Regional Science and Urban Economics 80 (2020) 103374

Contents lists available at ScienceDirect

Regional Science and Urban Economics

journal homepage: www.elsevier.com/locate/regec

Housing subsidies and property prices: Evidence from England[☆]

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ARTICLE INFO

JEL Classification:
H20
H22
H55
R30
R38

Keywords:
Housing subsidies
Housing benefit
Benefit cuts
Property prices

ABSTRACT

This paper analyses the effect of major cuts to housing subsidies on property prices in England. Governments commonly give rental subsidies to poor households, but it is not known whether or to what extent this distorts underlying property prices. Using a difference-in-differences-type estimator to exploit variation in scale of the cuts across local areas, we find that the cuts lowered house prices from the time of the policy announcement. The impact was seen predominantly for types of property typically rented by recipients of subsidies and in areas where demand for housing is low relative to supply. Analysis of survey data of individuals finds that benefit recipients were more likely to move home after the cuts relative to other renters. Overall, the results suggest that rental subsidies, while helping recipients to afford otherwise too expensive properties, could contribute to affordability problems for buyers.

1. Introduction

Housing rental subsidies are a common and big-ticket welfare policy for governments seeking to address concerns about the affordability of housing for low-income families. In the United Kingdom this subsidy is the second largest item of welfare expenditure - it comprised over 3% of all government spending in 2011/12, making it a greater expenditure than unemployment benefits and smaller only than the state pension. Any intervention on this scale to subsidise the demand for housing is likely to lead to distortions in the housing market. However, there is currently little evidence on how changes to rental subsidies might have unintended impacts on the prices of the underlying assets, i.e. the properties. In this paper, we attempt to fill this gap in the literature by investigating the effect of government reforms that cut housing subsidies in England in 2011, and in doing so we examine the possibility of a difficult public policy trade-off. Housing benefits can enable recipients to rent certain properties that they otherwise could not afford, but at the same time they might increase property prices and affect the affordability of owner-occupied housing.

A number of previous studies have analysed the impact of housing subsidies on rental prices. Evidence from the UK (Gibbons and Manning, 2006), the USA (Susin, 2002; Eriksen and Ross, 2015), France (Fack, 2006) and Finland (Kangasharju, 2010; Viren, 2013) suggests that rents generally increase with the availability of more generous subsidies – in the case of Eriksen and Ross (2015) primarily due to subsidy recipients choosing more expensive accommodation. However, while the majority of these papers draw a consistent link between housing subsidies and rental prices, the relationship between subsidies and property prices has been rarely examined. In fact, the only direct evidence of a link between housing subsidies and property prices is Rydell et al. (1982), who analyse both the rental and sales price effects of the Housing Assistance Supply Experiment, a decade-long experiment that gave housing allowance payments to eligible households in two US towns. Their findings point towards a fairly moderate increase in house prices, mainly due to the housing allowance causing a small increase in demand.

A link between housing subsidies and property prices follows straightforwardly from equilibrium asset price models in which current prices are a function of expected returns (Case and Shiller, 1989; Clayton, 1996; Ayuso and Restoy, 2006). Reductions in subsidies will lower expected rents and hence depress property prices. This will happen if the incidence of the subsidy cuts falls directly on landlords who have been forced to reduce rents (see, for example, the evidence by Gibbons and Manning, 2006), if recipients fall into arrears with greater probability, or if tenants are forced to move into cheaper accommodation and properties

☆ Stata files are available from the authors on request. The house price data covers the transactions received at Land Registry in the period 2nd January 2009 to 31st December 2013. © Crown copyright 2013. Census data contains National Statistics data © Crown copyright and database right 2011. We thank the editors and two anonymous reviewers for useful comments. Helpful comments at the 2016 conferences of the Royal Economic Society and the Scottish Economic Society and at the University of Dundee are gratefully acknowledged.

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https://doi.org/10.1016/j.regsciurbeco.2018.06.002
Received 30 October 2017; Received in revised form 28 May 2018; Accepted 9 June 2018
Available online 15 June 2018

are vacant more frequently. The price changes following these effects are also likely to extend beyond tenanted properties to the wider housing market, as rents and the value of owner-occupation are intrinsically linked, with the latter serving as a hedge against rent rises (Sinai and Souleles, 2005).

In this paper we exploit a reform to housing subsidies in England, known as Housing Benefit (and henceforth HB). Two major changes to HB were announced in June 2010 to come into effect from April 2011. These were a cap on the maximum benefits a household can receive for a property of a certain size and a reduction in the reference rent used to calculate recipients' maximum benefit entitlement from the local median rent to the 30th percentile. The latter change was expected to affect the majority (83%) of benefit recipients with an average loss of approximately 7% of HB per recipient (Department of Work and Pensions, 2010).

To analyse the effect of these changes on property prices, we exploit variation in the proportion of HB recipients across the 325 local government authorities in England. Property markets with fewer recipients prior to the changes can be expected to be less affected by the cuts. This enables the use of a difference-in-differences style estimator in which differences in treatment intensity across local housing markets give the necessary cross-sectional variation to identify the effects of the treatment. Supplementary evidence suggests that the identification strategy is valid.

