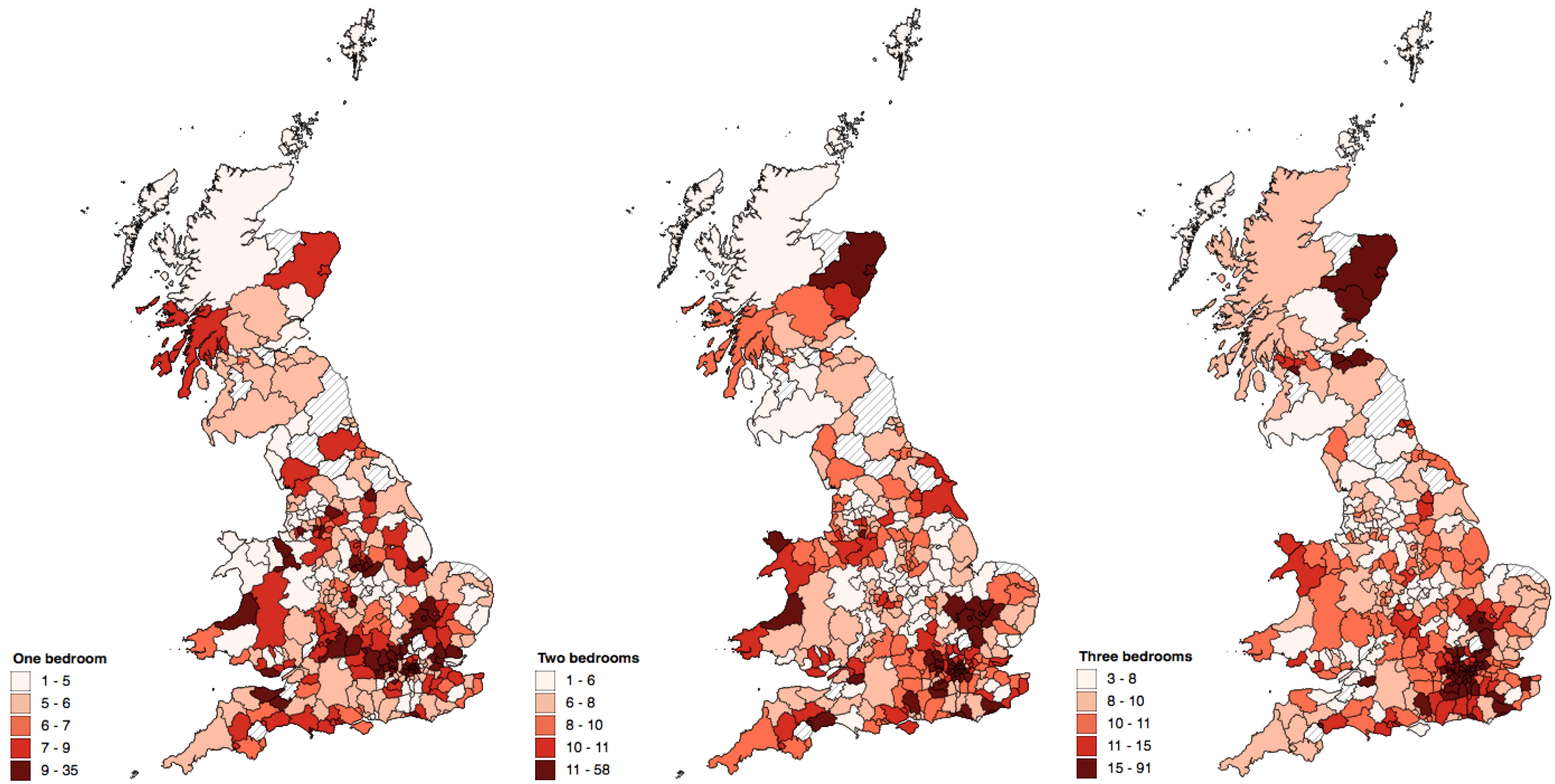
Figure A2: Affordability and the impact of housing benefit across the market segments



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: Estimated impact of reducing Local Housing Allowance from covering median to 30th percentile of rents at the district level for different types of properties: amounts expressed in GBP lost per claimant and week

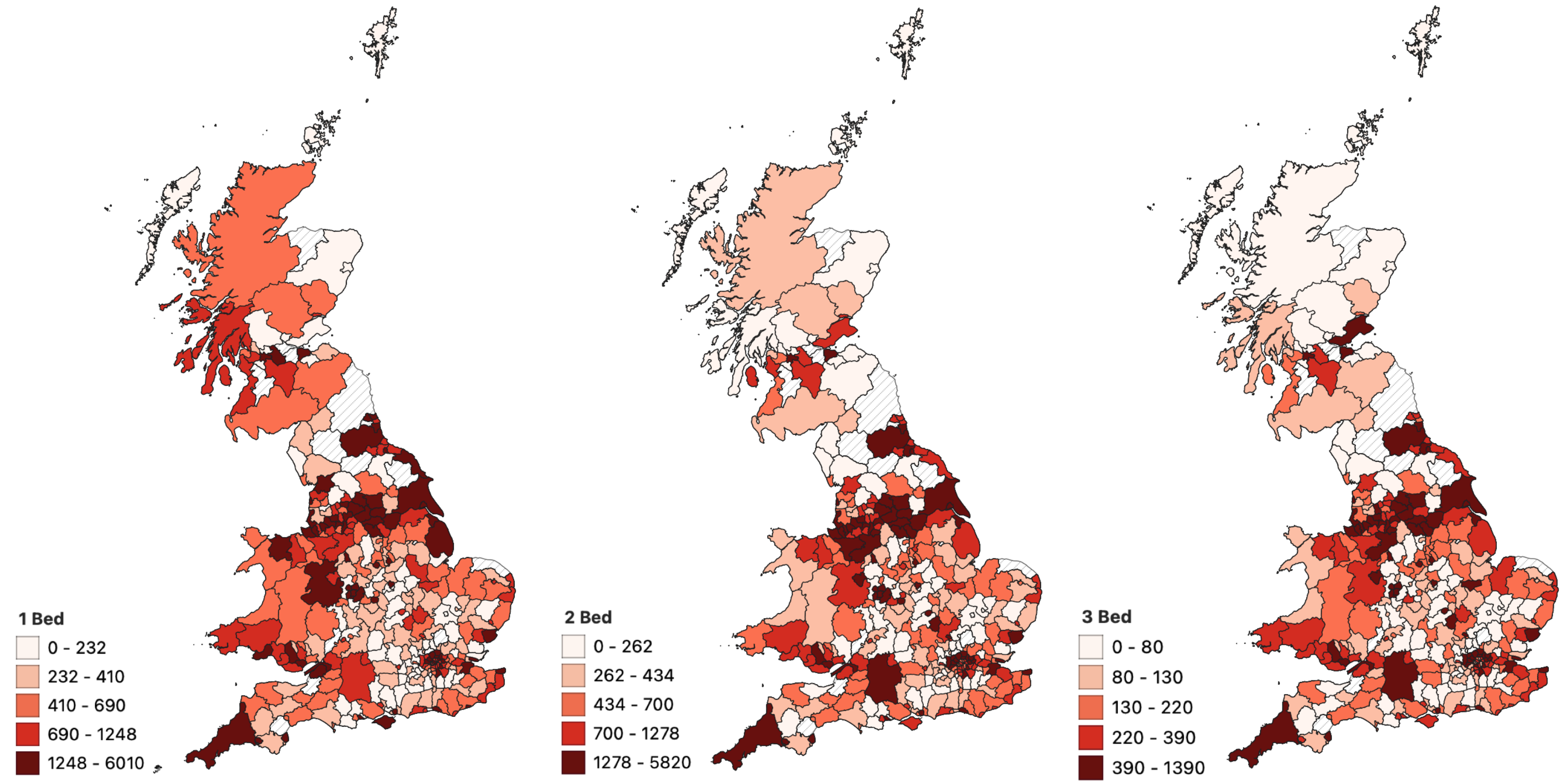
Panel A: One Bedroom flats   *Panel B*: Two Bedroom flats   *Panel C*: Three Bedroom flats



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

Figure A4: Estimated impact of reducing Local Housing Allowance from covering median to 30th percentile of rents at the district level for different types of properties: estimated number of claimants affected by respective cut

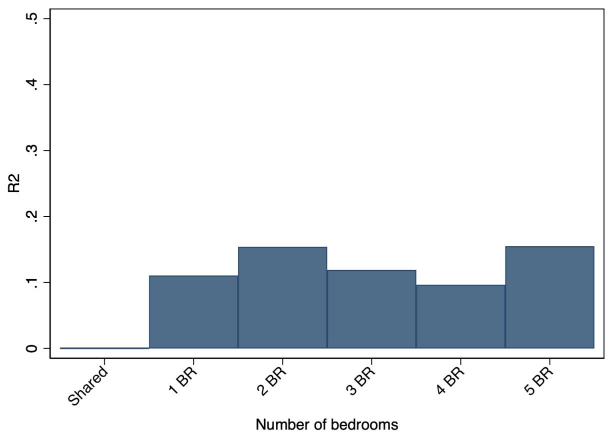
Panel A: One Bedroom flats *Panel B*: Two Bedroom flats *Panel C*: Three Bedroom flats



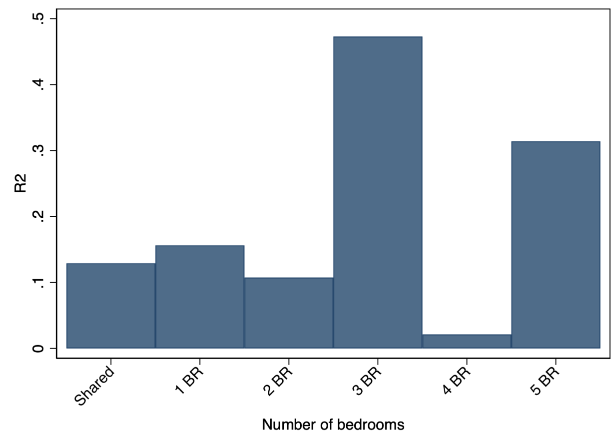
Notes: Figure plots the number of households that were estimated to be affected by the respective cuts to the local housing allowance rate covering the 50th percentile of private sector rents to only cover up to the 30th percentile of private sector rents. The figure highlights significant spatial variation of the incidence of the shock that is due to the different structure of housing benefit demand across districts..

