3 The impact of LHA reforms on entitlements, rents and property type for new claimants

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Summary of main findings

- Claimants in higher rent areas (especially London) were more likely to start a new claim just before the reforms were introduced in April 2011: these tenants would have more to lose in cash terms from the changes in Local Housing Allowance (LHA) rates caused by the reforms; these claimants did not, therefore, become subject to the new rules until late in 2012.
- The analysis estimates that the LHA reforms reduced maximum LHA entitlements for new claimants up to the end of 2011 by an average of **£8.21** per week, which was comprised of rent reductions o**f £0.46** per week and increased shortfalls (gaps between rent and LHA) o**f £7.76** per week over this immediate post-reform period: these estimates imply that 94 per cent of the initial incidence of reduced LHA entitlements was on tenants and six per cent on landlords.
- These results vary by claimant sub-group. Single claimants and younger claimants seem to have been able to pass a greater share of the incidence of the reductions on to landlords (via reduced rents), which may reflect their greater propensity to move house.
- The incidence of the reforms on landlords also varied geographically: it was greater in urban areas outside London and in London suburbs.
- A note of caution is needed about the results at this stage of the analysis: they may be short-term effects, in that it may take tenants time to negotiate lower rents with landlords; it may also take time for tenants to seek cheaper accommodation; and landlords may accept informally lower than the contracted rents from LHA tenants.
- The next stage of the analysis will incorporate existing claimants as well and will be able to investigate the longer term impacts of the reforms, to establish if this pattern of impacts on landlords and tenants is sustained.

3.1 Introduction

This report forms part of the first stage of the review of the impact of recent measures (introduced from April 2011 onwards) to change the system of LHAs in the private rented sector (PRS) in Great Britain. It analyses administrative data on Housing Benefit (HB) claims by claimants subject to LHA rules in order to examine the impact of the LHA measures introduced in April 2011 on new LHA claimants.[32]

This research project was commissioned by the Government in 2011. The evaluation is being undertaken by a research consortium from the Centre for Regional Economic and Social Research (CRESR) at Sheffield Hallam University, the Institute for Fiscal Studies (IFS), the Blatvatnik School of Government at the University of Oxford and Ipsos MORI. The overall research programme involves both quantitative and qualitative methods. The quantitative methods comprise spatial analysis and econometric analysis based on nationwide data on HB claimants, and surveys of claimants and landlords in 19 case study areas across Britain[33]. The qualitative methods comprise interviews with claimants, landlords and housing advisers in the case study areas. This broad-based approach to the research is designed to monitor some of the impacts of the LHA measures in the short to medium term, while being sensitive to different local housing market contexts, and to assess the extent to which LHA reforms are starting to induce attitudinal and behavioural changes among landlords and tenants in the PRS.

The research programme runs from April 2011 until December 2013. The first report from the research team examined the findings of the wave 1 large-scale face-to-face surveys of claimants and a postal survey of landlords in the 19 case study areas (DWP, 2012). These surveys were undertaken in autumn 2011: several months after most of the measures had been introduced for new LHA claimants, but before they had an impact on the rents and housing circumstances of existing (pre-April 2011) LHA claimants. (For further details of the overall research programme see DWP, 2012.)

The main outcomes of interest in this section are LHA entitlements, contractual rents, the differences between the two, and the types of properties that LHA claimants inhabit. We also examine whether the impacts of the LHA reforms differ across sub-groups of claimants.

The main reforms considered here are:

- setting LHA rates at the 30th percentile of PRS rents rather than the 50th percentile;
- abolition of the five-bedroom LHA rates;
- capping the LHA rates at £250, £250, £290, £340 and £400 per week for the shared room, one-bedroom, two-bedroom, three-bedroom and four-bedroom rates respectively;

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[32] This section uses the phrase ‘LHA claimants’ as shorthand for ‘Housing Benefit claimants subject to the LHA rules’. The phrase ‘new LHA claimants’ covers both those individuals who start a claim for the first time, and those who have had a previous claim which ceased and subsequently start another claim – these are the individuals who were immediately affected by the reforms in question in April 2011.

[33] Barking and Dagenham, Blackburn with Darwen, Bradford, Brent, Cardiff, Denbighshire, Edinburgh, Exeter, Fenland, Hackney, Newcastle, North Lanarkshire, Perth and Kinross, Portsmouth, Rhondda Cynon Taf, Tendring, Thanet, Walsall, Westminster.

- removal of the £15 per week excess; and
- increase in central government funding for Discretionary Housing Payments (DHPs) by £10 million in 2011-12.[34]

The section is organised as follows. In 3.2, we discuss what we might expect the key impacts of the reforms to have been, with reference to economic theory and previous relevant literature. Sub-section 3.3 describes the data and econometric methods used in order to obtain our empirical estimates of the impacts of the reforms on new claimants. Sub-section 3.4 presents and discusses the main results of our analysis for GB as a whole. Sub-section 3.5 presents sub-group analysis, with estimates of how the impacts of the LHA reforms varied by family type, age and area. We conclude with a summary of results and a reminder of the main limitations of this work at this interim stage.

3.2 What does economic theory suggest about the impact of the LHA measures on LHA claimants?

LHA is a rent subsidy, where the maximum cash subsidy depends upon household type and Broad Rental Market Area (BRMA). Economic theory suggests that the incidence of rent subsidies – in other words, who actually benefits from them –- is in general ambiguous. If the rent levels that landlords charge are completely insensitive to rent subsidies, then LHA is simply a transfer from taxpayers to tenants. Tenants may 'spend' this either by renting more expensive accommodation or by purchasing more of other consumption goods, or some combination of the two. Alternatively, rents may be higher in the presence of rent subsidies than they would otherwise have been: tenants are less sensitive to increases in rents if the taxpayer pays at least some of the cost, so landlords may charge higher rents in response. In that case, at least some of LHA spending is a transfer from the taxpayer to landlords.

For the same reasons, reductions to LHA may in general be incident on either landlords or tenants (or some combination), and the impact on rent levels is crucial in this regard. The LHA reforms considered in this report all act to reduce LHA entitlements for some subset of LHA claimants. Consider first the three reforms introduced in April 2011 which reduced LHA rates (the maximum rent that can be covered by LHA, given household type and BRMA). These were the switch from the 50th to 30th percentile of the non-LHA local PRS distribution, the introduction of the national caps for different property types, and the abolition of the five-bedroom rate. We would expect these LHA reductions to lead to some combination of the following:

- claimants facing a larger shortfall between their rent and the LHA they receive for a given type of accommodation (financing more of their rent via non-LHA resources, and hence reducing consumption of other goods);
- claimants spending less on rent by moving to cheaper accommodation; or
- claimants spending less on rent for a given type of accommodation due to landlords reducing rents.

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[34] This cost estimate comes from the June 2010 Budget (HM Treasury, 2010). Increases in DHP funding of £40 million were announced for 2012–13 onwards.

The first two items in the above list represent incidence of the LHA reductions on tenants, and the third represents incidence on landlords. To the extent that claimants move to accommodation where rents or LHA rates are lower than the LHA rate that they would have otherwise have been entitled to, the second item also implies further reductions in HB spending on the claimants concerned.

The overall incidence of the LHA reforms on landlords and tenants is, therefore, an empirical question. But economic theory provides guidance as to what types of factors will determine the answer. We know that the mechanical effect of a reduction in rent subsidies is to make a given level of rent more expensive for tenants. Therefore, their demand for rental accommodation at a given level of rent is reduced too. This is shown by a downwards shift in the demand curve in Figure 3.1, which shows supply and demand in a competitive rental market – after the reform, rents need to be lower than before in order for a given quantity of accommodation to be demanded, to offset the reduction in rent subsidies. In particular, this implies that supply would exceed demand if rents remained at their pre-reform equilibrium levels. The new equilibrium rent level, in which supply and demand are equalised once more, will, therefore, be lower. But how much lower – and hence, how much of the incidence of the reform is ultimately on landlords – depends crucially on the following types of factors:

- **How responsive the supply of rented accommodation is to changes in rent levels**. If the supply of accommodation to LHA recipients is very responsive ('elastic') to rents, these reforms would not affect the equilibrium rent level much. Conversely, if the supply of rented accommodation to the LHA sector was unresponsive ('inelastic') to rent levels, the new equilibrium rent level would be significantly lower. The intuition is as follows. As explained above (and depicted in Figure 3.1), a reduction in rent subsidies reduces demand at a given level of rent, and in particular means that supply would exceed demand if rents remained at their pre-reform level. Hence, rents in the new equilibrium – that is, the rent level at which supply and demand are equal once more – must be lower. If the supply of rental accommodation to the LHA sector is very elastic, then rents need to fall only a little in order for supply to fall enough that it is aligned once more with demand: in other words, the new equilibrium rent level will be only a little lower than the old one. This would imply that most of the incidence of the reforms would be on tenants via increased shortfalls, rather than landlords via reduced rents. The converse would apply for the case of inelastic supply, where rents would need to fall more before supply and demand were equalised in a post-reform equilibrium. Figure 3.1 illustrates these points for a competitive rental market. In the case where supply is responsive to rent levels, equilibrium rents fall by much less (on the left, where they fall from a to b) in response to a reduction in rent subsidies than in the case where it is unresponsive (on the right, where they fall from a to c). Factors affecting the elasticity of supply for private rental accommodation could include things which determine how easy it is to buy up new properties to let, such as planning regimes and Stamp Duty Land Tax.
- **The share of LHA tenants within a particular rental market, or the level of segmentation between the LHA rental market and the wider market**. As these reforms affect a subset of the private rental market – those claiming HB subject to the LHA rules – the demand for rental property in a particular area will fall by less if LHA tenants are a small share of the overall market. Where landlords are willing and able to let their properties to non-LHA tenants, they will be less likely to reduce their rents in response to the LHA reductions. With reference to the competitive market represented by Figures 3.1 and 3.2, the demand curve that these landlords face for their rental property would not shift down as much as it would do if all potential tenants were LHA recipients, and the

equilibrium rent level would, therefore, not need to change as much. However, where LHA tenants face little competition for properties from non-LHA recipients, landlords would be expected to reduce rents to a greater extent. Hence, in a market where given landlords would, in principle, let to either LHA or non-LHA claimants, the share of LHA claimants in the private rental market should affect the incidence of the reforms. On the other hand, if the market is segmented such that given landlords let only to LHA or non-LHA claimants, we would not expect this to be important: instead, the crucial factors would be those that affect the responsiveness to rent levels of supply and demand for rental accommodation specifically in the LHA sector. Evidence presented in Section 5 of these interim research outputs, from interviews with landlords, suggests that such segmentation does exist in some local private rental markets

- **How responsive LHA tenants' demand is to changes in rent levels**. If demand for rental property is more sensitive to rent levels, the equilibrium rent level will fall by more in response to any reductions in rent subsidies. Intuitively, a reduction in rent subsidies means that demand would fall short of supply if landlords continued to charge the same rents; but the more elastic demand is, the more it will fall short of supply at the old equilibrium rent level, and hence the further rents will have to fall before supply and demand are equalised again in the new equilibrium. Figure 3.2 again shows supply and demand in a competitive rental market: rents fall by more in the case where demand is more responsive to changes in rent levels (on the left, the fall from a to b is much larger than the fall from a to c on the right, where responsiveness is lower). Factors affecting the demand elasticity could include the costs (financial or otherwise) to LHA claimants of moving properties, which might vary with characteristics such as household type and age.