The results show that the reforms had a negative and economically large effect on house prices in England. This effect immediately followed the policy announcement, suggesting that prices adjusted to expectations of lower rental returns, and this result is consistent across different specifications. There is also evidence that the reforms had a lasting effect on prices, with the impact continuing to be felt through 2013, more than two years after the policy announcement.

We find that the impact of the reforms is greatest for the types of property most commonly rented by HB recipients, flats, and that it is sensitive to the conditions of local property markets. These vary enormously across England (Maclennan and Tu, 1996), as can be seen from Fig. 1, which plots average price developments over time in London, other metropolitan areas and non-metropolitan areas. A striking feature of many local housing markets in the UK, especially in London, is the existence of excess demand as strict planning laws and factors such as topography mean that growth in the supply of housing is continually outpaced by the growth in demand (Cameron et al., 2006; Cheshire and Hilber, 2008; Hilber and Vermeulen, 2015). In areas of high excess demand the reforms may be expected to have little effect on house prices as benefit recipients for whom rents become unaffordable will be displaced by non-subsidised renters or landlords will be able to sell the property to owner-occupiers.[1]

To examine this, we test whether the treatment effect varies with a simple indicator of local excess demand, the proportion of the local housing stock that is vacant. Our findings suggest that the negative effect of the cuts is indeed stronger in markets with higher vacancy rates.

Of course, the finding that the reduction in rental subsidies reduced house prices only implies the existence of a public policy trade-off if the cuts also adversely affected HB recipients. To investigate this we use data from Understanding Society, a large household panel for the UK, to examine the moving behaviour of HB recipients. We find some evidence that the reforms increased the likelihood of HB recipients moving house relative to other renters, which suggests that the cuts also had an effect on

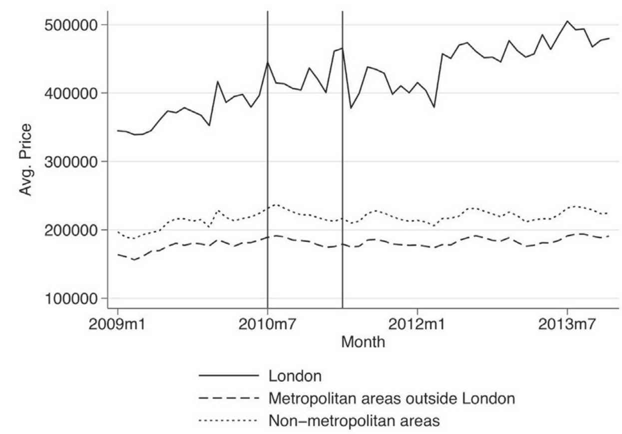

Fig. 1. Price developments over time by property market.

the affordability of rental properties for low-income households.

Overall, the results suggest the existence of a policy trade-off in which providing assistance to help recipients to live in properties (and areas) that they could otherwise not afford also contributes to higher property prices, and thus helps to fuel the affordability problem they are trying to address.

The paper proceeds as follows. A full description of the reform and the general structure of Housing Benefits can be found in section 2. Section 3 and 4 focus on the first part of our investigation – the link between housing benefits and property prices, while section 5 considers the effect of the cuts on moving behaviour by benefit recipients. Conclusions are given in section 6.

2. Housing benefit in the United Kingdom

Housing Benefit in the UK is payable to low-income individuals and couples who rent their homes. It can cover up to 100% of a tenant's rent, but it is means-tested and capped at a maximum amount. The means-test involves the withdrawal of the subsidy at a taper rate of 65% on all income exceeding a threshold level that varies according to household circumstances, so that the HB received by each household is:

$$\begin{aligned} HB &= \min(r, HB\text{max}) \text{ if } y \le y^{*} \text{ or } HB \\ &= \min(r, HB_{\max}) - 0.65(y - y^{*}) \text{ if } y > y^{*} \end{aligned} \tag{1}$$

where y is the household income, y^* is the threshold income, r is the rent, and HB_{max} is the maximum amount that will be paid. Approximately two-thirds of recipients are eligible for the maximum amount (Brown and Hood, 2012) and the maximum that can be claimed by tenants in the private rental sector is determined by Local Housing Allowance (LHA) rates. These LHA rates increase in the number of bedrooms, but fix payments for same-size properties within the same local rental market. The LHA rates for each local rental market are determined by a Rent Officer and until April 2011 these were set to be the median rental price for properties in the local market.

In June 2010, the UK government announced changes to HB affecting tenants in private accommodation in England and Wales from April 2011. The changes were announced by a new government immediately following a general election and had not been trailed in advance. The main reforms were:

- The LHA to be set at the 30th percentile of local rents rather than the 50th percentile.
- The maximum LHA payable for each property size to be capped at an absolute amount instead of being purely dependent on market rents. The caps are £250 a week for a one-bedroom property (including shared accommodation), £290 a week for a two-bed, £340 a week for

[1] Reductions in HB might even increase house prices if low-income households are priced out of neighbourhoods and spillover effects then make these areas more desirable, i.e. if they stimulate a process of gentrification, such as that modelled by Guerrieri et al. (2013). Recent empirical work has found evidence of these externalities affecting housing prices (Rossi-Hansberg et al., 2010; Autor et al., 2014). However, as we have data on HB recipients at the local authority level only, it is not possible to empirically identify such neighbourhood gentrification effects in this study.

a three-bed and £400 a week for a four-bed. Furthermore, the LHA band for 5-bedroom properties was abolished, with 4 bedrooms becoming the new maximum.

