# The incidence of targeted housing subsidies: evidence from reforms to UK housing benefit

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## Abstract

During 2011 and 2012 the UK government reduced the generosity of the housing subsidy it provides to low-income private renters. This paper estimates the incidence of this change on the recipients and on their landlords, using administrative monthly panel data on the universe of subsidy recipients. We exploit the phased roll-out of the reforms to estimate separate effects on rents for new claimants and for existing claimants, using different identifying assumptions in each case. The two sets of estimates are extremely similar. Rents paid by subsidy recipients were affected little overall: after adjusting for housing quality about 90% of the incidence of the reforms was on tenants. There is important heterogeneity however. Estimated incidence on tenants was substantially lower for some subgroups - in particular, for those previously subsidised to rent expensive properties. We find this to be the most likely reason why previous research, looking at reforms affecting smaller groups of high-rent claimants, has found much higher incidence on landlords. Overall our results suggest that the incidence of reforms to housing subsidy regimes can vary substantially within the range of real-world rental markets.

# I. Introduction

The redistributive programs of modern welfare states tend to include targeted subsidies for certain goods, with rented housing among the most significant. The economic incidence of these subsidies is of great importance. To the extent that subsidies raise the price of rented accommodation, governments are transferring resources to landlords rather than the intended recipients. In the US, on which much of the empirical literature on the incidence of housing subsidies is based, the federal government spends about 0.01% of GDP ($18 billion in 2010) subsidising the rents of 2.2 million families through Housing Choice Vouchers, the largest such federal program. Demand-side housing subsidies are substantially more significant elsewhere. In Great Britain, the government spends around £9 billion per year, or 0.5% of GDP, subsidising the rents of 1.6 million families in privately rented accommodation through housing benefit.[1] These demand-side subsidies have increasingly taken the place of intervention through public housing projects, and rising rent levels have further increased their cost to governments.[2] Their incidence is therefore of growing importance.

Economic theory suggests that the balance of incidence between tenants and landlords depends on the details of the rental market. For example, in a competitive market the key factors are the relative magnitudes of the supply and demand elasticities for rented accommodation: the lower is the supply elasticity relative to the demand elasticity, the greater is the share of the subsidy that will be incident on landlords.[3]

The conclusions of a small empirical literature have consistently suggested that a large portion (often the majority) of housing subsidies are incident on landlords. This paper comes to different conclusions. We use a previously unexploited administrative monthly panel dataset on the universe of housing benefit claimants in Great Britain, and we use a natural experiment provided by a substantial package of cuts to housing benefit that was rolled out during 2011 and 2012 to estimate the impact of these cuts on the quality-adjusted rents paid by subsidy recipients. The phased nature of the roll-out makes the reform an ideal natural experiment to study, as it allows us to estimate separately the impacts on the rents of new claimants and existing claimants, using a different set of identifying assumptions in each case.

We find that the large majority of the incidence of these cuts to housing subsidies reforms was on tenants. Our estimates for both new claimants and existing claimants are extremely similar, and indicate that the rents of subsidy recipients, conditional on property characteristics, were affected little on average (with the average effect not being statistically significantly

[1] US figures from Collinson and Ganong (2014). GB figures from Department for Work and Pensions (https://www.gov.uk/government/statistics/benefit-expenditure-and-caseload-tables-2015). In addition to the housing benefit programme, the UK government subsidises tenants in public housing through a combination of sub-market rents and housing benefit rebates.

[2] The evolution of the US system is described in Susin (2002). Hills (2007) provides a detailed account of the UK case.

[3] An alternative framework for thinking about the rental market is through a search model with negotiation between tenants and landlords. Gibbons and Manning (2003) give an overview of the relevant theory for that case.

different from zero). As a result, about 90% of the reduced housing benefit entitlements were incident on the tenants rather than their landlords. This is different to the consensus from the literature to date, but there are good reasons to believe that the internal validity of our analysis is high. We also uncover significant heterogeneity in the balance of incidence between tenants and their landlords. We argue that this provides the most plausible explanation for why our findings seem at odds with those of previous work. We find that the incidence on tenants who were previously subsidised to rent some of their area's highest-rent properties was much higher than average. This subgroup is likely to be more similar to the group affected by previous UK reforms analysed in Gibbons and Manning (2006), which is the most similar previous paper to ours. Our results suggest that the incidence of housing subsidy regimes varies substantially within the range of real-world rental markets, with potentially important consequences for their optimal design.

The basis of our estimates is a package of cuts to UK housing benefit. From April 2011, new housing benefit claims began to be assessed under the reformed (less generous) rules. We estimate effects on the rents of new claimants, controlling for property characteristics, by assuming that an extrapolation of a pre-April 2011 time trend in rents for new claimants provides a valid counterfactual. Existing claimants (i.e. those whose claim began before April 2011) were gradually rolled on to the reformed system across the course of 2012, in 12 separate cohorts defined (for most) by the calendar month in which their claim began.[4] We use a difference-in-differences design to estimate effects on rents for existing claimants, effectively using those not yet rolled onto the reformed system as a control group at each point in time. The staggered roll-out also means that we are able to allow for anticipation effects and for gradual adjustment, tracing the reforms' impacts between about one year before and one year after individual claimants started to be assessed under the new rules.

Our two sets of estimates – for both new and existing claimants – rely on different assumptions. Our estimates for new claimants do not rely on a control group and are therefore robust to any general equilibrium effects of the reforms. On the other hand, without a control group we rely on extrapolating a pre-reform time trend whilst being careful to avoid bias arising from anticipation effects. Our estimates for existing claimants effectively use claimants not yet rolled onto the reformed system as a control group at each point in time. This group is defined only by the calendar month in which their housing benefit claim began, so is highly comparable to the group already being treated. This has obvious advantages, but also carries risks, as control groups and treated groups this similar are clearly participants in the same housing market. A similar concern could therefore apply to that expressed by Gibbons and Manning (2006). They used a control group (in their case, existing claimants) when looking at rent effects of previous reforms on new claimants and noted: "our methods would seriously underestimate the impact of the reforms if the rental housing market is perfectly competitive and frictionless, when any reduction in

[4] The precise month in which claimants were rolled onto the new system was also determined by the last time they moved house, or saw a change in their family composition, but is closely correlated with the calendar month in which claims began. This is discussed in more detail in the data appendix.

benefit payments to one claimant group drives down rents throughout the rental sector". In reality, the rental market is not characterised by spot prices. Tenants' rents typically change at no more than annual frequency (and sometimes cannot be changed within-year) and there is evidence of limited awareness of the reforms in question on the part of both tenants and landlords before entitlements were reduced. In addition, we allow for anticipation effects that occur in the year before tenants are rolled onto the new system, and are related to their roll-over date rather than calendar time. Nevertheless, the fact that we can complement this analysis with estimates of effects on new claimants, which rely on a different set of assumptions, provides a useful additional source of verification.

As already mentioned, we add to evidence provided by a number of previous studies. For the United States, Susin (2002) effectively compared rent trends between areas where housing voucher supply has been expanded to different degrees.[5] He estimated that the existence of the voucher system had increased the rents of unsubsidised low-income households by 16%. More recently, Collinson and Ganong (2014) exploited a plausibly exogenous change in county-level price ceilings for housing vouchers (caused by incorporating new Census information in the allocation formula) in 2005. They concluded that the primary impact of raising the ceiling is to increase rents, rather than increase the housing quality of recipients. For France, Fack (2006) studied an extension of the housing benefit system to low-income households without children in the early 1990s. Using slightly higher-income households as a control group, difference-in-differences estimates suggested that the reform increased the rents of the treated group by 78 cents for each euro of housing benefit.[6] For the UK, Gibbons and Manning (2006) looked at a cut to housing benefit which applied to new claimants in the mid-1990s. They effectively compared the rent levels of new claimants and existing claimants after controlling for tenure length effects. They estimated that most (60% to two-thirds) of the cut was incident on landlords via reduced rents. Viren (2013) used Finnish panel data on rented properties and, having assumed that variation in trends in maximum allowable subsidies across areas is exogenous, estimated that one-third to one-half of a Finnish housing subsidy is incident on landlords.

The rest of the paper proceeds as follows. Section 2 provides more detail on the institutional background and the reforms to housing benefit that form the basis of our estimates. Section 3 describes the data we use and our empirical strategy. Section 4 presents our results. Section 5 discusses these results, while Section 6 concludes.

