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Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

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Summary

This report is one of a series of research papers that form the final outputs from the independent evaluation of the recent changes to Local Housing Allowances (LHAs) and Housing Benefit in the private rented sector in Great Britain. The evaluation has been undertaken by a research consortium from the Centre for Regional Economic and Social Research at Sheffield Hallam University, the Institute for Fiscal Studies, the Blavatnik School of Government at the University of Oxford and Ipsos MORI.

The LHA measures examined in this report were rolled out to existing claimants between April 2011 and December 2012. The dates at which particular claimants were affected by the reforms were linked to their annual claim reassessment dates. Otherwise-identical individuals observed at the same time in the same area could therefore face different LHA systems. The analysis in this report exploits this feature of the roll-out in order to estimate the causal impacts of the reforms on existing claimants, for up to 11 months after being rolled onto the reformed system, using administrative data on Housing Benefit claimants in Great Britain.

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Executive summary

- The Local Housing Allowance (LHA) measures examined in this report were rolled out to existing claimants between April 2011 and December 2012. The dates at which particular claimants were affected by the reforms were linked to their annual claim reassessment dates. Otherwise-identical individuals observed at the same time in the same area could therefore face different LHA systems. The analysis in this report exploits this feature of the roll-out in order to estimate the causal impacts of the reforms on existing claimants, for up to 11 months after being rolled onto the reformed system, using administrative data on Housing Benefit (HB) claimants in Great Britain (GB).
- Eleven months after being rolled onto the reformed system, the LHA reforms had reduced existing claimants' maximum entitlements in given property types by an estimated average of **£6.84** per week. This was comprised of average contractual rent reductions of **£0.79** per week and reduced LHA relative to contractual rent of **£6.06** per week. This implies that, in aggregate, 89 per cent of the incidence of reduced LHA entitlements was on tenants and 11 per cent on landlords. However, this masks substantial variation across some claimant groups (see below).
- The reforms reduced the probability that claimants move house by an estimated 0.3 percentage points (ppts) per month, on average, when transitional protection began (protection which would be forfeited with a house move); but increased the probability that claimants move house by an estimated 0.5ppts per month, on average, 11 months after being rolled onto the reformed system.
- There is some evidence that the reforms have reduced the number of bedrooms that claimants choose to rent, on average. We do not find significant effects of the reforms on the types of locality in which claimants choose to live, as measured by an indicator of local deprivation levels.
- The reforms' impacts in reducing LHA entitlements in given types of property were unsurprisingly higher for demographic and geographic groups who had higher entitlements to start with. These include claimants in London (average reduction of £13.39 per week) and lone parent claimants (£8.43 per week). The reduction in entitlements in London was also relatively large in proportionate terms, at 6.6 per cent of January 2011 entitlements, as opposed to 5.4 per cent for all claimants.
- There is evidence that the reforms led to relatively substantial, and statistically significant, reductions in rental values in the suburbs of London and in the East Midlands. In both of those areas – and in contrast to other parts of GB – the majority of the estimated incidence of LHA reductions fell on landlords rather than tenants. Our estimates also suggest that the small minority (six per cent) of claimants living as a couple without dependent children saw a fall in their average contractual rent level due to the reform which was sufficient to offset their reduction in LHA entitlement almost entirely (i.e. almost all of the incidence seems to be on their landlords). There is no clear pattern in how the estimated incidence of the LHA reductions varies with the density of LHA claimants in local private rental markets.

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- Other claimant groups who saw relatively large reductions in LHA include those particularly likely to be affected by the increased scope of the Shared Accommodation Rate (SAR), the national LHA caps (binding in parts of inner London), and the abolition of the five-room rate. There is evidence that the property choices of each of these groups were affected. Claimants likely to be affected by the SAR change – most single childless individuals aged 25 to 34 who were not previously in shared accommodation – were, due to the reform package, an estimated 12.9ppts more likely to be in shared accommodation 11 months after the point of impact; those likely to be affected by the national caps were more likely to move out of the capped areas of Inner London; and there is some evidence that those likely to be affected by the abolition of the five-room rate were more likely to move to cheaper properties with fewer bedrooms, reducing the average fall in their income after housing costs from £17.52 to £12.56 per week.
- For groups particularly likely to be affected by the SAR change and the abolition of the five-room rate, the reforms reduced rental values by a statistically significant amount (£4.80 and £11.69 per week respectively). As a result, more than one-third of the incidence of the LHA reductions in given properties for these groups fell on their landlords.
- There are important limitations to this analysis. First, the administrative data used record contractual rents, and there is no guarantee that this is what tenants are actually paying in all cases. If landlords informally accept lower rents from tenants without changing contractual rents, we will tend to understate the true incidence of the reforms on landlords and overstate the incidence on tenants (all else equal). Second, with these data we can observe only limited property characteristics. If, due to the reforms, claimants live in property types where contractual rents are lower for reasons unobservable in the Single Housing Benefit Extract (SHBE) data (for example, the quality of the housing), we would be wrongly attributing changes in housing choices by LHA claimants to rent reductions by landlords. Alternatively, the same properties may be presented or maintained to a lower standard than would have been the case in the absence of reform. If important, these factors could lead us to understate the true incidence of the LHA reductions on tenants. Third, responses to the package of reforms may continue to develop beyond the 11 months from the point of impact that were studied here. If so, the estimates in this report would not capture the long-run impacts of the reforms.

1 Introduction

This report is one of a series of research papers that form the final outputs from the independent evaluation of the recent changes to Local Housing Allowances (LHAs) and Housing Benefit (HB) in the private rented sector (PRS) in Great Britain (GB). The evaluation has been 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 Blavatnik School of Government at the University of Oxford and Ipsos MORI (IM). It is funded by the Department for Work and Pensions (DWP), the Department for Communities and Local Government (DCLG), the Scottish Government and the Welsh Government.

The report applies econometric techniques to administrative data on HB claims assessed under the LHA rules, in order to estimate the impact of these recent changes on existing LHA claimants.¹ This follows similar analysis published at the interim reporting stage of the evaluation (Brewer *et al.*, 2013), which looked at the impacts on new and repeat claimants, who were the first to be rolled onto the reformed system.

The reforms considered here are:

- setting LHA rates at the 30th percentile of PRS rents rather than the median (50th percentile);
- removal of the £15 per week excess;
- abolition of the five-bedroom LHA rates;
- capping the LHA rates at £250, £250, £290, £340 and £400 per week for the shared accommodation, one-bedroom, two-bedroom, three-bedroom and four-bedroom rates respectively;
- extension of the coverage of the Shared Accommodation Rate (SAR), to include most single adults without dependent children aged between 25 and 34 not living in shared accommodation;
- increase in central government funding for Discretionary Housing Payments (DHPs) by £10 million in 2011/12 and £40 million in each of 2012/13, 2013/14 and (although this will not be relevant for the data analysed here) 2014/15.

This is almost the same set of measures that was considered in the evidence presented at the interim reporting stage. That analysis was based on a comparison of the flow of new and repeat claimants shortly after the reforms took effect in April 2011² with the flow of new and repeat claimants shortly before the reforms took effect. A brief summary of the findings of that work can be found in Chapter 2.

These measures were rolled out to existing LHA claimants between April 2011 and December 2012. The dates at which particular claimants were rolled onto the reformed

¹ We use 'LHA claimants' as shorthand for 'Housing Benefit claimants assessed under the LHA rules'.

² This excludes the SAR change, which did not affect new and repeat claimants until January 2012 and hence was not considered in the econometric analysis for the interim report.

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system were linked to their annual claim reassessment date. As a result, otherwise-identical individuals observed at the time in the same area could face different LHA systems. The analysis in this report exploits this feature of the roll-out to estimate the causal impacts of the reforms on existing LHA claimants.

The main outcomes of interest are LHA entitlements, contractual rents, the difference between the two, mobility of claimants and the types of properties that they inhabit. We also examine whether the impacts of the LHA reforms differ across subgroups of claimants, according to their demographic characteristics, their location and the elements of the reform package that they were likely to be affected by.

The report is organised as follows. Chapter 2 outlines what impacts of the reforms might be expected, with reference to economic theory and previous relevant literature. Chapter 3 describes the data and econometric methods used in order to obtain empirical estimates of the impacts of the reforms. Chapter 4 presents and discusses the main results of the analysis for Great Britain as a whole. Chapter 5 presents analysis by demographic and geographic subgroup. Chapter 6 considers groups likely to be affected by specific elements of the reform package. Chapter 7 concludes with a summary of results and a reminder of the main limitations of this work.

2 Theory and previous evidence

2.1 What effects should we expect from changes in rent subsidies?

Housing Benefit (HB) is a rent subsidy. For claimants assessed under the Local Housing Allowance (LHA) rules, the maximum subsidy depends upon their household type and the Broad Rental Market Area (BRMA) in which they live.

Economic theory suggests that the incidence of rent subsidies – in other words, whose financial position is actually affected by them – depends on the details of the rental market. If the rent levels that landlords charge are completely insensitive to rent subsidies, then it 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 covers (some of) the cost, so landlords may charge higher rents in response. In that case, at least some of the subsidy 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 rental values is crucial in this regard. The reforms considered in this report all act to reduce LHA entitlements for some subset of claimants. Consider first the four reforms which reduced claimants’ 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 private rented sector (PRS) distribution; the introduction of the national caps; the abolition of the five-bedroom rate; and the extension of the Shared Accommodation Rate (SAR) to cover single childless individuals aged 25 to 34. We would expect these to lead to some combination of the following:

- claimants facing a gap, or a larger gap than they previously faced, between their rent and the HB 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 choosing cheaper accommodation; or
- claimants spending less on rent for a given type of accommodation due to landlords reducing rents towards the new LHA level.

The first two items in this 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 additional savings to the exchequer.

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The overall incidence of the LHA reforms on landlords and tenants is therefore an empirical question. It will depend 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 changes in rents, these reforms would not affect rents significantly. Conversely, if the supply of rented accommodation to the LHA sector was unresponsive ('inelastic') to changes in rent levels, rents would fall. For example, if supply were completely fixed ('perfectly inelastic'), then in market equilibrium – where supply equals demand – the level of demand after the subsidy reduction would have to be exactly the same as the level of demand before the reduction. For that to be the case, rents would need to fall one-for-one with rent subsidies, so that tenants are unaffected overall. Factors affecting the elasticity of supply could include things such as planning regimes, which determine how easy it is to buy up new properties to let. Figure 2.1 illustrates these points by showing supply and demand in a competitive market. In the case where supply is responsive to rent levels, 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).
- **The share of LHA tenants within a particular rental market, or the level of segmentation between the LHA rental market and the wider market.** If landlords are willing and able to let their properties to non-LHA recipients, they will be less likely to reduce their rents in response to the LHA reductions. Conversely, 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 landlords would in principle let to either LHA or non-LHA claimants, the share of LHA claimants in the PRS 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 elasticities of supply (see above) and demand (see below) for rental accommodation specifically in the LHA sector. Qualitative evidence collected from interviews with landlords as part of this evaluation suggests that such segmentation does exist in some parts of the PRS (Beatty *et al.*, 2013).
- **How responsive tenants are to changes in rent levels.** If demand for rental property is more sensitive to its cost to LHA claimants, rents will fall by more in response to any reductions in rent subsidies. 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 location. 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 sensitive demand is to rent levels, the more landlords will have an incentive to reduce rent levels to regain some of the demand, and hence the lower rents will be in the new equilibrium. Figure 2.2 again shows supply and demand in a competitive 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).

Figure 2.1 Impact of LHA reductions on rental market depending on responsiveness of supply of rented accommodation to rent levels

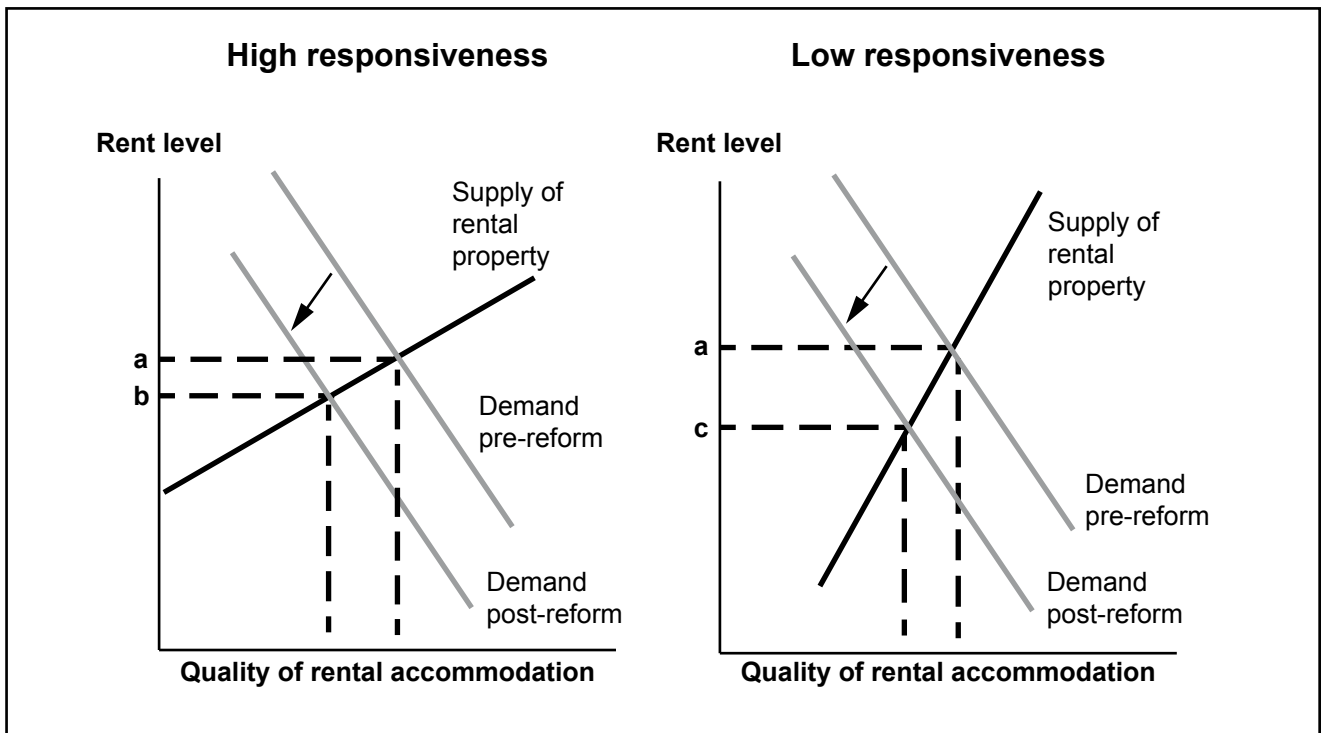
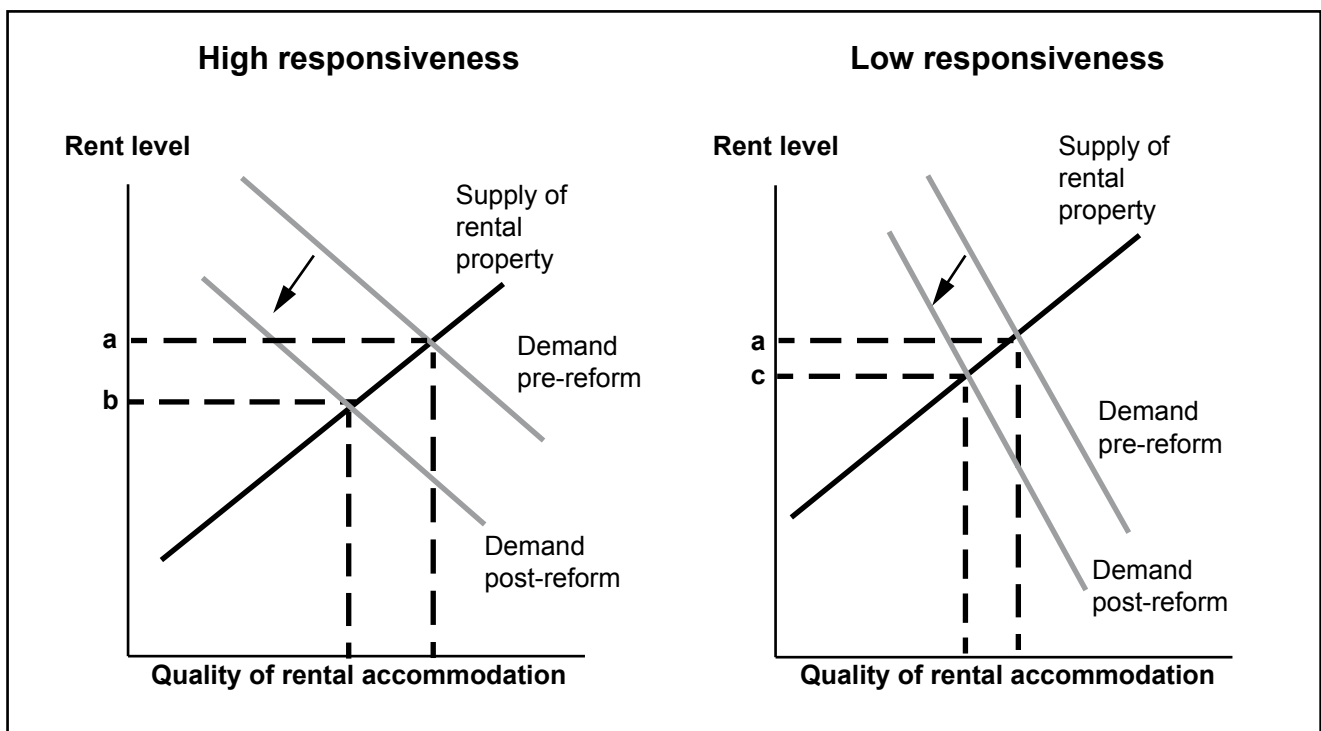