These changes applied immediately for new claims and on the anniversary of the claim plus a nine-month transitional period for existing recipients. This means that the reforms applied to all recipients by December 2012 as the latest affected case would be someone whose claim was made in March 2011, placing the anniversary in March 2012 and the end of the 9-month transition period in December 2012.

It was estimated that the setting of LHA rates at the 30th percentile would affect 83% of LHA cases, with an average loss of approximately 7% per recipient (Department of Work and Pensions, 2010). The impact was expected to vary across regions, with London having the lowest proportion of affected claimants (71%) and Humber the greatest (90%), but the average expected loss of those affected was relatively constant across all regions, being always within the 6.5–8.5% range. The benefit cap was set at a level above rental prices in most local markets and was predicted to affect only 2% of HB recipients. The majority of those affected are recipients living in London, but for whom this would be a very large reduction. For example, the LHA rate for a 3-bedroom property in the Central London rental market area pre-reform was £700.00 per week, but this was reduced to £340.00.

Further measures affecting HB recipients in the private rented sector were also announced in June 2010. These mostly reinforce the impact of the two measures outlined above by further reducing subsidies, but are either smaller in scale, such as increased non-dependent deductions and an increased upper age limit from 25 to 35 years old at which the recipient should live in shared accommodation, or were introduced at a later date, such as linking LHA increases to the Consumer Price Index (CPI) instead of changes to real local market rents from April 2013. The latter can be expected to cut subsidies over time since rental prices typically increase faster than general inflation. A final reform was the scrapping of an excess of up to £15 per week paid to recipients renting properties at rates below the LHA. The abolition of the excess removes an incentive for renters to search for lower rents, which could work against the other subsidy changes. However, the Department for Work and Pensions argue that the incentive was ineffective (Department of Work and Pensions, 2011), and there is no evidence to the contrary.[2]

Other welfare reforms were also announced in the June 2010 budget that tightened the eligibility criteria for some benefits and abolished others. Most prominently, these were changes to child benefits for higher-earners and changes to the disability living allowance. In principle, these other changes could cause problems for our analysis as they might lead to further income shocks for HB recipients. A precondition for this is an overlap between the target groups for these reforms and HB recipients. To gauge the potential impact of these other reforms, we used data from wave 1 of “Understanding Society” (see section 5 for details on the data). Child benefits and child tax credits are claimed by 42% and 37% of all HB recipients. However, the announced changes to these benefits only affected those with an annual income of at least £40,000.[3] In our sample, this amounts to 0.5% of all HB recipients, which suggests that bias from these reforms is negligible. Disability living allowance is claimed by 18% of all HB recipients. However, changes to this benefit did not come into effect before April 2013, which is at the end of our observation period and suggests that any bias from this benefit change should be small.

[2] The introduction of the so-called “bedroom tax” (or, officially, the removal of the “spare room subsidy”), whereby households with spare bedrooms lose between 14% (one spare bedroom) and 25% (two or more spare bedrooms) of their housing benefits, was a well-publicised and controversial change to housing subsides, but affects tenants in social (state-owned) housing only.

[3] The actual implementation of the changes to child benefits used an even higher threshold of £50,000.

Aside from these welfare reforms, the June 2010 budget held other fiscal measures aimed at reducing the government's deficit. These included a two-year pay freeze for public sector workers on medium and upper incomes and an announcement that from 2011 the measure of inflation used to uprate welfare payments would change from the Retail Price Index to the, typically lower, Consumer Price Index. We present further econometric evidence in the following sections, which suggests that these contemporaneous reforms do not drive our results.

3. Modeling the effect of housing benefit cuts on property prices

The analysis investigates the impact of the benefit reform on property prices and is conducted on the level of local authorities. Although the benefit reforms affected the whole of the UK, due to data restrictions we have to restrict the sample to England, which leaves 325 local authorities. The property data come from HM Land Registry, which is the central registry for all landowners in England (and Wales). The specific data used here is the ‘price paid data’ recording final transaction prices, which is publicly available from 1995 onwards.[4] We use a sample covering all property transactions in England from January 2009 (i.e., after the crash of Lehman Brothers and the resulting nationwide drop in house prices) until December 2013, which gives a total of 3,006,801 transactions. The data contain information on the full address of each property, the price paid, the date of the transaction, the property type (flat, terraced house, semi-detached house or detached house), whether the property is newly built and whether the property is freehold or leasehold. We also merge this data with information that we use in parts of the analysis. These are the pre-reform proportion of vacant dwellings per local authority from the Department for Communities and Local Government and the pre-reform proportions of unemployed and public sector workers and recipients of other benefits. Table 1 presents descriptive statistics on the estimation sample.

Data on property prices over time are insufficient to identify the effect of the HB reforms as the effect would be confounded by general time trends or potentially the effects of other policies enacted around the same time. As Fig. 2 shows, the changes were announced in a month with high transaction prices and came into effect in a month with very low transaction prices, followed by another increase. While part of this effect could be due to the policy changes, part of it is likely due to changes in other factors affecting house prices that occur at the same time.