## II. Policy background

Housing benefit is a large and growing part of social security spending in the UK. Any renter with sufficiently low income and financial assets is entitled to it. Its total cost to government therefore depends on the number of

[5] Olsen (2003) provides a brief discussion of the potential limitations of Susin's approach. Eriksen and Ross (2014) look at the impact of voucher expansion and find no statistically significant impact on average rents, though with some differences between submarkets.

[6] Laferrère and Le Blanc (2004) look at the same reform, and also find a significant effect of housing subsidies on rents.

individuals with qualifying characteristics who claim it and their average level of entitlement (rather than being set directly, as is the case with the Housing Choice Voucher Program in the US).

In 2015–16, spending on housing benefit is projected to be £24.5 billion: 12% of all government spending on cash transfers. £9.1 billion of that total is spent on rent subsidies for recipients in the private rented sector (the focus of this paper), with the remainder spent on tenants in public housing who are also subsidised directly through sub-market rent.[7] Spending on housing benefit for private renters increased by 136% in real terms between 2000–01 and 2010–11, thanks to a 94% increase in the caseload and a 22% increase in average entitlements during a period of rising real rents.[8] Since then, real expenditure has been roughly flat: further growth in the number of claimants has been offset by the impact of the reforms analysed in this paper, which cut the generosity of entitlements. These cuts were part of a wider post-recession fiscal consolidation implemented by the UK government in an attempt to reduce a large structural budget deficit. They accounted for about 12% of the cuts to social security and 1.7% of the whole consolidation package between 2010–11 and 2015–16.[9]

For claimants who rent from a private landlord and whose claim began in April 2008 or later, housing benefit entitlement is a function of actual rent and a cap known as the Local Housing Allowance (LHA) rate.[10] For a claimant with no private income or assets who lives with no more than a partner plus any dependent children,[11] the function under the pre-reform system was:

$$HB = \min\,(LHA\,rate, rent + £15\,per\,week)$$

The LHA rate varies geographically[12], and by the claimant's family type; the variation by family type arises through a set of rules (set out in Appendix A) that maps a claimant's family type to a 'reasonable' accommodation size (ranging from a room in a shared property to a five bedroom property). Before the reforms analysed in this paper, LHA rates were set equal to the median of private sector rents among properties of a similar type and in the same geographical area not being rented by HB recipients. As a result, the LHA rate that applied to a particular claimant was set at a level sufficient to fully subsidise them in the median property rented by non-subsidy recipients in their area of the size deemed appropriate for their family circumstances. If they rented a cheaper property than that, then claimants could effectively keep the first £15 a week of the difference.

[7] Department for Work and Pensions (2015).

[8] Department for Work and Pensions (2015).

[9] Browne and Hood (2015).

[10] Claims that began before April 2008 are not assessed under the LHA rules, were not affected by the reforms studied here and are ignored in the rest of the paper.

[11] For claimants living with an adult other than their partner, 'non-dependent deductions' (NDDs) are subtracted from 'rent' in the formula. In addition all housing benefit claims are subject to a means test. This withdraws entitlement at a rate of 65p for each £1 by which income, after direct tax, exceeds a threshold that varies by family type. The system of NDDs and the rules of the means test were unaffected by the set of reforms studied here, so we abstract from them throughout and focus simply on 'maximum' (pre-means test) entitlements ignoring the impacts of any NDDs.

[12] The relevant areas are known as Broad Rental Market Areas, which are deemed to represent self-contained housing markets. There are 192 BRMAs in Great Britain, and a further 8 in Northern Ireland.

The reform package studied in this paper had several elements. Onwe element removed the weekly ‘excess’ of up to £15 that claimants could keep if their rent was less than their applicable LHA rate, so that the function became

$$HB = \min(LHA\, rate, rent)$$

The other elements of the reform package affected the calculation of claimants’ applicable LHA rates. These changes were:

- setting LHA rates at the 30th percentile of local private sector rents among non-HB recipients (for the relevant property type) rather than at the median;
- abolishing the 5-bedroom rates, so that large families previously entitled to this became entitled only to the 4-bedroom rate;
- capping the rates at £250, £250, £290, £340 and £400 per week for the shared accommodation, 1-bedroom, 2-bedroom, 3-bedroom and 4-bedroom rates respectively (reducing rates below the 30$^{th}$ percentile of local rents in the highest-rent areas, which in practice means parts of inner London);
- reducing the entitlement of most single adults without dependent children aged 25-34 to the amount for a room in a shared property (known as the Shared Accommodation Rate or SAR), rather than the rate for a 1-bedroom property.

The switch to the 30$^{th}$ percentile and the removal of the excess affected a wide group of claimants. The other changes affected only small subgroups. In our empirical analysis we look separately at those subgroups.

The removal of the £15 excess applied to new claimants from April 2011, and to existing claimants on their first annual claim anniversary after April 2011 (i.e. at some point between April 2011 and March 2012). The changes to the calculation of LHA rates applied to new claimants from April 2011 (at the same time as the excess removal); typically, they applied to existing claimants nine months after their first annual claim anniversary after April 2011 (i.e. nine months after the excess removal, at some point between January and December 2012).

The changes that reduced the value of the LHA rate lower the cap on housing benefit payments. They therefore give claimants an incentive to seek cheaper properties or to pay less for a given property, and the empirical issue that we explore in Sections III and IV is how much of the incidence then falls on landlords. The removal of the £15 excess has different effects on incentives. The pre-reform system (which allowed claimants to keep £15 of any difference between their actual rent and their LHA rate) gave claimants an incentive to keep rent up to £15 below their LHA rate, either by choosing cheaper accommodation or by negotiating with landlords. Removing the excess means that claimants no longer have this incentive so, if they change their behaviour in response, we would expect them to choose more expensive types of accommodation or to accept a higher rent for a given property. Hence this change could effectively transfer the excess from tenants to landlords, rather than from either group to the taxpayer. There is no plausible mechanism by which it could lead to lower rents.

## III. Data and methods

Our analysis uses administrative panel data from the Single Housing Benefit Extract (SHBE). This is made up of returns submitted to central government each month by local authorities (LAs) in Great Britain. Hence our analysis excludes Northern Ireland, although it was also affected by the reforms in question. SHBE contains monthly information on the universe of live housing benefit claims, including the contractual level of rent and characteristics of the claimants.[13] This is the first paper to make use of these microdata. Details of the construction of key variables, including data cleaning, are given in Appendix B.

*New claimants*

Our first set of empirical estimates are for new claimants. The reformed, less generous housing benefit system was applied in full to new claims starting in April 2011 or later.

We sample the first observation in the panel for each new HB claim assessed under the LHA rules, meaning that we use information only on the circumstances that applied when the claim began. Although each LA submits a scan of its records on one day per month[14], the exact start date of the claim is part of each record. We therefore essentially observe new claims, and the characteristics of those claims, in continuous time.

Table 1 describes the family types and ages of new claimants assessed under the LHA rules, separately for periods shortly before and after the reforms took effect. Claimants are disproportionately likely to be single (about 80%) and to be single parents (about 25%), compared to the GB population as a whole. As housing benefit is means-tested, this is unsurprising. They are also a young group. Older individuals are much more likely to be owner-occupiers or in social housing than in the private rented sector; and, if private renters, they are less likely to be starting a new housing benefit claim.

Figure 1 shows a seven-day moving average of housing benefit entitlements for new claims made between June 2010 and November 2011 inclusive. The average claim was for about £110 per week. Figure 2 shows the same trend for the rents of new claimants, which averaged just above £120 per week. Perhaps the most striking feature of both figures is the large spike in mean entitlements and mean rents (as well as the rise in the number

[13] Actual rents paid might differ from contractual rents, either because tenants are in arrears or because landlords 'informally' accept a rent that differs from the contractual one. We are not able to tell whether the prevalence of these phenomena was affected by the reforms. Some qualitative evidence suggests that rent arrears did increase after the reforms (Beatty et al, 2014). The ultimate implication of this for the incidence of the reforms is not clear, as it depends whether or not these arrears are eventually settled. It is also not clear a priori whether survey-based measures of rents, as used in some other studies (including Gibbons and Manning (2006)), would be more likely to pick up actual rents paid where this differs from the contractual rent.