Figure 2.2 Impact of LHA reductions on rental market depending on responsiveness of demand for rented accommodation to rent levels



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, all else equal. The rationale for the excess was to encourage claimants to rent a cheaper property or negotiate rents downwards, rather than

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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 no immediate financial incentive to choose a cheaper property over a more expensive one or to bargain with landlords to reduce rents below the LHA rate.³ 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 – this would no longer act to increase tenants' demand for their property, because the tenant would no longer keep any of the difference. On the other 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. The reforms 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 this package of LHA reforms to be on tenants than in the case of other rent subsidy reforms examined in the academic literature, as discussed in Section 2.2.

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. The empirical analysis that follows uses data on individuals up to 11 months after they were rolled onto the reformed LHA system. It is possible that the market will not have adjusted fully by that point. If that is the case then our results may not reflect the ultimate shares of the incidence of the LHA reductions faced by landlords and tenants.

³ Tenants may still have an incentive to keep rents lower than their LHA rate if they are forward-looking, as they might expect to stop claiming LHA in future (for example, 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.

2.2 Previous empirical evidence

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, as well as the limitations of those studies for providing guidance on the likely impacts of the 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 property characteristics, as here – see Chapter 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. 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 other words, rent subsidies result in 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 housing 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.

Econometric analysis at the interim reporting stage of this evaluation (Brewer *et al.*, 2013) looked at the initial impacts of the LHA measures on new and repeat claimants, who were the first to be treated under the new rules from April 2011. The LHA reductions were found to have been mostly incident on tenants at that early stage, as rent levels were little different for those claiming shortly after the reforms. However, it was stressed that these results were not necessarily a good guide to long run effects; and that subsequent analysis on existing claimants – as presented in this report – would be better able to track longer-run impacts and to pick apart the different elements of the reform package.

3 Data and methodology

The empirical analysis in Chapters 4 to 6 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 Great Britain (GB) and contains information on the status of each claim. The key data available to the research team and utilised in the analysis include information on claimants' contractual rents, LHA rates, BRMAs, LAs, Local Housing Allowance (LHA) bedroom entitlements, actual number of bedrooms⁴, family type and age on a particular day each month between January 2010 and November 2013.

We add Discretionary Housing Payments (DHPs) to LHA entitlements when tracking what has happened to entitlements over time. 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 claimants who were claiming LHA shortly before the reforms took effect. Precisely, the sample on which the analysis is based is those claimants who were receiving Housing Benefit (HB) assessed under the LHA rules in January 2011. For purely computational reasons (i.e. the time taken for a high-powered computer to perform the analysis), we randomly select a one-in-three subset for the analysis in Chapters 4 and 5⁵. After dropping 15 per cent of these claimants because they are missing important information, this leaves us with 239,723 claimants, observed 28 times on average.⁶ Table 3.1 describes the full SHBE sample and the sample of claimants on which our analysis in Chapters 4 and 5 is based, in terms of basic demographic and geographic characteristics: age, family type, work status and region. This shows that the two samples are almost identical in terms of these key characteristics. Table 3.1 also presents average maximum LHA entitlements in January 2011 for each of the demographic and geographic sub-groups.

⁴ Information on whether the claimant is in non-self-contained (i.e. shared) accommodation is also included. The number of bedrooms in the property is not relevant for the HB entitlement of LHA claimants, and it is possible that it is therefore recorded less meticulously than other variables in the data. Measurement error would tend to make estimates of the effects of the reform on the number of bedrooms less precise (i.e. it would tend to make standard errors larger). An important implicit assumption is that any such measurement error does not vary systematically across claimants observed at the same time but at different stages relative to the 'point of impact' of the reforms (see Chapter 4).

⁵ The analysis in Chapter 6 uses SHBE data on all those particularly likely to be affected by each specific element of the reform package considered (rather than a one-in-three subset).

⁶ See the Appendix for precise details of the sample selection.

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Table 3.1 Characteristics of LHA claimants in January 2011

Characteristic	Percentage of claimants (full SHBE sample)	Percentage of claimants (estimation sample)	Average weekly maximum LHA entitlement (estimation sample)
Age of claimant			
Under 25	16.1	16.5	£107
25-34	31.6	31.8	£129
35-44	25.2	25.2	£139
45-59	19.0	18.9	£126
60 and above	8.2	7.6	£109
Family type			
Single men	28.9	29.2	£98
Single women	15.6	15.7	£103
Couples without children	6.4	6.3	£112
Lone parents	32.7	32.4	£148
Couples with children	16.4	16.3	£158
Family work status (working-age only)			
At least one adult in work	33.6	33.4	£143
No adults in work	66.4	66.6	£119
Government office region			
North East	4.3	4.4	£95
North West	13.6	13.5	£101
Yorkshire and Humberside	9.2	9.5	£95
East Midlands	6.5	6.8	£98
West Midlands	8.3	7.9	£105
East of England	7.6	8.1	£126
London	17.5	17.0	£203
South East	12.9	13.2	£137
South West	8.8	8.9	£117
Wales	5.4	5.1	£95
Scotland	6.0	5.7	£105
All	n/a	n/a	£126

Source: Single Housing Benefit Extract (SHBE).

The nature of the reforms' roll-out provides a robust way to estimate their impacts. The date at which claimants were rolled onto the reformed system was linked to the date of their last claim reassessment before April 2011. In the absence of changes in circumstances triggering a new claim reassessment, claimants lost any LHA excess 12 months after that date, at the point of their first annual reassessment after April 2011 (i.e. at some point between April 2011 and March 2012). They were then transitionally protected in cash terms from the other LHA reductions for nine months, before being rolled fully onto the new system (i.e. at some point

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

between January 2012 and December 2012).⁷ For example, those with a claim anniversary in April – for whom we shall adopt the terminology of the April ‘cohort’ – lost any excess in April 2011 and were rolled fully onto the new system in January 2012; but the March ‘cohort’ would not lose any excess until March 2012 and would not be rolled fully onto the reformed system until December 2012.

The result is that otherwise-identical individuals, observed at the same point in time and in the same BRMA, could face different LHA systems. This allows the impact of the reforms to be identified without strong assumptions about underlying trends in the outcomes. In technical terms, we adopt a difference-in-differences (DiD) strategy. The most standard application of DiD would essentially compare trends in outcomes (for example, rents) for cohorts as they move onto the reformed system with trends at the same point in time for cohorts who do not. We extend this slightly, by allowing the estimated impact of the reforms to change as claimants move further from the ‘point of impact’ (when they were rolled onto the new system). The logic is the same: we can identify these effects because we observe similar claimants at the same time, but at a different number of months before or since their ‘point of impact’. The key assumption throughout is that, in the absence of reform, trends would have been the same across groups of claimants who were rolled onto the new system at different times (i.e. across ‘cohorts’).

As with any empirical analysis of this kind, we can only obtain statistical estimates of the effects of the reforms, and there is a range of uncertainty around all estimates. For example, if we estimate that the reform affected rents, this essentially means that average rents in the SHBE data are different for claimants observed under the reformed system than for people with similar observed characteristics at the same time who had not been rolled onto the reformed system. The smaller this difference, the less confidently we can rule out the possibility that it is due to random other factors affecting rents for different claimants, rather than a genuine reform effect (all else equal). We use asterisks throughout the report to signify ‘statistical significance’. This indicates when an estimated effect is large enough, relative to the uncertainty that surrounds it, to be confident that the true effect is not zero. It is generally unwise to draw firm conclusions from results that are not statistically significant. Technical details of our regression specification and estimation procedure, including how we quantify uncertainty, are given in Box 3.1.

⁷ If a reassessment was triggered between April 2011 and the next annual claim anniversary, or during the nine-month period of potential transitional protection, the claimant was rolled fully onto the new system at that point.

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We allocate claimants into cohorts by identifying the month of their last claim reassessment prior to April 2011. Full details of the allocation procedure can be found in the Appendix. Conceptually, we make one key decision when defining cohorts: we disregard changes of circumstances if they trigger a claim reassessment between April 2011 and the date at which the claimant was due to be rolled onto the reformed system. We retain claimants who had these changes of circumstances in our sample, but the cohort we assign them to is the one to which they would have been assigned if the change of circumstance had not occurred. The reason for doing this is that such a change of circumstance would bring forward the application of the new LHA rules – they would apply immediately without transitional protection. But these changes might themselves be responses to the reforms (for example, moving house). We want to estimate how claimants' and landlords' behaviour responds to the roll-out of the reforms. This would be hampered if we instead allowed the definition of the roll-out to be affected by claimants' and landlords' responses.⁸

By definition, some cohorts of claimants could not have had certain changes of circumstances over certain periods before April 2011. For example, the January cohort could not possibly have moved to a different address in February or March 2011 (because such a change would have triggered a claim reassessment and hence put the claimant in the February or March cohort). Hence, in analyses where the outcome of interest relates to a change of circumstance (for example, where the outcome is the probability of a claimant moving), our estimates are obtained only from the post-April 2011 data. These outcomes of interest are: the probability of a claimant moving house, or moving further than the pre-reform median distance of 2.2km, or moving out of an inner London area where the national LHA caps bind; and the probability of living in shared accommodation.

⁸ In technical terms, we attempt to avoid 'endogeneity' in our definition of when the reforms were applied.

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Box 3.1 Model specification and estimation

To obtain the main estimates presented in Chapters 4 to 6, we estimate equations of the following form using Ordinary Least Squares regressions:

$$y_{ict} = \gamma'X_{ict} + f(t) + \alpha_c + \beta'ROLLOUTMONTH_{ct} + \varepsilon_{ict}$$

where y_{ict} is an outcome for individual i in cohort c at time t ; X_{ict} contains some explanatory variables; $f(t)$ is a flexible function of calendar time (described below); α_c is a cohort effect which allows for any sources of between-cohort variation in the outcome that are fixed over time; $ROLLOUTMONTH_{ct}$ is a set of indicators for the number of months before or since the 'point of impact' of the reforms; and ε_{ict} is an error term which incorporates all unobserved determinants of the outcome.

When the outcome of interest is binary, probit regression is used instead (unless otherwise stated), as it is the more appropriate technique in those cases. The probit specification is analogous: the left-hand-side variable is the probability that the outcome equals one, and the right-hand-side of the equation (minus the error term) is assumed to be inside a standard normal cumulative distribution function. The probit models are estimated by maximum likelihood. Estimates reported from probit models in this report are average marginal effects. Given the coefficient estimates, marginal effects are evaluated separately for each claimant at the values of their right-hand-side variables, and then averaged over the estimation sample.

We control completely flexibly for time at the national level by including a full set of month dummy variables in $f(t)$. In addition we allow each BRMA to have its own underlying linear time trend (buffered by those national month-by-month shocks).

$ROLLOUTMONTH_{ct}$ is, by definition, the same for everyone in the same cohort observed at the same time (emphasised by the lack of an i subscript). The objects of interest are the elements of β , which capture the effect on the outcome of being a given number of months before or after the 'point of impact'.

Our estimated standard errors are robust to flexible specifications of the error term. 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.