Figure 3.1 Impact of LHA reductions on rental market depending on responsiveness of supply of rented accommodation to rent levels

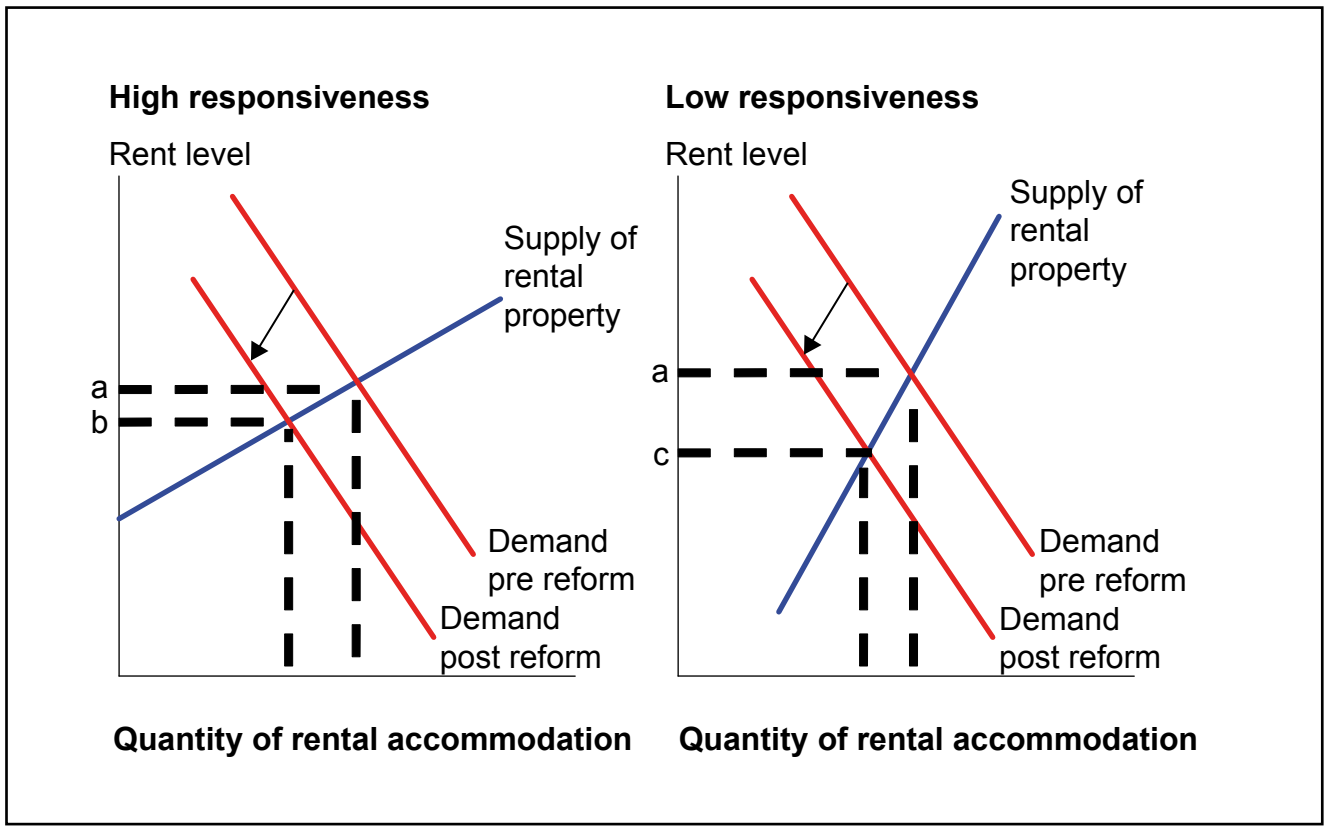
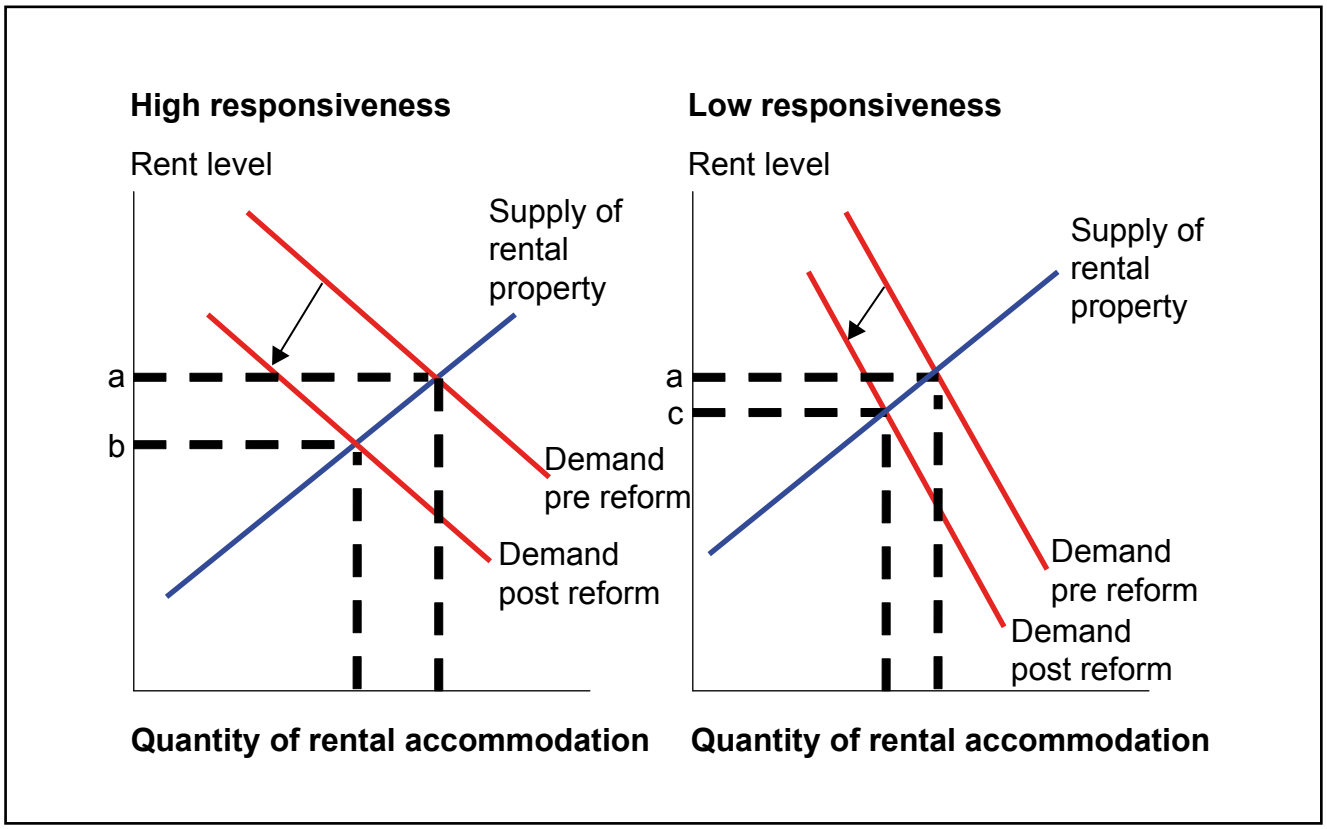


Figure 3.2 Impact of reforms on rental market depending on responsiveness of demand for rented accommodation to rent levels



3.2.1 Removal of the £15 per week excess

However, theory suggests that the incidence of the decision to end the £15 per week excess that LHA claimants can keep (over and above their rent) is likely to be different from the incidence of the other LHA measures. The rationale for the excess, which was a key part of the initial introduction of LHA in April 2008, was to encourage claimants to rent a cheaper property or negotiate rents downwards, rather than spending their full applicable LHA rate on rent. Removing the excess removes this incentive: LHA claimants no longer get any financial benefit from spending less than their LHA rate on rent, as they no longer keep any of the difference. In choosing between properties where the rent is no higher than the LHA rate, claimants, therefore, have little or no reason to choose a cheaper property over a more expensive one, and no immediate financial incentive to bargain with landlords to reduce rents below the LHA amount[35]. Similarly, if landlords know that a potential tenant is entitled to LHA, they have little or no reason to offer a rent that is less than the LHA rate. On the other

[35] Some tenants may still have an incentive to keep rents lower than their LHA rate if they are forward-looking, as they might expect that they may stop claiming LHA in future (e.g. because of a move into work or an increase in earnings), and there are costs associated with moving house again or renegotiating a rental contract in such an event. In that case, they have some incentive to seek lower rents in view of the fact that they may face the financial cost of higher rents in future. Nevertheless, these incentives are clearly weaker than they would be if tenants also kept £15 per week of the difference between the LHA rate and their rent.

hand, where landlords are unable to identify which potential tenants are LHA recipients or are unaware of the relevant details of the LHA system, rents would not be expected to rise in response to this reform. This reform in isolation should, therefore, have some combination of the following effects:

- landlords increasing rents to or towards the full LHA rate. To the extent that this happens, the effect of the reform is to transfer the excess from claimants to landlords rather than from claimants to the exchequer;
- tenants choosing more expensive properties, as they would no longer keep any of the difference between their LHA rate and their rent. Again, this would not result in any direct saving to the exchequer;
- claimants simply losing the excess, with no change in rent levels or property type rented, reducing the exchequer cost of LHA.

In none of these cases would the removal of the £15 excess lead to lower rents. Hence, we would not expect any of the incidence of this LHA reduction to be on the landlord. Indeed, because the first item in the list above implies some transfer of the excess from claimants to landlords (rather than to the exchequer), the extent to which tenants lose from this reform can exceed the amount by which their LHA entitlement is reduced (i.e. more than 100 per cent of the LHA reduction can be incident on tenants). These are unusual and potentially important features of this particular rent subsidy reform. They imply that, all else being equal, we should expect a greater share of the incidence of the April 2011 LHA reforms to be on tenants than in the case of other rent subsidy reforms examined in the academic literature (see below).

In the empirical analysis that follows, it is not possible to separate robustly the impacts of the removal of the £15 excess from the impacts of the other reforms to LHA in April 2011, for two reasons. First, many claimants who would have benefited from the £15 excess pre-reform would also have been affected by the switch from the 50th to 30th percentile for LHA rates (the exceptions are those whose rent was already below the 30th percentile). Second, we are analysing new LHA claims in this report, and we do not know which post-reform new claimants would have chosen properties that cost less than their LHA rate (and hence benefited from the £15 excess) in the absence of the reform. Nevertheless, in cautiously offering potential interpretations of our empirical results in Section 3.4, it will be useful to be aware of the types of new claimants who were most likely to benefit from the excess rule. Table 3.2 documents this, and shows that these groups include households with children and claimants in London and Scotland.

Subsequent analysis to be undertaken in this research project will examine the impact of the reform on existing claimants, and will attempt to disentangle the impact of this component of the reforms from that of other components. This will be possible both because existing claimants were affected by the removal of the £15 excess at a different time from the other reforms,[36] and because we can observe the pre-reform housing choices of existing claimants (and how they subsequently changed).

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[36] Existing claimants lost entitlement to any excess in April 2011. For the other elements of the LHA reductions analysed here, existing claimants were affected nine months after the first post-April 2011 anniversary of their claim (i.e. at some point between January and December 2012).