[14] In any month, this day can be different for different LAs. Furthermore, some LAs sometimes fail to submit records in a particular month. All regressions include LA dummies so that this should not induce any bias in our estimates.

of claims per week[15]) just before the reformed system starts to apply to new claimants on 1st April 2011. This increased volume of claims is consistent with the financial incentives created by their roll-out: a claimant starting a new claim just after 1st April would immediately face the reformed, less generous, housing benefit system, whereas a claimant starting a new claim just before 1st April would not face this system in full for another 21 months. Furthermore, we would expect the resulting distortion to the timing of claims to be largest for those making large claims, who tended to have more to lose from the reforms. This is what we see in the data. For example, the proportion of new claims occurring in London rose by three percentage points between January and March 2011, from 14.3% to 17.3%. The same proportion did not fluctuate by more than one percentage point over any other two-month period in these data. Similarly, average household size – as measured by individuals per household – rose from 1.86 to 1.95 between January and March 2011, also a larger fluctuation than over any other two-month period in the data. The grey line on Figure 2 plots mean residuals from a regression of rent on an area indicator (Broad Rental Market Area) and the number of bedrooms – both are strong correlates of rents and hence the size of a housing benefit claim. The spike in raw rents is largely (though not entirely) explained by these factors.

Because of these shifts in the timing of new claims, we exclude a window of data around the reform time from our analysis. There is inevitably a tradeoff: excluding more data is more likely to purge these short run timing responses, but it incurs a loss of sample size and means that estimates of time trends have to be extrapolated further. Since the sample size is very large and the time trends in our outcomes of interest look uncomplicated, we take a conservative approach and exclude all new claims made between 1st December 2010 and 31st May 2011. The excluded window is marked with vertical lines on Figures 1 and 2. We have conducted sensitivity analysis and, as the figures would suggest, our estimates are robust to small shifts in the window of data excluded.

Having removed the period around the introduction of the reforms in April 2011, time trends in entitlements and rents for new claims look straightforward: linear, and approximately flat. Housing benefit entitlements clearly settle at a lower level post-reform than they had generally been pre-reform; for rents, though, there is little difference. This is indicative of the main result on incidence that we obtain more formally in the next section.

We estimate impacts of the housing benefit cuts on new claimants using the following specification:

$$y_{iat} = f_a(t) + 1(t \geq April\ 2011)\beta + x'_{iat}\alpha + \varepsilon_{iat} \qquad (1)$$

Individuals, areas (specifically BRMAs) and time are indexed by i, a and t respectively. y is either rent, housing benefit entitlement or the difference between the two (in £s per week). All standard errors allow for heteroskedasticity and for errors clustered at the BRMA level.

The first term on the right hand side is a secular time trend. Identification of the effects of the reforms on new claimants rests crucially on

[15] See Figure 3.3 and surrounding text of Beatty et al (2013).

distinguishing them from this trend. Based on the descriptive analysis in Figure 1 and 2 we use a linear trend. This is allowed to vary between the pre- and post- reform periods and to be BRMA-specific.[16] The second term captures the 'treatment' effect: an indicator variable for whether the claim started on or after the reform happened in April 2011. $\beta$ is the coefficient of interest.

x is a vector of control variables. It includes dummies for the full set of interactions between BRMA and number of bedrooms in the property (which we have top-coded at 5). This is so that we do not confound changes in property choices with changes in the unit-price of accommodation. Local area and property size are strongly correlated with rents; to the extent that claimants adjust along other (unobserved) margins of property characteristics, we will pick this up as a price change rather than a quality change and will therefore over estimate the incidence on landlords and underestimate the incidence on tenants. In other words, any bias arising for this reason would mean that an even larger majority of the incidence of these cuts was on tenants than our estimates suggest. We also control for family type and age, as these may change over time for reasons unrelated to the reform in ways that are not adequately captured by our time trends; as shown in the next section, these demographic controls make a negligible difference to our estimates.

TABLE 1

*Demographic characteristics of new claimants*

| *Characteristic* | *June 2010 to November 2010 (% of claimants)* | *June 2011 to November 2011 (% of claimants)* |
|---|---|---|
| **Family type** | | |
| Single man | 35.7 | 34.5 |
| Single woman | 18.8 | 18.7 |
| Couples without children | 7.4 | 7.6 |
| Single parents | 24.3 | 24.7 |
| Couples with children | 13.7 | 14.7 |
| | | |
| **Age** | | |
| Under 25 | 23.2 | 22.6 |
| 25-34 | 32.8 | 33.1 |
| 35-44 | 22.8 | 22.8 |
| 45-54 | 13.0 | 13.4 |
| 55-64 | 5.6 | 5.6 |
| 65 and above | 2.6 | 2.7 |
| | | |
| N | 336,486 | 334,093 |

[16] The specification has the features of a regression discontinuity design, with time as the running variable. However, the exclusion of a window of data around the discontinuity (i.e. the date of the reform) perhaps makes this better thought of as a before-after analysis, with allowance for time trends.

FIGURE 1

*Average housing benefit entitlement of new claimants by date of claim (seven-day moving average)*

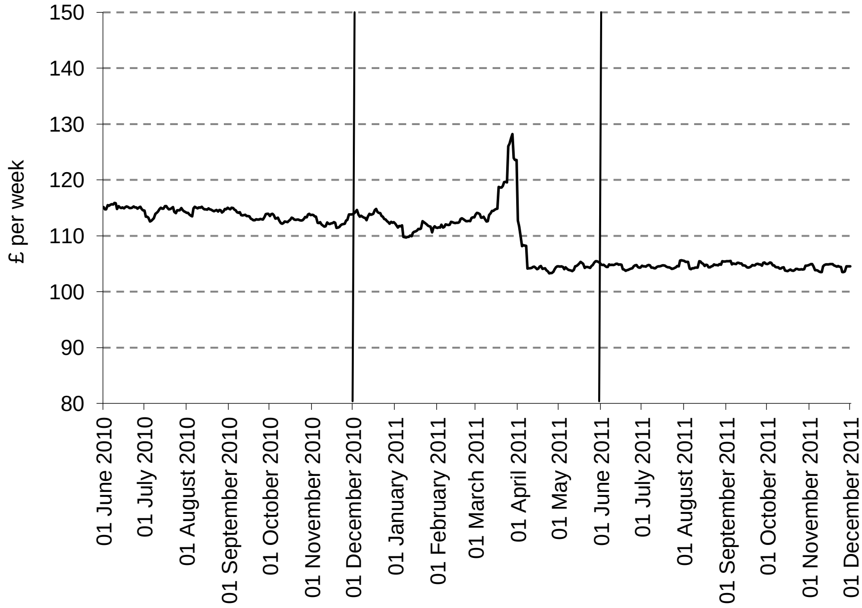
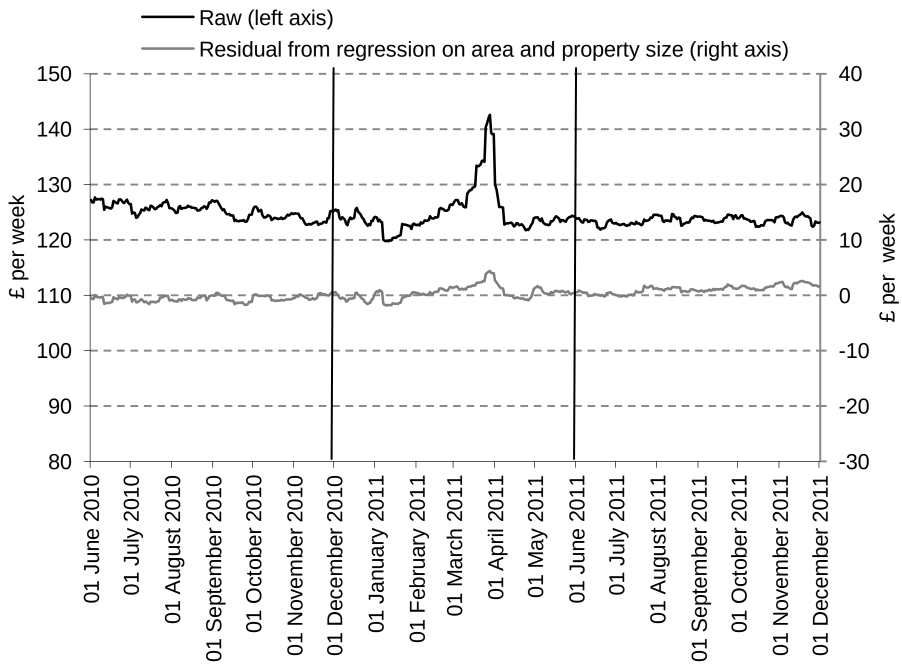



FIGURE 2

*Average rent of new claimants by date of claim (seven-day moving average)*



*Existing claimants*

Our second set of empirical estimates is for existing claimants. We focus on individuals receiving HB assessed under the LHA rules in January 2011, shortly before the reforms were implemented. We use monthly observations for these claimants between January 2010 and November 2013 inclusive.[17]

For computational reasons, a random one-in-three subset of claimants was taken for most of the regression analysis in Section IV. After dropping a further 15% of the sample because they are missing important information, this leaves us with an estimating sample of 239,723 claimants, observed 28 times on average. Table 2 shows the basic demographic characteristics of January 2011 claimants, both for the universe and for our final estimation sample. The two groups are almost identical in terms of these key characteristics.