4 Impacts on all existing claimants

4.1 Maximum Local Housing Allowance entitlements

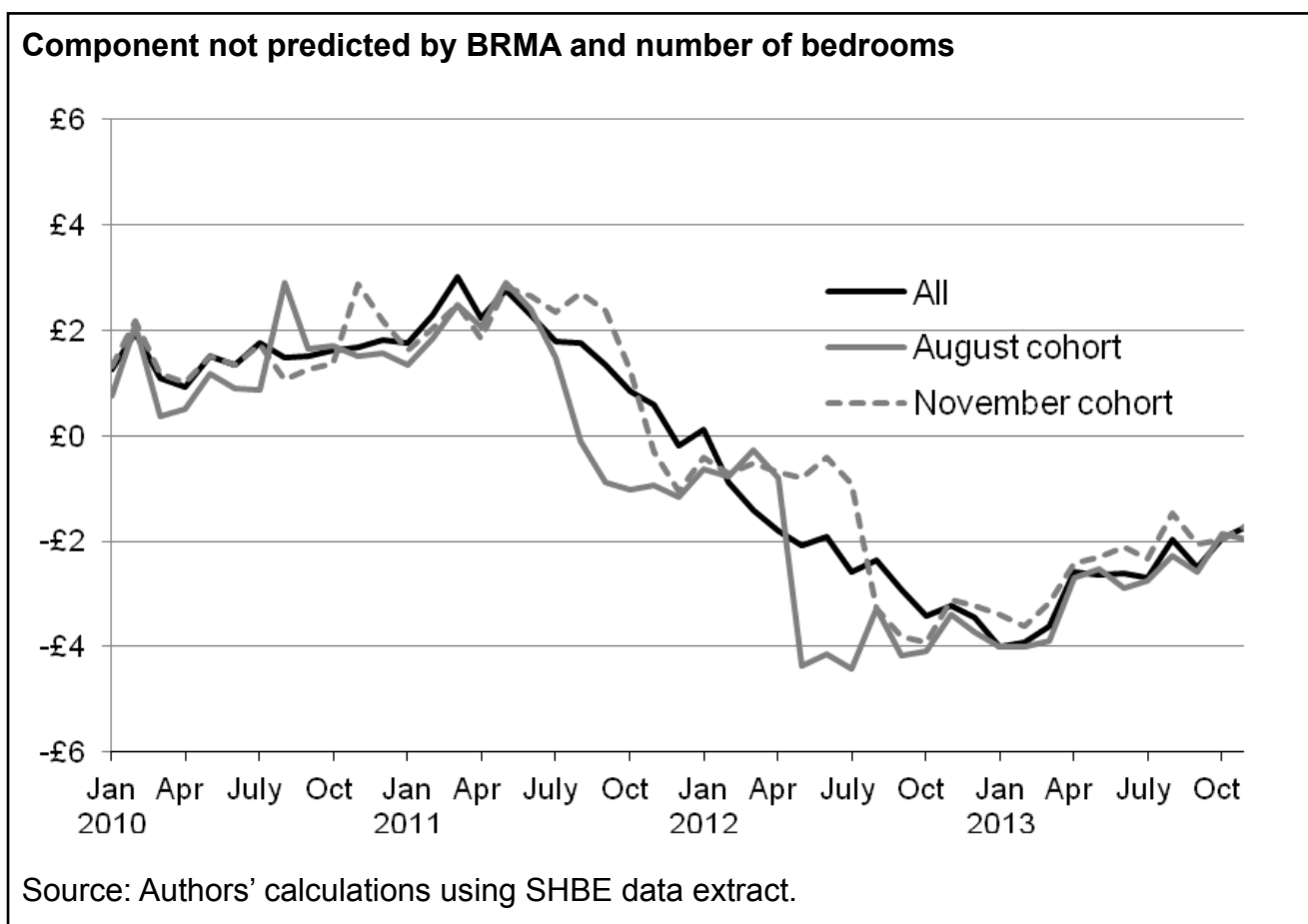
We begin by looking at maximum Local Housing Allowance (LHA) entitlements. We define these as entitlements before means tests and non-dependent deductions. They are therefore simple functions of rents and LHA rates. The reason for abstracting from the effects of means tests on entitlements is that claimants' resources from the point of view of the means test could be affected by the reform. For example, if a claimant moves into work in response, this increases their income and may subsequently reduce their means-tested entitlement. We would not want to count this as a further reduction in LHA due to the reforms in the same way that we would count reductions to pre-means test entitlements. This 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 the reform, and perhaps related to the state of the wider economy – could bias estimates of reform impacts, 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: the number of non-dependents in households may change over time and could in principle be affected by the reforms.

Figure 4.1 shows how average maximum LHA entitlements changed over time for our sample of LHA claimants. Here and throughout this chapter, we plot the component that is not 'explained' by the combination of the Broad Rental Market Areas (BRMA) in which people live and the number of bedrooms in their property. In other words, we take a claimant's LHA entitlement and subtract from that the average entitlement for claimants in the same BRMA with the same number of bedrooms over the sample period (and we later do the same for contractual rents and the gaps between LHA and rent).⁹ This is useful for two reasons. First, it isolates changes in LHA entitlements (or later, rent levels) that are not driven simply by claimants living in different kinds of properties (as defined by BRMA and number of bedrooms). Second, it helps to account for the fact that, in any particular month, some local authorities do not submit scans of their Housing Benefit (HB) records. This can make the 'raw' data volatile if, for example, a set of relatively high-rent areas submit data in one month and a set of relatively low-rent areas submit data the next month.

⁹ In practice we implement this by plotting the residuals from a regression on a full set of BRMA dummy variables, a full set of dummy variables for the number of bedrooms in the property (top-coded at 5), and a full set of interactions between the two. We treat shared accommodation cases as a distinct 'number of bedrooms' category.

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

Figure 4.1 Average maximum weekly LHA entitlement for January 2011 LHA claimants, by month



The 'All' line shows LHA entitlements declining consistently between April 2011 and December 2012. This is the period during which the LHA reforms were rolled out to existing claimants. The gradual nature of the fall in average entitlements does not reflect the impact of the roll-out on any particular claimant, but rather the fact that successive cohorts of claimants were being sequentially rolled onto the new system and seeing sudden reductions in LHA. This can be seen by considering two example cohorts. The August cohort (those whose first claim reassessment after April 2011 fell in August 2011) saw their LHA entitlements fall sharply in August 2011 at the time when they lost any excess. They then saw a further fall in May 2012, as transitional protection expired and they were affected by the rest of the package of reforms. The same pattern holds for the November cohort, but with the marked declines in LHA occurring three months later. As explained fully in Chapter 3, we will exploit this 'between-cohort' variation in when the reforms took effect in order to estimate the reforms' impacts. A convenient, alternative way of looking at how the evolution of LHA entitlements relates to the roll-out of the reforms is shown in Figure 4.2. The figure pools all cohorts together, and graphs their LHA entitlements not by calendar month, but by the number of months since being rolled onto the new system. This will differ across cohorts at any point in calendar time. For example, '0' denotes the month in which transitional protection expires. This would correspond to May 2012 for the August cohort and August 2012 for the November cohort.

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The figure confirms that the pattern shown for the two example cohorts in Figure 4.1 holds more generally. There are two clear downwards discontinuities in the series: the first at nine months before being fully rolled onto the new system (when any excess of up to £15 per week was removed) and the second at the point of full transition onto the new system (i.e. when transitional protection from the other measures expired).

Figure 4.2 Average maximum weekly LHA entitlement for January 2011 LHA claimants, by months since rolled onto new system

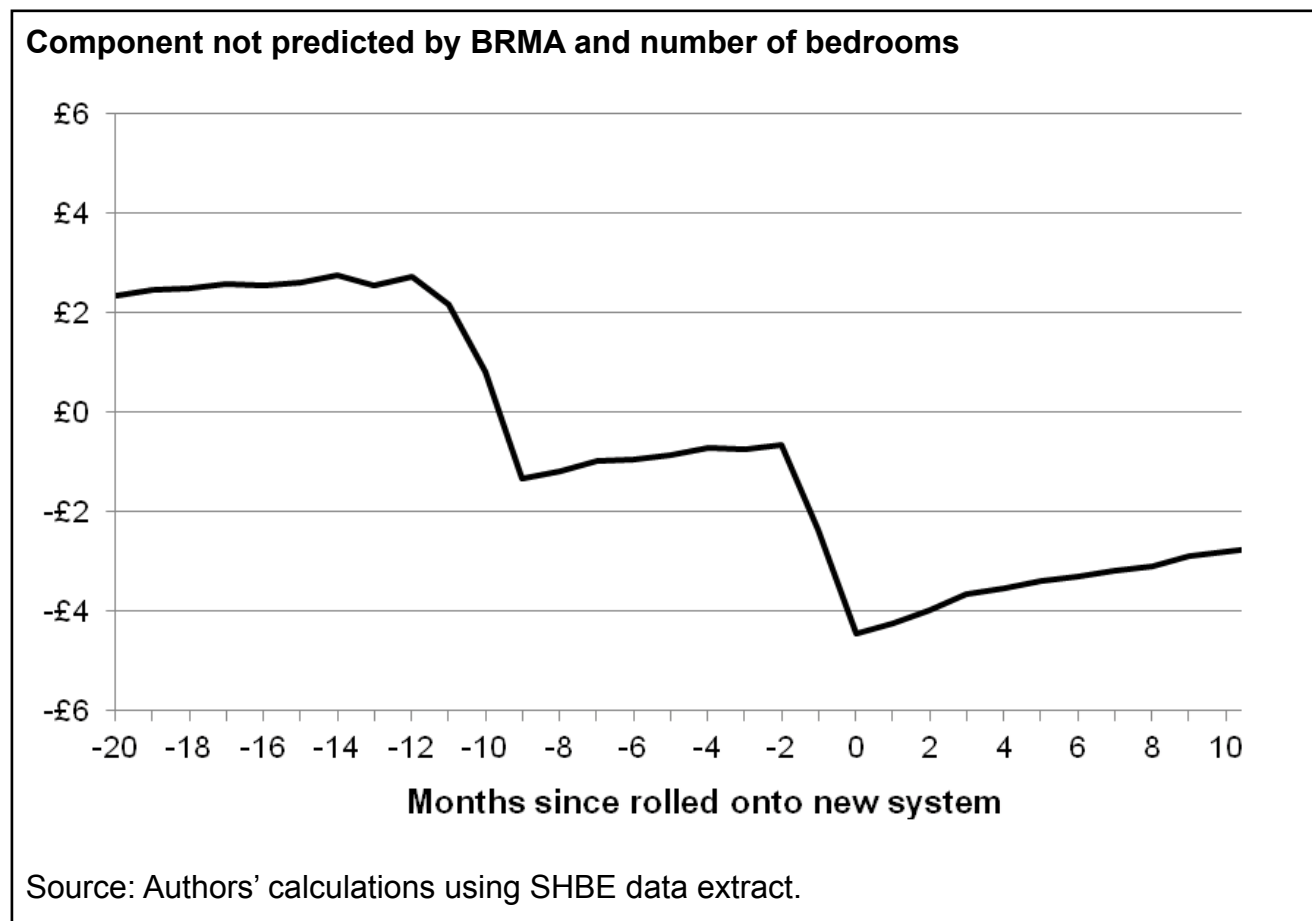


Table 4.1 shows the results of linear regression analysis of the data underlying Figure 4.2. By controlling for other factors that might have been influencing LHA entitlements over the period in question, we formally estimate the impacts on LHA entitlements of being rolled onto the reformed LHA system. Three separate estimated effects are presented: at nine months before being rolled on to the new system (the point at which any excess was lost, and the first point at which claimants could be affected by the other reforms if they had a change of circumstance which automatically ended their transitional protection period); at the point of transition, when transitional protection from the other changes expired; and at 11 months after transition.¹⁰

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 local authority (LA); model (3) adds controls for the number of bedrooms in the property, interaction terms that

¹⁰ This is the latest point at which we have data on all 12 cohorts.

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

capture all possible combinations of number of bedrooms and BRMA, and a measure of deprivation in the local area¹¹; model (4) adds controls for 'cohort' and time¹²; and model (5) adds controls for household type and age (jointly). The notes to the table contain precise definitions of the control variables. A formal description of the regression specification can be found in Box 3.1 in Chapter 3. Note that a small fraction of the sample is dropped from the analysis as we move from models 1 to 5 (see the bottom row of the table), because they are missing information with respect to the control variables that are added to the model.

The results show that claimants' 'raw' LHA entitlements were significantly lower (about £4 per week) when observed at the point of transition onto the new system than when observed well before the transition (column 1). But, in order to estimate the effects of the reforms on LHA entitlements for the same properties, we first need to control for property type. For example, LHA entitlements could change merely because claimants choose to rent properties in different BRMAs. The next two columns attempt to account for this, adding controls for the LA and BRMA that claimants are renting in and the number of bedrooms in the property.¹³

As we move from model 1 to model 2, and particularly from model 2 to model 3, the estimated effects on LHA entitlements become larger. Claimants observed later in the sample period tended to have larger families than those observed earlier, pushing up both the number of bedrooms in their house and their LHA entitlements. This does not mean that such a change was an effect of the reform, or even that this trend applies to the LHA claimant population as a whole. One important factor that we are controlling for here is that smaller families are more likely to stop claiming HB, and hence drop out of the data, after we sample them in January 2011.

Model 4 adds time trends to the model, along with a set of controls for any differences between cohorts that are constant over time (these controls have very little effect on the estimates). This can be viewed as providing the first reasonable estimate of the effects of the changes on LHA entitlements in given properties, as we have now controlled for property type and for general time trends unrelated to the reform. Having accounted for the fact that LHA entitlements were tending to rise in cash terms over time in the absence of reform, the estimated effect of the reforms in reducing those entitlements becomes even larger. The impact averages almost £5 per week when claimants lose any excess that they had, and about £7 to £8 per week once rolled fully onto the new system. These impacts are statistically significant.

Finally, model 5 adds controls for family 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 and not adequately captured by our modelled time trends. This could change housing choices, and LHA entitlements. In practice these extra controls make very little difference to the estimates.

¹¹ We use a UK-wide Index of Multiple Deprivation (IMD) score, calculated at the lower super-output area (LSOA) level. On average, each LSOA contains about 800 households, See Payne and Abel (2012) for further details.

¹² Our modelling of time trends allows for arbitrary month-by-month variation in LHA entitlements at the national level, on top of separate underlying trends in each BRMA. Formally, we include a full set of 'month' dummy variables plus interactions between BRMA and a linear time trend.

¹³ We also control for local area deprivation, which barely affects the estimates.

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

Table 4.1 Impact of the LHA reforms on maximum LHA entitlements of January 2011 LHA claimants (£ per week)

	Model				
	(1)	(2)	(3)	(4)	(5)
9 months before impact	-0.72	-2.09***	-3.40***	-4.90***	-4.98***
Point of impact	-4.12**	-4.65***	-6.67***	-8.31***	-8.31***
11 months after impact	-1.35	-1.64*	-5.04***	-7.08***	-6.84***
Clusters (BRMAs)	192	192	192	192	192
Observations	6,787,885	6,787,885	6,680,261	6,680,261	6,607,687

Notes: *** Statistically significant at one per cent level, ** Statistically significant at five per cent level, * Statistically significant at ten per cent level. Model (1) contains only a series of dummy variables corresponding to the number of months since being rolled onto the new system; 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); interaction terms that capture all possible combinations of number of bedrooms and BRMA; and UK-wide Index of Multiple Deprivation measure for the Local Super Output Area calculated by Payne and Abel (2012); model (4) adds linear time trends for each BRMA, a full set of month dummy variables and 'cohort' fixed effects; and model (5) adds joint controls for family type and age. We define 37 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, 45–59, 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 (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 within BRMAs.

Source: Authors' calculations using SHBE data.

4.2 Contractual rents

We now turn to the effects of the reform on contractual rent levels. Figure 4.3 is analogous to the series shown in Figure 4.2, but for contractual rents rather than LHA entitlements. It shows how average rents differ between claimants observed at different points in time relative to being rolled onto the reformed LHA system (after stripping out that part of rents that is predicted by BRMA and number of bedrooms).