It is crucial to note that the discussion so far has focused on what economic theory tells us about the incidence of the changes to rent subsidies in steady state – in other words, once the private rental market has adjusted to its new post-reform equilibrium. But such adjustment may take time. For example, nominal 'rent stickiness' could occur if there is imperfect awareness of the reforms on the part of landlords (as suggested by some of the evidence in Section 5), or if there are costs associated with drawing up new rental contracts mid-tenancy. It is perfectly possible, therefore, that the relative incidence of LHA reductions on landlords and tenants changes over time. In a future report we will investigate such changes, by tracking the new claimants analysed here over a number of months to look for evidence of delayed effects of the LHA reductions on rent levels.

Table 3.1 Proportions of new LHA claimants with contractual rent less than, equal to and greater than their LHA rate, June to November 2010

Characteristic	Rent less than LHA rate %	Rent equal to LHA rate %	Rent greater than LHA rate %
Household type			
Single man	33	10	57
Single woman	32	8	60
Couples without children	25	7	68
Single parents	41	15	44
Couples with children	46	10	43
Age of claimant			
Under 25	32	11	57
25–34	38	11	51
35–44	38	11	51
45–54	37	10	53
55–64	32	9	59
65 and above	30	7	62
Government Office Region			
North East	31	11	59
North West	34	12	54
Yorkshire and Humberside	36	11	53
East Midlands	33	10	58
West Midlands	32	11	57
East of England	33	10	57
London	43	14	43
South East	37	10	53
South West	37	9	55
Wales	27	9	65
Scotland	42	11	47
All	**36**	**11**	**53**

Notes: Rent is taken to be equal to the LHA rate here if it is within £1 per week of it Rows may not sum to 100 per cent due to rounding.
Source: Single Housing Benefit Extract (SHBE).

3.2.2 Previous evidence on the effects of changes to rent subsidies

As discussed, economic theory does not generally provide definitive guidance on the impact of LHA reductions: it suggests that the effects will depend upon the details of the private rented market. It is, therefore, instructive to consider the evidence provided by previous empirical studies of the impacts of rent subsidies, as well as the limitations of those studies for providing guidance on the likely impacts of the particular reforms considered here.

The last substantial changes to the UK HB system before the introduction of LHA for the PRS in April 2008 were in the mid-1990s. Gibbons and Manning (2006) studied the impacts of those reforms, which reduced the maximum amounts of rent that could be covered by an HB claim in given properties. Using survey data on England only, the authors found that at least about one half of the incidence of those HB reductions was on landlords via reduced rents (subject to the caveat that they had only limited controls for housing quality, as here – see sub-section 3.4).

A small number of studies have explored changes in rent subsidies in other countries in order to estimate their incidence. Fack (2006) looked at reforms to rent subsidies in France in the early 1990s, and estimated that 78 per cent of the incidence of those changes was on landlords. One limitation of that study is that the reforms in question affected only small households, so the findings may not generalise to the French population as a whole. But earlier work using different French data and a different methodology also found that a significant portion of the incidence of rent subsidies was on landlords (Laferrere and le Blanc, 2002). Susin (2002) studied the impact of rent vouchers for low-income households in 90 metropolitan areas of the USA, and found that they have increased rent levels for those households substantially (by about 16 per cent).

In summary, empirical studies of the impact of rent subsidies have tended to find that the incidence is largely on landlords in the form of higher rents, mainly because the supply of rental accommodation is unresponsive to changes in rent levels. This is true both in the UK and elsewhere. If the incidence of rent subsidies is indeed partly on landlords, then reductions to rent subsidies would reduce rents.

But of course, the direct relevance of the previous literature on the incidence of rental subsidies for this particular study may be limited. The structure of the rental market might be different in the UK now compared with the mid-1990s period studied by Gibbons and Manning; and one needs to be cautious in inferring too much from studies in other countries, which have different subsidy systems and different markets. Furthermore, as discussed, with all else being equal we would expect the overall incidence of the reforms studied here to be different from those studied elsewhere, as theory suggests that the removal of the £15 per week excess will be entirely incident on tenants.

3.3 Data and methodology

This sub-section describes the data and methodology used in the empirical analysis that follows.

The empirical analysis in sub-section 3.4, as in the spatial analysis in Section 2, uses administrative data from the Single Housing Benefit Extract (SHBE). This is made up of returns submitted to the Department for Work and Pensions (DWP) each month by all local

authorities (LAs) in England, Scotland and Wales and contains information on the status of each claim. The key data available to the research team and utilised in the proceeding analysis include information on claimants' contractual rents, LHA rates, BRMAs, LAs, LHA bedroom entitlements, actual number of bedrooms, household type, and age on a particular day each month.

We add DHPs to LHA entitlements when tracking what has happened to entitlements over time (see Section 6 for a fuller discussion of the use of DHPs in the research case study areas). Our estimates will, therefore, account for any effects of changes in DHP allocation alongside the LHA changes. For simplicity we continue to refer to entitlements simply as 'LHA entitlements'. Unsurprisingly, given the small monetary amounts involved relative to the reductions to LHA, the inclusion of DHP makes a negligible difference to our estimates.

The focus is on new claimants of LHA, rather than those who were already in receipt of LHA in April 2011 when the reforms took place. As described earlier, the latter group were not affected by all of these measures until 2012, and this group of existing claimants will be the subject of a future report on impact. Table 3.2 describes the composition of this flow of new claimants in terms of basic demographic characteristics: measures of household type, age, and region. This is done for June to November inclusive, separately for 2010 and 2011 (i.e. both before and after the LHA reforms considered here were implemented). As explained below, we use data from those periods to formally estimate the impacts of the reforms in sub-section 3.4.

Table 3.2 Demographic characteristics of new LHA claimants

Characteristic	Jun-10 to Nov-10	Jun-11 to Nov-11
Household type		
Single man	35.7	34.5
Single woman	18.8	18.7
Couples without children	7.6	7.6
Single parents	24.3	24.5
Couples with children	13.7	14.7
Age of claimant		
Under 25	23.2	22.6
25–34	32.8	33.1
35–44	22.8	22.6
45–54	13.0	13.4
55–64	5.6	5.6
65 and above	2.6	2.7
Government Office Region		
North East	4.9	4.6
North West	13.8	13.5
Yorkshire and Humberside	9.1	9.7
East Midlands	7.0	7.2

Continued

Table 3.2 Continued

Characteristic	Jun-10 to Nov-10	Jun-11 to Nov-11
West Midlands	8.3	8.5
East of England	7.8	8.0
London	16.0	14.5
South East	12.4	12.5
South West	9.2	9.5
Wales	5.3	5.5
Scotland	6.3	6.6

Source: Single Housing Benefit Extract (SHBE).

Because the reforms affect all new LHA claimants from the same date, there is no natural control group of new LHA claimants who can be used to estimate the impacts of the reforms. Therefore, our strategy is effectively to compare outcomes for those who claimed LHA shortly after April 2011 with those who claimed LHA shortly before April 2011.

The empirical analysis in the following sub-section includes descriptive graphical analysis of how the key outcomes of interest (including rents, different measures of property type, the amount of LHA received and the shortfall between rent and the amount of LHA received) changed over time. Such analysis will not necessarily give us an unbiased impression of the causal impact of the reform, for at least two reasons. First, any change could reflect a general underlying time trend independent of the April 2011 reforms. For example, rents tend to increase over time (although this does not appear to have been happening for new LHA claimants in the period before the reforms in question, as shown in sub-section 3.4). These underlying time trends, therefore, need to be modelled if the causal effect of the reforms on rents is to be estimated. Second, any changes over time could be due to changes in the composition of LHA recipients: for example, if the number of single claimants (who are typically entitled to smaller properties with lower rents) increased after the reform for reasons that were unconnected with the changes to LHA, it would be wrong to conclude that the reform had caused rents to fall.

In order to estimate the impact of the reforms formally, we use multivariate regression analysis. This allows us to control for the changing composition of LHA recipients, and to allow the outcomes (e.g. rents) to have an underlying time trend (i.e. one that exists even after controlling for the observed characteristics of LHA recipients) both before and after the reform date. We then look for a change in the outcome in the period after the reform beyond what would be predicted by any changes in our control variables and the underlying time trends. As described below, in order to avoid bias in our estimates of the reform due to the effects of people anticipating the reforms and changing their behaviour accordingly, we exclude a window of data surrounding the time of the reform (April 2011) from this analysis. In technical terms, our identification strategy, therefore, lies somewhere between a before-after study and a regression discontinuity design.

We estimate equations of the following form using Ordinary Least Squares regressions:

$$\Upsilon_{it} = \chi'_{it}\beta + f(t) + Y.POST_t + \varepsilon_{it},$$

Where Υ_{it} is an outcome of interest for individual i starting an LHA claim at time t, χ_{it} contains some explanatory variables, $f(t)$ is a flexible time trend, $POST_t$ is an indicator for whether an individual started their LHA claim after April 2011, and ε_{it} is an error term which captures the

unobserved determinants of the outcome. The parameter of interest is, therefore Y, which is the effect of the reforms on the outcome of interest. In all cases our estimated standard errors around the estimate of Y are robust to flexible specifications of the error term.[37]

3.4 Empirical analysis

3.4.1 The pattern of new LHA claims

The dark (top) line in Figure 3.3 shows the number of new LHA claims being made over the period. On average over the period from June 2010 to December 2011 there were around 1,900 new LHA claims each week, with more new claims typically falling in the first week of each month and slightly fewer claims typically falling in later weeks in a month. Two other periods exhibit a slightly different pattern, in terms of the number of new claims. First, the period from Christmas to New Year has fewer new claims. Second, the spike in new claims close to the start of April 2011 is particularly high and the number of claims later in April 2011 and early May 2011 is particularly low.

The atypical pattern in the number of new claims at around the introduction of the April 2011 LHA reforms suggests that the number and/or timing of new claims may have been affected by the reforms. This is consistent with the financial incentives created by their roll-out: they affected new LHA claimants immediately, but affected existing claimants only nine months after the anniversary of their claim (except for the removal of the £15 excess). Those whose claim started prior to April 2011 were, therefore, not affected by most of the reforms until 2012, with the exact date depending on precisely when their claim started. Therefore, households who started a claim shortly before April 2011 would not have been affected by most of the LHA measures until up to 21 months after households who started a claim just after April 2011. Some households who were planning to make an HB claim at around the reform date would, therefore, have had much stronger financial incentives than usual to sure that this was done quickly, so that the claim started before 1st April 2011.[38]

Other possible anticipatory responses could have included ceasing an existing claim shortly before the reform so that a new one could be started. The extent to which this was widespread would depend on the extent of awareness about the reforms before their

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37 Technically, we allow for arbitrary forms of heteroscedasticity and correlation in the error term across different observations within a BRMA, using a cluster-robust variance estimator (Liang and Zeger, 1986). Allowing for heteroscedasticity means that analysis is robust to the variance of the error term depending on the values of the explanatory variables. Clustering at the BRMA level allows for the error term to be both serially and cross-sectionally correlated within BRMAs. Failure to account for any intra-BRMA correlation would tend to result in underestimates of the true standard errors (see e.g. Moulton, 1990).