TABLE 2

*Demographic characteristics of existing claimants in January 2011*

| *Characteristic* | *Full SHBE sample (% of claimants)* | *Estimation sample (% of claimants)* |
|---|---|---|
| **Household type** | | |
| Single man | 28.9 | 29.2 |
| Single woman | 15.6 | 15.6 |
| Couples without children | 6.4 | 6.3 |
| Single parents | 32.7 | 32.4 |
| Couples with children | 16.4 | 16.3 |
| | | |
| **Age** | | |
| Under 25 | 16.1 | 16.5 |
| 25-34 | 31.6 | 31.8 |
| 35-44 | 25.2 | 25.2 |
| 45-59 | 19.0 | 18.9 |
| 60 and above | 8.2 | 7.6 |
| | | |
| N | 850,249 | 239,723 |

Existing claimants were affected by the removal of the £15 excess on their first annual claim anniversary[18] from April 2011. They were typically affected by the other elements of the reform package nine months after that. More precisely, the nine-month interval was a period of ‘transitional protection’ from the reforms (other than the removal of the excess). Protection expired immediately if a claimant had a change of circumstance which triggered a claim reassessment within those nine months, such as a change in family type or a move to another area. However, the probability of such a change could itself be affected by the reforms. Hence, to avoid

[17] The date of observation within a month can differ across claimants according to the local authority that they live in.

[18] Where there had been a claim ‘reassessment’, the relevant anniversary is the anniversary of the most recent reassessment rather than the anniversary of the start date of the claim. For ease of exposition, the rest of the discussion abstracts from this and just refers to ‘claim anniversaries’.

endogeneity, we define the timing of the roll-out for a particular claimant based on what it would have been without any changes in circumstances from April 2011 onwards.

Figure 3 illustrates the nature of the roll-out. It tracks the covariate-adjusted average weekly housing benefit entitlements for existing claimants by calendar month.[19] The "All" line shows entitlements gradually declining between April 2011 and December 2012, when claimants were migrating to the new system. This gradual decline across all existing claimants is the result of successive cohorts being sequentially rolled onto the new system and seeing sudden reductions. Two example cohorts are shown in the figure. The August cohort (those whose claim anniversary fell in the month of August) saw entitlements fall in August 2011, when they lost any excess. They then saw a further fall nine months later in May 2012, as transitional protection expired and they were affected by the rest of the reform package. The same pattern holds for the November cohort but with the declines occurring three months later.

FIGURE 3

*Average maximum entitlement of existing claimants by month*

*(Residual from regression of entitlements on BRMA and number of bedrooms, £pw)*

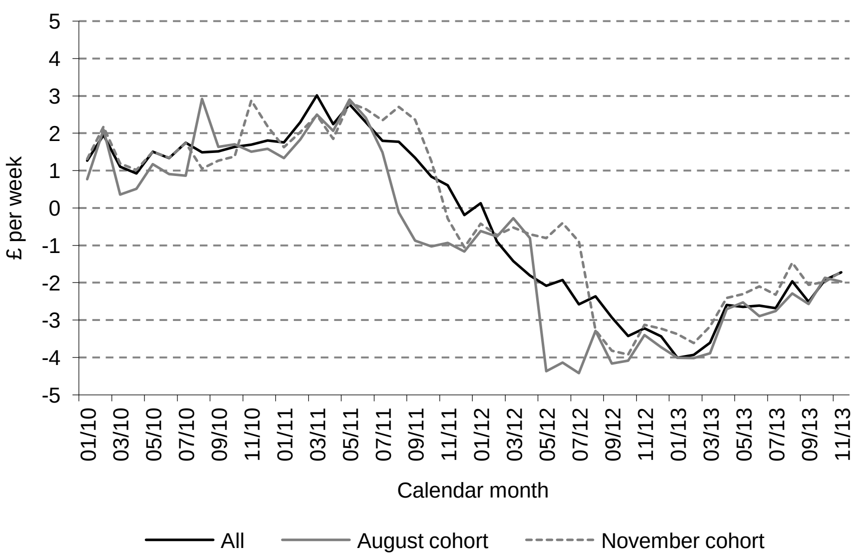



Figure 4 pools all cohorts and graphs their entitlements not by calendar month, but by the number of months since being rolled onto the new system (which differs across cohorts at any point in calendar time). We define month "0" as that in which claimants are rolled fully onto the new system – the month in which transitional protection expires; month "-9" is therefore the point at which any excesses are removed. The figure highlights the

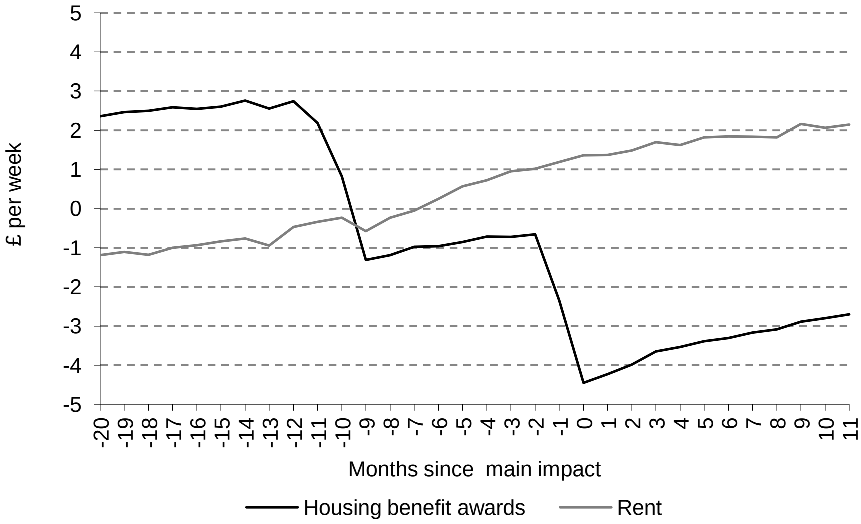
[19] Specifically, we plot residuals from a regression of LHA entitlements on all interactions between BRMA and number of bedrooms. This accounts for the fact that, in a given month, some local authorities fail to submit scans of their housing benefit records. Stripping out fixed area effects therefore results in a smoother series from month to month. It also means we are closer to isolating 'quality-adjusted' entitlements, which is of most interest when thinking about incidence and is what we do in our regressions in the next section.

discrete points of impact for specific cohorts. It also shows the same series for rents. It is apparent from this that little of the reduction in housing benefit entitlements seems to have been reflected in rental values, i.e. the incidence seems to have been largely on tenants.

FIGURE 4

*Average maximum entitlements and rents of existing claimants by months since main impact*

*(Residual from regression on BRMA and number of bedrooms, £pw)*




Our analysis exploits the between-cohort variation in when the reforms took effect that means that otherwise-identical individuals observed at the same point in time face different housing benefit systems if their claim anniversary fell in a different calendar month. We therefore apply a difference-in-differences (DiD) design to estimate impacts of the reform, and the incidence of the cuts, for existing claimants. The specification is

$$y_{iact} = f_a(t) + \pi_c + z'_{ct}\beta + x'_{iact}\alpha + \varepsilon_{iact} \qquad (2)$$

We now use repeated observations of the same claimants, so that each individual i is observed at multiple values of t. Compared to equation (1), we also have an additional ‘c’ subscript, denoting cohorts of individuals whose claim anniversary falls within the same calendar month (i.e. c can take 12 values).