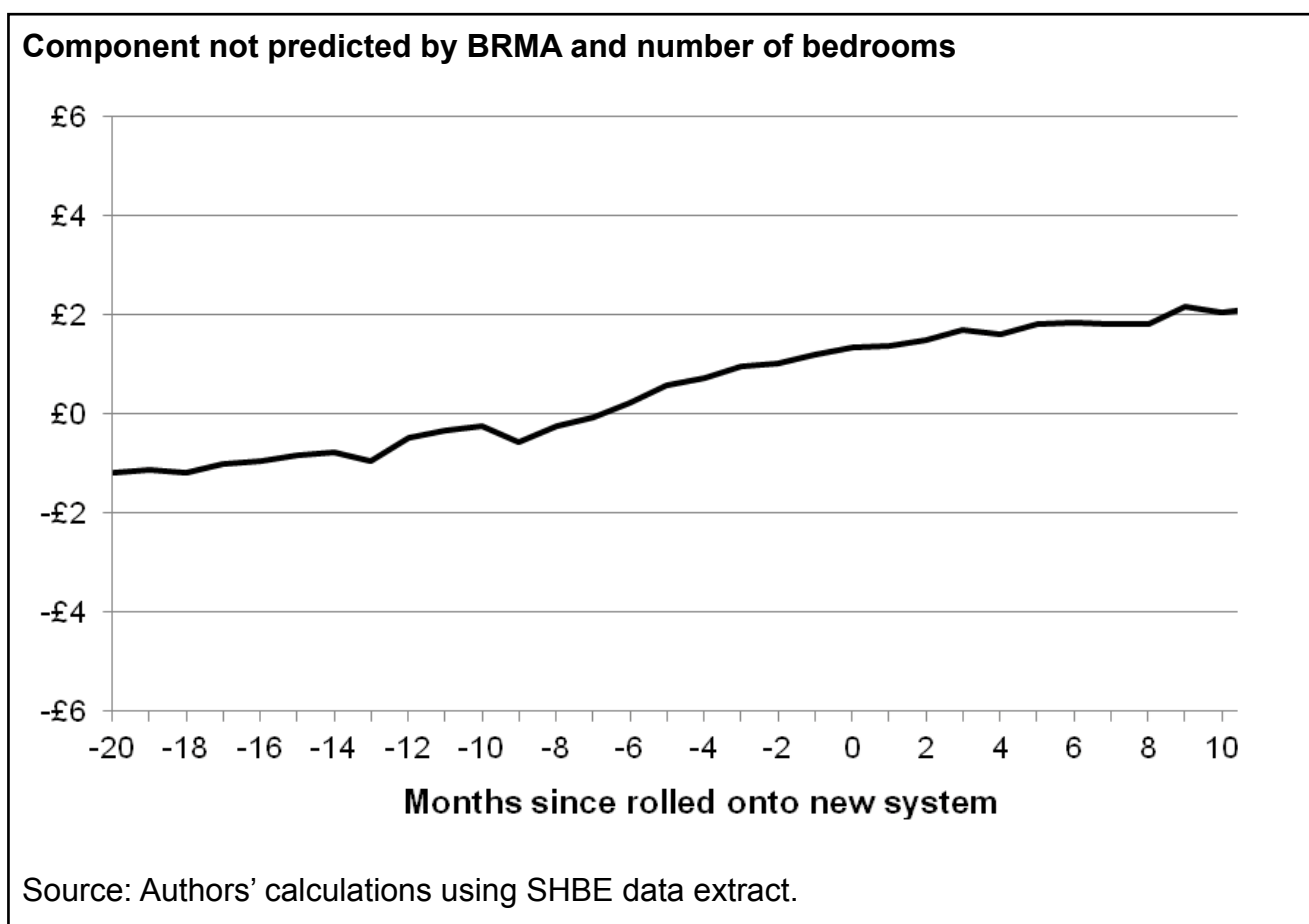
The series is generally upward sloping. This is unsurprising, because rents tend to rise in cash terms over time, and claimants observed later relative to the reforms' roll-out also tend to be observed later in calendar time. The regression analysis presented below controls flexibly for general trends in rent levels.

We do not see large falls in rents at the stages of the roll-out when LHA entitlements fell sharply (see Figure 4.2). There are also no very striking signs of rents responding to the reform in a more gradual way – the general rising trend does not appear to slow appreciably once claimants are rolled onto the reformed system. This suggests that, on average, the reductions in LHA were largely incident on the tenants themselves, rather than being passed on to landlords via lower rents – something that our regression analysis below confirms more formally.

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

However, there is a small visible fall in rents at the point of the first claim anniversary after April 2011, when any excess was lost and the nine months of transitional protection began (labelled '-9' on the x-axis). For those whose claim anniversary coincides with an anniversary of their rental contract (for example, because their claim began when they moved into their current property), this would perhaps have been the last natural point at which to renegotiate rents before being hit by the reforms. Hence, this stage of the roll-out may be significant not just because of the mechanical impact on LHA awards of removing claimants' excesses, but also as a likely point for market adjustments to begin in anticipation of the other reforms nine months later.

Figure 4.3 Average maximum contractual rents for January 2011 LHA claimants, by months since rolled onto new system (£ per week)



We corroborate these observations with the formal regression analysis presented in Table 4.2. This shows that, after accounting for time trends and control variables in the same way as previously (see Section 4.1), the estimated impact of the reforms on rental values is, if anything, negative, but small on average: less than £1 per week and far smaller than the fall in LHA entitlements.

It is important to note at this point that the Single Housing Benefit Extract (SHBE) data record contractual rents. We cannot be certain that this is always what tenants actually pay. Qualitative analysis from interviews with landlords and housing advisers (Beatty *et al.*, 2014), 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.

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

Table 4.2 Impact of the LHA reforms on weekly contractual rents of January 2011 LHA claimants (£ per week)

	Model				
	(1)	(2)	(3)	(4)	(5)
9 months before impact	3.73***	2.44***	0.93***	-0.74**	-0.81***
Point of impact	5.17***	4.81***	2.78***	-0.65	-0.73
11 months after impact	6.97***	6.83***	3.43***	-0.79	-0.79
Clusters (BRMAs)	192	192	192	192	192
Observations	6,787,885	6,787,885	6,680,261	6,680,261	6,607,687

Notes and source: as for Table 4.1.

4.3 Contractual rents net of maximum LHA entitlements

For completeness, Figure 4.4 and Table 4.3 show the time series and regression results when using the difference between rents and LHA entitlements as the outcome of interest. Note that this difference could be negative in the pre-reform period: the excess rule meant that LHA entitlements could exceed rents (we narrow the focus to positive rent shortfalls below). Any increase in this measure means a decrease of the same magnitude in after-housing-cost income, and vice versa. For example, if rents net of HB increase by £10, then the income that the claimant has to spend after paying their rent decreases by £10. As such, this is a useful measure of the net impact of the changes on claimants' financial position. Note that the estimates shown here are implied by the combination of results shown previously for contractual rent and maximum LHA individually.

Figure 4.4 confirms that the reforms clearly reduced LHA entitlements relative to contractual rents (and hence, reduced after-housing-cost incomes) – implied by the fact that the reductions in maximum LHA entitlements were larger than any reductions in contractual rents. Table 4.3 shows that this remains true after controlling for property types and time trends. We estimate that a statistically significant amount of the reduction in LHA entitlements in given properties was incident on tenants: about £7.50 per week at the point of transition onto the new system, falling slightly to about £6 per week 11 months later.

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

Figure 4.4 Average maximum contractual rents net of LHA for January 2011 LHA claimants, by months since rolled onto new system (£ per week)

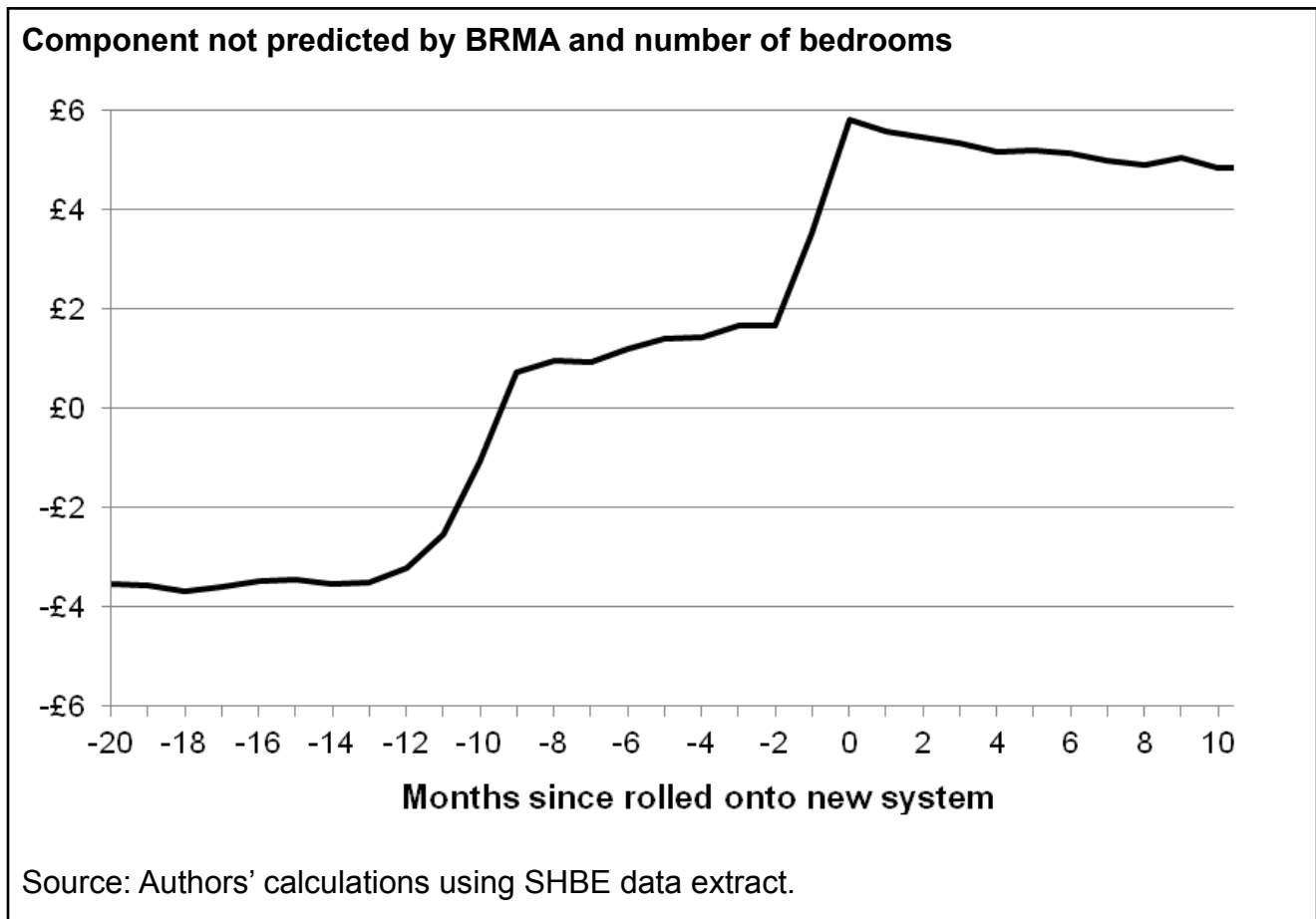


Table 4.3 Impact of the LHA reforms on contractual rents net of LHA for January 2011 LHA claimants (£ per week)

	Model				
	(1)	(2)	(3)	(4)	(5)
9 months before impact	4.46***	4.53***	4.33***	4.17***	4.17***
Point of impact	9.29***	9.46***	9.45***	7.66***	7.58***
11 months after impact	8.32***	8.47***	8.47***	6.29***	6.06***
Clusters (BRMAs)	192	192	192	192	192
Observations	6,787,885	6,787,885	6,680,261	6,680,261	6,607,687

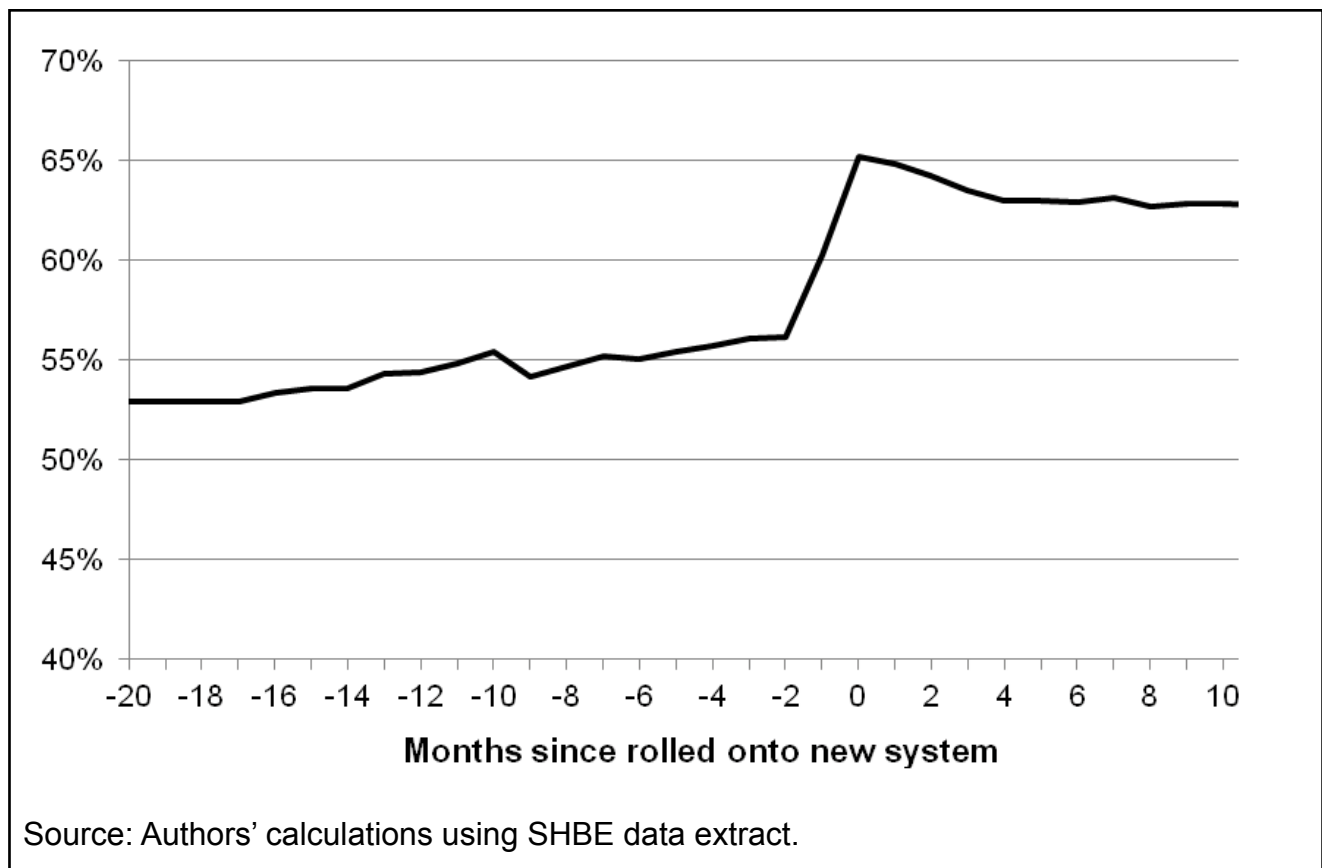
Notes and source: as for Table 4.1.

Before the reforms were introduced, it was possible for LHA entitlements to exceed rents by the 'excess' of up to £15 per week. In other words, rents net of LHA could be negative. A possible alternative outcome of interest is the proportion of claimants who face a strictly positive shortfall (i.e. who have to finance their rent at least partially from sources of income other than LHA). This is not directly affected by the removal of the excess. Figure 4.5 and Table 4.4 present the analogous analysis using a binary indicator for 'having a shortfall greater than zero' as the outcome of interest.