Further, since this is a nationwide change to benefits there is no clear control region that can be considered to be fully unaffected by the changes. However, the extent to which a local property market is affected will depend on the number of HB recipients relative to the total population. To exploit this cross-sectional variation, the property data is merged with local authority-level information on the share of HB recipients in the total population directly before the announcement of the reform in May 2010. Fixing the share of recipients at some pre-reform level is necessary, since shares may change due to the reforms and the use of later shares might lead to endogeneity problems.

The empirical approach is a difference-in-differences-type estimator with a continuously varying treatment intensity, similar to those commonly used in the evaluation of nationwide minimum wages (e.g., Card, 1992; Dolton et al., 2012, 2015). Specifically, we estimate versions of the following equation:

$$Y_{ilrt} = \alpha_l + \varphi_{rt} + \beta^{'} X_i + \sum_{P=1}^{4} \tau_P * HBR_l * T_P + \varepsilon_{ilrt}, \tag{2}$$

[4] See http://www.landregistry.gov.uk/public/information/public-data/price-paid-data. There is no equivalent house ‘price paid’ data available for Scotland or Northern Ireland. Furthermore, their legal frameworks around housing markets are markedly different from England, which raises doubts about their suitability for comparability. Data on vacant dwellings was not available for Wales.

Table 1
Descriptive statistics, property sample.

Variable	Mean	Std. dev.	Min	Max
Ln(price)	12.14	0.63	8.52	17.82
Price (£)	237,307	268,314	5000	55 million
Transaction 07/2010 to 03/2011	0.14	0.34	0	1
Transaction 04/2011 to 12/2011	0.16	0.37	0	1
Transaction in 2012	0.18	0.39	0	1
Transaction in 2013	0.23	0.42	0	1
Newly build property	0.07	0.25	0	1
Leasehold	0.23	0.42	0	1
Detached house	0.24	0.43	0	1
Flat	0.19	0.39	0	1
Semi-detached house	0.28	0.45	0	1
Share of housing benefit recipients in May 2010	7.29	2.63	2.51	16.22
- in London	9.57	3.12	5.00	16.22
- in metropolitan areas outside of London	8.40	2.57	2.51	13.54
- in non-metropolitan areas	6.18	1.81	2.59	12.47
Share of property transactions in				
- London	14.2			
- outside London	85.8			
Avg. share of vacant properties	3.0	1.0	0.9	8.0
- in London	2.2	0.6	1.0	4.2
- in metropolitan areas outside of London	3.4	1.0	1.2	6.9
- in non-metropolitan areas	3.0	0.8	0.9	8.1
Observations	3006801			

Source: Land Registry and Department of Work and Pension, *Housing benefit caseload statistics*.

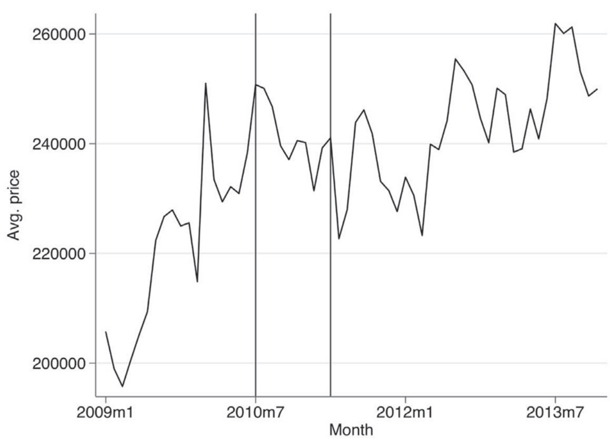

Fig. 2. Mean property prices over time.

where Y_{ilrt} is the (log) price of property i in local authority l in government office region r in month t.[5] Equation (2) summarises our basic estimating equation including local authority fixed effects (α_l), government office region-month fixed effects (ϕ_{rt}), i.e., non-parametric time trends by region, and property characteristic controls (X_i). We also estimate simpler specifications that exclude all of the previously mentioned controls and more comprehensive versions that include local authority-specific linear time trends, the local unemployment rate, local shares of public sector employment, and the proportion of local populations in receipt of other types of benefit as a robustness check. HBR_l is the share of the population in local authority l who received HB in May 2010, which gives the necessary cross-sectional variation in the benefit changes. This is interacted with a vector of dummy variables for four time periods, T_P. These are the period following the announcement of the reforms, but before they came into effect, i.e. July 2010 to March 2011, and three subsequent periods, April to December 2011, January to December 2012, and January to December 2013. We define four time periods as the announcement of the reform may immediately cause changes in prices if expected future rents fall or some HB recipients may move in anticipation of the changes, while allowing the treatment effect to vary by time in the post-reform period acknowledges that increasing numbers of recipients are affected by the changes over time. By the last time period all HB recipients are affected by the reform. The parameters of interest are τ_P, which give the effect of the reform in period P when the share of HB recipients in the local population increases by one additional percentage point.