The time trend, $f_a(t)$, contains a full set of monthly dummy variables. For additional flexibility we also allow each BRMA to have its own underlying linear trend (buffeted by the national-level monthly shocks). $\pi_c$ are cohort (i.e. month-of-claim-anniversary) fixed effects.

z is a vector of dummies denoting numbers of months before or after the claimant was rolled onto the new system. In this way, we allow the reform’s impacts to evolve as claimants spend longer under the reformed system, and as they approach the point of transition (i.e. anticipation effects). The dummies run from twelve months before the point of full transition onto the reformed system (three months before the loss of excess) to eleven months after that transition. These variables vary at the cohort-time level. $\beta$ is the coefficient vector of interest. x includes a similar set of demographic and property type controls as in the specification for new claimants (listed in the notes to Tables 1 and 2).

The familiar DiD assumption is that, in the absence of reform, trends would have been the same across different cohorts of claimants. It seems reasonable to assume that there are not systematic differences between claimants whose claim anniversary falls in a different calendar month. The inclusion of the cohort fixed effects has negligible impacts on our estimates, which increases confidence that these groups are essentially the same.

A further assumption required for identification in a DiD design is that the treatment does not affect the outcomes of the control group. This might be compromised by general equilibrium effects. Indeed, precisely because the cohorts are so similar, this may be the more serious threat to identification. Different cohorts are certainly part of the same rental market. Without any frictions and with perfect competition, there would be a single rental price at all times – all cohorts' rents would change in the same way, regardless of whether they have yet been rolled onto the reformed system. Hence, we would expect to estimate zero impacts of the reforms on rents using our specification, regardless of the true impact. More generally, general equilibrium effects would attenuate our estimates of the incidence of the reforms on the landlords of existing claimants.

In reality, the rental market is of course not characterised by spot prices. Tenants' rents typically change at no more than annual frequency (and sometimes cannot be changed within-year). There is evidence of limited awareness of the reforms in question, on the part of both tenants and landlords, before entitlements were reduced (Beatty et al, 2013), which suggests that adjustments were more likely to take place once claims were actually being assessed under the new system. It is also important to stress that we will explicitly estimate any adjustments that happen before entitlements are reduced, if their timing relates to claimants' roll-over dates rather than calendar time (as long as they occur in the year before tenants are rolled onto the fully reformed system).

It is also the case that the first set of estimates obtained for new claimants are a useful additional robustness check on these results, as no assumption about general equilibrium effects is required for identification in that case.

## IV. Results

### *Estimates for new claimants*

Table 3 shows the estimated effects of the reforms for new housing benefit claims from equation (1). The units of coefficients are pounds per week. Standard errors, which are robust to clustering at the BRMA level and heteroskedasticity, are in parentheses.

The different columns build up to our preferred specification by gradually adding regressors. Column 1 shows the results from regressions of housing benefit, rent, and the difference between the two, on a dummy variable indicating whether or not the new claim began on or after 1st April 2011 (when the reforms applied to new claimants). As one would expect given Figures 1 and 2, this shows that housing benefit entitlements were significantly lower in the post-reform period, and that rents changed much less. Columns 2 and 3 add controls for housing quality (local area, number of bedrooms and their interaction), with the results suggesting that the small fall in rents unadjusted for quality in the post-reform period can be explained by claimants moving to cheaper areas and renting smaller houses in that period. Column 4 adds the time trends discussed above to the model. This can be viewed as the first reasonable estimate of the effects of the reform on the price of rental accommodation, having controlled for property characteristics and underlying time trends. It suggests that: the reforms had a very small (and not statistically significant) downwards effect on rents.

Our preferred specification (column 5) adds controls for family type and age, on the basis that they might be changing over time for reasons unrelated to the reforms. Under this specification, we estimate that the reforms reduced housing benefit awards by an estimated average of £8.20 per week for new claimants. Almost all of this – an estimated £7.80, or 95% – was incident on the tenants. The point estimate suggests that rent fell slightly due to the reforms, but this is not statistically significantly different from zero – and hence, neither is the proportion of the cut to housing benefit that was incident on landlords. We obtain similar results for a wide of demographic subgroups (including age, family type and region).[20]

TABLE 3

*Estimated impact of cuts to housing benefit on new claimants*

| | Model | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Housing benefit | -9.28*** | -7.73*** | -6.36*** | -7.87*** | -8.21*** |
| | (1.18) | (0.63) | (0.48) | (0.52) | (0.50) |
| Rent | -1.57 | 0.12 | 1.62*** | -0.21 | -0.46 |
| | (1.11) | (0.52) | (0.43) | (0.66) | (0.64) |
| Rent net of HB | -7.71*** | 7.85*** | 7.97*** | 7.66*** | 7.76*** |
| | (0.34) | (0.35) | (0.35) | (0.49) | (0.49) |
| N | 667,278 | 667,278 | 662,764 | 662,764 | 659,892 |

*Note:* *** Statistically significant at 1% level, ** Statistically significant at 5% level, * Statistically significant at 10% level. Standard errors in brackets are robust to heteroskedasticity and clustering at the BRMA level. Figures given in UK pounds per week. Model (1) contains only a post-reform dummy variable; model (2) adds controls for BRMA and LA; model (3) adds controls for the number of bedrooms in the property (shared accommodation, 1 bedroom, 2 bedrooms, 3 bedrooms, 4 bedrooms, 5 or more bedrooms), and interaction terms that capture all possible combinations of number of bedrooms and BRMA; model (4) adds linear time trends for each BRMA, which are allowed to differ before and after the reform; and model (5) adds joint controls for family type and age. We define 40 mutually exclusive combinations of family type and age: families without children are split jointly by family type (single men, single women, couples) and age of claimant (under 25, 25-34, 35-44, 45-54, 55-64, 65 or more); families with dependent children are split jointly by whether lone parents or couple parents, age of claimant (under 25, 25-34, 35-44, 45 or more), and number of children (1 or 2 or more for under 25s, and 1, 2 or 3 or more for other ages).

[20] These are reported in full in the interim impact evaluation of these reforms carried out by some of the present authors (Beatty et. al., 2013).

*Estimates for existing claimants*

Table 4 presents analogous estimates for existing claimants, based on from the difference-in-differences design in equation (2). We now focus on our preferred specification (containing a similar set of controls to those used in model 5 above), and show three sets of coefficients, capturing impacts at three different stages: the point at which claimants reach their first annual claim anniversary after April 2011, at which any excess they had was removed; the point nine months later when they were fully rolled onto the new system and subject to all other elements of the reform package; and eleven months after that, which is the latest point in our data at which we observe all cohorts.

Existing claimants lost an average of about £5 per week as a result of the removal of excesses, with around 40% of claimants affected. At this point their rents were also reduced by about £0.80 per week, which is statistically significant. As discussed in Section II, we would not expect landlords to lose out from the excess removal in isolation, as there is no reason why this should result in lower rents (and it might increase them). However, given the likely rigidity of rents mid-contract and the fact that tenancy agreements tend to last for at least one year, it would not be surprising if there were some adjustment to the other (impending) reforms at this stage. Indeed for tenants whose claim anniversary coincides with the anniversary of their tenancy, this would be the last opportunity to change rents before the reforms take effect (unless they renegotiate mid-tenancy).

The loss of housing benefit rose to £8.30 per week nine months later at the point when the rest of the reforms took effect. There was essentially no further impact on rents, meaning that about £7.60 per week (90%) of the cuts were incident on tenants at this point. One might expect this to be partly due to short-run rent rigidity. However, the proportion of the cut estimated to be incident on tenants remained very stable, at around 90%, eleven months later. Again, we obtain similar results for a wide range of demographic subgroups.[21]

It is important to recall that all these estimates control for local area and number of bedrooms. If tenants adjust to the reforms partly by choosing different kinds of properties in ways not captured by these controls, then that would be an additional mechanism by which the reforms were incident on tenants. The inclusion of local area and number of bedrooms in the regression makes little difference to our estimates (as shown in Appendix C). This suggests that, on average, there was little adjustment by tenants along those margins and that a reduction in housing benefit in given property types was the main route by which they were made worse off.

[21] Brewer et. al, 2014, reports full results for a variety of sub-groups.