Figure A5: Decomposition of Variation in Treatment Intensity Measure

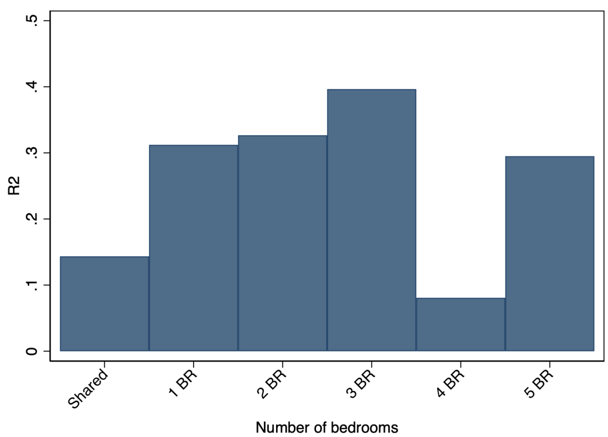
Panel A: Housing benefit cut per week & type



Panel B: # of claimants affected by type

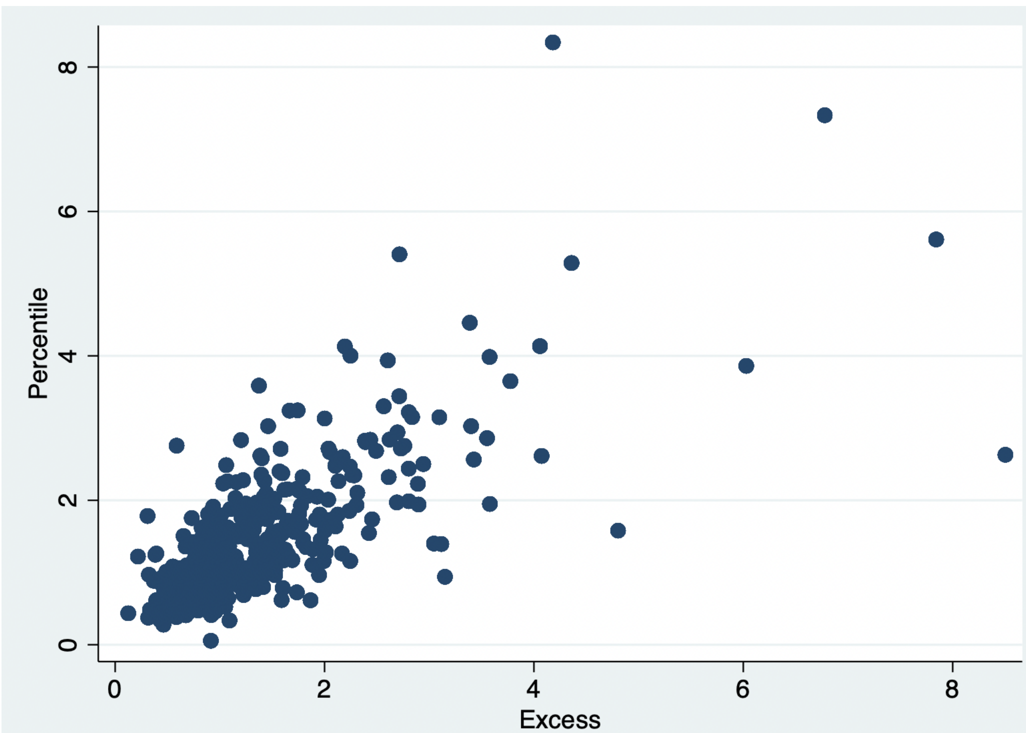


Panel C: # of claimants x Average Cut by type



Notes: Figure plots the R^2 capturing the variation that can be explained in the combined housing-benefit cut treatment exposure measure across districts zooming in on the variation that can be attributed to the size of the cut across the different types of accommodation (Panel A), the number of claimants affected by each cut (Panel B) or the combination of the two (Panel C).

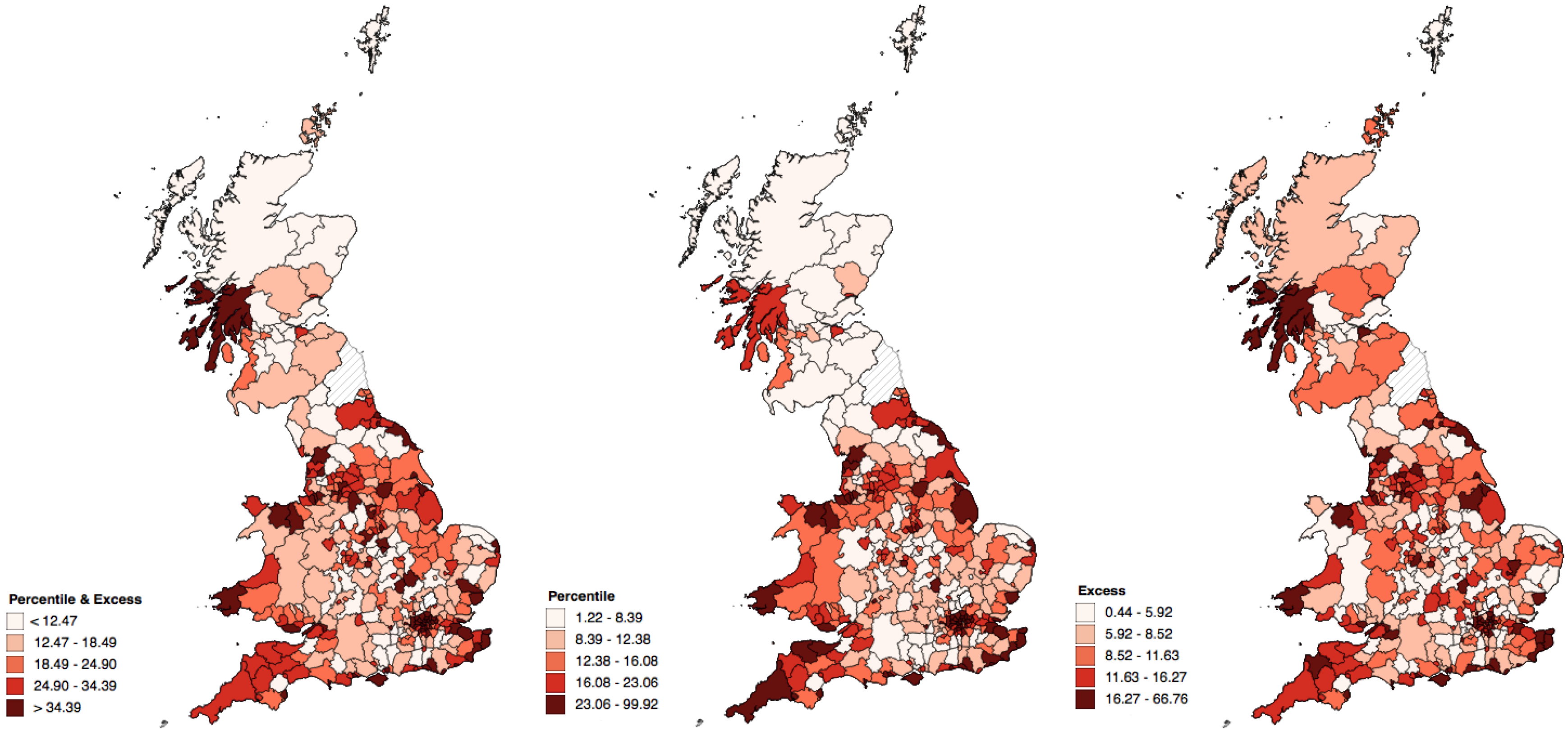
Figure A6: Excess versus percentile shocks



Notes: Figure plots the percentile and excess shock per households in a district as a result of the April 2011 entitlement cut.

Figure A7: Ex-ante estimated impact of cuts to housing benefit: spatial distribution of financial losses per resident households

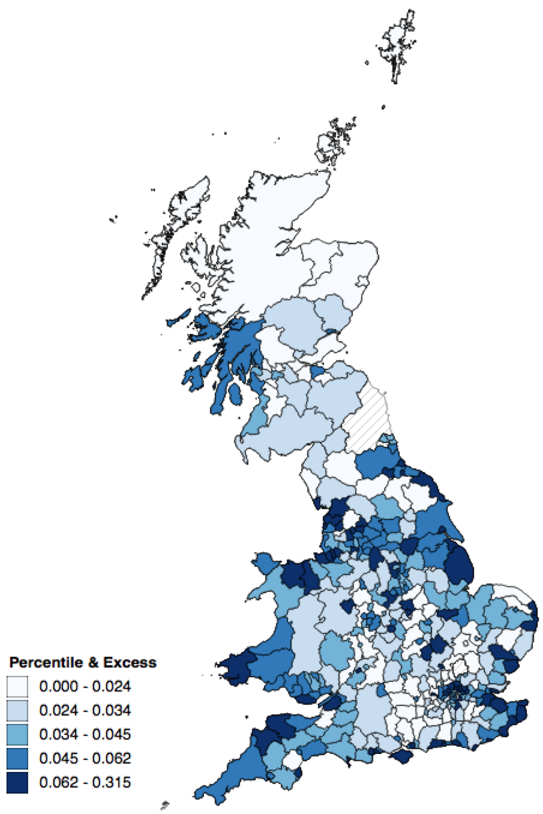
Panel A: Percentile & excess *Panel B*: Percentile *Panel C*: Excess



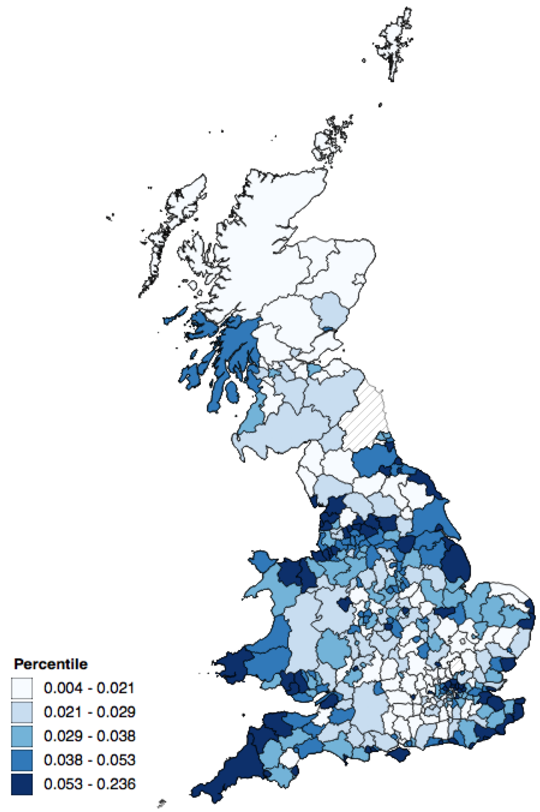
Notes: Map plots out the average financial losses due to the housing benefit cuts in a year and district divided by the number of households living in a district at baseline. Panel A presents the measure combining the percentile- and the excess cut, Panel B focuses on the measure pertaining to the cut in reference rents from the median to the 30th percentile, while Panel C presents the measure pertaining to the removal of the excess payments.