It is important to be clear about the main threats to identification in a treatment intensity-based difference-in-difference setting. The main worry in a case such as ours are potential other changes over time that are correlated with the pre-reform share of housing benefit recipients HBR_l. For example, a general change in expectations regarding further government policies would only matter if it affects regions with a high proportion of HB recipients differently from those with fewer HB recipients. We can think of a range of potential confounding effects that might be problematic in this context. Firstly, it is possible that the proportion of HB recipients is higher in certain local authorities (for example, housing benefits recipients are more common in certain cities). We include local authority fixed effects to control for this possibility. Secondly, it is possible that HBR is correlated with unobserved trends on the local authority or regional level. In our more comprehensive specifications, we include both region*month-of-year FEs and local authorities to account for this possibility. Thirdly, it is possible that other government reforms affecting other benefit recipients might affect house prices in a similar way that the housing benefit cuts, which would be problematic if the proportion of other benefit recipients in an area was correlated with HBR_l. To allow for this possibility, we estimate a specification that includes further interactions between the (pre-reform) share of other benefit recipients and the post-treatment dummies, specifically the proportions of people receiving income support, job seekers allowance, incapacity benefits, employment and support allowance, carer's allowance and pension credits. We also allow for the effects of general austerity by introducing further interactions with the respective pre-reform employment in the private and public sector and also control for the local unemployment rate. Finally, it is possible that HB recipients cluster in specific areas of cities, which could introduce a correlation between HBR_l and unobserved housing quality and amenities. To allow for this possibility we also present results from a specification that restricts the sample to streets with multiple purchases and replaced local authority with street fixed effects.

Since there are comparatively high number of groups that are treated with different intensity, we are able to cluster standard errors at the level of the local authorities, i.e., at the level at which the treatment varies, to avoid the Moulton-problem (Moulton, 1986). This approach also accounts for the problems with autocorrelation in difference-in-differences estimators described by Bertrand et al. (2004).

Fig. 3 presents a simple comparison of property price trends by local authorities, which are grouped into quintiles based on the share of recipients. It shows that prices are always higher in local authorities with fewer recipients. Pre-reform trends are broadly similar, although a price spike can be observed in late 2009/early 2010 in local authorities with lower shares of recipients. The timing of these spikes corresponds to a change to a property transaction tax. This transaction tax, known as "stamp duty", was levied at a variable rate increasing in the value of the property of up to a maximum of 7%. Until changes in December 2014 – which are not relevant for this paper – stamp duty began at a 1% rate on properties above the threshold value of £125,000. However, between September 2008 and December 2009 the threshold level was temporarily raised to £175,000, a so-called "stamp-duty holiday". Higher mean prices

[5] Adopting the former Government Office classifications, we have nine English regions.

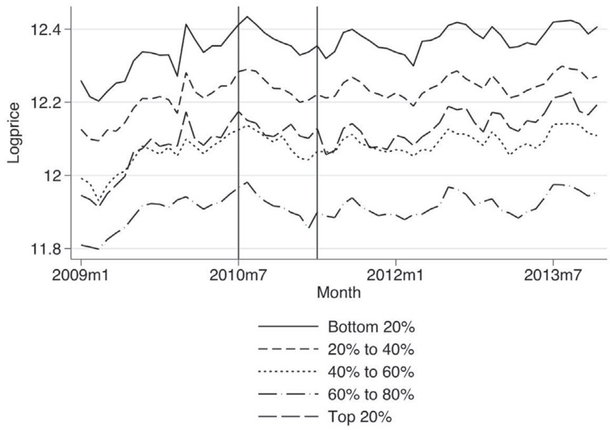

Fig. 3. Mean property prices by quintiles of treatment variable.

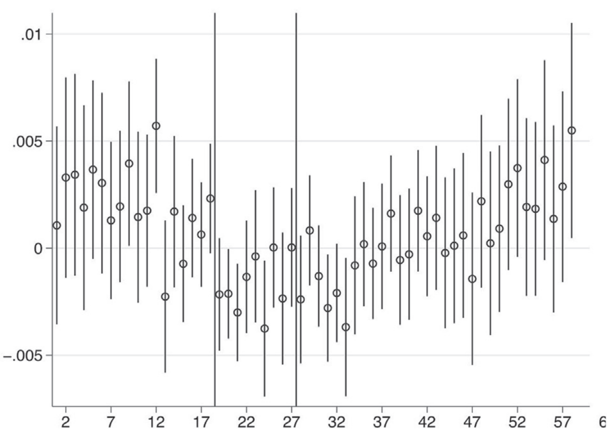

Fig. 4. Coefficient plot of monthly interactions with treatment variable.

are generally observed in December 2009 suggesting that transactions of properties worth more than £125,000 were pushed through to take advantage of the tax break. As we can see from Fig. 3, the extent of this varied across local authorities.

This evidence of unconditional pre-existing trends is not necessarily a threat to identification if they disappear after conditioning on the various fixed effects and other controls that are included in our preferred specification. To test whether these differences persist in this specification, the interactions in (2) were replaced with interactions between the share of HB recipients and each month from 2009 until 2013. This specification, which closely follows an idea by Autor (2003), effectively includes leads and lags of the treatment effect.[6] Ideally, all interactions prior to the announcement should be small and insignificant. Fig. 4 plots these interactions along with 95%-confidence bounds using July 2010 as the base. The estimated coefficients are generally quite small and statistically insignificant, although there is again a relatively pronounced spike at the time the stamp holiday ended and a noticeably lower point estimate for the following month.