TABLE 4

*Estimated impact of cuts to housing benefit on existing claimants*

| | Housing benefit | Rent | Rent net of HB |
|---|---|---|---|
| Loss of excess | -4.98*** | -0.81*** | 4.17*** |
| | (0.42) | (0.27) | (0.34) |
| Point of main impact | -8.31*** | -0.73 | 7.58*** |
| | (1.01) | (0.68) | (0.85) |
| 11 months after main impact | -6.84*** | -0.79 | 6.06*** |
| | (0.92) | (1.09) | (0.83) |

*Note:* *** Statistically significant at 1% level, ** Statistically significant at 5% level, * Statistically significant at 10% level. Standard errors in brackets are robust to heteroskedasticity and clustering at the BRMA level. Figures given in UK pounds per week. Includes controls for BRMA, local authority, number of bedrooms in the property, local area deprivation, 'cohort', calendar month, linear time trends in each BRMA, and joint controls for family type and age: families without children are split jointly by family type (single men, single women, couples) and age of claimant (under 25, 25-34, 35-44, 45-59, 60 or more); families with dependent children are split jointly by whether lone parents or couple parents, age of claimant (under 25, 25-34, 35-44, 45 or more), and number of children (1 or 2 or more for under 25s, and 1, 2 or 3 or more for other ages). N = 238,782.

### *Groups affected by specific elements of the reform package*

An advantage of the analysis of existing claimants is that we observe their circumstances before the reforms took effect. We can therefore investigate heterogeneity in the reforms' impacts across exogenously-defined groups of claimants. Here we look at claimants with pre-reform characteristics such that they would have been affected by specific elements of the reform package had their circumstances remained the same. We find substantial heterogeneity between these subgroups and the population as a whole, and argue that this may be informative of why the results presented in the previous section differ from those elsewhere in the literature.

One subgroup for which one might expect the incidence to be different is those who had the full £15 excess in January 2011 (ie. their rent was at least £15 lower than their LHA rate). We identify this group according to their circumstances in January 2011 because they might have changed their circumstances in response to the reforms, meaning that the composition of the group at a later stage would be endogenous. As discussed in section 2, there is no reason to expect the removal of the excess to reduce rents (and it might increase them). All else equal, this would suggest that tenants in this group should bear a greater share of the incidence of the reform package, relative to their landlords, than other tenants. However, one might also expect this group to be selected on important unobserved characteristics, such as preferences between housing and other consumption, and their willingness and ability to negotiate – by definition, this is a group paying relatively low rents.

Table 5 shows our estimates for the incidence of the reform package as a whole on those who stood to lose the full £15 excess. At the point when claimants lost their excess, the falls in both housing benefit and rents were slightly larger for this group, meaning that the share of the cuts to housing benefit incident on the tenants was similar for this group as for the population as a whole. Estimates for later periods, when the other elements of the reform package applied, show that if anything a smaller share (but still a comfortable majority) of the cuts were incident on the tenants than in the claimant population as a whole, with falls in rents that are statistically significant. This may indicate that this selected group of individuals were more responsive to the cuts than others (perhaps because their housing demand is more price elastic, or because they are more pro-active negotiators).

TABLE 5

*Estimated impact of cuts to housing benefit on existing claimants with a £15 excess in January 2011*

| | Housing benefit | Rent | Rent net of HB |
|---|---|---|---|
| Loss of excess | -12.44*** | -1.20*** | 11.24*** |
| | (0.52) | (0.24) | (0.37) |
| Point of main impact | -14.80*** | -3.13*** | 11.68*** |
| | (1.09) | (0.56) | (0.91) |
| 11 months after main impact | -14.31*** | -3.59*** | 10.72*** |
| | (0.80) | (0.79) | (0.79) |

*Note:* *** Statistically significant at 1% level, ** Statistically significant at 5% level, * Statistically significant at 10% level. Standard errors in brackets are robust to heteroskedasticity and clustering at the BRMA level. Figures given in UK pounds per week. Includes controls for BRMA, local authority, number of bedrooms in the property, local area deprivation, 'cohort', calendar month, linear time trends in each BRMA, and joint controls for family type and age: families without children are split jointly by family type (single men, single women, couples) and age of claimant (under 25, 25-34, 35-44, 45-59, 60 or more); families with dependent children are split jointly by whether lone parents or couple parents, age of claimant (under 25, 25-34, 35-44, 45 or more), and number of children (1 or 2 or more for under 25s, and 1, 2 or 3 or more for other ages). N = 134,246.

Table 6 shows separate estimates for three additional subgroups, again defined according to their January 2011 characteristics. For brevity we focus just on estimated impacts at 11 months after being fully rolled onto the reformed system. For each group, the first row (labelled 'quality-adjusted') shows estimates with controls for contemporaneous property characteristics analogous to those presented in Tables 3 and 4. The second row (labelled 'unadjusted') shows estimates without those controls (but with a control for initial BRMA, based on circumstances in January 2011).

The first group is single adults without dependent children living in self-contained (i.e. not shared) accommodation[22] who were due to be aged 25-34 at the point that the changes to the calculation of LHA rates took effect. Under the pre-reform system these people would have been able to claim the 1-bedroom rate. After the reforms they could claim only the shared accommodation rate (SAR). Conditional on property characteristics they lost an average of about £13 per week in housing benefit entitlement from the reforms; but we estimate that their rents fell by about £4.80 per week, implying that just over one third of the incidence was on their landlords. The estimates that do not adjust for property characteristics show larger falls in both housing benefit entitlements and rents, suggesting that some of the individuals affected responded by moving to cheaper properties – something we show directly below.

The second group is large families who were entitled to the 5-bedroom LHA rate in January 2011.[23] These families were therefore likely to be

[22] All of those living in shared accommodation are entitled only to the SAR, even if they would otherwise be entitled to a higher level of support.

[23] To be entitled to this rate, one would have needed a minimum of seven dependent children if all were aged under 16 or a minimum of four if all were 16 or over (in each case the necessary number of children could be greater than this, depending on their gender composition).

affected by the abolition of that rate, and to have to claim the 4-bedroom rate instead after the reforms. Conditional on property characteristics they lost an average of about £29 per week in housing benefit entitlement from the reforms; but we estimate that their rents fell by almost £12 per week, implying that about 40% of the incidence was on their landlords. Again, comparison with the estimates that do not adjust for property characteristics suggests that these claimants may also have responded by living in cheaper types of properties than they would otherwise have done.

The third group is claimants who were, in January 2011, living in one of five London BRMAs in which the overall national caps on LHA rates bind (i.e. in which those caps were below the 30th percentile of local private sector rents)[24] and whose housing benefit award exceeded the impending cap for their family type. These families therefore would (absent a change of circumstances) be affected by the national caps. Conditional on property characteristics they lost an average of about £42 per week in housing benefit entitlement from the reforms. We estimate that their quality-adjusted rents fell relatively little, though their raw rents fell more, suggesting some possible quality adjustments (but neither the adjusted nor unadjusted rent changed by a statistically significant amount).

Table 7 explores some of the likely behavioural responses of these subgroups, looking at whether these claimants moved to a different property in response to the reforms, and, if so, to what type of accommodation. For binary outcomes, a probit specification is used and marginal effects at the mean are reported.

The estimates suggest that, after 11 months under the new system, each of these subgroups was about 1 percentage point more likely to move house in that month as a result of the reforms.[25] The group affected by the extended coverage of the Shared Accommodation Rate were 13 percentage points more likely to be living in shared accommodation. There is also some evidence that those affected by the abolition of the 5-bedroom rate responded by renting fewer bedrooms, and that those affected by the national caps responded by moving out of the capped areas, although these effects are not statistically significant.

[24] These BRMAs are Central London, Inner North London, Inner East London, Inner West London and Inner South West London.

[25] The impact is stable from five months after being rolled onto the reformed system.