Figure A8: Ex-ante estimated impact of cuts to housing benefit: spatial distribution of share of resident households affected

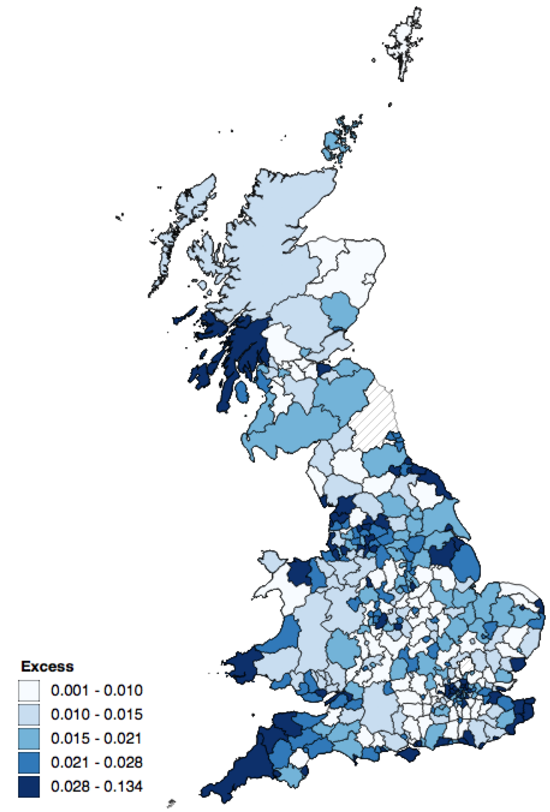
Panel A: Percentile & excess



Panel B: Percentile



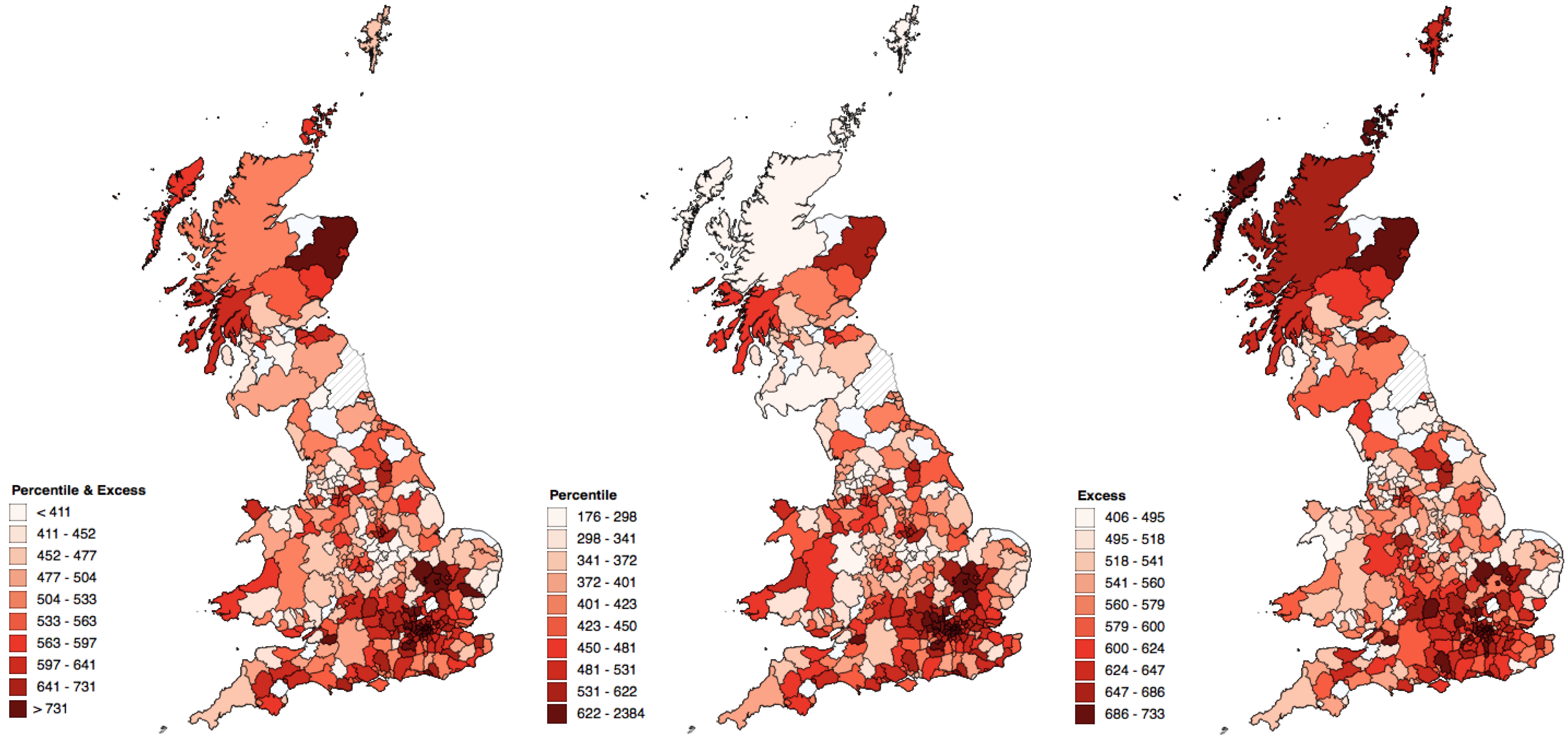
Panel C: Excess



Notes: Map plots out the share of households affected by the housing benefit reforms implemented from April 2011. Panel A presents the measure combining the percentile- and the excess cut, Panel B focuses on the measure pertaining to the cut in reference rents from the median to the 30th percentile, while Panel C presents the measure pertaining to the removal of the excess payments.

Figure A9: Ex-ante estimated impact of cuts to housing benefit: spatial distribution of financial losses per affected households

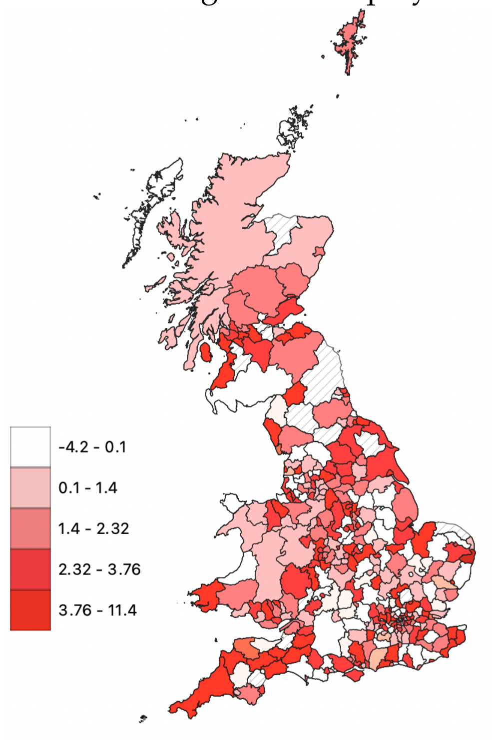
Panel A: Percentile & excess *Panel B*: Percentile *Panel C*: Excess



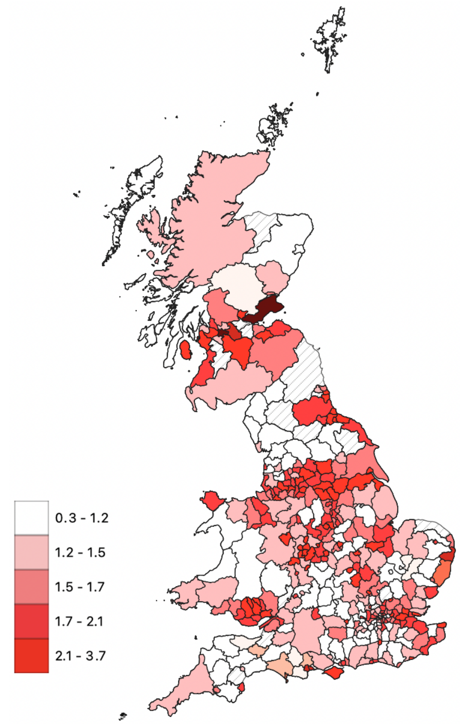
Notes: Map plots out the financial losses per household that was affected by the housing benefit cuts implemented from April 2011. Panel A presents the measure combining the percentile- and the excess cut, Panel B focuses on the measure pertaining to the cut in reference rents from the median to the 30th percentile, while Panel C presents the measure pertaining to the removal of the excess payments.

Figure A10: Exposure to the Great Recession

Panel A: Change in Unemployment Estimates, 2010-2008



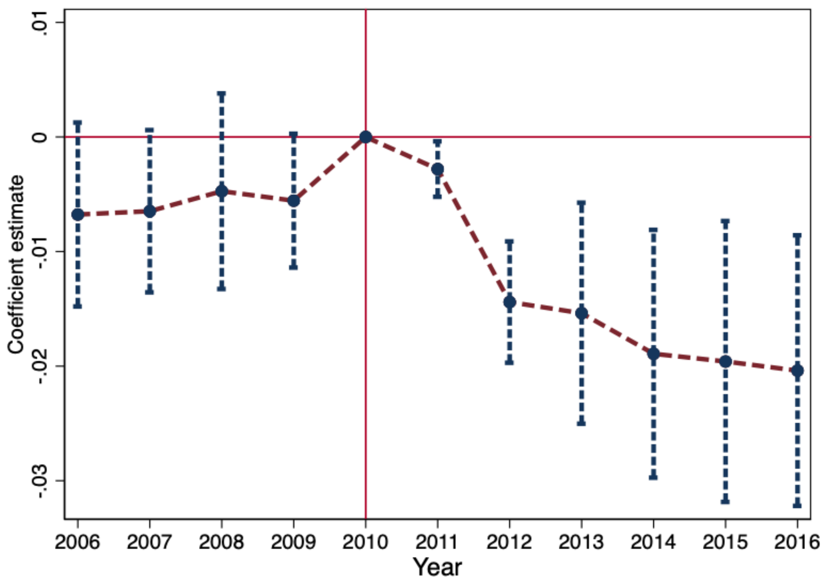
Panel B: Change in Job Allowance Claimant rate, 2010-2008



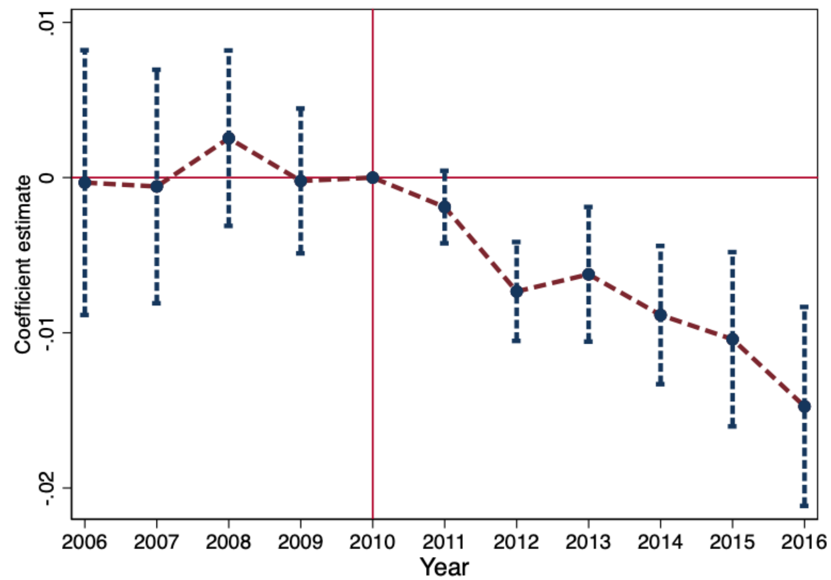
Notes: Map plots the exposure to the Great Recession, as measured by the change in the Change in Unemployment estimates (Panel A) and Jobseeker Allowance Rante (JSA), between April 2008 and April 2010.