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

Figure 4.5 shows that a narrow majority of claimants already faced a shortfall before the reforms' implementation. There was no increase in the proportion of claimants with a shortfall at the time when excesses were removed, nine months before the 'point of impact' of the other reforms (labelled '-9' on the x-axis). This highlights the fact that, although claimants were on average made worse off at this point (i.e. their average rent net of LHA increased sharply, as shown in Figure 4.4), the removal of the excess alone does not make any additional claimants finance rent out of non-LHA resources: it merely caps their LHA at 100 per cent of their contractual rent. There is, however, an increase in the probability that a claimant will face a shortfall nine months later once the other reductions in LHA kicked in – the proportion with a shortfall settled about 7ppts higher once fully rolled onto the reformed system than before the point of impact. Estimates from the corresponding regression analysis, shown in Table 4.4, indeed suggest that the reforms had increased the proportion of claimants with a shortfall by about 7ppts at 11 months after impact.¹⁴

Figure 4.5 Proportion of the January 2011 LHA claimants with a strictly positive shortfall, by months since rolled onto new system



¹⁴ Maximum likelihood estimation of a probit model did not converge on a solution. This is likely to be due to computational difficulties arising from the very large number of observations and very large number of variables. Hence, despite the fact that the outcome is binary, results in Table 4.4 are from a linear probability model estimated using Ordinary Least Squares. This is reasonable in this context: linear probability models are typically good approximations to the truth as long as the probability that the outcome equals one is not 'extreme' (i.e. not very close to 0 or 1). As Figure 4.5 shows, having a positive shortfall satisfies this criterion.

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

Table 4.4 Impact of the LHA reforms on probability of having a strictly positive shortfall for January 2011 LHA claimants

	Model				
	(1)	(2)	(3)	(4)	(5)
9 months before impact	0.02***	0.02***	0.02***	0.02***	0.02***
Point of impact	0.13***	0.14***	0.13***	0.12***	0.12***
11 months after impact	0.11***	0.11***	0.11***	0.08***	0.07***
Clusters (BRMAs)	192	192	192	192	192
Observations	6,787,885	6,787,885	6,680,261	6,680,261	6,607,687

Notes and source: as for Table 4.3.

4.4 The incidence of LHA reductions in given property types

Combining the results from the final columns of Tables 4.1, 4.2 and 4.3, we can estimate the extent to which reductions in LHA entitlements in given types of properties were incident on tenants (via increased contractual rents net of LHA) or their landlords (via reduced contractual rents).

If the reforms did not affect rent levels, 100 per cent of LHA reductions would be incident on tenants: each £1 of reduced entitlement would simply mean that tenants have £1 less, after paying their rent, to spend on other things (or to save). If rents fell as a result of the reform, then some fraction of the incidence of the LHA reductions would instead be on landlords. In an extreme case, where rents fell by as much as LHA entitlements, 100 per cent of the incidence would be on landlords: each £1 of reduced entitlement for tenants would mean £1 less rental income for landlords, and tenants would have just as much money left over after paying rent as they had before. If rents rose due to the reform, tenants would be worse off by an amount greater than the reduction in LHA they experience: the reform would result in some transfer from tenants to landlords, as well as from tenants to taxpayers, and therefore more than 100 per cent of the incidence of LHA reductions would be on tenants. Economic theory suggests that the latter effect should not typically occur when rent subsidies are reduced, but that it could occur as a result of the excess being removed from some claimants (see Chapter 2).

We estimated that, 11 months after the point of impact, the reform package had reduced maximum LHA entitlements in given property types by an average of £6.84 per week, while resulting in contractual rent reductions in given property types averaging £0.79 per week. As shown in Table 4.5, this implies that 89 per cent of the incidence of reductions in LHA entitlements in given properties resulting from the reforms was on tenants, with the other 11 per cent on landlords. We show in Chapters 5 and 6 that this aggregate analysis does mask variation, with some sub-groups experiencing more substantial rent reductions.

It is important to note that the indicators of property characteristics captured in the SHBE data – LA, BRMA, number of bedrooms and local area deprivation – may not be the only relevant ones. Claimants could have responded to the LHA reductions by moving to properties where rents are lower for reasons unobserved in the SHBE data (for example,

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related to the quality of the housing); or the same properties could be fitted or maintained to a different standard than would have been the case without the reforms. Evidence presented by consortium partners for this independent evaluation showed that some landlords did report reductions in maintenance expenditures (Beatty *et al.*, 2014). If there are widespread changes such as this to the quality of tenants' accommodation, which we do not observe in the data, this would act to understate the true incidence of the LHA reductions on tenants.

On the other hand, as highlighted in the discussion about rents, this analysis will not pick up instances in which landlords informally accept rent lower than the contractual level. Again, work by consortium partners for this evaluation has suggested that some landlords have informally accepted lower rents from tenants without changing contractual rents (Beatty *et al.*, 2014). If such behaviour is widespread then we will tend to understate the true incidence of the reforms on landlords and overstate the true incidence on tenants, all else equal.

Table 4.5 Estimated incidence of reductions to maximum LHA entitlements

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
9 months before impact	-4.98***	-0.81***	+4.17***	84%***	16%***
Point of impact	-8.31***	-0.73	+7.58***	91%***	9%
11 months after impact	-6.84***	-0.79	+6.06***	89%***	11%

Notes: The first three columns of numbers are from model 5 in Tables 4.1, 4.2 and 4.3 respectively. The percentage of the incidence on tenants is $(- \text{change in rent net of LHA})/(\text{change in maximum LHA})$. The percentage of the incidence on landlords is $(\text{change in contractual rent})/(\text{change in maximum LHA})$.

Source: Authors' calculations using SHBE data extract.

4.4.1 Excluding claimants who had an excess in January 2011

It was noted in Chapter 2 that, all else equal, we would not expect any of the incidence of the removal of the excess to be on landlords (because it should not have a downwards effect on rents). This contrasts with the other LHA reductions introduced, which theory suggests could – depending on the details of the rental market – be incident on landlords. With this in mind, Table 4.6 reproduces the analysis in Table 4.5, but only for those claimants who did not have an excess in January 2011. We classify this group based on January 2011 information so that any subsequent responses to the reform which change claimants' 'excess status' – such as a move to a lower-rent property – do not change our classification.

It is important to note that some of these claimants could have had an excess shortly after January 2011 (for example, due to the uprating of their LHA rate, or a fall in their contractual rent), which was subsequently removed by the reform. This is one reason why there is a small, but statistically significant, fall of £1.60 per week in the group's average maximum LHA entitlement at the start of the transitional protection period ('nine months before impact') when any excesses were removed. A second reason is that claimants could be affected by the other LHA reductions at this point if they had a change of circumstance which triggered a claim reassessment (as this automatically ended the transitional protection period). However, both at the start of the transitional protection period and subsequently, this group clearly lost less LHA entitlement from the reform package than the sample as a whole.

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The results also suggest that, for this group just as for the sample as a whole, the incidence of the LHA reductions tended to be on tenants: there were no statistically significant reductions in rental values. All else equal we might have expected to find less incidence on tenants here than in Table 4.5, because the removal of the excess should be incident only on tenants. However, all else may not be equal: tenants who did not have an excess in January 2011 may be systematically different from other tenants in relevant ways. For example, they may be less effective at negotiating lower rents (which could explain why they did not have an excess in the first place).

By 11 months after impact the central estimate suggests that the reform had actually increased rental values for the group slightly, by about £2 per week (suggesting that more than 100 per cent of the incidence of LHA reductions was therefore on tenants). This is statistically significant at the ten per cent level (but not the conventional five per cent level). We would caution against inferring much from this, particularly given that it seems inconsistent with what theory suggests. By definition, if the true effect is zero then estimated effects should still be statistically significant at the ten per cent level with a probability of ten per cent (due to random unobserved factors affecting the rents of different claimants, which our model cannot disentangle from reform effects). This report splits results by a large number of subgroups, so the occasional erroneous statistically significant result is to be expected.

Table 4.6 Estimated incidence of reductions to maximum LHA entitlements for claimants without an excess in January 2011

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
9 months before impact	-1.61***	-0.05	+1.56***	97***	3%
Point of impact	-6.11***	+0.95	+7.07***	116%***	-16%
11 months after impact	-3.95***	+2.11*	+6.05***	153%***	-53%*

Notes: Model specifications and estimates reported are analogous to those in Table 4.4.

Source: Authors' calculations using SHBE data extract.

4.5 Changes in property and property type

This chapter has so far 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. If people move to properties with different rent levels this could also affect HB expenditure. It is therefore important to examine whether claimants responded to the reforms by renting different types of properties, to get a clearer idea of the reforms' overall impacts.

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Table 4.7 shows estimates of the reforms' impacts on a number of additional outcomes. The estimated impacts are obtained after controlling for a number of other factors, in a similar way to the model underlying Table 4.5. Details are given in the notes to Table 4.7, but the two main differences are as follows. First, we control only for initial LA, BRMA, number of rooms and local area deprivation (as measured in January 2011) rather than their contemporaneous measures. This means that, unlike the estimates presented so far, these estimates will include the effect of any changes in claimants' locations and number of rooms. Second, we add additional controls for whether a claimant is observed at or around the anniversary of their LHA claim or rental contract. This is to account for the fact that claimants are more likely to change their circumstances at that point (for example, by moving house).

The first column shows estimated effects on the probability of moving to a different property. We identify moves as changes in the census output area (COA) recorded for a given claimant.¹⁵ Interestingly we estimate that the reforms had a statistically significant downwards effect on the number of claimants moving around the start of their transitional protection period, 'nine months before impact' (and immediately prior to this, although this is not shown in the table). This is consistent with the financial incentives that claimants faced. If a house move between April 2011 and the point of impact resulted in a claim reassessment (or an entirely new claim), claimants would have been rolled fully onto the reformed system – without transitional protection – at that point. By not moving at this time, claimants could instead spend longer under the unreformed system with higher entitlements.

By 11 months after impact, our central estimate is that the reform had increased the monthly probability of a claimant moving properties by 0.5ppts, from a baseline probability of 2.2 per cent in the pre-reform period. This is a statistically significant effect. We show in Chapter 5 that this effect is driven by working-age claimants (Table 5.4). We cannot be sure of the extent to which this reflects claimants having simply delayed moves that they would otherwise have made earlier (see above), versus additional moves that would not have taken place at all in the absence of the reforms.

The second column of the table shows estimated effects on the probability of moving further than 2.2km. This is the median distanced moved observed in the data among claimants who moved more than 12 months before they were due to be rolled onto the new system (i.e. over a period before the reforms were likely to have had an effect). In other words, before the reforms had any impact, half of the moves among our sample of claimants covered distances greater than 2.2km, and half covered less. The central estimates suggest that, 11 months after impact, the reforms had increased the monthly probability of moving further than the pre-reform median by 1.0ppts. This is a larger effect than the effect on moving at all, which suggests that some claimants who would have moved less far than the pre-reform median instead moved further than the pre-reform median as a result of the reform. However, these estimates are less precise than those obtained for the probability of moving at all, so the estimated effect is not statistically significant.

¹⁵ This means that we will fail to capture moves within COAs. Because COAs contain an average of only 125 households, this is unlikely to have much bearing on our estimates.

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The final two columns look at the characteristics of claimants' properties. There is some evidence that the reform resulted in some claimants renting smaller properties (as proxied by the number of bedrooms).¹⁶ The central estimate of an effect of '-0.04' at 11 months after impact would correspond to a decision by four per cent of claimants to rent a property with one fewer bedroom as a result of the reforms. This is not statistically significant. However, we can be more confident about effects of the reform on property size for particular sub-groups of claimants (see Chapters 5 and 6). The outcome used in the last column is a measure of deprivation in the local area, with higher numbers indicating more deprivation.¹⁷ We do not find significant effects of the reforms on the types of locality in which claimants choose to live, as measured by local deprivation levels.

Changes in the types of property that claimants inhabit could have further implications for rent levels and LHA entitlements – in addition to the impacts of the reforms in given property types, which were the subject of the sections. To explore this, Table 4.8 shows estimated effects of the reforms on LHA entitlements, contractual rents and rents net of LHA. The underlying analysis is the same as that presented earlier (in Table 4.5), except that we do not control for the number of bedrooms and only control for the location of the property in January 2011, rather than controlling for contemporaneous location and number of bedrooms. This means that, unlike in the previous sections, the estimates include the effect of any changes in LHA entitlement or rents that are explained by tenants switching to different BRMAs, or to properties with different numbers of bedrooms, due to the reform.

The results are very similar to those shown in Table 4.5. This suggests that, at the aggregate level, changes in claimants' property choices (at least, according to those characteristics of properties that we are able to control for) are relatively unimportant in accounting for the effects of the reforms on LHA entitlements and on contractual rents. However, we show in Chapters 5 and 6 that effects on property choices are an important part of what happened among specific sub-groups.

¹⁶ The actual number of bedrooms may be measured less accurately than other variables in the administrative data such as rent and LHA entitlements: it is not a variable that needs to be accurately reported for households' LHA entitlements to be assessed, so the reporting of it may be less meticulous. When using the number of bedrooms as an outcome variable, we treat shared accommodation cases as having zero bedrooms. This analysis will therefore pick up shifts from self-contained one-bedroom properties into shared accommodation. We look separately at propensities to live in shared accommodation in Section 6.1, for those claimants for whom it is most relevant – claimants likely to be affected by the increased coverage of the Shared Accommodation Rate (SAR).

¹⁷ We use a UK-wide measure of an Index of Multiple Deprivation (IMD) score, calculated at the LSOA level. Each LSOA contains about 800 households, on average. The IMD score for an LSOA is a weighted average of indicators of deprivation in terms of income, employment, health, education and training, access to service, living environment and housing quality, physical environment and crime.

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Table 4.7 Impact of the LHA reforms on the probability of moving property (each month), number of bedrooms and deprivation level of local authority, for January 2011 LHA claimants

	Impact of the reform on:			
	Probability of moving (ppts)	Probability of moving further than 2.2 km (ppts)	Number of bedrooms	IMD percentile
9 months before impact	-0.3***	-0.0	-0.01*	-0.04
Point of impact	-0.1	+0.3	-0.02	-0.30
11 months after impact	+0.5**	+1.0	-0.04	-0.50
Pre-reform mean	2.2	1.1	1.83	64.27
Clusters (BRMAs)	192	192	192	192
Observations	4,298,206	4,298,206	6,619,042	6,726,791

Notes: *** Statistically significant at one per cent level, ** Statistically significant at five per cent level, * Statistically significant at ten per cent level. The two left-hand columns contain estimates from a probit model containing controls for BRMA in January 2011, a full set of month dummy variables, a full set of cohort dummy variables, joint controls for family type and age (as for table Y), and dummy variables for rental contract and LHA claim anniversaries. The model is run only on data from April 2011 onwards, as cohort and moving house are jointly determined before that point. The two right-hand columns contain estimates from an OLS regression model including all the controls listed above and linear time trends in each BRMA. Number of bedrooms is counted as zero for those in shared accommodation. Standard errors are robust to heteroscedasticity and clustering within BRMAs. Source: Authors' calculations using SHBE data extract.