TABLE 6

*Estimated impact of cuts to housing benefit on existing claimants likely to be affected by certain elements of the reform package (11 months after main impact)*

| | | Housing benefit | Rent | Rent net of HB | N |
|---|---|---|---|---|---|
| Increased scope of shared accommodation rate | Quality-adjusted | -13.05*** (1.36) | -4.80*** (1.31) | 8.25*** (1.73) | 49,569 |
| | Unadjusted | -15.55*** (1.59) | -7.36*** (1.55) | 8.18*** (1.78) | 49,635 |
| Abolition of 5-bedroom LHA rate | Quality-adjusted | -29.21*** (8.49) | -11.69** (5.48) | 17.52*** (5.44) | 5,699 |
| | Unadjusted | -31.60*** (9.99) | -19.04** (9.27) | 12.56** (5.36) | 5,703 |
| National caps on LHA rates | Quality-adjusted | -41.93*** (9.96) | -5.68 (10.19) | 36.25*** (12.31) | 16,992 |
| | Unadjusted | -48.48*** (12.59) | -17.07 (14.20) | 31.41*** (12.13) | 16,992 |
| All existing claimants | Quality-adjusted | -6.84*** (0.92) | -0.79 (1.09) | 6.06*** (0.83) | 238,782 |
| | Unadjusted | -7.40*** (0.91) | -1.11 (1.12) | 6.28*** (0.84) | 239,279 |

*Note:* *** Statistically significant at 1% level, ** Statistically significant at 5% level, * Statistically significant at 10% level. Standard errors in brackets are robust to heteroskedasticity and clustering at the BRMA level. Figures given in UK pounds per week. "Adjusted" figures include controls for BRMA, local authority, number of bedrooms in the property, local area deprivation, 'cohort', calendar month, linear time trends in each BRMA, and family type and age."Unadjusted" figures do not include controls for contemporaneous BRMA, LA, number of bedrooms and local area deprivation, but do include controls for BRMA in January 2011.
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TABLE 7

*Estimated impact of cuts to maximum entitlements on property choices of existing claimants likely to be affected by certain elements of the reform (11 months after main impact)*

| | | | N |
|---|---|---|---|
| Increased scope of shared accommodation rate | Probability of moving (ppts per month) | 1.0*** (0.5) | 43,655 |
| | Probability of living in shared accommodation (ppts) | 17.0*** (0.4) | 43,564 |
| Abolition of 5-bedroom LHA rate | Probability of moving (ppts per month) | 0.6 (0.8) | 5,406 |
| | Number of bedrooms | -0.14 (0.16) | 5,700 |
| National caps on LHA rates | Probability of moving (ppts per month) | 0.8* (0.6) | 16,163 |
| | Probability of moving out of capped area (ppts per month) | 0.3 (0.5) | 16,163 |
| All existing claimants | Probability of moving (ppts per month) | 0.4*** (0.2) | 219,592 |

*Note:* *** Statistically significant at 1% level, ** Statistically significant at 5% level, * Statistically significant at 10% level. Standard errors given in brackets are robust to heteroskedasticity and clustering at the BRMA level. Figures given in UK pounds per week. Includes controls for BRMA in January 2011, calendar month, cohort, family type and age and rent and claim anniversaries.

## V. Discussion

Taken together, our results indicate significant heterogeneity in the incidence of recent cuts to UK housing benefit. On average, about 90% of the incidence was on tenants (and we cannot reject the possibility that 100% was on tenants). However, for two groups affected more than average by the changes, we estimate that less than two-thirds of the cut was incident on them (and we can reject the possibility that it was all incident on them).

Why is this? We cannot be conclusive, but differences in demand elasticities are one plausible possibility. All else equal we would expect the incidence on landlords to be higher when demand is more price elastic. It is likely that a sizeable fraction of 25-34 year-old single adults without children are relatively indifferent between living in self-contained or shared accommodation, and hence relatively sensitive to the relative price of these options. Indeed, we found that a significant number of individuals in this group *did* choose to move into shared accommodation as a result of the policy. The abolition of the 5-bedroom rate affects a group of families with large numbers of children who were in many cases fully subsidised to rent some of the largest and hence highest-rent properties in their area. This group might not be prepared to pay very much for an additional bedroom once they face (more of) the marginal cost. Again, we find some evidence that some members of this group moved to smaller accommodation as a result of the reforms, though note that the reduction in the average number of bedrooms that we estimate is not statistically significant.

These findings may help to explain why our main result – that the incidence of changes to housing subsidies were so heavily incident on tenants – is at odds with the findings of previous empirical work. The only other paper to look at the incidence of UK housing benefit is Gibbons and Manning (2006). They studied reforms in the mid-1990s that introduced caps on rents eligible for housing benefit, based on average market rents in the local area. The reforms studied here extended restrictions much further down the rent distribution (typically to the 30$^{th}$ percentile of local rents at a given number of bedrooms, and sometimes lower) and affected the large majority of claimants – rather than a relatively high-rent minority who might be able to substitute more easily towards cheaper accommodation. Where we do focus on a subgroup whose ability to live fully subsidised in some of their area's highest-rent properties was removed or restricted, our results on incidence are closer to those of Gibbons and Manning.

If the heterogeneity in the incidence of the cuts that we find is indeed explained by heterogeneity in demand elasticities, it would have important implications for the likely impacts of other reforms to housing subsidy regimes, and for the optimal design of such regimes. If, as cuts to subsidies bite further down the distributions of rent and housing quality, the average demand elasticity of the affected tenants falls, then less generous subsidies will tend to be proportionately more incident on tenants. Given a fixed level of total expenditure, a system with lower subsidies for a larger number of recipients would, in this scenario, lead to a larger share of the subsidy being incident on tenants than a system with higher subsidies for a smaller number of recipients.[26]

[26] Collinson, Ellen and Ludwig (2015) provide a helpful discussion of this trade-off in the US context.

An alternative explanation for our results might be that the groups for which a smaller proportion of the cut was incident on them (and a larger proportion on their landlords) were hit harder than average by the changes, and this simply made them quicker to notice and to make an effort to respond. If this were the key explanation, it would suggest that the small effects on rents for other claimants simply reflect short run rigidity. However, there are good reasons to doubt this. Previous studies of changes to rent subsidies, which have tended to find higher incidence on tenants than this paper, have also either typically looked either at near-contemporaneous responses of rents to subsidies (e.g. Viren, 2013) or have shown that effects on rents occurred quickly (Gibbons and Manning, 2013). In addition, our results show that tenants affected by the national caps lost by far the *most* housing benefit of the subgroups considered, and yet the estimated incidence on them is high (and we cannot reject that it was 100%). There seems no obvious reason why they should adjust more slowly as a group. Instead, we might expect that the explanation lies in the relative supply and demand elasticities. For example, the margin by which this group would most likely need to respond to reduce their rent substantially would be to move area (rather than to move further down the quality distribution within-area), which could be a relatively unattractive option.

## VI. Conclusion

During 2011 and 2012 the UK government reduced the generosity of the housing subsidy it provides to low-income private renters. This paper has estimated the incidence of this change on the recipients and on their landlords, using previously unexploited administrative monthly panel data on the universe of subsidy recipients. We have utilised the phased roll-out of the reforms to estimate separate effects on rents for new claimants and for existing claimants, using different identifying assumptions in each case.

The two sets of estimates paint a consistent picture. Rents paid by subsidy recipients were affected little overall, and, having adjusted for housing quality, about 90% of the incidence of the reforms was on tenants. This is different to the results of most previous studies of the incidence of rent subsidies, which have tended to find that much of the incidence is on landlords. There is also important heterogeneity within our own estimates, however: the estimated incidence on landlords was substantially higher for some subgroups. These facts may well be related, as different reforms implemented at different times affect different kinds of people. We have argued that this heterogeneity may explain why our overall results on incidence differ from those obtained in the only previous study of the incidence of changes to UK housing benefit. Taken together, our results suggest that the incidence of reforms to housing subsidy regimes can vary substantially within the range of real-world rental markets.

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## Appendix A: Rules governing the housing benefit entitlement of different family types

As discussed in the main text, the Local Housing Allowance (LHA) rate (maximum housing benefit entitlement) applicable for each claimant depends on both their geographical location and their family type (which determines the number of bedrooms to which they are entitled). The purpose of this appendix is to describe in detail the mapping between family type and number of bedrooms, known as the 'size criteria'.

Under the LHA rules, claimants are allowed one bedroom for each of the following occupiers (up to a maximum of 5 bedrooms before April 2011, and 4 bedrooms afterwards), each coming only into the first category for which they are eligible:

- a couple each aged 16 or over
- an individual aged 16 or over
- two children under 16 of the same sex
- two children under 10
- a child

So for example, a couple with two children aged 12 of opposite sex are entitled to three bedrooms, but a couple with two children aged 12 of the same sex are entitled to two bedrooms.

Individuals living in shared (rather than self-contained) accommodation are entitled to the lower 'shared accommodation rate'. Before January 2012, this 'shared accommodation rate' also applied to all single childless individuals aged under 25. From January 2012, that age threshold was rased to 35.

## Appendix B: Data appendix

*Definition of key variables*

The derivation of weekly contractual rents in the SHBE data is typically straightforward, using a combination of the rent amount reported and the periodicity that it is reported to cover (weekly, monthly, etc).