Figure A11: Impact of cuts to housing benefit on housing benefit spending

Panel A: Percentile

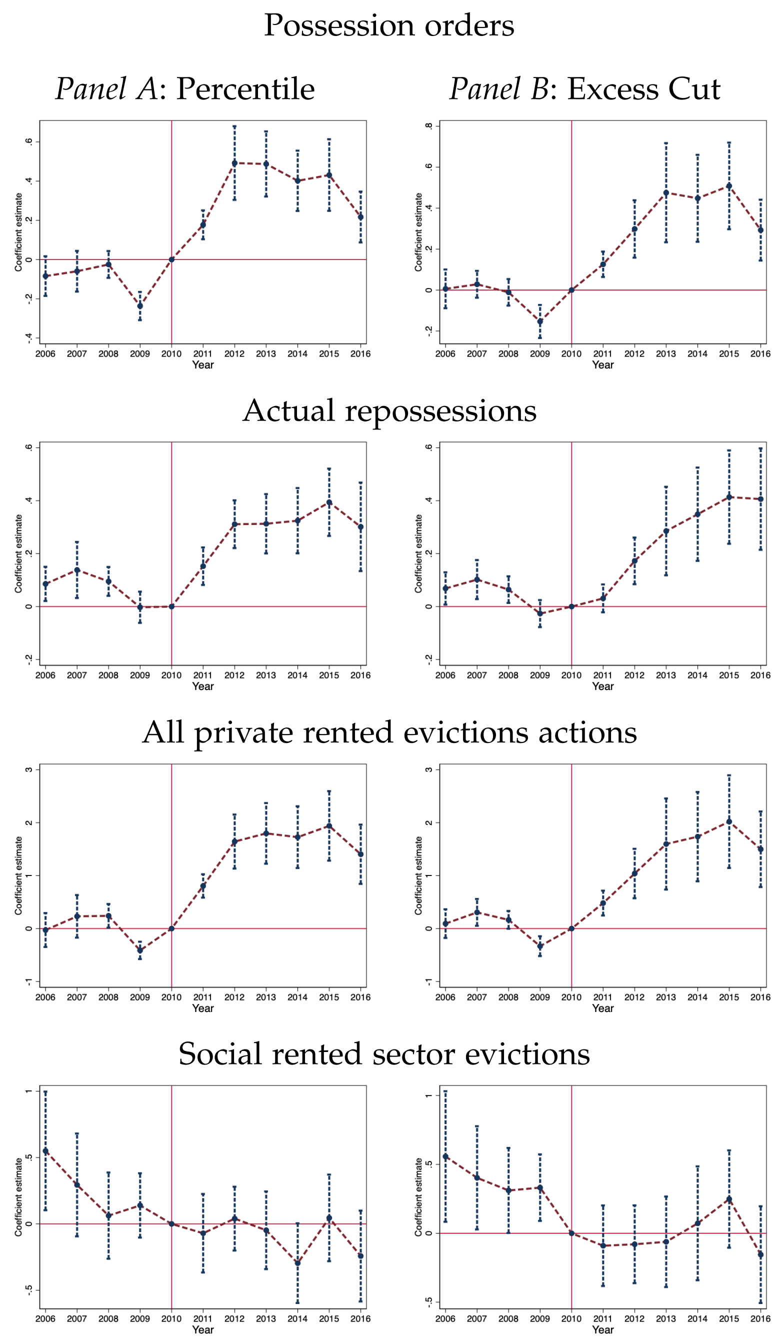


Panel B: Excess Cut



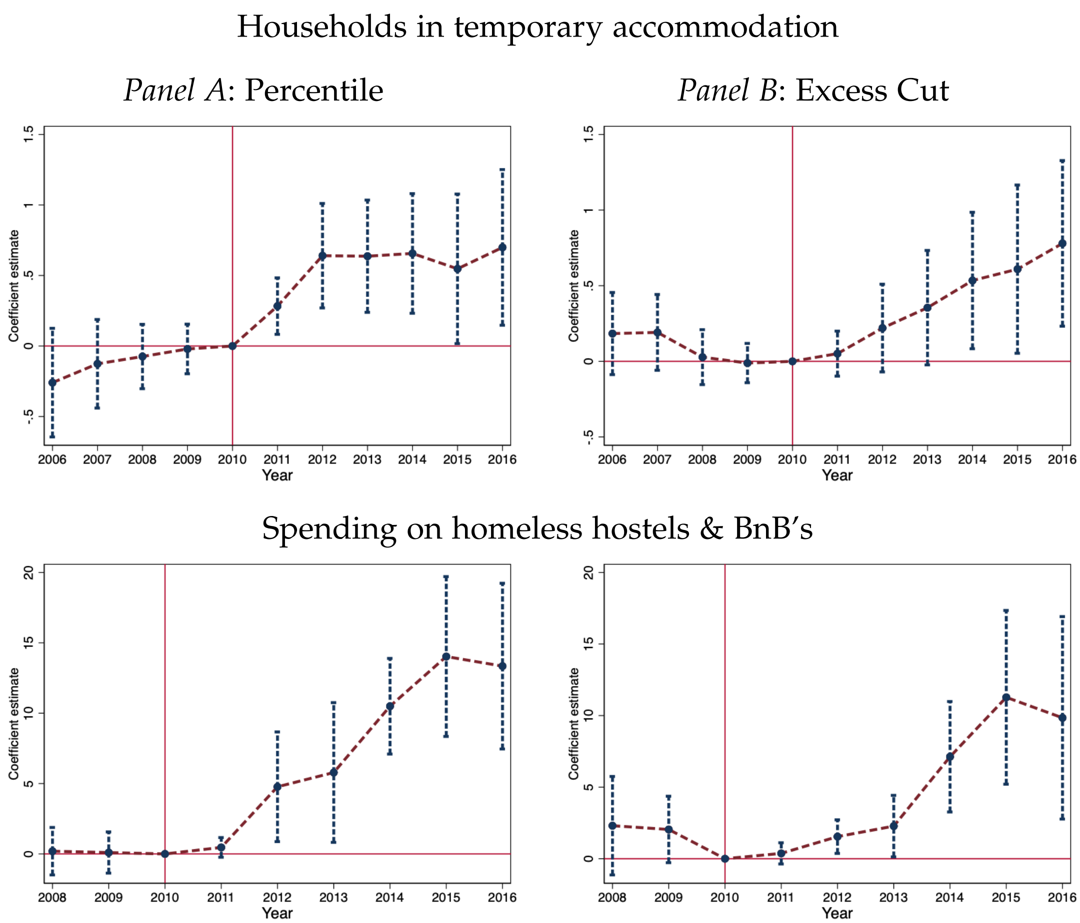
Notes: Figure plots regression coefficients obtained from estimating specification in Equation (2). The dependent variable is the log value of housing benefit spending. All regressions control for local authority district fixed effects and year effects. 90% confidence bands obtained from clustering standard errors at the district level are indicated.

Figure A12: Impact of cut to housing benefit on forced evictions of people living in rental accommodation



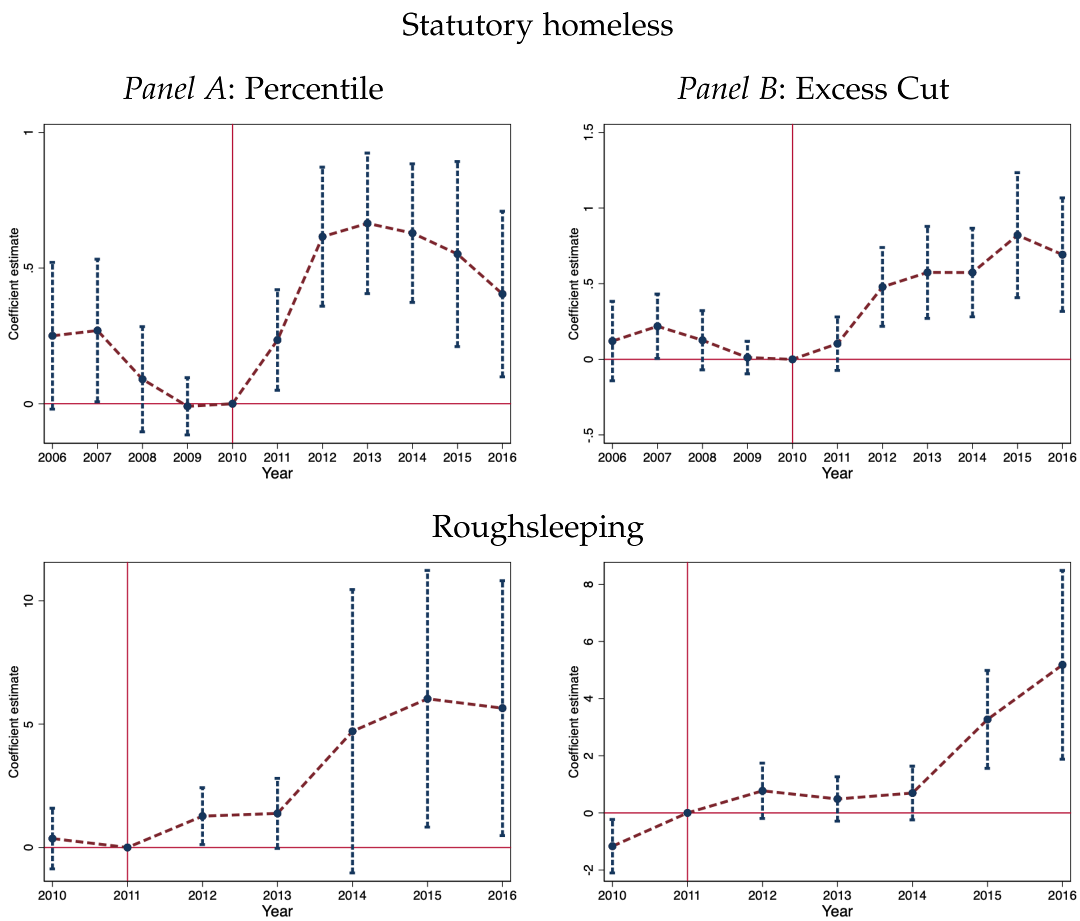
Notes: All dependent variables are measured as rates relative to the number of resident households in a district. Figure plots regression coefficients obtained from estimating specification in Equation (2). The dependent variable in Panel A measures all Landlord possession claims raised. Panel B studies 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. 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 A13: Impact of cut to housing benefit on rate of residence and spending in temporary accommodation



Notes: Figure plots regression coefficients obtained from estimating specification in Equation (2). 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.