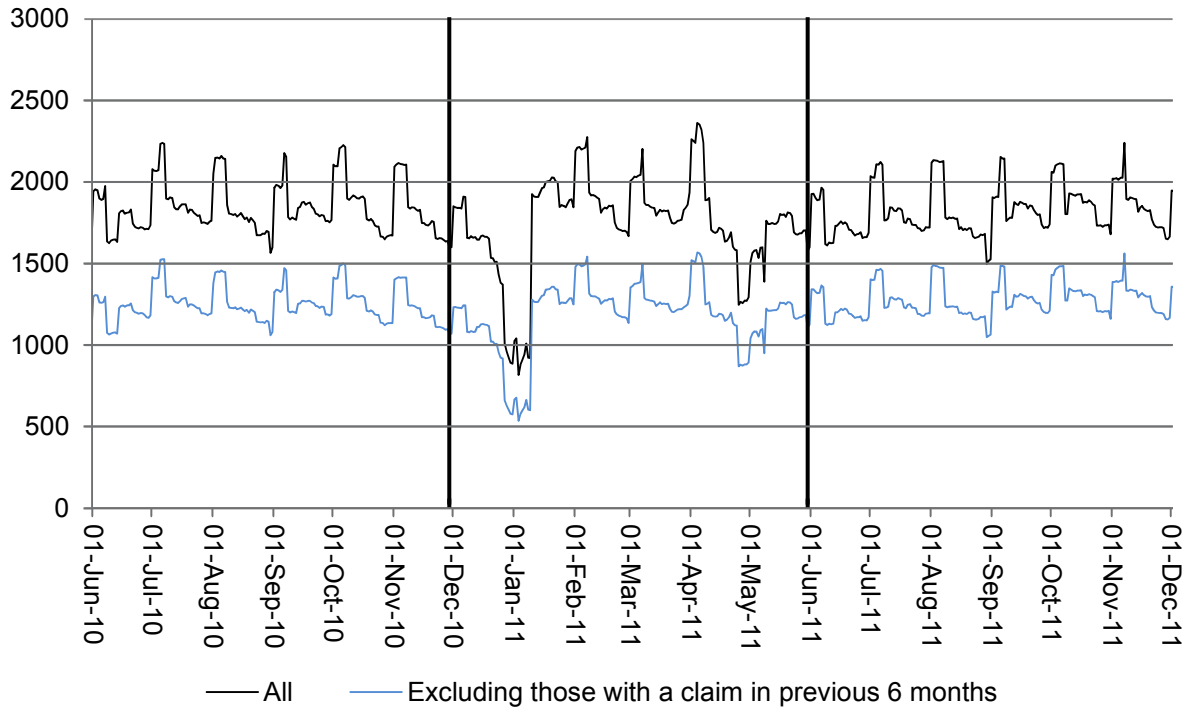
38 The peak in the number of new claims around the reform date actually occurs on 1 April 2011, though the spike starts some days before that. This might seem odd, as 1 April 2011 was the first day on which new claimants would have been affected by the LHA reductions. This may just reflect claimants rushing and narrowly failing to get their claim started before the reform date. Alternatively, there could be some measurement error in the data, perhaps because LAs have a tendency to record claims as starting at the beginning of a month even if, in fact, they started a few days either side of that. In any case, our estimates of the effect of the reform are insensitive to this, as we exclude the window of data surrounding the reform date from our regression analysis.

implementation. Evidence presented in Section 4 and Section 5 of this analysis indicated generally low awareness of the reforms on the part of tenants, but, on the other hand, a good understanding of the reforms on the part of some landlords. (As shown in Section Five, one large landlord in the case study areas allocated all their LHA tenants to different properties within their portfolio shortly before the reforms came in.) If this were the only explanation, however, we would expect the abnormal patterns of claims at around the reform date to 'disappear' if we exclude from consideration new LHA claims which followed other claims by the same claimant that had been active within the previous six months. This is not the case, as shown by the lower line in Figure 3.3.

As analysis later in this sub-section shows, it was claimants in higher-rent areas (in particular, London) who were more likely to start a new claim just before the reforms were introduced. This is consistent with the anticipatory responses discussed above. Such claimants would in general have more to lose in cash terms from the shift from the 50th to 30th percentile for LHA rates, and would be more likely to be affected by the national caps.

In order to ensure that anticipation effects do not bias our estimates of the impact of the LHA reductions, we exclude all claims made in the window between 1 December 2010 and 31 May 2011 from the regression analysis. This excluded window of data is marked with vertical lines on Figure 3.3 and on subsequent Figures. The drawbacks of excluding a window of data are loss of sample size and the fact that parametric estimates of time trends have to extrapolate further outside of the range of the estimation sample. But the sample size is very large, and the time trends in our outcomes of interest look uncomplicated (see below). As reported later, we have conducted sensitivity analysis which shows that our key estimates are robust to small shifts in the window of data excluded.

Figure 3.3 HA on-flow by day: all new claims, and excluding those following a claim active within last 6 months (seven-day moving average)



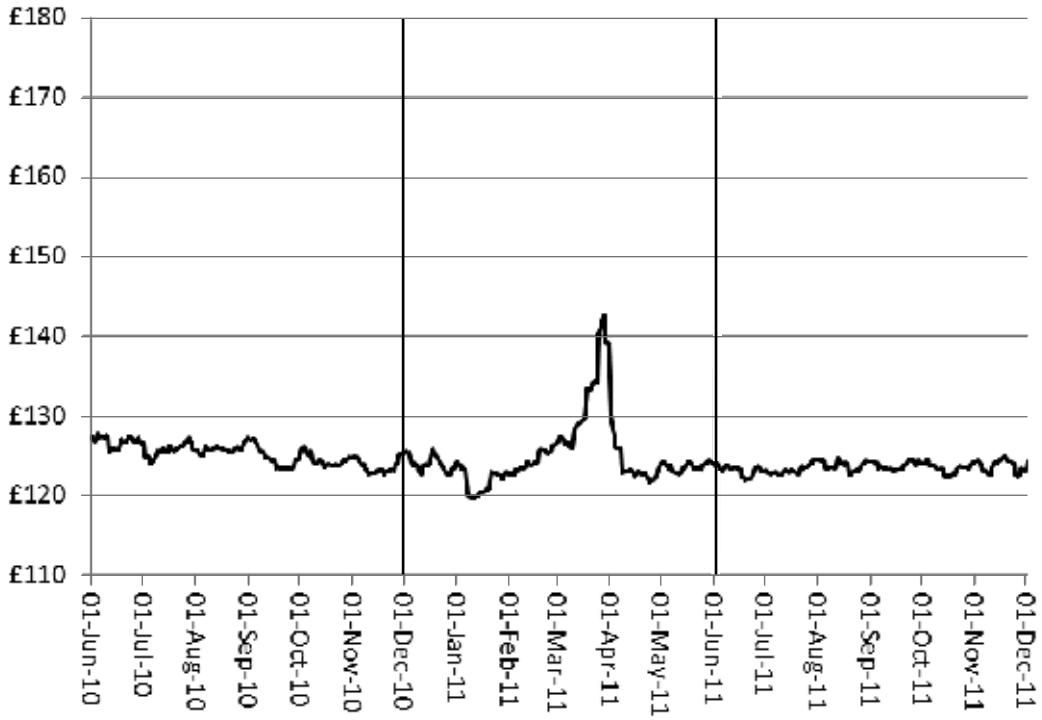
Source: Authors' calculations using SHBE data extract.

The analysis that follows examines the impact of the reforms on claimants' rents, property types, maximum LHA entitlements, and shortfalls between rent paid and maximum LHA entitlement.

3.4.2 Contractual rents

Figure 3.4 shows what happened to new claimants' contractual rents around the time that the reform was introduced.

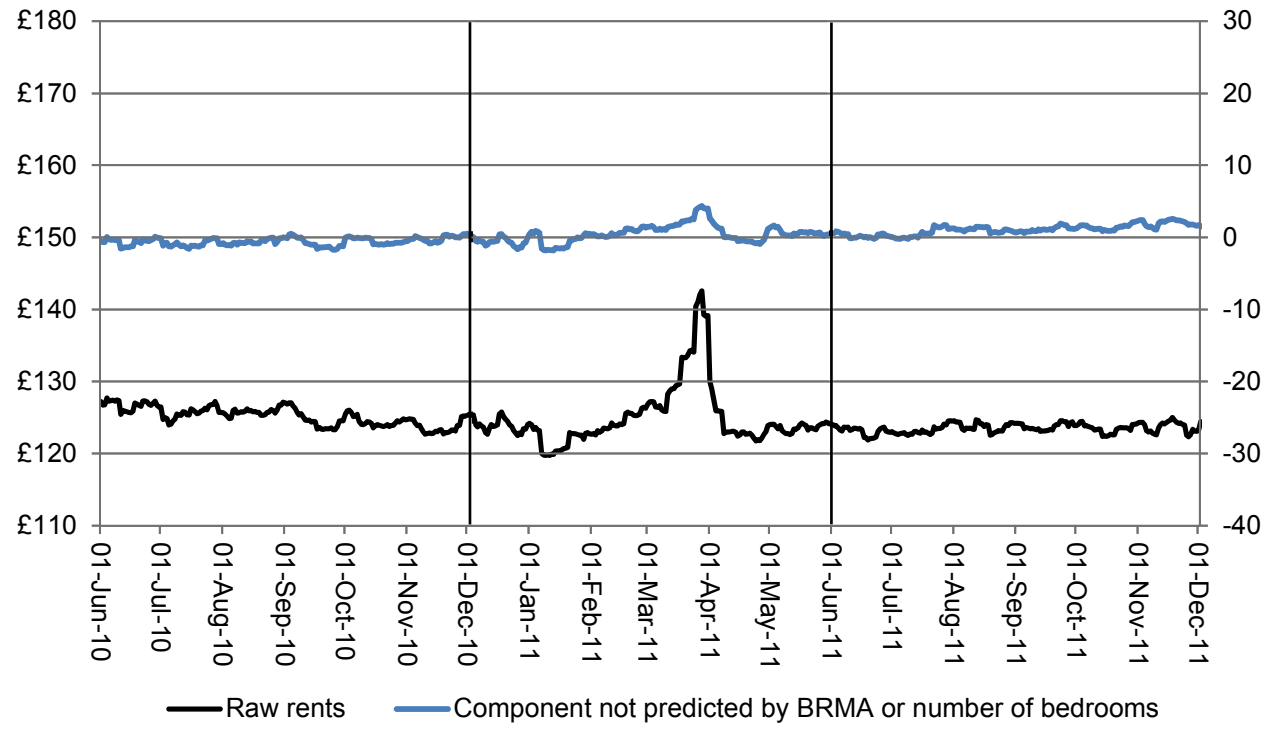
Figure 3.4 Average weekly rents of new LHA claimants by date of claim (seven-day moving average)



Source: Authors' calculations using SHBE data extract.

The most striking feature of Figure 3.4 is the spike in rents just before the reforms were introduced at the start of April 2011. This can be predominantly explained by the composition of LHA claimants over the period. For example, in the data underlying Figure 3.4, the proportion of new LHA claims being made in London rose by three percentage points between January and March 2011, from 14.3 per cent to 17.3 per cent. The same proportion never fluctuated by more than one percentage point over any other two-month period in these data, so it seems very likely that this is related to the reforms. Similarly, the average number of individuals per household – which is unsurprisingly correlated with rent levels, presumably due mainly to property size – among new LHA claimants rose from 1.86 to 1.95 between January and March 2011, and again this is a larger fluctuation than over any other two-month period in the data. As stated above, this is consistent with the fact that claimants with higher rents would have had larger financial incentives to start new claims just before April 2011, because they generally had more to lose from the reforms.