Additional data cleaning was required in some cases where the periodicity was recorded as weekly when in fact it was monthly. This issue was almost exclusively confined to cases recorded by a single software provider (Civica) and for monthly records no later than early 2011. Misrecording is evident from the fact that average weekly rents in affected Local Authorities appeared to fall by approximately 75% in a single month when the issue was resolved. We corrected for this error by identifying claimants for whom, when comparing one month's record with the next, periodicity changed from weekly to monthly with no change to the reported rent. For such claimants we assume that the periodicity had always been monthly when reported weekly in prior months, and hence multiplied reported rents in prior months by (12/52) in order to convert them into weekly amounts. For the small number of Civica cases with periodicity recorded as weekly where the claim *ended* no later than early 2011 (specifically, where the last record of the claim is from a scan submitted before 1st March 2011), we record weekly rents as missing. This is because we know that these periodicities are relatively likely to be incorrect, but some will be correct (i.e. some claimants genuinely report weekly amounts), and we are unable to distinguish between the two without being able to observe a change in periodicity when the error was corrected.

We set rents to missing in four other circumstances:

- A joint tenancy is recorded and the software provider is Saffron/Camino, as there appears to be a tendency for the full rent for the dwelling to be recorded in such cases (rather than just the share of the rent for which the claimant is liable);
- rent is recorded as zero;
- dummy values (beginning 9999) appear to have been used for recorded rents;
- periodicity is recorded as daily, as implied weekly rents tend to be very high in these cases.

Maximum weekly housing benefit entitlements, ignoring non-dependent deductions, are known functions of rent and the applicable LHA rate. Where the excess 'rule' still applies, we define them as the minimum of the LHA rate and the rent plus £15. Otherwise, we define them simply as the minimum of the LHA rate and rent. We set maximum housing benefit entitlement to missing in rare cases where the LHA rate is recorded as zero.

Analyses that use rent, maximum housing benefit, or rent net of housing benefit as the dependent variable are all conducted on the common sample for which all three of these variables are non-missing.

Data cleaning on other variables was also carried out where necessary. For example, certain local authorities at certain times incorrectly record whether or not claimants are in shared accommodation. Instances of this are identifiable from the fact that, in certain local authorities in certain months, a clear majority of claimants are recorded as residing in shared accommodation – with the proportion very close to the proportion of claimants in self-contained accommodation elsewhere. It seems clear that these cases have simply been recorded the wrong way round, and it is therefore straightforward to correct.

*Sample selection – new claimants*

For our analysis of new claimants, we ignore any SHBE records for LHA claims that had already started before the period of data used for analysis (i.e. before June 2010). For the records that remain – those of new LHA claims – we look at the circumstances of the claimant the first time that they were recorded. Since local authorities submit scans of their records once per month, this means that we extract the first monthly scan for each claim, and ignore all subsequent monthly scans

One piece of data cleaning was required in order to ensure that we were defining new claims robustly. Scans from some Local Authorities have a tendency to include claim start dates that have been erroneously reset on a particular date, making the number of new claims appear larger than it really is in that Local Authority on that day and making the start dates of some existing claims appear more recent than they actually are. We were able to detect instances of this by identifying claims which appear to have started soon after (within six months of) a previous active claim by the same claimant, and looking at the proportion of apparent new claims in each Local Authority on each date which have those characteristics. This proportion is far higher than normal in certain Local Authorities on particular days. Where the proportion exceeds 70% on a day in which at least five apparent new claims were made in a certain Local Authority, we conclude that any apparent new claim in that Local Authority on that day which shortly follows a previous active claim by the same claimant is likely to be erroneous. We therefore exclude such claims.

To guard against using information that did not genuinely apply at the beginning of a claim, we exclude from analysis claims for which the first monthly scan appears more than four months after the recorded start date of the claim. For example, if a claim is recorded as having started in January 2011, but the first scan of the relevant Local Authority's records which included that claim was submitted in or after June 2011, we would exclude this claim from the analysis.

*Sample selection – existing claimants*

The basis for our analysis of all existing claimants is a random one-in-three sample of all LHA claimants in January 2011. We take a one-in-three sample purely for computational reasons. (In our analysis of the removal of the five bedroom rate, the extension of the Shared Accommodation Rate, and the introduction of national LHA rate caps, we use data on all those particularly likely to be affected by these reforms, rather than a one-in-three subset.) Focusing on LHA claimants in the one-in-three subset yields a sample of 283,574 claimants.

43,851 claimants are dropped from this sample because the point in time at which they would be affected by the reforms analysed cannot be robustly determined, leaving us with a final sample of 239,723 claimants. In the absence of behavioural response (which we do not incorporate in order to preserve the exogeneity of our treatment), the point at which a claimant was affected by the reforms was determined by the date of the last LHA claim reassessment or claim anniversary in the year prior to April 2011 (or the date on which the claim began, if it began in the year prior to April 2011 and there had been no reassessment since). For full details on how this date is calculated, see Brewer et al (2014). In short, there are three reasons why the point at which a claimant would have been affected can be impossible to determine robustly:

1. Some individuals whose claim began before April 2010 do not appear to have had any claim reassessments or anniversaries between April 2010 and March 2011, because their LHA rate remained constant throughout this period. For most of these individuals, it is therefore impossible to determine the anniversary of their claim. It is possible for a claimant's LHA rate after a claim reassessment or anniversary genuinely to be the same as their previous one. We can use publicly available LHA rates in different BRMAs over time to identify the

claimants for which this was the case (and those claimants are not dropped).

2. Some claimants have large gaps in their records, because local authorities do not always submit scans every month. If a gap of more than 60 days occurs prior to the point at which we identify a claimant as having had their last claim reassessment or anniversary before April 2011, we are unable to calculate the date on which it occurred with sufficient accuracy.
3. Where an individual's claim has never been visibly reassessed, and they have not been dropped as a result of rule 1 (because their claim began after April 2010 or because a reassessment or anniversary during 2010–11 should not have changed their LHA rate), the point at which they will be affected (in the absence of behavioural response) depends on the start date of their claim. For some of these cases, the start date recorded in the SHBE data extract is not deemed sufficiently reliable, for one of the following reasons:
    a. The start date recorded is more than three months earlier than the first observation we have for that individual;
    b. The start date recorded is later than the first observation we have for that individual;
    c. The start date is in April 2009, and the individual lives in one of a number of local authorities in which all start dates from 2008–09 were reset to April 2009.[27]

[27] These local authorities are Stockton-on-Tees, Gateshead, Blackpool, Rochdale, Fylde, Rushcliffe, South Staffordshire, Taunton Deane and Wrexham.

## Appendix C: Results for existing claimants under different specifications

TABLE A1

*Estimated impact of cuts to housing benefit on new claimants*

| | | Model | | | | |
|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | (5) |
| Loss of excess | Housing benefit | -0.72 | -2.09*** | -3.40*** | -4.90*** | -4.98*** |
| | | (0.73) | (0.31) | (0.41) | (0.44) | (0.42) |
| | Rent | 3.73*** | 2.44*** | 0.93*** | -0.74** | -0.81*** |
| | | (0.74) | (0.27) | (0.31) | (0.29) | (0.27) |
| | Rent net of HB | 4.46*** | 4.53*** | 4.33*** | 4.17*** | 4.17*** |
| | | (0.23) | (0.24) | (0.27) | (0.34) | (0.34) |
| Point of main impact | Housing benefit | -4.12** | -4.65*** | -6.67*** | -8.31*** | -8.31*** |
| | | (1.93) | (0.84) | (0.85) | (1.09) | (1.01) |
| | Rent | 5.17*** | 4.81*** | 2.78*** | -0.65 | -0.73 |
| | | (1.43) | (0.47) | (0.40) | (0.78) | (0.68) |
| | Rent net of HB | 9.29*** | 9.46*** | 9.45*** | 7.66*** | 7.58*** |
| | | (0.71) | (0.79) | (0.79) | (0.84) | (0.85) |
| 11 months after main impact | Housing benefit | -1.35 | -1.64* | -5.04*** | -7.08*** | -6.84*** |
| | | (2.02) | (0.99) | (0.82) | (1.07) | (0.92) |
| | Rent | 6.97*** | 6.83*** | 3.43*** | -0.79 | -0.79 |
| | | (1.84) | (0.83) | (0.56) | (1.19) | (1.09) |
| | Rent net of HB | 8.32*** | 8.47*** | 8.47*** | 6.29*** | 6.06*** |
| | | (0.39) | (0.45) | (0.48) | (0.80) | (0.83) |
| | N | 239,576 | 239,576 | 239,094 | 239,094 | 238,782 |