Table 4.8 Impact of the LHA reforms on 'unadjusted' rents and LHA entitlements for January 2011 LHA claimants (£ per week)

	Impact of the reform on weekly:		
	Maximum LHA	Rent	Rent net of LHA
9 months before impact	-4.51***	-0.12	4.40***
Point of impact	-8.33***	-0.56	7.76***
11 months after impact	-7.40***	-1.11	6.28***

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 4.5 (and the final columns of Tables 4.1 to 4.3), except that the controls for contemporaneous BRMA, LA, number of bedrooms and local area deprivation are replaced by controls for BRMA in January 2011. Source: Authors' calculations using SHBE data extract.

5 Impacts on demographic and geographic sub-groups

This chapter examines how the impacts of the Local Housing Allowance (LHA) reforms studied in Chapter 4 varied by demographic and geographic group. Chapter 6 will consider how impacts varied between groups who were particularly likely to be affected by specific elements of the reform package.

5.1 Demographics: age, work status and family type

Table 5.1 shows the estimated impact of the reforms according to the claimant's age. Claimants under 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 were entitled only to the lowest LHA rates (the shared room rate).

There is no clear pattern by age in terms of the estimated share of the LHA reductions that were incident on the tenants themselves rather than their landlords. For all age-groups, we estimate that the majority of the incidence was on the tenants (and that this incidence is statistically significant), and the incidence on landlords (i.e. the effect of the reforms on rents) was not statistically significant. The same is true when we split the working-age population by family work status¹⁸, as in Table 5.2.

Table 5.3 shows results by family type (including both working-age and pensioner claimants¹⁹). Two points stand out. First, families with dependent children experienced bigger cash losses in LHA entitlement than childless families. This is unsurprising given that, all else being equal, they have higher entitlements. As a proportion of their total LHA entitlement in January 2011 (see Table 3.1), lone parent claimants lost an estimated 5.7 per cent and couple parent claimants lost an estimated 5.0 per cent – both close to the 5.4 per cent figure for all claimants. Second, the estimates suggest that rental values for couples without children had fallen by a statistically significant £5.47 per week due to the reforms 11 months after impact; and that almost the entire incidence of the LHA reductions for this group therefore fell on their landlords. This contrasts starkly with other family types, for whom the estimates on incidence look much more similar to the sample as a whole.

Couples without children are a relatively small group of LHA claimants: only six per cent of the sample (see Table 3.1). The results could at least partly reflect that the fact that they are better able to threaten credibly to look elsewhere in order to negotiate lower rents (either with current landlords or prospective new landlords) than, for example, couples with dependent children. But there could be other characteristics of this quite narrow claimant group which are different, on average, from other family types in receipt of LHA. One obvious example

¹⁸ We define in-work families as those where either the claimant or partner has positive recorded earned income.

¹⁹ A large majority (95.6 per cent) of claimants aged 60 or above do not have dependent children, and are therefore found within the first three family type categories listed in Table 5.3. Note, however, that only 7.6 per cent of the sample are aged 60 or above.

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is age – about 31.6 per cent of this group are aged 60 or above, compared to just 7.6 per cent for the sample as a whole – but the results for different age-groups shown in Table 5.1 suggest that this is not the explanation. It may be a characteristic not directly related to family type itself and not captured in the Single Housing Benefit Extract (SHBE) data which is driving this result.

Table 5.1 Impact of the reforms by age of claimant, 11 months after impact

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
Under 25	-5.11***	-0.32	4.79***	94%***	6%
25–34	-7.23***	-0.94	6.28***	87%***	13%
35–44	-8.42***	-0.46	7.96***	95%***	5%
45–59	-6.29***	-1.81	4.48***	71%***	29%
60+	-5.96***	+0.27	6.23***	105%***	-5%
All	-6.84***	-0.79	6.06***	89%***	11%

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 are the same as in Table 4.5 (and the final columns of Tables 4.1 to 4.3), except we include interaction terms between the dummy variables corresponding to the number of months since being rolled onto the new system 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 the interaction term between the subgroup dummies and the dummy indicating the claimant was affected 11 months previously. We also interact the full sets of calendar month and cohort dummies with the series of dummy variables indicating membership of each subgroup, allowing for different time trends and cohort effects in each subgroup.

Source: Authors' calculations using SHBE data extract.

Table 5.2 Impact of the reforms by family work status as of January 2011 (working-age households only), 11 months after impact

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
At least one adult in work	-7.62***	-0.60	7.02***	92%***	8%
No adults in work	-6.68***	-1.09	5.60***	84%***	16%
All	-6.84***	-0.79	6.06***	89%***	11%

Notes and source: as for Table 5.1.

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Table 5.3 Impact of the reforms by family type, 11 months after impact

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
Single men	-5.81***	-0.66	+5.15***	89%***	11%
Single women	-5.34***	-0.16	+5.19***	97%***	3%
Couples without children	-5.85***	-5.47***	0.39	6%	94%***
Lone parents	-8.43***	-1.14	7.29***	86%***	14%
Couples with children	-7.96***	-0.31	7.65***	96%***	4%
All	-6.84***	-0.79	6.06***	89%***	11%

Notes and source: as for Table 5.1.

It is also of interest to look by family type at effects on the mobility of claimants and the types of properties they inhabit. We might expect these effects to be different for certain groups, such as working-age claimants, to the sample as a whole (which was analysed in Table 4.6 in the previous chapter). Table 5.4 suggests that, 11 months after impact, the reforms had reduced the proportion of older claimants who were changing properties. This is a particularly interesting result given that it contrasts strongly with that for working-age claimants (the large majority group of claimants – see Table 3.1), who were more likely to move as a result of the reform 11 months after impact. The lower panel of Table 5.4 shows that lone parent claimants in particular were more likely to move house due to the reforms, by a statistically significant 0.8ppts per month 11 months after impact.

The table also suggests that older individuals were, if anything, less likely than working-age claimants to respond to the reform by living in a property with fewer bedrooms (although the difference is not statistically significant). Estimated effects on working-age groups are stronger and, in the case of families without children, statistically significantly different from zero. We estimate that this group had five fewer bedrooms for every 100 claimants due to the reforms 11 months after impact. We might expect this result as many of those in this group were affected by the increased scope of the shared accommodation rate (see Chapter 6), and we capture moves into shared accommodation here (as a change from one to zero bedrooms).

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Table 5.4 Impact of the LHA reforms on the probability of moving property (each month) and number of bedrooms, by family type, 11 months after impact

	Probability of moving (ppts)	Number of bedrooms
Age and children		
Age <60, with children	0.7***	-0.06
Age <60, without children	0.6*	-0.05**
Age 60+	-1.1**	-0.03
Partnership status, sex and children		
Single men	0.5	-0.04
Single women	0.6	-0.04
Couples without children	-0.9	-0.08
Lone parents	0.8**	-0.04
Couples with children	0.6	-0.12
All	+0.5**	-0.04

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 the results for each outcome are the same as in Table 4.7, except that we now include interaction terms between the dummy variables corresponding to the number of months since being rolled onto the new system and a series of dummy variables indicating membership of one of the mutually exclusive subgroups defined in the left-hand column. The results are then the estimated coefficients on the interaction term between the subgroup dummies and the dummy indicating the claimant was affected 11 months previously. We also interact the full sets of calendar month and cohort dummies with the series of dummy variables indicating membership of each subgroup, allowing for different time trends and cohort effects in each subgroup. Number of bedrooms is counted as zero for those in shared accommodation.

Source: Authors' calculations using SHBE data extract.

5.2 Geography

Since the nature of the rental market varies across the country, we might expect some variation in the impact of the reforms geographically. One potential reason for this is variation in the density of LHA recipients in an area relative to the total size of the private rented sector (PRS). As discussed in Chapter 2, if 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 density of LHA claimants is greater. On the other hand, if the PRS is segmented so 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 5.5 reports results after splitting claimants according to the proportion of private rented households in their local authority (LA) who are LHA claimants. We combine data from the 2011 Census on the size of the PRS in each LA with data from the SHBE extract on the size of the LHA claimant population, in order to rank LAs by the percentage of privately rented properties that are occupied by LHA claimants. We then divide LAs into quintiles on the basis of this ranking, with the LAs with the lowest share of LHA claimants in the first quintile.

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The table shows that, according to our central estimates, there is no clear pattern in how the incidence of the LHA reductions varies with 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 (see Chapter 2). It is also possible that this measure of density is simply correlated with other factors which affect the incidence of the reforms in the opposite direction.

Table 5.5 Impact of the LHA reforms by share of LHA recipients in LA's PRS, 11 months after impact

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
1st (lowest)	-9.79***	-4.47	5.31***	54%***	46%
2nd	-6.13***	-0.62	5.51***	90%***	10%
3rd	-7.58***	-4.98***	2.60	34%	66%***
4th	-6.70***	+0.17	6.87**	103%**	-3%
5th (highest)	-5.33***	+2.72***	8.05***	151%***	-51%***
All	-6.84***	-0.79	6.06***	89%***	11%

Notes and source: as for Table 5.1.

Table 5.6 presents an analysis by government office region and Office for National Statistics LA classification. The reduction in maximum LHA entitlements was largest in London, where rents and entitlements are highest. The proportionate reduction in entitlements was also relatively large in London, at 6.6 per cent of January 2011 entitlements rather than 5.4 per cent for the sample as a whole. This is not surprising given that the national LHA caps bind only in parts of inner London. In the suburbs of London, due to statistically significant reductions in rental values, about two-thirds of the estimated incidence of these LHA reductions was on landlords. This continues a theme picked up at the interim reporting stage when looking at new and repeat claimants. There is also evidence of relatively high incidence on landlords in Central London – although we caution that this is estimated much more imprecisely and is not statistically significant. Elsewhere, just as for the sample as a whole, estimated impacts of the reforms on rental values are typically much smaller than impacts on LHA entitlements and are rarely statistically significant. Exceptions are the East Midlands and, to a lesser extent, the 'Prospering UK' category (which mostly contains LAs located in the geographic centre of England²⁰). In the East Midlands we estimate that rental values fell by about £3.70 per week due to the reforms. This is statistically significant and implies that the majority of the incidence of LHA reductions in that region was on landlords.

It is worth noting that the reforms' impacts within different areas can only be estimated relatively imprecisely. This is partly a straightforward consequence of estimating effects within a large number of smaller subsets of the sample, rather than among the whole

²⁰ For more detail, see www.ons.gov.uk/ons/guide-method/geography/products/area-classifications/ns-area-classifications/index/cluster-summaries/health-areas/prospering-uk.pdf

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sample.²¹ When looking at large numbers of results together, as in this chapter, it is also important to bear in mind that the estimates which most 'stand out' may simply be the estimates that are most driven by random fluctuations in the outcomes of interest (for example, rents), rather than effects of the reform. For example, there are three occasions in this report in which a statistically significant increase in rents is estimated (two of them in this chapter). Given the number of estimates presented, this is perfectly consistent with there being no such effect for any group in reality. By definition, estimated effects should be statistically significant at the ten per cent level with a probability of ten per cent if the true effect is zero.

Table 5.6 Impact of the LHA reforms by government office region and LA classification, 11 months after impact

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
Government office region					
London	-13.39***	-3.47	+9.92**	74%**	26%
East Midlands	-6.31***	-3.69***	+2.62*	42%*	58%***
South West	-6.07***	-1.94	+4.13***	68%***	32%
West Midlands	-5.92***	-0.17	+5.76***	97%***	3%
Scotland	-5.77***	-1.28	+4.49***	78%***	22%
North West	-5.76***	+1.07	+6.83***	119%***	-19%
South East	-5.75***	+0.28	+6.03***	105%***	5%
East of England	-5.74***	-2.43	+3.32	58%	42%
Wales	-5.58***	-0.60	+4.98**	89%**	11%
North East	-5.07***	+5.30	+10.38***	205%***	-105%
Yorkshire and Humber	-4.72***	+0.84	+5.56***	118%***	-18%
LA classification					
London centre	-27.52*	-16.29	11.23***	41%***	59%
London cosmopolitan	-10.01	+6.17	16.18	162%	-62%
London suburbs	-11.88***	-7.83**	4.06	34%	66%**
Cities and services	-6.47***	-0.44	6.03***	93%***	7%
Prospering UK	-5.52***	-2.09*	3.43***	62%***	38%*
Large seaside towns	-7.45***	+1.15	8.59***	115%***	-15%
Coastal and countryside	-5.66***	-0.19	5.47***	97%***	3%
Mining and manufacturing	-4.56***	+2.24*	6.80***	149%***	-49%*
All	-6.84***	-0.79	+6.06***	89%***	11%

Notes and source: as for Table 5.1.

²¹ Another likely factor is that rents tend to move together within areas (regardless of whether the area is being impacted in any particular way by a reform to LHA). This makes it harder to be confident that even relatively substantial fluctuations in rents between areas are due to differential impacts of the reform between those areas. In technical terms, this is a standard case of clustered error terms. We allow for this clustering, at the BRMA level, when estimating standard errors (see Box 3.1).

6 Impacts on groups likely to be affected by specific elements of reform package

The analysis presented so far has looked at the impact of the package of Local Housing Allowance (LHA) reforms on existing claimants as a whole and on certain demographic and geographic subgroups of that population. In this chapter we focus on claimants who were particularly likely to be affected by specific components of the reform package. We examine, in turn, those particularly likely to be affected by the increased scope of the Shared Accommodation Rate (SAR), the removal of the £15 a week excess, the introduction of national LHA caps and, finally, the removal of the five-bedroom rate of LHA.

We take those LHA recipients who, in January 2011, had characteristics such that they would have been affected by the relevant component of the reform package had it been in place at that point. The strength of this approach is that it allows us to look at whether individuals responded in such a way as to move outside the group directly affected by the reform. For example, individuals might change where or with whom they live, or attempt to renegotiate a lower rent, to mitigate the reforms' effects (perhaps before they are even rolled onto the new system).

This approach does mean that our analysis will include some who are not actually affected by a particular component of the reform at a time when it is, nevertheless, in place for their cohort. This is because, even in the absence of reform, some individuals would have experienced changes in their circumstances after January 2011 that made that component of the reform immaterial for their LHA entitlement. This would tend to make estimated impacts on LHA entitlements lower than if one simulated how much they would lose were the reforms implemented at the point when the group were sampled in January 2011 (which would be closer to the analysis undertaken in the Department for Work and Pensions' (DWP's) impact assessments, for example). Even in the absence of any reform, the following changes will occur in some cases:

- some single childless individuals aged 25 to 34 not living in shared accommodation will start to live with someone else;
- some with an excess will experience a rise in rent that eliminates their excess;
- some eligible for the five-bedroom rate of LHA will see some children leave home so that they become eligible for fewer rooms.

It is also important to note that we continue to estimate the impact of the whole package of reforms on the sub-groups of claimants examined here. We are not separately identifying the individual impacts of specific elements of the reform package. For example, many of those affected by the change to the coverage of the SAR were also affected by the reduction in LHA rates from the 50th to the 30th percentile of local rents. When we examine impacts on the group particularly likely to be affected by SAR, we will also be capturing the impacts on the same group of that other reform.

The estimates in this chapter are based on the whole sample of individuals within the relevant category receiving LHA as of January 2011, with individuals followed for as long as they are receiving LHA up to and including November 2013.