Therefore, to address potential problems arising from these differences, we use two alternative sample definitions in addition to the full sample from January 2009. The first simply omits all transactions prior to February 2010, so that it excludes the last month of the stamp holiday as well as the following month. In this sample, the pre-treatment period ranges from February until June 2010 with the other periods unchanged from the base estimates. However, there is also a potential concern regarding the comparability of the pre- and post-periods, as we compare February to June pre-reform with full years post reform. To address this problem, we form a further (and preferred) sample that includes only February to June in each year. In this sample, the pre-reform period ranges from February to June 2010, the announcement period covers February and March 2011 and the post-enactment period cover April to June in 2011 and February to June in both 2012 and 2013. While all of the above specifications yield similar results, we present additional analyses based on the third sample definition and our most comprehensive specification.

Additionally, it is possible that the stamp duty holiday had an impact on the timing of house purchases and thus the volume of transactions, as some transactions might have been brought forward to benefit from the stamp duty holiday. Note that we cannot simply include the number of transactions in the main regressions as both the volume of transactions and their price are market equilibrium outcomes and thus jointly determined. Fig. 5 plots the number of housing transactions for various types of properties and three price groups. The stamp duty holiday should mainly affect property transactions with prices between £125,000 and £175,000 as these could benefit from the reduction in stamp duty. Fig. 5 indeed suggests a spike in the number of transactions in this price category right before the end of the stamp duty holiday and a corresponding drop in transactions the following month. After this the number of transactions appears to return to a normal level, suggesting that the impact of the stamp duty holiday was short-term. This finding is also in line with evidence by a more formal analysis of the stamp duty holiday by Besley et al. (2014).

4. Results

Table 2 presents base estimates for the whole country based on a variety of sample definitions and specifications. Columns (i) to (vi) are based on the whole sample from January 2009. Column (i) is the most basic specification that just includes local authority fixed effects and dummies for the pre-treatment and the various post-treatment periods. In columns (ii) and (iii) these broad time effects are replaced with either month-year effects or region-month-year effects. Column (iv) adds property characteristics, column (v) adds local authority linear time trends and column (vi) adds the local unemployment rate as well as interactions between the treatment period dummies and the pre-reform shares of other benefit recipients and private and public-sector employment. The results are consistent across all of these columns, with the estimates being robust to the inclusion of all additional controls. Columns (vi) and (vii) are based on the same specification as column (v), but use the alternative sample definitions discussed in section 3. Our preferred specification is column (vii) that uses the most comparable sample to the pre-treatment period and that is uncontaminated by any potential impact of the stamp duty holiday. Finally, column (ix) restricts the sample to streets with multiple purchases and replaces the local authority fixed effects with street fixed effects.

Overall, the results suggest that property prices initially fell after the changes were announced, i.e. during the period 07/2010 to 03/2011, and remained at this lower level for the rest of 2011. To judge the economic significance of these results, one can compare how prices change for typical variation in the share of HB recipients across local authorities, for example, for a one standard deviation change (equivalent to 2.6 percentage points). The results are economically large, where significant, with prices dropping between approximately 0.5 (for a coefficient value of 0.002 in column (v)) and 1.6% (for a value of 0.06 in column(viii)) following a one standard deviation increase in the number of HB recipients. The results are consistent across all three samples and the immediate impact of the policy announcement on prices offers support for the contention that prices are determined by expected returns to property ownership.

From 2012 onwards the picture diverges depending on the sample used. In the more comparable samples using similar time periods in each year and avoiding the possible distortion through the stamp duty holiday

[6] See Angrist and Pischke, 2009, pp. 178–180, for a textbook treatment.