Figure 3.5 Average weekly rents of new LHA claimants by date of claim (seven-day moving average)



Source: Authors' calculations using SHBE data extract.

Figure 3.5 shows more formally that we can explain much of the spike in rents with reference to the changing composition of claimants in the immediate pre-reform period. It plots the component of rent levels that is not explained by the combination of BRMA and number of bedrooms.[39] When we do this, the spike in rents is almost entirely eliminated, confirming that the spike is (at least primarily) due to particular kinds of claimants being more likely to start claims in the immediate pre-reform period.

Abstracting from what appear to be the anticipation effects of the reforms, the other noteworthy feature of Figure 3.4 is that rents do not appear to be very different in the post-reform period relative to the pre-reform period. This is certainly the case in the context of the size of the corresponding changes in LHA entitlements, as shown in Figure 3.6.

Table 3.3 shows the results of linear regression analysis of this time series to investigate the changes in rents, and whether they can be explained by changes in the (limited) measures of property type in the SHBE data. Moving from left to right across the table, more controls are added: 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, 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 controls for household type and age (jointly). The notes to the Table contain precise definitions of the control variables.

[39] Technically, this is a plot of the residuals from a regression of rents on indicators for BRMA, the number of bedrooms, and all possible combinations of the two.

The results show a small fall in raw rents (column 1) in the post-reform period relative to the pre-reform period. But this change in rents is not statistically significantly different from zero (indicating that the fall is not large enough to be clearly out of line with the normal level of volatility in rent levels). In order to estimate whether any of the incidence of the LHA reductions was on landlords rather than tenants, we need to control for property type – raw rents may change simply because tenants move to properties with different rent levels. The next two columns do this, adding controls for the LA and BRMA that claimants are renting in and the number of bedrooms in the property. As we move from model 1 to model 2 the estimated change in rent turns positive; and from model 2 to model 3 it becomes more positive and statistically significant. This suggests that the small fall in unadjusted rents in the post-reform period can be explained by claimants moving to cheaper areas and renting smaller houses in that period (note that these changes were not necessarily caused by the reforms, as there may have been wider time trends – we explore this in more detail later, examining the impact of the reforms on the limited measures of property type that are available in the SHBE data).

Model 4 adds general time trends to the model, allowing them to vary by BRMA and between the pre-reform and post-reform periods. This can be viewed as the first reasonable estimate of the effects of the reform on the price of rental accommodation, as we have now controlled for property type and for general time trends unrelated to the reform. The estimate is that the reforms had a very small downwards effect on rents, but this effect is not statistically significantly different from zero.

Table 3.3 Impact of the LHA reforms on contractual rents of new LHA claimants (£ per week)

	Model				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Post-reform coefficient	-1.57	0.12	1.62***	-0.21	-0.46
Standard error	(1.11)	(0.52)	(0.43)	(0.66)	(0.64)
R^2	0.000	0.256	0.499	0.500	0.513
Clusters (BRMAs)	191	191	191	191	191
Number of observations	667,278	667,278	662,764	662,764	659,892

Notes: *** Statistically significant at 1 per cent level, ** Statistically significant at 5 per cent level, * Statistically significant at 10 per cent level. 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, one bedroom, two bedrooms, three bedrooms, four bedrooms, five 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 (one or two or more for under 25s, and one, two or three or more for other ages). Standard errors are robust to heteroscedasticity and clustering at the BRMA level.
Source: Authors' calculations using SHBE data from June 2010 to November 2010 and June 2011 to November 2011.

Finally, model 5 adds controls for household type and age. The argument for adding them to the specification is that they may be changing over time for reasons unrelated to the reform, therefore, changing the kinds of properties that are rented – and hence changing rent levels – in ways that are not adequately captured by our parametric time trends. This makes little difference to our estimates of the impact of the reforms on rents.

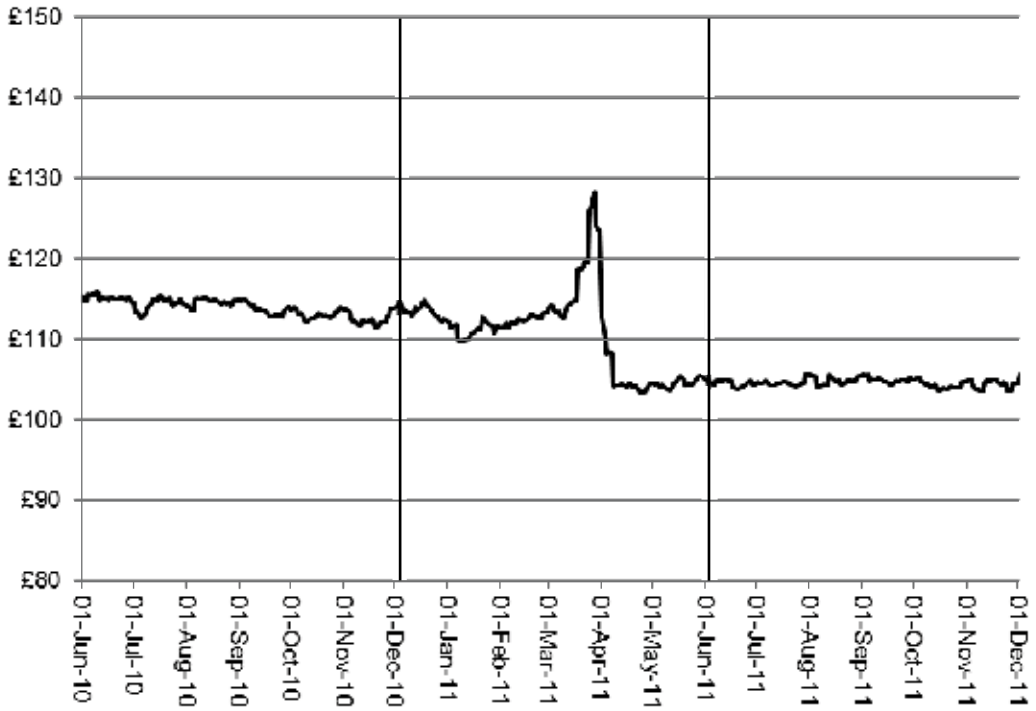
It is worth noting at this point that the SHBE data record contractual rents. We cannot be certain that this is always what tenants actually pay. Qualitative analysis from landlords (Section 5) and housing advisers (Section 6) suggests that some landlords agreed to accept a lower rent payment from their tenants following the reform without any formal contractual change – an impact that would not be picked up by examining these data. If this is an important phenomenon, we would expect to find different impacts on contractual rents when we look at the same claimants some time later, once tenancy agreements have been formally renewed. This will be investigated in a future report from the research team.

3.4.3 Maximum LHA entitlements

We now turn to the effects of the reform on maximum LHA entitlements. We define these as entitlements before means tests and non-dependent deductions, which are simple functions of rents and LHA rates. The reason for abstracting from the effects of means tests on entitlements is that it is possible that claimant's resources from the point of view of the HB means test are themselves affected by the reform. For example, if a claimant moves into work in response to LHA reductions, this increases their income and may subsequently reduce their means-tested LHA entitlement. We would not want to count this as a further reduction to LHA due to the reforms in the same way that we would count reductions to pre-means test entitlements. Our approach also guards against the risk that changes in the relative proportions of in-work and out-of-work LHA claimants over time – unrelated to any effects of the reform, and perhaps related to the state of the economy – could bias estimates of the effects of the reform by changing the proportion of LHA claimants who are entitled to maximum LHA. We ignore the effects of non-dependent deductions on entitlements for analogous reasons, as the number of non-dependents in households may change over time.

Figure 3.6 shows the evolution of average maximum LHA entitlements for new LHA claimants. There is a spike in the period immediately before the April 2011 LHA reductions. This is unsurprising given the spike in rents over the same period discussed previously, due to more families with high rents making new claims, seemingly in anticipation of the reforms. Figure 3.6 also shows maximum LHA entitlements clearly settling at a lower level after the reform than they had been pre-reform. Again, this is entirely unsurprising given that the reforms in question were explicitly designed to reduce LHA entitlements. The fall in LHA entitlements is due to a combination of reductions in LHA rates, the removal of up to £15 per week of entitlements in excess of rents (both of which are mechanical effects of the LHA reforms), and any changes in rent levels. But the key information provided by the figure is that the fall in LHA entitlements was clearly much larger than any fall in rents in the post-reform period (see Figure 3.4). This is suggestive that the majority of the initial incidence of the LHA reductions was, therefore, on new LHA claimants rather than their landlords. We verify this with the formal regression analysis presented in Table 3.4. This shows that, after accounting for time trends and control variables in the same way as previously (see previous sub-section on Contractual Rents), the estimated impact of the reforms on average maximum LHA entitlements is negative, and both economically and statistically significant, at -£8.21 per week.

Figure 3.6 Average maximum weekly LHA entitlement for new claimants by date of claim (seven-day moving average)



Source: Authors' calculations using SHBE data extract.

Table 3.4 Impact of the LHA reforms on maximum LHA entitlements of new LHA claimants (£ per week)

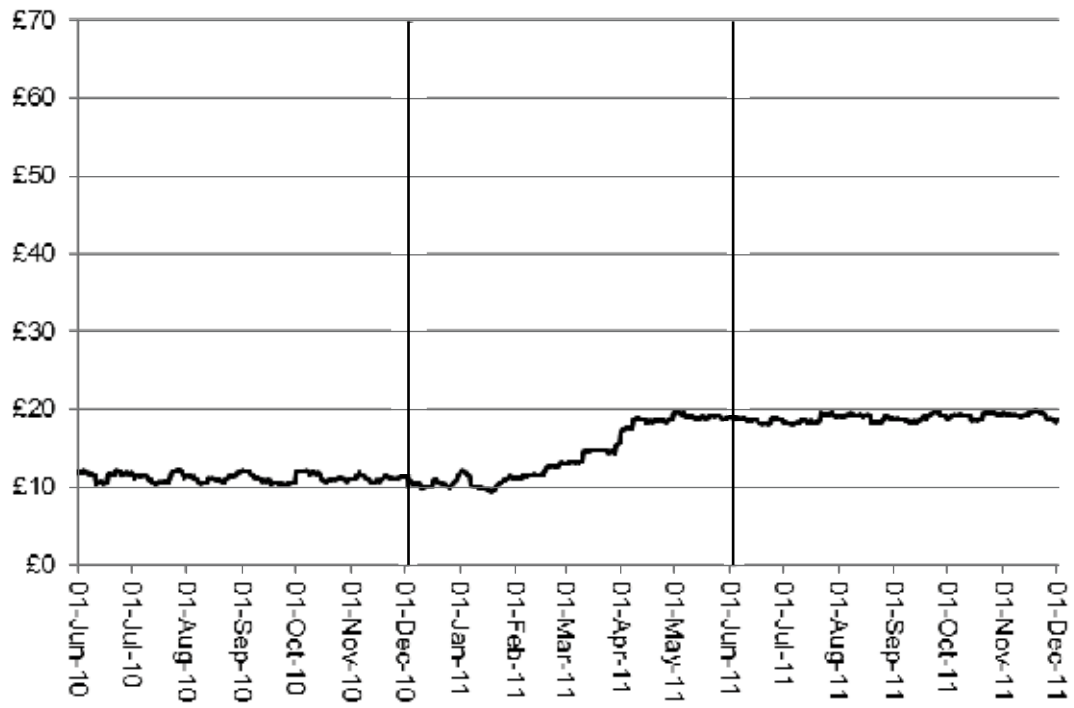
	Model				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Post-reform coefficient	-9.28***	-7.73***	-6.36***	-7.87***	-8.21***
Standard error	(1.18)	(0.63)	(0.48)	(0.52)	(0.50)
R^2	0.008	0.411	0.792	0.795	0.864
Clusters (BRMAs)	191	191	191	191	191
Number of observations	667,278	667,278	662,764	662,764	659,892

Source: As for Table 3.3.