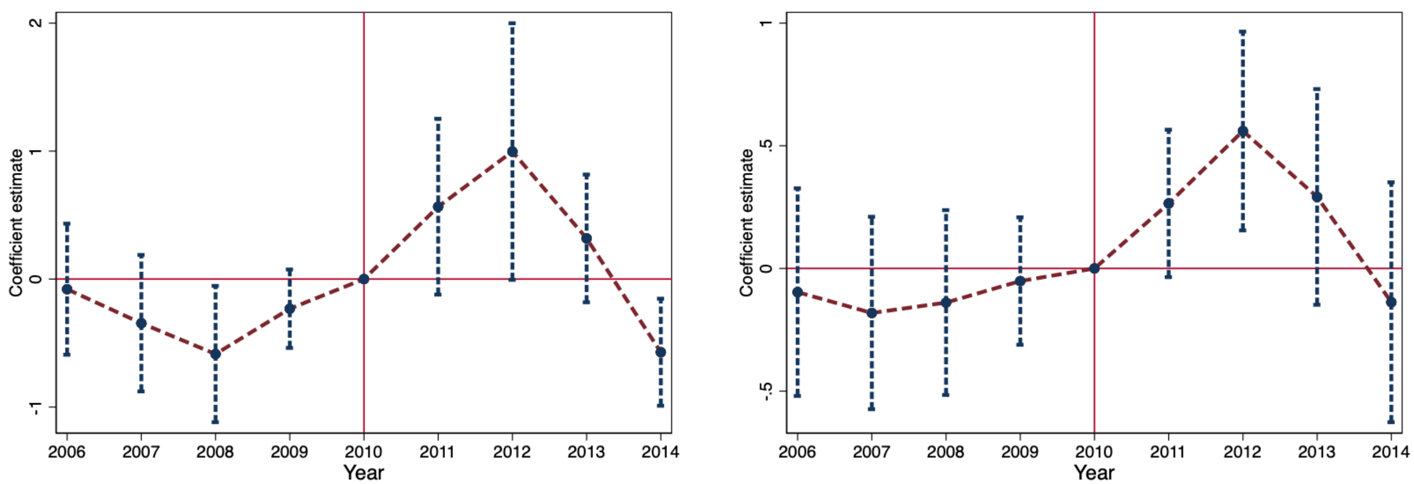
Figure A14: Impact of cut to housing benefit on measures of statutory homelessness



Notes: Figure plots regression coefficients obtained from estimating specification in Equation (2). 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 A15: Impact of housing benefit cut on crime

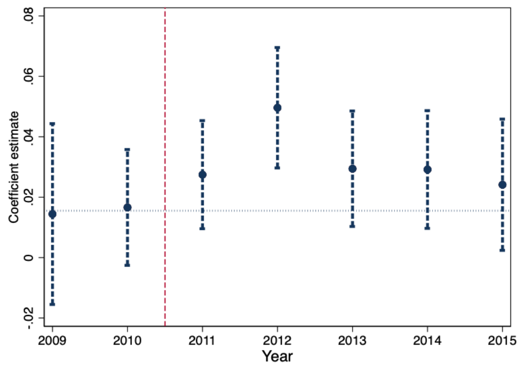
Panel A: Theft from persons   *Panel B*: Burglaries



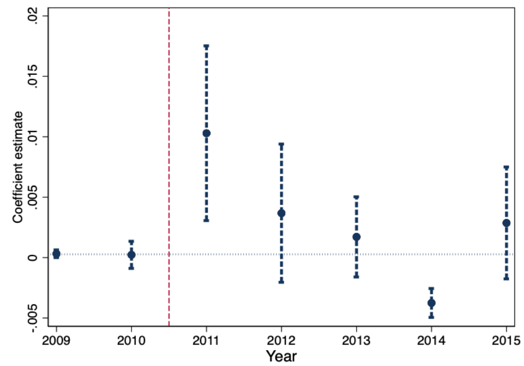
Notes: Figure plots regression coefficients obtained from estimating specification in Equation (2). All dependent variables are measured as rates relative to the number of resident households in a district. The dependent variable in Panel A measures the reported cases of theft from individuals; Panel 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.

Figure A16: Individual-level event studies

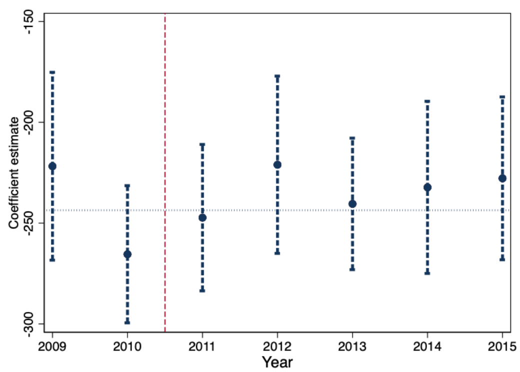
Panel A: Rent Arrears



Panel B: Evicted



Panel C: Labor Income



Notes: All dependent variables are measured as rates relative to the number of resident households in a district. Figure plots regression coefficients obtained from estimating specification in Equation (2). 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. 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.

Table A1: Impact of housing benefit cut on eviction measures: focusing on percentile and excess-shock separately

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	percentile shock					*excess shock*				
Panel A: Possession claims due to rent arrears										
post × $S^{\text{percentile}}$	0.500***	0.503***	0.480***	0.503***	0.289***					
	(0.079)	(0.073)	(0.110)	(0.083)	(0.098)					
post × S^{excess}						0.363***	0.325***	0.311***	0.448***	0.257
						(0.099)	(0.089)	(0.100)	(0.114)	(0.166)
Mean of DV	2.13	2.02	2	2.13	2.22	2.13	2.02	2	2.13	1.92
Local authority districts	366	366	354	365	92	365	365	353	364	80
Observations	4025	2927	3893	4014	1012	4014	2919	3882	4003	880
Panel B: Repossessions										
post × $S^{\text{percentile}}$	0.264***	0.209***	0.300***	0.265***	0.201**					
	(0.055)	(0.037)	(0.077)	(0.053)	(0.078)					
post × S^{excess}						0.207***	0.118***	0.192***	0.266***	0.125
						(0.064)	(0.042)	(0.069)	(0.073)	(0.096)
Mean of DV	1.46	1.35	1.37	1.46	1.43	1.46	1.35	1.37	1.46	1.34
Local authority districts	366	366	354	365	92	365	365	353	364	80
Observations	4025	2927	3893	4014	1012	4014	2919	3882	4003	880
Panel C: All private rented-sector eviction actions										
post × $S^{\text{percentile}}$	1.725***	1.526***	1.664***	1.724***	1.153***					
	(0.271)	(0.216)	(0.372)	(0.278)	(0.311)					
post × S^{excess}						1.282***	0.997***	1.085***	1.558***	0.926
						(0.344)	(0.266)	(0.347)	(0.394)	(0.627)
Mean of DV	5.28	4.86	4.92	5.28	5.55	5.28	4.86	4.93	5.29	4.69
Local authority districts	366	366	354	365	92	365	365	353	364	80
Observations	4025	2927	3893	4014	1012	4014	2919	3882	4003	880
Panel D: All social-rented rented-sector eviction actions										
post × $S^{\text{percentile}}$	-0.354***	-0.298**	-0.233*	-0.387**	-0.220					
	(0.121)	(0.119)	(0.132)	(0.156)	(0.346)					
post × S^{excess}						-0.374**	-0.425***	-0.247*	-0.413**	-1.156***
						(0.147)	(0.145)	(0.133)	(0.185)	(0.367)
Mean of DV	10.8	11	10.4	10.8	10.4	10.8	11	10.4	10.8	10.9
Local authority districts	366	366	354	365	92	365	365	353	364	80
Observations	4025	2927	3893	4014	1012	4014	2919	3882	4003	880
London included?	X	X		X	X	X	X		X	X
Include data after 2013	X		X	X	X	X		X	X	X
$C^j_{d,c,2010}$ trends				X					X	
Matched Pair x Year effects					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. Columns (1) and (6) study the effects of the shocks separately. Columns (2) and (7) drops data post-2013 when welfare reforms were implemented. Estimates in Column (3) and (8) exclude London. Columns (4) and (9) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^j_{d,c,\text{baseline}}$. Column (5) and (10) include matched pair-by-year fixed effects. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A2: Impact of housing benefit cut on bankruptcies: focusing on percentile and excess shock separately

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	percentile shock					*excess shock*				
Panel A: Total individual bankruptcies										
post × $S^{\text{percentile}}$	0.166***	0.127**	0.247***	0.138**	0.238**					
	(0.048)	(0.064)	(0.061)	(0.054)	(0.092)					
post × S^{excess}						0.135**	0.112	0.148**	0.084	0.159
						(0.060)	(0.071)	(0.067)	(0.069)	(0.110)
Mean of DV	6	6.39	6.1	6	5.7	6	6.39	6.09	6	5.82
Local authority districts	338	338	326	337	92	337	337	325	336	80
Observations	3717	2703	3585	3706	1012	3706	2695	3574	3695	880
Panel B: Individual voluntary arrangements										
post × $S^{\text{percentile}}$	0.356***	0.106	0.613***	0.261**	0.305					
	(0.116)	(0.101)	(0.151)	(0.129)	(0.239)					
post × S^{excess}						0.359***	0.126	0.431***	0.246*	0.580**
						(0.113)	(0.106)	(0.129)	(0.133)	(0.255)
Mean of DV	10.5	10.6	10.7	10.5	10	10.5	10.6	10.7	10.5	10.4
Local authority districts	338	338	326	337	92	337	337	325	336	80
Observations	3718	2704	3586	3707	1012	3707	2696	3575	3696	880
London included?	X	X		X	X	X	X		X	X
Include data after 2013	X		X	X	X	X		X	X	X
$C^{j}_{d,c,2010}$ trends				X					X	
Matched Pair x Year effects					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 but require the permission of the landlord, which typically prefer to use court action. Columns (1) and (6) study the effects of the shocks separately. Columns (2) and (7) drops data post-2013 when welfare reforms were implemented. Estimates in Column (3) and (8) exclude London. Columns (4) and (9) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. Column (5) and (10) include matched pair-by-year fixed effects. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A3: Impact of housing benefit cut on council spending on temporary housing and accommodation: focusing on percentile and excess shock separately