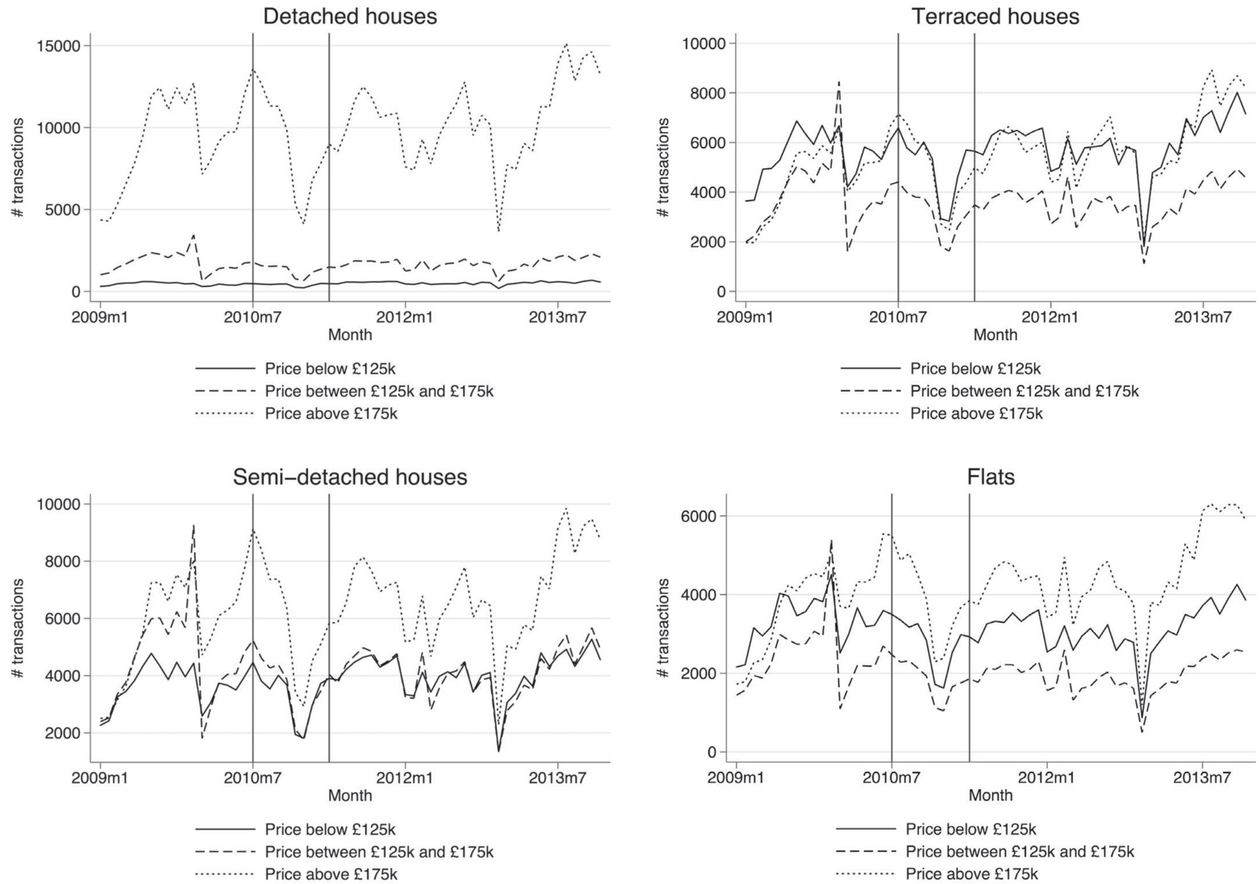

Fig. 5. Property transactions by month, property type and price category.

Table 2
Base estimates, dependent variable: Ln(price).

	Full sample from January 2009						Sample from February 2010	Sample from February 2010, only February to June transactions	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	All postcodes	Only streets with multiple purchases
								(viii)	(ix)
07/2010 to 03/2011 * % recipients in May 2010	−0.004***	−0.004***	−0.003***	−0.003***	−0.003***	−0.004***	−0.004***	−0.005**	−0.0025**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.0010)
04/2011 to 12/2011 * % recipients in May 2010	−0.003**	−0.003**	−0.003***	−0.003***	−0.002***	−0.004***	−0.005***	−0.006**	−0.0028***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.008)
2012 * % recipients in May 2010	−0.001	−0.001	−0.001	−0.002	−0.001	−0.002	−0.004***	−0.009*	−0.0024**
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.005)	(0.0011)
2013 * % recipients in May 2010	0.002	0.002	−0.000	−0.001	0.001	−0.000	−0.004*	−0.013*	−0.0029**
	(0.003)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)	(0.0013)
Local authority fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Street fixed effects	No	No	No	No	No	No	No	No	Yes
Broad period dummies	Yes	No	No	No	No	No	No	No	No
Month-year dummies	No	Yes	No	No	No	No	No	No	No
Month-year*Region dummies	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property characteristics	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Local authority linear time trends	No	No	No	No	Yes	Yes	Yes	Yes	No
Local unemployment rate	No	No	No	No	No	Yes	Yes	Yes	Yes
Public/private sector employment * treatment periods	No	No	No	No	No	Yes	Yes	Yes	Yes
Other benefit recipients * treatment periods	No	No	No	No	No	Yes	Yes	Yes	Yes
Observations	3006801	3006801	3006801	3006801	3006801	3006801	2381954	954274	677997

Notes: Coefficients, standard errors adjusted for clustering on the local authority level in parentheses. */**/***denote statistical significance on the 10%, 5% and 1% level respectively.

in 2009, the point estimates remain negative and suggest a lasting decrease in house prices. In the case of the most restrictive sample, including transactions in February and June of each year only, prices are estimated to continue to decrease, with the point estimate indicating that a one standard deviation increase in the number of HB recipients is associated with approximately a 0.8% (0.029*2.6, column (ix)) to a 3.4% (0.013*2.6, column (viii)) fall in house prices by 2013, although the latter estimates are only marginally statistically significant. By contrast, the less comparable full sample suggests that the impact of the cuts had only a temporary effect and there was no impact on prices from 2012 onwards. Given the comparability problems with the full sample, it seems advisable to put more trust in the estimates in column (viii).

If the effects in Table 2 are indeed due to changes to housing benefits, they should be stronger for properties that are more likely to be rented rather than occupied by the owners, and especially so for properties more likely to be rented by HB recipients. Table 3 shows ownership status and HB receipt by accommodation types using data from the 2010/11 English Housing Survey. It shows that the majority of houses are occupied by owners, while flats are predominantly occupied by renters and HB recipients, so that we should find the strongest price effects on flats.

This is investigated in Table 4, which repeats the estimates from Table 2 separated by property type. Given the differences between the London property market and the rest of the UK, we also present separate estimates for these regions. The estimates show that the HB reform had a negative impact on the prices for flats outside of London, but that little effect is found for flats in London or for any other property type anywhere in England. London is arguably the tightest housing market in the UK with demand regularly outstripping supply, and these estimates point towards local housing market conditions being important for whether the benefit cuts had an impact on property prices.