6.1 Increased scope of the SAR

To examine the impact of the increased scope of the SAR we take all LHA recipients in January 2011 who were single, without dependent children, aged 25 to 34²², not living in shared accommodation, not living with a non-dependent and not entitled to the severe disability premium.²³ This is broadly the group who would, in the absence of changes of circumstance²⁴, have seen their LHA entitlement reduced from the one-bedroom rate to the SAR as they were rolled onto the new system (at the 'point of impact'). They accounted for 6.9 per cent of LHA claimants in January 2011.

The estimated impact of the overall package of reforms on the average LHA award, contractual rent and amount of contractual rent net of LHA for individuals in this group are presented in Table 6.1. The top panel shows the effect where, as far as possible, we control for property type (by controlling for Broad Rental Market Areas (BRMA), number of bedrooms, and local area deprivation). This shows that, on average, by 11 months after the reforms came in individuals in this group saw their LHA awards fall by £13.06 per week. This is larger than the estimated impact of the reforms on the whole sample (£6.84 per week in Table 4.1) and the impact measured across all 25 to 34-year-olds (£7.23 per week in Table 5.1). There is evidence that this reduction in LHA is being shared between tenants and their landlords; at the same point we find that, again on average, contractual rents for a given property have been reduced by £4.80 per week. This suggests that less than two-thirds of the reduction in LHA is being felt by these private sector tenants (in the sense that LHA awards are falling by more than contractual rents for a given property) and over one-third is being felt by their landlords (in the sense that contractual rents for a given property are falling). After taking account of the lower contractual rents individuals were, on average, £8.25 per week worse off as a result of the reduction in LHA.

²² The precise age condition is that recipients must be due to be aged between 25 and 34 at the point of full transition onto the reformed system.

²³ Claimants who receive the middle or higher rate of the care component of Disability Living Allowance are eligible for this premium.

²⁴ Some other exemptions also apply, such as for some ex-offenders and some who were previously living in a homeless shelter, but we are unable to identify these individuals in our data. In any case the numbers benefiting from these exemptions will presumably be relatively small.

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Table 6.1 Estimated incidence of reductions to maximum LHA entitlements, ‘unadjusted’ contractual rents and LHA entitlements for the January 2011 potentially SAR-affected group (£ per week)

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
Adjusted entitlements and contractual rents					
9 months before impact	-6.54***	-1.87***	4.66***	71%***	29%***
Point of impact	-11.50***	-2.63***	8.88***	77%***	23%***
11 months after impact	-13.05***	-4.80***	8.25***	63%***	37%***
Unadjusted entitlements and contractual rents					
9 months before impact	-5.73***	-0.97***	4.75***	n/a	n/a
Point of impact	-12.26***	-2.67***	9.59***	n/a	n/a
11 months after impact	-15.55***	-7.36***	8.18***	n/a	n/a

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 the results in the top panel are the same as in Table 4.5 (and the final columns of Tables 4.1 to 4.3), and the regression specifications underlying the results in the bottom panel are the same as in Table 4.8, with one exception: controls for family type and age are replaced with controls for sex and age (because all claimants in this sub-sample were single and without dependent children in January 2011). Source: Authors’ calculations using SHBE data extract.

The bottom panel of Table 6.1 shows the equivalent estimates where we do not attempt to control for any changes in property type (we do not control for number of bedrooms and control only for the location of the property in January 2011, rather than controlling for contemporaneous location and number of bedrooms). This shows that contractual rents fell by £7.36 per week, which is more than the £4.80 per week suggested in the top panel and suggests that some individuals responded to the reform by changing where, or with whom, they live. Overall these individuals saw their LHA awards fall by just over £8 per week more than their contractual rents.

We are also able to test directly whether these individuals moved house and moved into shared accommodation as a result of the reform. These results are presented in Table 6.2. We find evidence that individuals particularly likely to be affected by the increased scope of the SAR were more likely to move house (1.4ppts per month) 11 months after the point of impact. However, just as for existing claimants as a whole (see Chapter 4), they were less likely to move house (0.6ppts per month) – and potentially forfeit their transitional protection – at the start of their transitional protection period. We also find that the reform led to many more of this group living in shared accommodation: this is increased by 12.9ppts by 11 months.

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Table 6.2 Impact of the LHA reforms on the probability of moving property (each month) and the probability of being in shared accommodation, for the January 2011 potentially SAR-affected group

	Impact of the reform on:	
	Probability of moving (ppts)	Probability of being in shared accommodation (ppts)
9 months before impact	-0.6***	-0.0
Point of impact	0.0	4.3***
11 months after impact	1.4***	12.9***

Notes: *** Statistically significant at one per cent level, ** Statistically significant at five per cent level, * Statistically significant at ten per cent level. The left-hand column contains estimates from a probit model containing controls for BRMA in January 2011, a full set of month dummy variables, a full set of cohort dummy variables, joint controls for sex and age, and dummy variables for rental contract and LHA claim anniversaries. The right-hand column contains estimates from a probit model including all the controls listed above as well as linear time trends in each BRMA. Both models are run only on data from April 2011 onwards. Standard errors are robust to heteroscedasticity and clustering within BRMAs.

Source: Authors' calculations using SHBE data extract.

6.2 Removal of £15 per week excess

To examine the impact of the removal of the excess we look specifically at claimants who, in January 2011, had contractual rent less than their maximum LHA entitlement. Within this group, we look separately at those claimants with the full £15 per week excess (i.e. claimants whose contractual rent was at least £15 less than their maximum LHA entitlement) and those claimants with an excess that was positive but less than £15 per week. These claimants accounted for 18.9 per cent and 23.4 per cent of LHA claimants in January 2011 respectively. Analysis on the group of claimants **without** any excess in January 2011 was presented in Table 4.6 in Section 4.4.

Without any change in their circumstances these individuals would experience the loss of their excess at their first annual claim anniversary from April 2011, and could then be affected by other parts of the reform package nine months later (at the 'point of impact').

The top panel of Table 6.3 shows the estimated impact of the LHA reforms on those entitled to a full £15 excess in January 2011. At the point at which excesses were removed (nine months before 'point of impact') individuals in this group saw their LHA entitlements in given property types reduced, on average, by £12.44 per week. The fact that this is slightly less than £15 per week indicates that some in this group were receiving an excess of less than £15 per week by the time that the reform came in: this suggests that (for reasons unrelated to the reform) some individuals experienced an increase in their contractual rent after January 2011 which reduced their excess. At the point at which excesses were removed we estimate that contractual rental values fell by an average of £1.20 per week.

Looking further ahead, once the rest of the reforms were in place, we estimate that these individuals saw their average LHA entitlements in given property types fall by £14.31 per week, their contractual rents fall by £3.59 per week and, therefore, their contractual rent net of their LHA award increase by £10.72 per week. This suggests that for this group about 70

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per cent of the fall in LHA awards were incident on the tenant (in the sense that contractual rents less LHA awards rose) and 30 per cent were incident on landlords (in the sense that contractual rents fell).

The finding that more of the incidence of the LHA reforms is on contractual rents among this group than among the sample as a whole (25 per cent in Table 6.3 compared to 11 per cent in Table 4.5) might seem surprising, as theory would suggest that the removal of the excess itself should be entirely incident on the tenant rather than the landlord (see Chapter 2). However, there could well be other differences between the types of claimant with a full excess and other LHA recipients. For example, they might be better able to negotiate lower rents (which could explain why they had a full excess in the first place) and therefore might also be better able than other claimants to negotiate a lower rent when their LHA is reduced by other elements of the reform package.

The second half of the top panel of Table 6.3 shows the equivalent estimates where we do not control for changes in property type (we do not control for number of bedrooms and control only for the location of the property in January 2011, rather than controlling for contemporaneous location and number of bedrooms). This shows similar estimates for the impact on LHA awards and contractual rents to those where we do control for property type (shown in the top panel of the table). This suggests that, for this group, the package of reforms has had little effect on where, or with whom, they live.

The bottom panel of Table 6.3 repeats the analysis for claimants with an excess that was positive, but less than the maximum of £15 per week, in January 2011. This group lost about half as much LHA entitlement, on average, as claimants who had the full £15 per week excess in January 2011. Again we estimate that a statistically significant minority of the reduction in LHA was incident on the landlords of these claimants (via lower rental values), but that the majority was incident on the tenants.

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Table 6.3 Estimated incidence of reductions to maximum LHA entitlements, ‘unadjusted’ rents and LHA entitlements for claimants with an excess in January 2011 (£ per week)

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
Excess = £15 per week					
Adjusted entitlements and contractual rents					
9 months before impact	-12.44***	-1.20***	11.24***	90%***	10%***
Point of impact	-14.80***	-3.13***	11.68***	79%***	21%***
11 months after impact	-14.31***	-3.59***	10.72***	75%***	25%***
Unadjusted entitlements and contractual rents					
9 months before impact	-12.38***	-1.37***	11.00***	n/a	n/a
Point of impact	-15.23***	-3.81***	11.42***	n/a	n/a
11 months after impact	-15.32***	-4.93***	10.40***	n/a	n/a
Excess < £15 per week					
Adjusted entitlements and contractual rents					
9 months before impact	-6.14***	-0.48**	5.66***	92%***	8%**
Point of impact	-8.19***	-1.02*	7.17***	88%***	12%*
11 months after impact	-7.44***	-1.48*	5.96***	80%***	20%*
Unadjusted entitlements and contractual rents					
9 months before impact	-5.94***	-0.43***	5.51***	n/a	n/a
Point of impact	-8.37***	-1.32***	7.05***	n/a	n/a
11 months after impact	-8.03***	-2.29***	5.74***	n/a	n/a

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 the results in the top half of each panel are the same as in Table 4.5 (and the final columns of Tables 4.1 to 4.3). The regression specifications underlying the bottom half of each panel are the same as in Table 4.8. Source: Authors’ calculations using SHBE data extract.

6.3 Caps on LHA rates, binding in parts of inner London

To examine the introduction of the nationwide LHA caps, we look at those who in January 2011 lived in one of the BRMAs in which the cap binds and who had an LHA award in excess of the cap for their household type. These claimants accounted for 2.4 per cent of LHA claimants in January 2011. The five BRMAs in which these claimants resided are all in inner London: Central London, Inner North London, Inner East London, Inner West London and Inner South West London.

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Without changes of circumstances (for example, a move to a different BRMA) these claimants would be directly affected by the cap when rolled onto the new system (the 'point of impact'). The estimated impact of the overall reforms on the average LHA award, contractual rent and amount of contractual rent net of LHA award for individuals in this group are presented in Table 6.4. The top panel shows the effect where, as far as possible, we control for property type (by controlling for the size and location of the property).

Even at the beginning of the transitional protection period ('9 months before impact'), the central estimate is that the group's average LHA award had been reduced by about £20 per week as a result of the reforms. This is partly due to the loss of any excesses at this point: in January 2011, 57 per cent of these claimants had an excess and 44 per cent had the maximum weekly excess of £15 (compared to 42 per cent and just 18 per cent respectively for other claimants). Another part of the reduction in LHA awards at this point will simply reflect the fact that rental values were already falling as a result of the reform, by a statistically significant £4.20 per week on average. For claimants whose LHA claim anniversary coincides with an annual rental contract renewal (for example, because the claim began when they last moved properties), this would perhaps have been the last natural point at which to negotiate a rent change before transitional protection expired (when the group stood to lose substantially more housing benefit on average than other claimant groups).

By 11 months after the reforms came in individuals in this group saw their LHA awards fall by £41.93 per week. This is much larger than the estimated impact of the reforms on the whole sample (£6.84 per week in Table 4.1). There is evidence that this reduction in LHA is being shared between tenants and their landlords; at the same point we find that, again on average, contractual rents for a given property have been reduced by £5.68 per week. This suggests that 86 per cent of the reduction in LHA is being felt by these private sector tenants (in the sense that LHA awards are falling by more than contractual rents for a given property) and 14 per cent is being felt by their landlords (in the sense that contractual rents for a given property are falling).²⁵ After taking account of the lower contractual rents individuals were, on average, just over £36 per week worse off as a result of the reduction in LHA.

The bottom panel of Table 6.4 shows the equivalent estimates where we do not attempt to control for a given property type (we do not control for number of bedrooms and control only for the location of the property in January 2011, rather than controlling for contemporaneous location and number of bedrooms). This shows that actual LHA awards fell by £48.48 per week while their rents were £17.07 lower (although again this latter effect is not statistically different from zero). The fact that unadjusted LHA awards and contractual rents both fell by more than adjusted ones suggests that some are moving to cheaper areas as a result of the reform.

²⁵ This is a relatively small group and our estimates are therefore less precise, particularly when attempting to estimate how the effects evolved many months after the point of impact: the £5.68 per week fall in rents 11 months after impact – and therefore the incidence on landlords by that point – is not statistically significantly different from zero. However, the earlier impact at the start of the transitional protections period was more precisely estimated, and was statistically significant even though smaller in magnitude. Taking these results together, it would therefore be reasonable to conclude that it is likely that the reforms did affect rental values in a sustained way for this group of claimants.

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Table 6.4 Estimated incidence of reductions to maximum LHA entitlements, ‘unadjusted’ rents and LHA entitlements for those whose January 2011 LHA award exceeds the subsequent national caps

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
Adjusted entitlements and contractual rents					
9 months before impact	-20.52***	-4.23*	16.28***	79%***	21%*
Point of impact	-54.15***	-4.20	49.95***	92%***	8%
11 months after impact	-41.93***	-5.68	36.25***	86%***	14%
Unadjusted entitlements and contractual rents					
9 months before impact	-18.71***	-2.89	15.82***	n/a	n/a
Point of impact	-56.74***	-9.79	46.94***	n/a	n/a
11 months after impact	-48.48***	-17.07	31.41**	n/a	n/a

Notes and source: as for Table 6.3.

We are also able to test directly whether these individuals moved house as a result of the reform and whether they moved to a BRMA where the cap was not binding. These results are presented in Table 6.5. We find evidence that individuals particularly likely to be affected by the national caps were more likely to move house (1.1ppts per month) and to move out of the areas affected by the national caps (0.4ppts per month). These increases are from pre-reform levels of 1.6 per cent per month and 0.1 per cent per month respectively

Table 6.5 Impact of the LHA reforms on the probability of moving property (each month) and the probability of moving out of capped BRMAs, for whose January 2011 LHA award exceeds the subsequent national caps

	Impact of the reform on:	
	Probability of moving (ppts)	Probability of moving out of capped BRMAs (ppts)
9 months before impact	-0.0	0.0
Point of impact	0.9**	0.4*
11 months after impact	1.1*	0.4

Notes: *** Statistically significant at one per cent level, ** Statistically significant at five per cent level, * Statistically significant at ten per cent level. Numbers are estimates from a probit model containing controls for BRMA in January 2011, a full set of month dummy variables, a full set of cohort dummy variables, joint controls for family type and age (as in Tables 4.1-4.5), and dummy variables for rental contract and LHA claim anniversaries. Standard errors are robust to heteroscedasticity and clustering within BRMAs.

Source: Authors’ calculations using SHBE data extract.

6.4 Abolition of the five-bedroom LHA rate

To examine the abolition of the five-bedroom LHA rate we look at those who were entitled to the five-room rate in January 2011. These individuals would be likely to be affected by the abolition of the five-bedroom rate as they were rolled onto the new system (at the ‘point of impact’). They accounted for 0.8 per cent of LHA claimants in January 2011.