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	percentile shock					*excess shock*				
Panel A: Temporary accommodation										
post × $S^{\text{percentile}}$	0.584**	0.500**	0.282	0.681**	0.813*					
	(0.259)	(0.247)	(0.222)	(0.278)	(0.421)					
post × S^{excess}						0.249	0.128	0.252	0.369	-0.025
						(0.220)	(0.185)	(0.229)	(0.261)	(0.102)
Mean of DV	3.17	3.04	2.75	3.18	2.41	3.18	3.05	2.75	3.19	1.64
Local authority districts	363	363	351	362	92	362	362	350	361	78
Observations	3865	2866	3735	3854	950	3854	2858	3724	3843	808
Panel B: Council spending on hostels and BnB's										
post × $S^{\text{percentile}}$	9.165***	4.104***	6.517***	8.943***	6.548					
	(1.931)	(1.374)	(2.063)	(1.832)	(4.451)					
post × S^{excess}						5.035**	1.290	2.924	5.300**	3.412
						(2.024)	(1.217)	(1.801)	(2.093)	(3.132)
Mean of DV	9.83	7.58	7.97	9.85	7.59	9.85	7.6	7.99	9.88	5.76
Local authority districts	366	366	354	365	92	365	365	353	364	80
Observations	3243	2195	3135	3234	828	3234	2189	3126	3225	720
Panel C: Total council spending on temporary housing										
post × $S^{\text{percentile}}$	18.264***	8.754***	10.818***	18.899***	11.508*					
	(3.329)	(2.589)	(3.154)	(3.623)	(6.489)					
post × S^{excess}						9.816***	3.119**	6.835***	10.106***	5.781
						(2.443)	(1.422)	(2.298)	(2.588)	(4.586)
Mean of DV	18.2	14.6	13.4	18.2	12.9	18.2	14.6	13.4	18.3	7.37
Local authority districts	366	366	354	365	92	365	365	353	364	80
Observations	3293	2195	3185	3284	828	3284	2189	3176	3275	720
London included?	X	X		X	X	X	X		X	X
Include data after 2013	X		X	X	X	X		X	X	X
$C^{j}_{d,c,2010}$ trends				X					X	
Matched Pair x Year effects					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. Columns (1) and (6) study the effects of the shocks separately. Columns (2) and (7) drops data post-2013 when welfare reforms were implemented. Estimates in Column (3) and (8) exclude London. Columns (4) and (9) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. Column (5) and (10) include matched pair-by-year fixed effects. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A4: Impact of housing benefit cut on homelessness and rough sleeping: focusing on percentile and excess shock separately

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	percentile shock					*excess shock*				
Panel A: Statutory homelessness										
post × $S^{\text{percentile}}$	0.321***	0.304***	0.274*	0.394***	0.073					
	(0.113)	(0.108)	(0.149)	(0.147)	(0.130)					
post × S^{excess}						0.188	0.130	0.166	0.475***	0.102
						(0.124)	(0.115)	(0.136)	(0.133)	(0.139)
Mean of DV	4.5	4.68	4.46	4.51	2.41	4.5	4.68	4.46	4.51	2.31
Local authority districts	366	366	354	365	92	365	365	353	364	80
Observations	3968	2882	3841	3957	988	3957	2874	3830	3946	854
Panel B: Rough sleeping street counts										
post × $S^{\text{percentile}}$	3.560**	0.996**	2.577***	3.694**	1.626**					
	(1.530)	(0.391)	(0.694)	(1.709)	(0.790)					
post × S^{excess}						2.919***	0.999**	2.910***	2.870***	2.400*
						(0.753)	(0.416)	(0.752)	(0.788)	(1.329)
Mean of DV	8.56	6.79	7.78	8.57	6.47	8.56	6.79	7.77	8.57	6.56
Local authority districts	316	316	304	315	92	315	315	303	314	80
Observations	2212	1264	2128	2205	644	2205	1260	2121	2198	560
London included?	X	X		X	X	X	X		X	X
Include data after 2013	X		X	X	X	X		X	X	X
$C^{j}_{d,c,2010}$ trends				X					X	
Matched Pair x Year effects					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. Columns (1) and (6) study the effects of the shocks separately. Columns (2) and (7) drops data post-2013 when welfare reforms were implemented. Estimates in Column (3) and (8) exclude London. Columns (4) and (9) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. Column (5) and (10) include matched pair-by-year fixed effects. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A5: Robustness of results controlling for exposure to Great Recession

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Possession claims due to rent arrears						
post × $S^{\text{percentile \& excess}}$	0.422***			0.380***	0.429***	0.479***
	(0.120)			(0.104)	(0.124)	(0.126)
post × $S^{\text{percentile}}$		0.460***				
		(0.111)				
post × S^{excess}			0.278**			
			(0.109)			
Mean of DV	2.22	2.22	2.22	2.1	2.07	2.22
Local authority districts	319	320	319	319	307	319
Observations	3509	3520	3509	2552	3377	3509
Panel B: Temporary accommodation						
post × $S^{\text{percentile \& excess}}$	0.451***			0.446***	0.451***	0.521***
	(0.103)			(0.107)	(0.106)	(0.117)
post × $S^{\text{percentile}}$		0.423***				
		(0.093)				
post × S^{excess}			0.381***			
			(0.093)			
Mean of DV	-.0894	-.089	-.0894	-.206	-.0818	-.0894
Local authority districts	319	320	319	319	307	319
Observations	3363	3374	3363	2503	3235	3363
Panel C: Total council spending on temporary housing						
post × $S^{\text{percentile \& excess}}$	12.915***			3.932**	11.503***	14.165***
	(3.595)			(1.759)	(3.666)	(3.632)
post × $S^{\text{percentile}}$		14.926***				
		(3.621)				
post × S^{excess}			7.626***			
			(2.692)			
Mean of DV	19.9	19.8	19.9	15.8	14.4	19.9
Local authority districts	319	320	319	319	307	319
Observations	2871	2880	2871	1914	2763	2871
Panel D: Statutory homelessness						
post × $S^{\text{percentile \& excess}}$	0.587***			0.444**	0.568***	0.760***
	(0.190)			(0.183)	(0.193)	(0.183)
post × $S^{\text{percentile}}$		0.599***				
		(0.182)				
post × S^{excess}			0.438**			
			(0.182)			
Mean of DV	4.64	4.63	4.64	4.83	4.6	4.64
Local authority districts	319	320	319	319	307	319
Observations	3455	3466	3455	2510	3328	3455
Panel E: Rough sleepers						
post × $S^{\text{percentile \& excess}}$	3.501***			1.236**	3.329***	3.422***
	(1.060)			(0.597)	(1.054)	(1.071)
post × $S^{\text{percentile}}$		3.259***				
		(0.996)				
post × S^{excess}			2.948***			
			(0.950)			
Mean of DV	9.05	9.05	9.05	7.18	8.16	9.05
Local authority districts	273	274	273	273	261	273
Observations	1911	1918	1911	1092	1827	1911
Great Recession Exposure x Time	X	X	X		X	X
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^{j}_{d,c,2010}$ trends						X

Notes: All regressions include district and year fixed effects and control for a vector of six measures capturing local labor market exposure to the Great Recession. This is captured through an interaction with a set of year fixed effect of both the level as well as the changes between 2008 and 2010 in the economic inactivity rate, the unemployment rate and the job seeker allowance claimants rate. Columns (1), (2) and (3) study the effect of the reform on the combined, percentile and excess shocks separately. Column (4) drops data post-2013 when welfare reforms were implemented. Estimates in Column (5) exclude London. Column (6) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A6: Robustness of results controlling for unemployment and inactivity rates

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Possession claims due to rent arrears						
post × $S^{\text{percentile \& excess}}$	0.518***			0.501***	0.455***	0.532***
	(0.099)			(0.094)	(0.119)	(0.111)
post × $S^{\text{percentile}}$		0.540***				
		(0.082)				
post × S^{excess}			0.387***			
			(0.109)			
Mean of DV	2.18	2.18	2.18	2.08	2.03	2.24
Local authority districts	346	347	346	345	334	319
Observations	3233	3243	3233	2614	3113	3048
Panel B: Temporary accommodation						
post × $S^{\text{percentile \& excess}}$	0.572***			0.555***	0.451***	0.614***
	(0.102)			(0.092)	(0.111)	(0.113)
post × $S^{\text{percentile}}$		0.558***				
		(0.089)				
post × S^{excess}			0.482***			
			(0.100)			
Mean of DV	-.123	-.122	-.123	-.205	-.11	-.127
Local authority districts	346	347	346	345	334	319
Observations	3127	3137	3127	2566	3011	2946
Panel C: Total council spending on temporary housing						
post × $S^{\text{percentile \& excess}}$	14.550***			7.868***	8.044***	17.218***
	(3.223)			(2.653)	(2.919)	(3.999)
post × $S^{\text{percentile}}$		16.745***				
		(3.138)				
post × S^{excess}			8.461***			
			(2.324)			
Mean of DV	18.2	18.1	18.2	15.5	13.2	19
Local authority districts	346	347	346	345	334	319
Observations	2588	2596	2588	1969	2492	2438
Panel D: Statutory homelessness						
post × $S^{\text{percentile \& excess}}$	0.369***			0.365***	0.360***	0.699***
	(0.114)			(0.115)	(0.138)	(0.151)
post × $S^{\text{percentile}}$		0.390***				
		(0.110)				
post × S^{excess}			0.268**			
			(0.125)			
Mean of DV	4.63	4.62	4.63	4.72	4.59	4.8
Local authority districts	346	347	346	345	334	319
Observations	3183	3193	3183	2571	3068	2999
Panel E: Rough sleepers						
post × $S^{\text{percentile \& excess}}$	2.687**			1.201***	1.850***	3.492**
	(1.198)			(0.435)	(0.557)	(1.405)
post × $S^{\text{percentile}}$		2.859**				
		(1.449)				
post × S^{excess}			1.951***			
			(0.550)			
Mean of DV	8.24	8.23	8.24	7.12	7.38	8.57
Local authority districts	297	298	297	297	285	272
Observations	1655	1661	1655	1128	1583	1537
Unemployment and benefit claimant controls	X	X	X		X	X
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^{j}_{d,c,2010}$ trends						X

Notes: All regressions include district and year fixed effects and additionally include measures of the unemployment, inactivity and job seeker allowance claimant rates. Columns (1), (2) and (3) study the effect of the reform on the combined, percentile and excess shocks separately. Column (4) drops data post-2013 when welfare reforms were implemented. Estimates in Column (5) exclude London. Column (6) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A7: Robustness of results controlling for other cuts implemented from 2011