3.4.4 Shortfall (contractual rent minus maximum LHA entitlement)

For completeness, Figure 3.7 and Table 3.5 show the raw data and regression results when using the LHA shortfall as the outcome of interest. Since this is simply the difference between rent and maximum LHA entitlement, the results here are directly implied by the results shown previously for rent and maximum LHA individually. The Figure confirms that LHA shortfalls were clearly higher on average after the reform than before it – implied by the fact that the reductions in maximum LHA entitlements were far larger than any reductions in contractual rents. Table 3.5 shows that this remains true after controlling for property type and time trends. We estimate that an economically and statistically significant amount – about £7.80 per week, on average – of the reduction in LHA entitlements in given properties (subject to our limited controls for property type) was incident on tenants.

Figure 3.7 Average shortfall between maximum LHA entitlement and contractual rent for new LHA claimants (seven-day moving average)



Source: Authors' calculations using SHBE data extract.

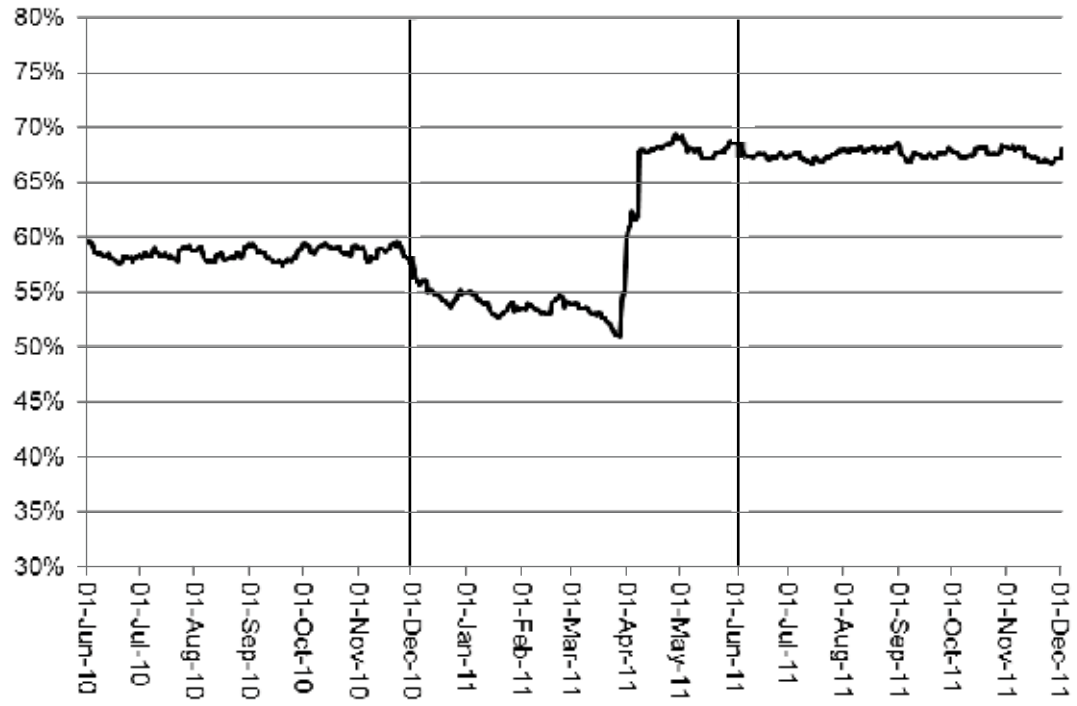
Table 3.5 Impact of the LHA reforms on shortfalls of new LHA claimants (£ per week)

	Model				
	(1)	(2)	(3)	(4)	(5)
Post-reform coefficient	7.71***	7.85***	7.97***	7.66***	7.76***
Standard error	(0.34)	(0.35)	(0.35)	(0.49)	(0.49)
R^2	0.007	0.022	0.042	0.044	0.061
Clusters (BRMAs)	191	191	191	191	191
Number of observations	667,278	667,278	662,764	662,764	659,892

Source: As for Table 3.3.

Note that, in the pre-reform period, it was possible for shortfalls to be negative, because the excess rule meant that maximum LHA entitlement could exceed rent by up to £15 per week. Part of the increase in shortfalls is, therefore, caused by the removal of the ability of claimants to keep this surplus. A possible alternative outcome of interest, which is not directly affected by the removal of the £15 per week excess, is the proportion of claimants who face a strictly positive shortfall (i.e. who have to finance their rent at least partially from non-LHA resources). Figure 3.8 and Table 3.6 below, therefore, present the analogous analysis using a binary indicator for 'having a shortfall greater than zero' as the outcome of interest.[40] These make clear that the observations above were not driven simply by the removal of the £15 excess. We estimate that the reform increased the probability of new LHA claimants in given types of properties having a maximum LHA entitlement less than their rent by 11 percentage points. This is statistically significant.

Figure 3.8 Proportion of new LHA claimants with a strictly positive shortfall by date of claim (seven-day moving average)



Source: Authors' calculations using SHBE data extract.

[40] As this is a binary outcome, the results presented in Table 3.5 are based upon probit estimation rather than Ordinary Least Squares.

Table 3.6 Impact of the LHA reforms on probability of having a strictly positive LHA shortfall for new claimants

	Model				
	(1)	(2)	(3)	(4)	(5)
Post-reform marginal effect	0.09***	0.09	0.09	0.09***	0.11**
Standard error	(0.01)	(0.08)	(0.08)	(0.01)	(0.04)
Clusters (BRMAs)	191	191	191	191	191
Number of observations	667,278	667,277	662,669	662,669	659,797

Notes and source: As for Table 3.3.

3.4.5 Summary: the incidence of LHA reductions in given property types

Combining the results from the final columns of Tables 3.3 to 3.5 (i.e. from the models which account for any underlying time trends unrelated to the reform, as well as any changes in the household types and ages of claimants), we can form an estimate of a key aspect of the impacts of the LHA reductions: the extent to which reductions in LHA entitlements in given types of properties (defined by LA, BRMA and number of bedrooms) were incident on tenants (via increased shortfalls) or their landlords (via reduced rents). We estimated that the reform reduced maximum LHA entitlements in given property types by an average of £8.21 per week, while resulting in rent reductions in given property types averaging £0.46 per week. As shown in Table 3.7, this implies that 94 per cent of the incidence of reductions in LHA entitlements in given properties resulting from the reforms was on tenants, with the other 6 per cent on landlords. Note that, since the change in rents resulting from the reforms is not statistically significantly different from zero, the incidence on landlords is not statistically significantly different from zero either.

The results are robust to the precise time periods chosen to form our estimating sample (to avoid bias in our estimates arising from anticipation effects, we exclude a window of data around the reform date – see the beginning of this sub-section for details). If we extend the window of data by one month in the pre-reform period and one month in the post-reform period, thereby including December 2010 and May 2011, the estimate of the reduction in maximum LHA entitlements resulting from the reforms changes from £8.21 per week to £8.92 per week, and 96 per cent of that is estimated to be incident on tenants.

It is important to note that the indicators of property type captured in the SHBE data – LA, BRMA and number of bedrooms – may not be the only relevant ones. Even after controlling for these aspects of property type, new LHA claimants after the reforms could be in properties that would have been cheaper than the properties rented by new claimants before the reforms even in the absence of reforms. If true, this would imply that we are picking up changes in housing choices by LHA claimants as reductions in rents by landlords, and hence under-estimating the true incidence of the LHA reductions on tenants.

Table 3.7 Estimated incidence of reductions to maximum LHA entitlements, conditional on LA, BRMA and number of bedrooms in the property

	Impact of the reform on weekly:			% of incidence on:	
	Maximum LHA amount	Contractual rent	Shortfall	Tenants	Landlords
Estimated effect	-£8.21***	-£0.46	+£7.76***	94%***	6%
Standard error	(0.50)	(0.64)	(0.49)	-	-
R^2	0.864	0.513	0.061	-	-
Clusters (BRMAs)	191	191	191	191	191
Sample size	659,892	659,892	659,892	659,892	659,892

Notes: The first three columns of numbers are from model 5 in Tables 3.2, 3.3 and 3.4 respectively. The percentage of the incidence on tenants is (- change in shortfall) / (change in maximum LHA). The percentage of the incidence on landlords is (change in contractual rent) / (change in maximum LHA).
Source: Authors' calculations using SHBE data extract.

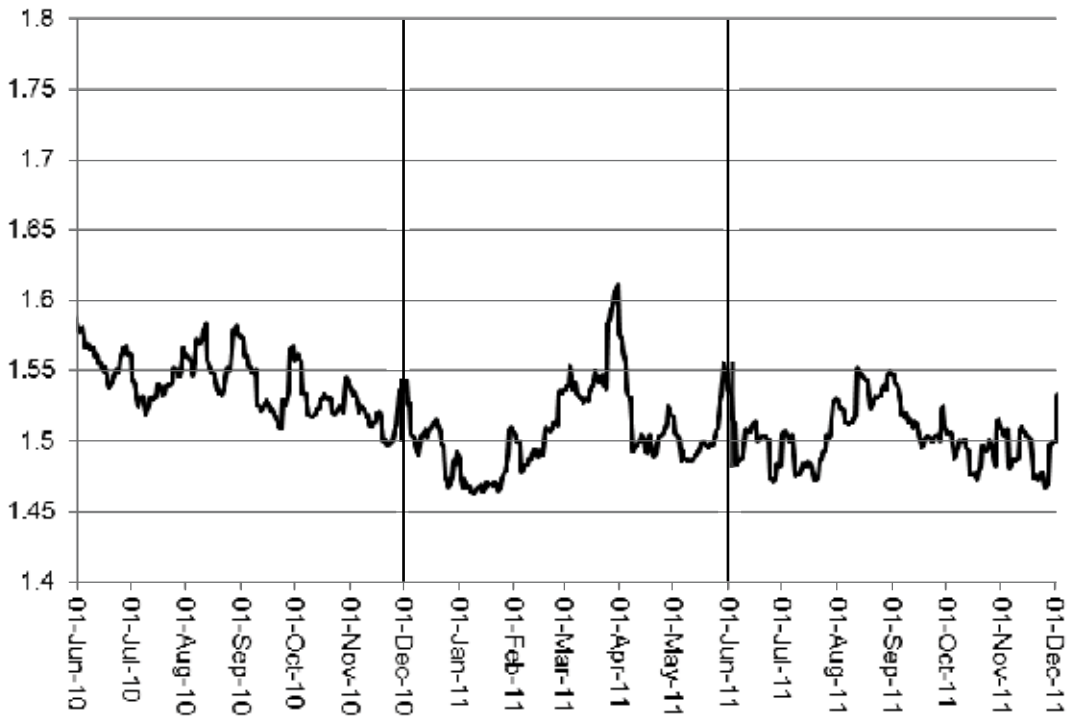
3.4.6 Property size

The previous analysis focused on the incidence of LHA reductions in given types of property – specifically, the extent to which they were incident on tenants (via increased shortfalls) or landlords (via reduced rents). But if the reform also caused changes in the types of property rented by LHA claimants, this would reflect another mechanism by which the reforms were incident on tenants. It is, therefore, important to examine what happened to property types in order to get a clearer idea of the overall impact of the reforms.