The estimated impact of the overall reforms on the average LHA award, contractual rent and amount of contractual rent net of LHA award for individuals in this group are presented in Table 6.6. The top panel shows the effect where, as far as possible, we control for property type (by controlling for the size and location of the property). This shows that, on average, by 11 months after the reforms came in individuals in this group saw their LHA awards fall by £29.21 per week. This is much larger than the estimated impact of the reforms on the whole sample (£6.80 per week in Table 4.1).

There is evidence that this reduction in LHA is being shared between tenants and their landlords; at the same point we find that, again on average, contractual rents for a given property have been reduced by £11.69 per week. This suggests that 60 per cent of the reduction in LHA is being felt by these private sector tenants (in the sense that LHA awards are falling by more than contractual rents for a given property) and 40 per cent is being felt by their landlords (in the sense that contractual rents for a given property are falling). One possible reason why a substantial proportion of this LHA reduction could be incident on landlords is that, for some large families formerly entitled to the five-bedroom rate, a five-bedroom property may be relatively closely substitutable for a four-bedroom property. This would strengthen their bargaining power in rent negotiations. However, this is not a hypothesis that we are able to test. After taking account of the lower contractual rents these claimants were, on average, £17.52 per week worse off as a result of the reduction in LHA.

Table 6.6 Estimated incidence of reductions to maximum LHA entitlements, ‘unadjusted’ rents and LHA entitlements for those whose January 2011 LHA award is for five bedrooms

	Impact of the reform on weekly:			Percentage of incidence on:	
	Maximum LHA	Rent	Rent net of LHA	Tenants	Landlords
Adjusted entitlements and contractual rents					
9 months before impact	-11.32**	-3.35	7.97**	70%**	30%
Point of impact	-38.64***	-5.65	32.99***	85%***	15%
11 months after impact	-29.21***	-11.69**	17.52***	60%***	40%**
Unadjusted entitlements and contractual rents					
9 months before impact	-10.62**	-3.86	6.76**	n/a	n/a
Point of impact	-38.97***	-8.29	30.68***	n/a	n/a
11 months after impact	-31.60***	-19.04**	12.56**	n/a	n/a

Notes and source: as for Table 6.3.

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The bottom panel of Table 6.6 shows the equivalent estimates where we do not attempt to control for changes in property type (we do not control for number of bedrooms and control only for the location of the property in January 2011, rather than controlling for contemporaneous location and number of bedrooms). This shows that actual LHA awards fell by £31.60 per week while their rents were £19.04 per week lower. This provides some evidence that individuals moved to cheaper properties as a result of the reform, and this reduced the impact on their after-housing-cost incomes to £12.56 per week from about £17.52 per week (shown in the top panel).

We are also able to test directly whether these individuals moved house as a result of the reform and whether they moved to a smaller property. These results are presented in Table 6.7. We find some weak evidence that individuals particularly likely to be affected by the abolition of the five-room LHA rate were more likely to move house (0.8ppts per month) and that, on average, they moved to smaller homes (the number of bedrooms fell by 0.14) but neither of these effects is statistically significantly different from zero.

Table 6.7 Impact of the LHA reforms on the probability of moving property (each month) and the number of bedrooms, for whose January 2011 LHA award is for five bedrooms

	Impact of the reform on:	
	Probability of moving (ppts)	Number of bedrooms (ppts)
9 months before impact	0.0	-0.000
Point of impact	0.6	-0.040
11 months after impact	0.8	-0.135

Notes: *** Statistically significant at one per cent level, ** Statistically significant at five per cent level, * Statistically significant at ten per cent level. The left-hand column contains estimates from a probit model containing controls for BRMA in January 2011, a full set of month dummy variables, a full set of cohort dummy variables, joint controls for family type and age (as for table 4.5), and dummy variables for rental contract and LHA claim anniversaries. The model is run only on data from April 2011 onwards. The right-hand column contain estimates from an OLS regression model including all the controls listed above and linear time trends in each BRMA. Standard errors are robust to heteroscedasticity and clustering within BRMAs.

Source: Authors' calculations using SHBE data extract.

7 Conclusions

This report has used administrative data on Housing Benefit (HB) claimants assessed under the Local Housing Allowance (LHA) rules in order to estimate the impacts of recent LHA measures on existing claimants as they were rolled onto the reformed system.

Eleven months after being rolled onto the reformed system, the LHA reforms had reduced existing claimants' maximum entitlements in given property types by an estimated average of **£6.84** per week. This was comprised of average contractual rent reductions of **£0.79** per week and reduced LHA relative to contractual rent of **£6.06** per week. These estimates imply that 89 per cent of the incidence of reduced LHA entitlements was on tenants and 11 per cent on landlords.

There is evidence that the reforms influenced property choices. We estimate that they reduced the probability that claimants move house by an estimated 0.3ppts per month, on average, when transitional protection began (protection which would be forfeited with a house move); but increased the probability that claimants move house by an estimated 0.5ppts per month, on average, 11 months after being rolled onto the reformed system. If anything, the reforms also appear to have reduced the number of bedrooms that claimants choose to rent, on average.

The most striking impacts of the reforms are evident when looking at particular sub-groups of claimants. Unsurprisingly, reductions to LHA entitlements in given types of property were higher for demographic and geographic groups who had higher entitlements to start with. These include claimants in London (£13.39 per week), who lost about twice the average amount, and lone parents (£8.43 per week). We estimate that the reforms led to relatively substantial, and statistically significant, reductions in rental values in the suburbs of London and in the East Midlands. In those areas the majority of the estimated incidence of LHA reductions fell on landlords rather than tenants. Our estimates also suggest that the small minority (six per cent) of claimants living as a couple without dependent children saw a fall in their average rent level due to the reform which was sufficient to offset their reduction in LHA entitlement almost entirely (i.e. almost all of the incidence was on their landlords).

Other claimant groups who saw relatively large reductions in entitlement include those particularly likely to be affected by the increased scope of the Shared Accommodation Rate (SAR), the national LHA caps, and the abolition of the five-room rate. There is clear evidence that the property choices of these groups responded to the reforms. Our estimates suggest that claimants likely to be affected by the SAR change were, due to the reform package, 12.9ppts more likely to be in shared accommodation 11 months after the point of impact of the reforms. Those likely to be affected by the national caps were more likely to move (and to move out of the capped areas of Inner London) as a result of the reforms. Those likely to be affected by the abolition of the five-room rate were, if anything, more likely to move to cheaper properties with fewer bedrooms, reducing the average fall in their after-housing-cost incomes resulting from the reform from £17.52 to £12.56 per week.

For groups likely to be affected by the SAR change and the abolition of the five-room rate, the reforms also reduced rental values by a statistically significant amount (£4.80 and £11.69 per week respectively). As a result, more than one-third of the incidence of the LHA reductions in given properties for these groups fell on their landlords.

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As ever, there are limitations to what can be inferred from the analysis. One possibility is that the market continues to adjust, and that effects of the reforms continue to change, beyond the time horizon that we have been able to study. We have traced the reforms' impacts for up to about one year after claimants were rolled onto the reformed system. This allows for a fuller picture of the reforms' impacts than was possible at the interim reporting stage of this evaluation, but it may still not capture the ultimate post-reform equilibrium in the rental market.

In addition, 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. It is not possible for us to quantify the importance of this but, if it is common for landlords to accept lower rents from tenants informally without changing contractual rents, our estimates may be understating the true incidence of the reforms on landlords.

On the other hand, with these data we can control only for limited kinds of property characteristics. Claimants could have responded to the LHA reductions by moving to types of properties where rents are lower for reasons unobserved in the Single Housing Benefit Extract (SHBE) data (for example, the quality of the housing). If so, we would be wrongly attributing changes in housing choices by LHA claimants to rent reductions by landlords. Alternatively, the same properties may be presented or maintained to a lower standard than would have been the case in the absence of reform. If important, these factors could lead us to understate the true incidence of the LHA reductions on tenants.

Appendix

Methods and data

Defining the ‘point of impact’ of the reforms for each claimant

As explained in Section 3.3, our method for identifying the causal impact of the Local Housing Allowance (LHA) reforms relies on the fact that similar claimants were rolled onto the reformed system at different times.

The point at which a claim started to be assessed under the new LHA rules was determined by two things:

- 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);
- the date of any changes of circumstance after April 2011 that triggered a reassessment of the claim. If there was such a change, the claimant was immediately rolled onto the new system, without transitional protection, at that point.

However, for the purposes of defining the ‘point of impact’ for each claimant, we ignore changes of circumstances after April 2011 (i.e. we ignore the second of the two items above). We retain claimants who had these changes of circumstances in our sample, but the cohort we assign them to is the one to which they would have been assigned if the change of circumstance had not occurred. This is because we do not want our measure of when individuals were affected by the reforms to be influenced by behaviour that might itself be a response to the reforms. This would create bias in our estimates of the reforms’ impacts.²⁶ For example, imagine that a claimant changes circumstances because they anticipate the effects of being rolled onto the new system in 2 months time. Perhaps, for instance, a potentially Shared Accommodation rate (SAR)-affected claimant moves in with a partner to continue being eligible for the 1-room rate. We want to capture that as an effect of being two months away from the point of impact. If we reset the point of impact to be the date of the change of circumstance, we would instead count it as an effect of being at the point of impact.

The ‘point of impact’ that we define is thus determined solely by the month of the last claim reassessment or claim anniversary in the year prior to April 2011 (or the date on which the claim began, if it began in the year prior to April 2011 and there had been no reassessment since). By default, LHA awards are simply updated to reflect the current applicable LHA rate on each annual anniversary of the claim start date. However, any change in the maximum eligible rent (caused by changes to the number of bedrooms to which a claimant is entitled or a change in address) triggers a reassessment and resets the claimant’s claim anniversary month to the month in which that change took place.

²⁶ In technical terms, we want to avoid any potential endogeneity in our treatment variable.

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

We consider a claimant to have had a claim anniversary or a claim reassessment in a given month if we observe either a change in their LHA rate or a change of circumstances. Specifically, we consider a claimant to have had a reassessment or claim anniversary if any of the following are true:

- the applicable LHA rate is different to the previous month in which a scan of their record appears;
- the number of bedrooms to which the claimant is entitled for the purposes of the LHA calculation is different to the previous month in which a scan of their record appears;
- the census output area (the finest-level geographical indicator available to the research team²⁷) is different to the previous month in which a scan of their record appears.

Any claim reassessment or anniversary should lead to the creation of what is known as a 'T-record' in the SHBE data. This provides a way of cross-validating the results of the process described above. We consider a reassessment or anniversary 'validated' if there exists a T-record for the relevant individual in which at least one of the scan extract date, notification date, and date at which the change took effect is between the extract date immediately prior to the putative reassessment or anniversary and the putative reassessment or anniversary itself. In other words, validation of a reassessment or anniversary on a particular date depends on a T-record existing with dates that correspond. The 'point of impact' that we assign each claimant is determined by their latest validated reassessment or anniversary before April 2011.

Additionally, we ignore cases where we suspect that a recorded change in a claimant's LHA rate was not genuine. In particular, the largest software supplier, Northgate, changed a large number of LHA rates in the data in late 2010 and early 2011, with such changes being heavily concentrated in particular LAs at particular times. We ignore (for the purpose of defining points of impact) changes to LHA rates in months where over a quarter of claims in the same local authority exhibit a change to their recorded LHA rate, except for claimants between 11 and 13 months since their previous reassessment.

Sample selection

The basis for our analysis in Chapters 4 and 5 is a random one-in-three sample of all LHA claimants in January 2011.²⁸ We identify LHA claimants in the Single Housing Benefit Extract (SHBE) data as those with a private rented sector (PRS) deregulated tenancy (or who are private borders), to which the LHA rules have been applied, who are not subject to the local reference rent restrictions, whose claims began after April 2008 and whose LHA rate is recorded. These restrictions yield a sample of 283,574 claimants.

43,851 claimants are dropped from this sample because their 'point of impact' cannot be robustly defined using the process described above, leaving us with a final sample of 239,723 claimants. There are three reasons why the point of impact can be impossible to determine robustly:

²⁷ Each census output area contains 125 households on average.

²⁸ We take a one-in-three sample for purely computational reasons. In our analysis of the removal of the five-bedroom rate, the extension of the SAR, and the introduction of national LHA rate caps in Chapter 6, we use data on all those particularly likely to be affected by these reforms, rather than a one-in-three subset.

Econometric analysis of the impacts of Local Housing Allowance reforms on existing claimants

- 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. 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 Broad Rental Market Areas (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 (LAs) 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), their estimated 'point of impact' 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 LAs in which all start dates from 2008–09 were reset to April 2009.²⁹

Accounting for transitional protection

The SHBE data record the LHA rate to which individuals are entitled ignoring any transitional protection. However, for the purposes of tracking the impacts of the reforms, we want to include the effects of transitional protection. We therefore modify the data on LHA rates to account for transitional protection.

First, we calculate when transitional protection would be due to apply for each claimant. It starts at the earlier of the first annual claim anniversary after April 2011 and the date of any increase in the number of bedrooms to which they were entitled after April 2011. Transitional protection ends nine months later, or if the claimant moves house or sees a reduction in the number of bedrooms to which they are entitled.

Second, we calculate each claimant's 'transitionally protected' LHA rate as the maximum of their current LHA rate and their March 2011 LHA rate. Their current LHA rate is then replaced with this 'transitionally protected' rate during the period of transitional protection, unless the individual moves house or sees a reduction in the number of bedrooms to which they are entitled between April 2011 and the date at which transitional protection was due to

²⁹ These LAs are Stockton-on-Tees, Gateshead, Blackpool, Rochdale, Fylde, Rushcliffe, South Staffordshire, Taunton Deane and Wrexham.

begin.³⁰ We then recalculate claimants' maximum LHA entitlements using these transitionally protected LHA rates.

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 LAs appeared to fall by approximately 75 per cent 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 1 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.

³⁰ Claimants were not entitled to any transitional protection if, at the point when it was due to begin, their LHA rate is higher than that at which they would be transitionally protected. This only has an effect on entitlements if, within the subsequent nine-month period, a claimant's LHA rate drops below the rate at which they would have been transitionally protected. However, the only way that a claimant's LHA rate can change without them automatically losing transitional protection is if the number of bedrooms to which they are entitled has increased. The rule above therefore only affects cases in which the new LHA rate for a higher bedroom entitlement is lower than the transitionally protected LHA rate, which is itself lower than the old LHA rate for a lower bedroom entitlement. We do not account for this rule in adjusting the data to account for transitional protection. We judge that the error from incorrectly incorporating transitional protection in the rare cases described above is likely to be extremely small – and smaller than the error that would arise from assuming no transitional protection for those whose recorded LHA rates fall with a lag in the SHBE data, who would therefore have erroneously high LHA rates at the point that transitional protection was due to be applied.

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Maximum weekly LHA 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 LHA entitlement to missing in rare cases where the LHA rate is recorded as zero. We also add on any Discretionary Housing Payments (DHPs) so that our estimates capture any effects of the increase in DHP funding.

Analyses that use rent, maximum LHA or shortfall 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 LAs at certain times incorrectly record whether or not claimants are in shared accommodation. Instances of this are identifiable from the fact that, in certain LAs 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.

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