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Possession claims due to rent arrears						
post × $S^{\text{percentile \& excess}}$	0.508***			0.525***	0.448***	0.504***
	(0.084)			(0.087)	(0.105)	(0.091)
post × $S^{\text{percentile}}$		0.508***				
		(0.067)				
post × S^{excess}			0.388***			
			(0.095)			
Mean of DV	2.13	2.13	2.13	2.02	2	2.13
Local authority districts	365	366	365	365	353	364
Observations	4014	4025	4014	2919	3882	4003
Panel B: Change in temporary accommodation						
post × $S^{\text{percentile \& excess}}$	0.613***			0.617***	0.482***	0.611***
	(0.089)			(0.083)	(0.097)	(0.095)
post × $S^{\text{percentile}}$		0.583***				
		(0.078)				
post × S^{excess}			0.516***			
			(0.090)			
Mean of DV	-.0875	-.0871	-.0875	-.196	-.0808	-.0876
Local authority districts	365	366	365	365	353	364
Observations	3842	3853	3842	2863	3714	3831
Panel C: Temporary accommodation						
post × $S^{\text{percentile \& excess}}$	0.763***			0.588**	0.489*	0.816***
	(0.273)			(0.259)	(0.278)	(0.292)
post × $S^{\text{percentile}}$		0.802***				
		(0.291)				
post × S^{excess}			0.453*			
			(0.233)			
Mean of DV	3.18	3.17	3.18	3.05	2.75	3.19
Local authority districts	362	363	362	362	350	361
Observations	3854	3865	3854	2858	3724	3843
Panel D: Total council spending on temporary housing						
post × $S^{\text{percentile \& excess}}$	19.496***			8.897***	11.562***	19.745***
	(3.676)			(2.750)	(3.161)	(3.948)
post × $S^{\text{percentile}}$		21.149***				
		(3.446)				
post × S^{excess}			12.446***			
			(2.722)			
Mean of DV	18.2	18.2	18.2	14.6	13.4	18.3
Local authority districts	365	366	365	365	353	364
Observations	3284	3293	3284	2189	3176	3275
Panel E: Statutory homelessness						
post × $S^{\text{percentile \& excess}}$	0.551***			0.423***	0.576***	0.682***
	(0.128)			(0.126)	(0.161)	(0.141)
post × $S^{\text{percentile}}$		0.530***				
		(0.123)				
post × S^{excess}			0.456***			
			(0.142)			
Mean of DV	4.5	4.5	4.5	4.68	4.46	4.51
Local authority districts	365	366	365	365	353	364
Observations	3957	3968	3957	2874	3830	3946
Panel F: Rough sleepers						
post × $S^{\text{percentile \& excess}}$	4.356***			1.369***	3.842***	4.318***
	(1.511)			(0.447)	(0.914)	(1.629)
post × $S^{\text{percentile}}$		4.151**				
		(1.672)				
post × S^{excess}			3.734***			
			(0.867)			
Mean of DV	8.56	8.56	8.56	6.79	7.77	8.57
Local authority districts	315	316	315	315	303	314
Observations	2205	2212	2205	1260	2121	2198
Other Cuts x Time	X	X	X	X	X	X
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^j_{d,c,2010}$ trends						X

Notes: All regressions include district and year fixed effects and control for an area's exposure to the child tax credit cuts and the child benefit cut interacted with a set of year fixed effects. These two cuts were implemented from 2011 onwards and could have interacted with the housing benefit cut's impact. Columns (1), (2) and (3) study the effect of the reform on the combined, percentile and excess shocks separately. Column (4) drops data post-2013 when welfare reforms were implemented. Estimates in Column (5) exclude London. Column (6) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^j_{d,c,\text{baseline}}$. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.

Table A8: Robustness of results controlling for various forms of spillovers

	(1)	(2)	(3)	(4)	(5)	(6)
Spillover W_{ij} matrix based on	No spillovers	*Broad Rental market areas*		*Moving flows*	*Commuting flows*	
		Contiguous	Max		all flows	train
Panel A: Possession claims due to rent arrears						
post × $S^{\text{percentile \& excess}}$	0.478***	0.382***	0.416***	0.406***	0.368***	0.449***
	(0.092)	(0.087)	(0.091)	(0.083)	(0.079)	(0.085)
post × Spillover		0.391***	7.220***	1.106***	0.835***	0.696***
		(0.080)	(1.790)	(0.206)	(0.147)	(0.115)
Mean of DV	2.13	2.13	2.13	2.13	2.13	2.13
Local authority districts	365	365	365	365	365	365
Observations	4014	4014	4014	4014	4014	4014
Panel B: Total individual bankruptcies						
post × $S^{\text{percentile \& excess}}$	0.166***	0.203***	0.180***	0.195***	0.193***	0.179***
	(0.049)	(0.056)	(0.054)	(0.054)	(0.056)	(0.052)
post × Spillover		-0.161**	-1.474	-0.497***	-0.210**	-0.389***
		(0.067)	(1.388)	(0.128)	(0.095)	(0.093)
Mean of DV	6	6	6	6	6	6
Local authority districts	337	337	337	337	337	337
Observations	3706	3706	3706	3706	3706	3706
Panel C: Change in temporary accommodation						
post × $S^{\text{percentile \& excess}}$	0.531***	0.388***	0.456***	0.448***	0.413***	0.504***
	(0.097)	(0.079)	(0.088)	(0.087)	(0.085)	(0.093)
post × Spillover		0.578***	8.808***	1.242***	0.885***	0.609***
		(0.106)	(2.470)	(0.224)	(0.166)	(0.141)
Mean of DV	-.0875	-.0875	-.0875	-.0875	-.0875	-.0875
Local authority districts	365	365	365	365	365	365
Observations	3842	3842	3842	3842	3842	3842
Panel D: Temporary accommodation						
post × $S^{\text{percentile \& excess}}$	0.494**	0.440*	0.417*	0.504**	0.475**	0.517**
	(0.237)	(0.228)	(0.224)	(0.238)	(0.233)	(0.239)
post × Spillover		0.433	25.821***	0.879	1.108**	0.625***
		(0.309)	(9.582)	(0.556)	(0.477)	(0.238)
Mean of DV	3.18	3.18	3.18	3.18	3.18	3.18
Local authority districts	362	362	362	362	362	362
Observations	3854	3854	3854	3854	3854	3854
Panel E: Total council spending on temporary housing						
post × $S^{\text{percentile \& excess}}$	15.988***	12.869***	12.637***	14.060***	13.048***	15.490***
	(3.361)	(2.885)	(2.540)	(3.236)	(3.170)	(3.373)
post × Spillover		12.608***	387.868***	29.630***	22.218***	11.584***
		(3.796)	(140.472)	(7.200)	(5.797)	(3.617)
Mean of DV	18.2	18.2	18.2	18.2	18.2	18.2
Local authority districts	365	365	365	365	365	365
Observations	3284	3284	3284	3284	3284	3284
Panel F: Statutory homelessness						
post × $S^{\text{percentile \& excess}}$	0.287**	-0.009	0.210	0.115	0.067	0.217*
	(0.117)	(0.128)	(0.130)	(0.118)	(0.121)	(0.115)
post × Spillover		1.214***	9.187**	2.592***	1.652***	1.503***
		(0.251)	(3.885)	(0.442)	(0.291)	(0.290)
Mean of DV	4.5	4.5	4.5	4.5	4.5	4.5
Local authority districts	365	365	365	365	365	365
Observations	3957	3957	3957	3957	3957	3957
Panel G: Rough sleepers						
post × $S^{\text{percentile \& excess}}$	3.532***	3.489***	3.335***	3.768***	3.880***	3.595***
	(1.298)	(1.242)	(1.097)	(1.373)	(1.446)	(1.322)
post × Spillover		0.168	18.926	-3.775	-2.556	-1.437
		(0.898)	(32.265)	(2.599)	(1.737)	(1.696)
Mean of DV	8.56	8.56	8.56	8.56	8.56	8.56
Local authority districts	315	315	315	315	315	315
Observations	2205	2205	2205	2205	2205	2205

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 table summarizes results studying spillovers of the shock across districts, accounting for a broad range of possible spillover matrices W_{ij} across the different column heads. Column (1) presents the headline coefficient without accounting for spillovers; columns (2) - (3) model spillovers across districts that are part of the same Broad Rental Market Area at which level local housing allowance rates are determined; column (4) uses data from the 2011 Census measuring intra-UK migration between districts in the year prior to census date; column (5) and (6) measure spillover based on commuting flows across districts also inferred from the 2011 UK Census based on usual places of work and place of residence. 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 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 × $S^{\text{percentile \& excess}}$	12.889	13.064	25.590**	17.391				
	(10.442)	(10.080)	(10.132)	(12.342)				
post × $S^{\text{percentile}}$					11.968	46.749		
					(11.484)	(32.192)		
post × S^{excess}							11.420	-17.259
							(9.198)	(25.677)
Mean of DV	707	669	640	708	707	647	707	708
Local authority districts	360	354	348	359	361	92	360	78
Observations	3049	2018	2941	3040	3058	736	3049	612
Panel B: Internal migration inflow rate								
post × $S^{\text{percentile \& excess}}$	-12.133***	-9.490***	-9.096***	-8.635***				
	(2.720)	(2.332)	(1.893)	(2.881)				
post × $S^{\text{percentile}}$					-12.809***	-7.503**		
					(2.748)	(3.139)		
post × S^{excess}							-9.005***	-6.002
							(2.021)	(4.093)
Mean of DV	497	485	485	496	497	534	497	523
Local authority districts	364	364	352	363	365	92	364	80
Observations	3276	2184	3168	3267	3285	828	3276	720
Panel C: Internal migration outflow rate								
post × $S^{\text{percentile \& excess}}$	-1.265	-2.083	-0.716	0.490				
	(1.301)	(1.354)	(1.242)	(1.434)				
post × $S^{\text{percentile}}$					-1.471	-0.585		
					(1.391)	(1.844)		
post × S^{excess}							-0.693	-2.251
							(1.155)	(3.428)
Mean of DV	485	475	470	485	485	509	485	500
Local authority districts	364	364	352	363	365	92	364	80
Observations	3276	2184	3168	3267	3285	828	3276	720
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 effects						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.