Results shown above suggested that much of the raw change in contractual rents observed following the reform can be explained by changes in the (limited) indicators of property type available in the SHBE data, namely number of bedrooms, LA and BRMA. In itself, this does not establish that such changes were caused by the reforms. However, the spatial analysis presented in Section 2 of these interim research outputs provided evidence that the reforms had indeed caused some geographical shifts in new LHA claims, especially in terms of the reduction in on-flows in central London since April 2011. Here we estimate the effects of the reforms on the other indicator of property type available in the SHBE data – property size, as captured by the number of bedrooms.

Figure 3.9 shows the average number of bedrooms for new claimants according to the start date of their claim. As elsewhere in this section, this is the actual number of bedrooms (a measure of property size) rather than the number of bedrooms that claimants are deemed to need for the purposes of calculating LHA entitlements (which is a measure of household size and structure). This series is more volatile than the others examined so far. One possible explanation is that, as this is not a variable that needs to be accurately reported for households' LHA entitlements to be assessed, LAs are less meticulous in their reporting of it. Nevertheless, the figure suggests two things. First, property size increased significantly in the period just before the April 2011 reforms. This is likely to reflect the increase in the household size of new claimants over this period which, as discussed, may be due to anticipation effects of the reform. Second, ignoring the window of data immediately around the reform date, there does seem to be some reduction in property size following the introduction of the reform, but it is unclear from the raw data whether this is simply the continuation of a downwards trend that began before the reform.

Figure 3.9 Average number of bedrooms for new LHA claimants by date of claim (seven-day moving average)



Source: Authors' calculations using SHBE data extract.

In order to isolate the impact of the LHA reforms on property size from other factors, Table 3.8 shows results for linear regressions where the dependent variable is the number of bedrooms in the property a claimant chooses to rent. Again, additional controls are added when moving from left to right across the Table: model (1) does not contain any control variables; model (2) adds time trends, which are allowed to differ before and after the reform; and model (3) also controls for the age and household type of claimants. The results show that there was a slight fall in the number of bedrooms in the properties claimants chose following the reform, but that this is simply the result of the continuation of a trend from before the reform: adding time trends changes the estimated impact of the reform on the number of bedrooms from negative to positive, and the estimated effect disappears altogether once controls for age and household type are added.

Table 3.8 Impact of the LHA reforms on number of bedrooms for new LHA claimants

	Model		
	(1)	(2)	(3)
Post-reform coefficient	-0.04***	+0.03***	-0.00
Standard error	(0.01)	(0.02)	(0.01)
R^2	0.000	0.000	0.407
Clusters (BRMAs)	191	191	191
Number of observations	665,954	665,954	663,076

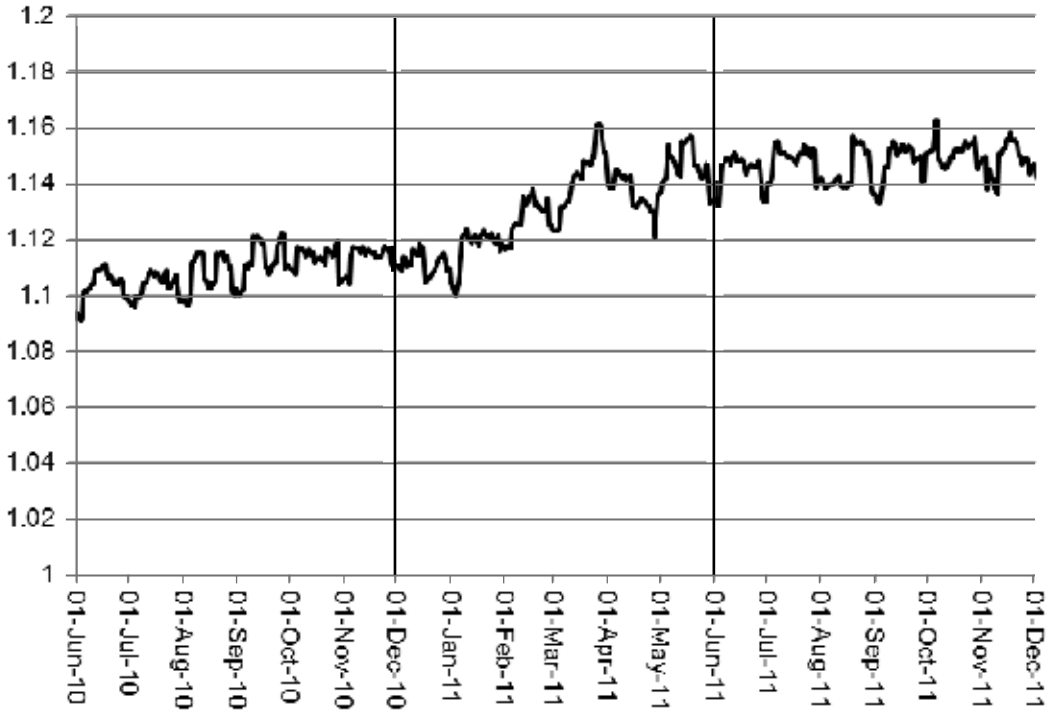
Notes: *** Statistically significant at one per cent level, ** Statistically significant at five per cent level, * Statistically significant at ten per cent level. The dependent variable is number of bedrooms in the property, where shared accommodation is treated as a zero. Model (1) does not contain any control variables; model (2) adds time trends, which are allowed to differ before and after the reform; and model (3) also adds controls for the age of the claimants and their family type, as defined in notes to Table 3.2. Standard errors are robust to heteroscedasticity and clustering at the BRMA level.
Source: Authors' calculations using SHBE data from June 2010 to November 2010 and June 2011 to November 2011.

A related way of examining changes in housing choices as a result of the reforms is to examine what happened to the number of people per bedroom in the households of LHA recipients. As Figure 3.10 and Table 3.9 show, the results closely reflect those obtained when looking just at the number of bedrooms. The raw data show an increase in the average number of people per bedroom over time, and the regression analysis suggests that this is due to general time trends rather than effects of the LHA reforms.

In summary, we do not find evidence as yet that the April 2011 LHA reductions have affected the average size of the properties rented by LHA recipients, as captured by the number of bedrooms; or the average size of rented properties relative to the number of people inhabiting them, as captured by people per bedroom.

Of course, these very specific results do not imply that the reforms have not affected housing choices. It may be the case that metrics of property type not recorded in the SHBE data did change as a result of the reforms; and it is shown in Section 2 that the reforms appear to have had some impact on the geographical distribution of new LHA claimants, notably with a marked reduction in on-flows in central London districts.

Figure 3.10 Average number of people per bedroom for new LHA claimants by date of claim (seven-day moving average)



Source: Authors' calculations using SHBE data extract.

Table 3.9 Impact of the LHA reforms on number of people per bedroom for new LHA claimants

	Model		
	(1)	(2)	(3)
Post-reform coefficient	+0.04***	+0.01	0.00
Standard error	(0.01)	(0.01)	(0.01)
R^2	0.001	0.001	0.356
Clusters (BRMAs)	191	191	191
Number of observations	665,954	665,954	663,076

Notes: *** Statistically significant at one per cent level, ** Statistically significant at five per cent level, * Statistically significant at ten per cent level. The dependent variable is the number of people per bedroom in the property. Shared accommodation cases are treated as having one bedroom for these purposes. Model (1) does not contain any control variables; model (2) adds time trends, which are allowed to differ before and after the reform; and model (3) also adds controls for the age of the claimants and their family type, as defined in notes to Table 3.2. Standard errors are robust to heteroscedasticity and clustering at the BRMA level.
Source: Authors' calculations using SHBE data from June 2010 to November 2010 and June 2011 to November 2011.

3.5 Sub-group analysis

This sub-section examines how the impacts of the LHA reforms studied in the previous sub-section varied by sub-group.

3.5.1 Variation in impact by household type and age

The impact on claimants by household type is analysed in Table 3.10 below. Unsurprisingly, the biggest cash losses in LHA entitlement from the reforms were among households with children, who are entitled to more LHA in cash terms, all else being equal. The estimated incidence of the LHA reductions on single people tends to be lower than for other groups, particularly for single men without children. We might expect this, all else being equal, if individuals without partners and/or children are more mobile than others. They may be better able to move to properties where rents fall in response to the reform, as suggested later, or better able to credibly threaten to move and thereby negotiate rent reductions in their existing property.

Single adults without dependent children were also less likely than households with children to benefit from the £15 excess that claimants were allowed to keep if their LHA rate exceeded their rent before the reforms, as their rents more often equalled or exceeded their LHA rates (see Table 3.2). As discussed in sub-section 3.2, the incidence of any reduction in LHA resulting from the abolition of the £15 excess must be on tenants; and indeed the extent to which tenants lose from this reform can exceed the amount by which their LHA entitlement is reduced (i.e. more than 100 per cent of the LHA reduction can be incident on tenants), because it can result in a transfer from tenants to landlords (rather than from tenants to the taxpayer). Hence, this may also play a role in explaining why less of the LHA reductions appear to have been incident on single adults without dependent children. Future work will determine the extent to which the incidence of the reform varies according to whether a household benefited from the £15 excess prior to the reform, by exploiting data on the stock of existing LHA claimants.

Table 3.10 Impact of the reforms by household type

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA amount	Contractual rent	Shortfall	Tenants %	Landlords %
Single men	-£7.19***	-£1.28**	£5.91***	82***	18**
Single women	-£7.13***	-£0.37	£6.76***	95***	5
Couples without children	-£6.84***	£0.50	£7.35***	107***	-7
Lone parents	-£10.22***	-£0.41	£9.82***	96***	4
Couples with children	-£9.51***	£0.89	£10.40***	109***	-9
All	**-£8.21*** **	**-£0.46**	**£7.76*** **	**94*** **	**6**

Notes: *** Statistically significant at one per cent level, ** Statistically significant at five per cent level, * Statistically significant at ten per cent level. The regression specifications underlying these results are the same as in Table 3.7 (and the final columns of Tables 3.3 to 3.5), except that we now include interactions terms between the dummy variable indicating a post-reform claim and a series of dummy variables indicating membership of one of the mutually exclusive subgroups defined in the left-hand column. The first three columns of numbers are estimated coefficients on these interaction terms.
Source: Authors' calculations using SHBE data extract.

Table 3.11 shows the impact of the LHA reforms according to the age of the claimant. Claimants under the age of 25 tended to lose less LHA entitlement in cash terms as a result of the reforms, as we would expect, given that those aged under 25 who are single and without children are entitled only to the lowest LHA rates (the shared room rate). The results also suggest that smaller proportions of the reductions in maximum LHA entitlements were incident on younger claimants than older ones. Again, this could be explained by younger claimants being more mobile.

Table 3.11 Impact of the reforms by age of claimant

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA amount	Contractual rent	Shortfall	Tenants %	Landlords %
Under 25	-£6.83***	-£1.00	£5.83***	85***	15
25–34	-£8.67***	-£0.63	£8.04***	93***	7
35–44	-£8.73***	-£0.13	£8.60***	99***	1
45–54	-£8.57***	-£0.00	£8.57***	100***	0
55–64	-£8.19***	-£0.15	£8.05***	98***	2
65+	-£7.85***	£0.84	£8.69***	111***	-11
All	**-£8.21*** **	**-£0.46**	**£7.76*** **	**94*** **	**6**

Notes and source: As for Table 3.10.

3.5.2 Geographic variation in the impact of the reforms

Since the nature of the rental market varies by region, we might expect some variation in the impact of the reforms geographically. One potential reason for geographical variation in the impacts of the reforms is variation in the density of LHA recipients in an area relative to the total size of the private rental market. As discussed in sub-section 3.2, if the private rental market is such that landlords consider letting to both LHA and non-LHA claimants, economic theory would predict that the incidence on landlords would be greater in areas where the relative density of LHA recipients is greater. On the other hand, if the private rental market is segmented such that given landlords let only to LHA or non-LHA claimants, we would not expect this to be important: instead, the crucial factors would be those that affect the responsiveness to rent levels of supply and demand for rental accommodation specifically in the LHA sector. This may include factors such as planning rules (on the supply side) and the mobility of LHA claimants (on the demand side).

Table 3.12 below reports results after splitting claimants up according to the share of private rented households in their LA who are LHA claimants. Due to data availability constraints, we are forced to do this somewhat imprecisely: although we can directly observe in the SHBE data the number of LHA claimants in each LA (the numerator), we have to rely on 2001 Census estimates of the number of private renting households in each LA (the denominator). Clearly there will be a tendency for the denominator to be under-estimated due to growth in the number of households since 2001, but what matters here is our ranking of LAs (by quintile group). Measurement error will be introduced to the extent that some LAs have experienced faster growth in the number of private rented households than others over the past decade. 2011 Census data will soon be available, and we will, therefore, be able to refine this analysis in a future report.

Table 3.12 shows that, according to our central estimates, the incidence of the LHA reductions has not varied significantly according to our measure of the density of LHA claimants in local private rental markets. This could be taken as tentative evidence that the rental markets for LHA and non-LHA claimants are segmented to a substantial degree. But it is also possible that this measure of density is correlated with some other factor which affects the incidence of the reforms in the opposite direction. Importantly, the likely measurement error in our ranking of LAs by the share of LHA recipients in the PRS (see above) would also mean that any estimates of differences by LAs along this dimension are downwards biased. We may, therefore, be able to draw more confident conclusions on this point in a future report, where we will be able to refine the measure due to the availability of new data.

Table 3.12 Impact of the LHA reforms by share of LHA recipients in private rented sector

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA amount	Contractual rent	Shortfall	Tenants %	Landlords %
1st (lowest)	-£9.37***	-£0.09	£9.28***	99***	1
2nd	-£8.17***	-£0.18	£7.99***	98***	2
3rd	-£8.00***	-£1.05	£6.95***	87***	13
4th	-£8.28***	-£0.25	£8.04***	97***	3
5th (highest)	-£7.70***	-£0.45	£7.25***	94***	6
All	**-£8.21*** **	**-£0.46**	**£7.76*** **	**94*** **	**6**

Notes and source: As for Table 3.10.

Table 3.13 presents an analysis by Government Office Region. The reduction in maximum LHA rates was largest in London, where rents are highest. As some of the reforms particularly reduced support for those with the highest rents, this is unsurprising.[41] The central estimates also suggest that, conditional on property type, a larger share of the incidence of reductions to maximum LHA entitlements has been on landlords in the West Midlands, Scotland and London. It should be noted, however, that the results by region are estimated relatively imprecisely: only in the West Midlands can the hypothesis that the impact on the price of rental accommodation was zero (and hence that none of the incidence was on landlords) be rejected at the 10% significance level.[42]

41 Indeed, the national caps on LHA rates are binding only in certain areas of London.

42 The relatively high degree of uncertainty surrounding estimates of effects by region is not surprising. Unobserved determinants of changes in rents are very likely to be correlated spatially, since rents in similar areas are likely to be affected by many common factors. This makes it less clear that changes in average rents within regions are due to the impacts of the reforms rather than normal volatility in rent levels within regions. We account for this by allowing for clustering of error terms at the BRMA level when estimating standard errors (see Section 3.2).

Table 3.13 Impact of the LHA reforms by Government Office Region

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA amount	Contractual rent	Shortfall	Tenants %	Landlords %
London	-£10.69***	-£2.17	£8.52***	80***	20
East of England	-£9.44***	-£0.03	£9.41***	100***	0
Scotland	-£9.19***	-£2.22	£6.97***	76***	24
South East	-£8.11***	-£0.47	£7.64***	94***	6
North West	-£7.84***	£0.06	£7.90***	101***	-1
Wales	-£7.77***	£1.82	£9.60***	124***	-24
North East	-£7.35***	-£0.50	£6.85***	93***	7
Yorkshire and Humber	-£7.31***	£0.22	£7.54***	103***	-3
West Midlands	-£7.21***	-£1.73*	£5.48***	76***	24*
East Midlands	-£7.20***	£0.69	£7.89***	110***	-10
South West	-£6.39***	£0.78	£7.18***	112***	-12
All	**-£8.21*** **	**-£0.46**	**£7.76*** **	**94*** **	**6**

Notes and source: As for Table 3.10.

To investigate more closely what might be driving these results, and to gain a richer understanding of how the incidence of the reforms has varied geographically, Table 3.14 splits people according to area classifications of local authorities (as also used in Section 2). Table 3.14 suggests that the price of rental accommodation fell by more (and hence the proportion of changes in LHA incident on landlords rather than their tenants) in urban areas outside of inner London. In particular, we estimate that about one-fifth of reductions in LHA in given property types were incident on landlords via lower rents in the 'Cities and Services' category, which includes substantial urban areas such as Manchester, Liverpool and Cambridge. This is statistically significant. There is also evidence of more incidence on landlords in the suburbs of London, although we caution that this is estimated much more imprecisely (for the reasons given in footnote 42 above). A possible interpretation is that these categories capture more mobile groups of people. For example, perhaps LHA claimants living in the suburbs of London do not have strong preferences about precisely where around London they locate, and can, therefore, credibly threaten to move in the absence of rent reductions.

3.5.3 Summary

There is evidence that single claimants and younger claimants were able to pass more of the reduction in their maximum LHA amounts on to their landlords in the form of lower rents.

There is also some evidence that more of the LHA reductions were incident on landlords via reduced rents in urban areas outside London and in the suburbs of London; or, by Government Office Region, in the West Midlands, Scotland and London. However, these results are estimated relatively imprecisely, limiting the extent to which firm conclusions can be drawn.

Table 3.14 Impact of the LHA reforms by area type classification

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA amount	Contractual rent	Shortfall	Tenants %	Landlords %
London Centre	-£19.85***	£1.91	£21.76***	110***	-10
London Cosmopolitan	-£14.35***	-£0.06	£14.29***	100***	0
London Suburbs	-£7.03***	-£3.68	£3.35*	48*	52
Cities and Services	-£8.24***	-£1.61**	£6.63***	80***	20**
Prospering UK	-£7.41***	£0.56	£7.96***	107***	-7
Large Seaside Towns	-£7.79***	-£0.23	£7.56***	97***	3
Coastal and Countryside	-£7.56***	£0.37	£7.93***	105***	-5
Mining and Manufacturing	-£7.38***	£0.33	£7.71***	104***	-4
All	**-£8.21*** **	**-£0.46**	**£7.76*** **	**94*** **	**6**

Notes and source: As for Table 3.10.

3.6 Conclusions

One possible mechanism by which the April 2011 LHA reductions could have been incident on tenants is through having less of their rents covered by LHA in given types of properties. Based on comparisons between new claimants after the reforms and new claimants before the reforms who rented similar properties, and allowing for local time trends independent of the reforms, we have estimated that the majority (94 per cent) of the LHA reductions were indeed incident on tenants (via increased shortfalls) rather than landlords (via reduced contractual rents).

Another possible mechanism by which the reforms could have been incident on tenants is if it led them to rent different kinds of properties in response. We do not find evidence that the reforms have yet affected the sizes of the properties claimants rent (as measured by the number of bedrooms). It has been shown elsewhere in these interim research outputs (Section 2), however, that there is evidence of some effects of the reforms on the geographical location of new LHA claimants in the London area. It is possible that there are other aspects of property type, not recorded in the SHBE data used here, which also changed as a result of the reforms. If so, this would act to increase the incidence on tenants further. The next stage of reporting will explore this using indicators of housing quality at a local level.

There is variation in these results by sub-group. Single claimants and younger claimants seem to have been able to pass a greater share of the incidence of the reforms onto landlords via reduced rents. We have also provided evidence that the incidence on landlords was greater in urban areas outside London, and in the suburbs of London.

The headline results may initially appear surprising: most of the relevant academic literature has found that rent subsidies have a large effect on landlords by resulting in higher rents, which would imply that reductions to rent subsidies significantly reduce rents. What might the explanations be? As explained, the removal of the £15 per week excess that claimants could keep if their maximum LHA entitlement exceeded their rent is different to a typical kind of reduction to rent subsidies, as there is no reason to expect rents to fall as a result (and indeed they may instead rise). This, therefore, seems extremely likely to be a factor.

Nevertheless, the removal of the £15 excess was only one of several LHA measures implemented at the same time, and was expected to raise less than half as much for the exchequer as the switch for LHA rates from the 50th to the 30th percentile of local rents, for example.

There may well be other reasons for the low incidence of the reforms on landlords estimated here, and there are various reasons to be cautious about the results at this stage. First, we have examined the impact of the reforms on rents by looking at new LHA claimants only right at the start of their claims and only in short periods before and after the reforms were implemented. It may take time for market rents to adjust in response to the reform, and it may take tenants time to either negotiate a lower rent or move to cheaper accommodation (see Section 5 and Section 6). Under either of these hypotheses, the LHA reductions would lead to a larger reduction in rents in the long-run than in the short-run, and, therefore, the data examined here may not represent the ultimate post-reform equilibrium in the rental market.

Second, with the administrative data used here it is possible to examine the impact of the reform only on contractual rents. There is no guarantee that this is what tenants are actually paying in all cases. The qualitative interviews with landlords (Section 5) and housing advisers (Section 6) indicated that one response to the reform has been for some landlords to informally accept lower rents from LHA tenants without changing their contractual rents. If this response is widespread, our estimates may be understating the real incidence of the reform on landlords. Future work will use additional data from after the introduction of the reform to investigate its longer term impact on landlords and tenants. In particular, it will examine whether the rents of those new claimants studied in this report changed later, the impact of the reforms on later new claimants of LHA, and the impact on existing claimants, who started to be affected by the reforms in January 2012. This analysis will, therefore, provide a much fuller assessment of the long-run impact of the LHA reforms.