Table A10: Impact of housing benefit cut on international migration indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Short-term international migration								
post × $S^{\text{percentile \& excess}}$	0.942*	0.338	1.545**	0.223				
	(0.566)	(0.558)	(0.638)	(0.715)				
post × $S^{\text{percentile}}$					0.604	1.724		
					(0.475)	(1.130)		
post × S^{excess}							1.294**	-0.315
							(0.654)	(0.903)
Mean of DV	19.7	17.6	17.7	19.7	19.7	16.6	19.7	15.7
Local authority districts	336	336	324	335	337	92	336	80
Observations	2688	2016	2592	2680	2696	736	2688	640
Panel B: Long-term international migration								
post × $S^{\text{percentile \& excess}}$	11.668	-65.672	-19.634	10.385				
	(43.341)	(46.443)	(40.146)	(47.214)				
post × $S^{\text{percentile}}$					22.862	43.460*		
					(47.956)	(25.616)		
post × S^{excess}							-6.866	50.683
							(37.694)	(41.444)
Mean of DV	1548	1500	1336	1551	1546	1196	1548	1140
Local authority districts	365	365	353	364	366	92	365	80
Observations	3285	2190	3177	3276	3294	828	3285	720
Panel C: New migrant GP registrations								
post × $S^{\text{percentile \& excess}}$	-1.620	-3.115*	-3.305	-2.959				
	(1.843)	(1.687)	(2.081)	(2.175)				
post × $S^{\text{percentile}}$					-0.783	1.551		
					(2.056)	(2.984)		
post × S^{excess}							-2.575*	-5.384***
							(1.470)	(1.684)
Mean of DV	95.6	92.1	87.1	95.7	95.5	83.7	95.6	81.8
Local authority districts	336	336	324	335	337	92	336	80
Observations	3024	2016	2916	3015	3033	828	3024	720
Panel D: New National Insurance (NINO) issue								
post × $S^{\text{percentile \& excess}}$	-1.419	-6.153**	-2.160	-1.462				
	(1.424)	(2.392)	(1.692)	(1.558)				
post × $S^{\text{percentile}}$					-1.318	-1.443		
					(1.416)	(2.712)		
post × S^{excess}							-1.252	-3.348**
							(1.456)	(1.407)
Mean of DV	89.2	82.3	78.8	89.3	89.2	76	89.2	73.5
Local authority districts	364	364	352	363	365	92	364	80
Observations	3276	2184	3168	3267	3285	828	3276	720
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 effects						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 A11: Impact of housing benefit cut on unemployment and economic inactivity rates

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Unemployment rate						
post × $S^{\text{percentile \& excess}}$	-0.023			0.068	0.122**	-0.015
	(0.065)			(0.082)	(0.058)	(0.070)
post × $S^{\text{percentile}}$		-0.050				
		(0.066)				
post × S^{excess}			0.018			
			(0.060)			
Mean of DV	6.48	6.48	6.48	6.67	6.41	6.49
Local authority districts	363	364	363	363	351	362
Observations	3449	3459	3449	2787	3329	3443
Panel B: Self employment rate						
post × $S^{\text{percentile \& excess}}$	0.100			-0.010	0.054	0.128
	(0.072)			(0.070)	(0.082)	(0.085)
post × $S^{\text{percentile}}$		0.101				
		(0.073)				
post × S^{excess}			0.086			
			(0.066)			
Mean of DV	9.87	9.87	9.87	9.71	9.82	9.85
Local authority districts	364	365	364	364	352	363
Observations	3640	3650	3640	2912	3520	3630
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^{j}_{d,c,2010}$ trends						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. Columns (1), (2) and (3) study the effect of the reform on the combined, percentile and excess shocks separately. Column (4) drops data post-2013 when welfare reforms were implemented. Estimates in Column (5) exclude London. Column (6) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. 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 property prices

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All property types						
post × $S^{\text{percentile \& excess}}$	0.018**			0.010	-0.001	0.024**
	(0.009)			(0.006)	(0.008)	(0.009)
post × $S^{\text{percentile}}$		0.022**				
		(0.009)				
post × S^{excess}			0.009			
			(0.007)			
Mean of DV	12.1	12.1	12.1	12.1	12.1	12.1
Local authority districts	337	338	337	337	325	336
Observations	3707	3718	3707	2696	3575	3696
Panel B: Flats						
post × $S^{\text{percentile \& excess}}$	0.032***			0.021**	0.006	0.036***
	(0.012)			(0.009)	(0.012)	(0.013)
post × $S^{\text{percentile}}$		0.038***				
		(0.011)				
post × S^{excess}			0.019*			
			(0.011)			
Mean of DV	11.8	11.8	11.8	11.7	11.7	11.8
Local authority districts	337	338	337	337	325	336
Observations	3707	3718	3707	2696	3575	3696
Panel C: Semi-detached houses						
post × $S^{\text{percentile \& excess}}$	0.031***			0.021**	0.007	0.037***
	(0.011)			(0.009)	(0.009)	(0.012)
post × $S^{\text{percentile}}$		0.036***				
		(0.011)				
post × S^{excess}			0.020**			
			(0.009)			
Mean of DV	12.1	12.1	12.1	12.1	12.1	12.1
Local authority districts	336	337	336	336	324	335
Observations	3696	3707	3696	2688	3564	3685
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^j_{d,c,2010}$ trends						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 A13: Impact of housing benefit cut on broader rental market developments

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average private sector rent						
post × $S^{\text{percentile \& excess}}$	0.590*			0.370	-0.082	0.931**
	(0.332)			(0.234)	(0.359)	(0.360)
post × $S^{\text{percentile}}$		0.867***				
		(0.306)				
post × S^{excess}			0.084			
			(0.299)			
Mean of DV	83.2	83.1	83.2	77.7	82.5	83.2
Local authority districts	315	316	315	315	303	314
Observations	3465	3476	3465	2520	3333	3454
Panel B: Social rent						
post × $S^{\text{percentile \& excess}}$	0.252			0.231	-0.259	0.315
	(0.329)			(0.297)	(0.378)	(0.390)
post × $S^{\text{percentile}}$		0.429				
		(0.307)				
post × S^{excess}			-0.081			
			(0.279)			
Mean of DV	74.8	74.8	74.8	69.9	73.2	74.8
Local authority districts	176	176	176	175	164	176
Observations	1818	1818	1818	1331	1686	1818
Panel C: log(private sector rent)						
post × $S^{\text{percentile \& excess}}$	0.000			-0.000	-0.004*	0.000
	(0.002)			(0.002)	(0.003)	(0.003)
post × $S^{\text{percentile}}$		0.002				
		(0.002)				
post × S^{excess}			-0.002			
			(0.002)			
Mean of DV	4.4	4.4	4.4	4.34	4.39	4.4
Local authority districts	315	316	315	315	303	314
Observations	3465	3476	3465	2520	3333	3454
Panel D: log(social rent)						
post × $S^{\text{percentile \& excess}}$	-0.008***			-0.006***	-0.010***	-0.010***
	(0.002)			(0.002)	(0.003)	(0.003)
post × $S^{\text{percentile}}$		-0.007***				
		(0.002)				
post × S^{excess}			-0.008***			
			(0.002)			
Mean of DV	4.29	4.29	4.29	4.23	4.27	4.29
Local authority districts	176	176	176	175	164	176
Observations	1818	1818	1818	1331	1686	1818
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^j_{d,c,2010}$ trends						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 A14: Impact of cut to housing benefit on crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Theft from person						
post × $S^{\text{percentile \& excess}}$	0.763**			1.109**	0.033	0.591
	(0.378)			(0.500)	(0.164)	(0.387)
post × $S^{\text{percentile}}$		0.943**				
		(0.442)				
post × S^{excess}			0.353*			
			(0.183)			
Mean of DV	4.3	4.29	4.3	4.38	3.49	4.31
Local authority districts	324	325	324	324	312	323
Observations	2749	2758	2749	2458	2641	2740
Panel B: Burglaries						
post × $S^{\text{percentile \& excess}}$	-0.111			0.049	-0.200	0.118
	(0.183)			(0.177)	(0.222)	(0.231)
post × $S^{\text{percentile}}$		0.018				
		(0.177)				
post × S^{excess}			-0.295			
			(0.193)			
Mean of DV	12.1	12.1	12.1	12.4	11.8	12.1
Local authority districts	324	325	324	324	312	323
Observations	2749	2758	2749	2458	2641	2740
Panel C: Bodily harm						
post × $S^{\text{percentile \& excess}}$	-0.166			-0.252	-0.253	-0.067
	(0.204)			(0.217)	(0.260)	(0.252)
post × $S^{\text{percentile}}$		-0.072				
		(0.197)				
post × S^{excess}			-0.294			
			(0.207)			
Mean of DV	20.4	20.4	20.4	20.4	20.1	20.4
Local authority districts	324	325	324	324	312	323
Observations	2749	2758	2749	2458	2641	2740
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^{j}_{d,c,2010}$ trends						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 A15: Impact of housing benefit cut on housing benefit spending per capita

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: log(Housing benefit per capita)						
post $\times$ $S^{\text{percentile \& excess}}$	-0.015***			-0.012***	-0.010**	-0.012***
	(0.004)			(0.004)	(0.005)	(0.004)
post $\times$ $S^{\text{percentile}}$		-0.015***				
		(0.004)				
post $\times$ S^{excess}			-0.013***			
			(0.004)			
Mean of DV	6.53	6.53	6.53	6.49	6.5	6.53
Local authority districts	365	366	365	365	353	364
Observations	4015	4026	4015	2920	3883	4004
Include data after 2013	X	X	X		X	X
London included?	X	X	X	X		X
$C^{j}_{d,c,2010}$ trends						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 A16: Impact of housing benefit cut on attrition from survey

	(1)	(2)	(3)	(4)
DV: Attrition				
Post 2011 $\times$ 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 A17: 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 $\times$ 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 whether 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 A18: Impact of housing benefit cut on rent-arrears and evictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Rent arrears							
Post April 2011 $\times$ Cut in Housing benefit	0.021**	0.024**	0.027***	0.032***	0.022**	0.025***	0.025***
	(0.009)	(0.009)	(0.009)	(0.011)	(0.010)	(0.010)	(0.005)
Mean of DV	.178	.178	.179	.168	.178	.179	.186
Local Authority Districts	378	378	378	346	378	378	379
Observations	94713	93785	60694	47481	85248	84118	73554
Panel B: Evictions							
Post April 2011 $\times$ Cut in Housing benefit	0.003**	0.003*	0.006**	0.002	0.004*	0.004	0.004***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)
Mean of DV	.00648	.00646	.00541	.00539	.007	.00696	.007
Local Authority Districts	378	378	378	346	378	378	379
Observations	98080	97179	62876	49305	88395	87300	73554
Panel C: Non-benefit household income							
Post April 2011 $\times$ Cut in Housing benefit	16.495	40.141	53.358	13.652	48.849*	34.091	29.013*
	(29.920)	(35.893)	(35.915)	(42.572)	(26.078)	(28.680)	(15.832)
Mean of DV	1787	1790	1668	1565	1743	1746	1472.18
Local Authority Districts	378	378	378	346	378	378	379
Observations	97872	96968	62872	49301	88154	87058	73554
District & Time FE	x				x		
District x Time FE		x	x	x		x	
Individual FE					x	x	
Drop post 2013			x				
Drop London				x			
Heckman Correction							x

Notes: The dependent variable in Panel A is an indicator equal to 1 in case the household is behind their rent, while Panel B studies on evictions. The dependent variable in Panel C is non-benefit household income. The sample includes all individuals that live in rental accommodation. Columns (1), (2) and (3) study the effect of the reform on the combined, percentile and excess shocks separately. Column (4) drops data post-2013 when welfare reforms were implemented. Estimates in Column (5) exclude London. Column (6) controls for set of year fixed effects interacted with the distribution of claimants across different property types c affected by the reform j, $C^{j}_{d,c,\text{baseline}}$. Standard errors are clustered at the Local Government Authority District Level with standard errors presented in parentheses.