Given these findings, it seems worthwhile to investigate the role of local market conditions further. In particular, a high level of excess demand would make it easier for landlords to replace tenants, thus lowering the risk of income losses from vacancies and also providing less of an incentive to lower rents in response to the HB cuts. We test this idea directly by looking at treatment effect heterogeneity with respect to a direct indicator of excess demand, specifically the (pre-reform) share of local dwellings that are vacant, which has been found to be strongly negatively correlated with excess demand (Rydell, 1982). We use two different specifications. In the first, we simply test for treatment effect heterogeneity between local authorities with a vacancy rate above and below the median. The estimates can be found in the top panel of Table 5, which suggest that the HB cuts affected house prices only in those local authorities with a vacancy rate above the median where demand is expected to be less strong.

In a second specification, we allow treatment effects to vary continuously with the vacancy rate. We centre this variable to a mean of zero for the interactions, which means that the base estimates give the treatment effect for a local authority with an average share of vacant dwellings. The estimates in the bottom panel Table 5 confirm the results of the first specification: Effects are zero in local authorities with an average vacancy rate, but become more negative in markets with more vacant dwellings, i.e. those where demand can be expected to be low relative to supply. The interaction terms in the lower half of Table 5 are always negative and fairly substantial relative to the base effects. Overall, this pattern of results lends support to the idea that property prices suffer more from the HB cuts in areas where it is harder to replace benefit recipients with other tenants.

Table 3
Ownership status and benefit receipt by accommodation type.

	Detached house	Semi-detached house	Terraced house	Flat
Owner-occupier	94.3%	76.4%	62.9%	27.0%
Tenant, no housing benefit	1.3%	11.6%	18.9%	36.0%
Tenant, housing benefit	4.4%	11.9%	18.2%	37.0%
Total	100%	100%	100%	100%

Source: English Housing Survey 2010/11, authors' own calculations.

Table 4
Estimates by property type.

	Preferred sample from February 2010, only February to June transactions		
	All England	London	Outside London
Detached houses			
07/2010 to 03/2011 * % recipients in May 2010	−0.0118*	−0.0715	−0.0109*
	(0.0065)	(0.1101)	(0.0063)
04/2011 to 12/2011 * % recipients in May 2010	−0.0040	−0.0232	−0.0045
	(0.0071)	(0.1287)	(0.0070)
2012 * % recipients in May 2010	−0.0123	−0.0223	−0.0131
	(0.0133)	(0.2376)	(0.0129)
2013 * % recipients in May 2010	−0.0202	−0.0854	−0.0208
	(0.0199)	(0.3581)	(0.0193)
Observations	220139	6843	213296
Semi-detached houses			
07/2010 to 03/2011 * % recipients in May 2010	0.0039	0.0359	0.0008
	(0.0042)	(0.0415)	(0.0043)
04/2011 to 12/2011 * % recipients in May 2010	0.0034	0.0850*	−0.0009
	(0.0051)	(0.0491)	(0.0047)
2012 * % recipients in May 2010	0.0073	0.1047	0.0006
	(0.0085)	(0.0866)	(0.0083)
2013 * % recipients in May 2010	0.0119	0.1600	0.0009
	(0.0129)	(0.1309)	(0.0123)
Observations	269086	21787	247299
Terraced houses			
07/2010 to 03/2011 * % recipients in May 2010	−0.0005	0.0395*	−0.0057
	(0.0044)	(0.0208)	(0.0048)
04/2011 to 12/2011 * % recipients in May 2010	−0.0034	0.0341	−0.0081
	(0.0047)	(0.0246)	(0.0054)
2012 * % recipients in May 2010	−0.0058	0.0692	−0.0150
	(0.0084)	(0.0435)	(0.0093)
2013 * % recipients in May 2010	−0.0072	0.0873	−0.0190
	(0.0127)	(0.0651)	(0.0140)
Observations	283706	39904	243802
Flats			
07/2010 to 03/2011 * % recipients in May 2010	−0.0073	0.0174	−0.0144*
	(0.0064)	(0.0202)	(0.0080)
04/2011 to 12/2011 * % recipients in May 2010	−0.0129*	0.0142	−0.0212**
	(0.0073)	(0.0238)	(0.0092)
2012 * % recipients in May 2010	−0.0133	0.0212	−0.0303*
	(0.0122)	(0.0422)	(0.0155)
2013 * % recipients in May 2010	−0.0162	0.0447	−0.0456*
	(0.0182)	(0.0634)	(0.0238)
Observations	181343	71132	110211

Notes: Coefficients, standard errors adjusted for clustering on the local authority level in parentheses. */**/***denote statistical significance on the 10%, 5% and 1% level respectively. Estimates based on specification (viii) from Table 2.

5. Effects on benefit recipients' moving behaviour

A possible consequence of the HB cuts is displacement of recipients if they cannot afford to stay in the same home after the cuts. HB recipients being forced to move into cheaper accommodation or areas might also help to explain the negative effects on property prices (if they cannot easily be replaced with other renters). To understand whether this is the case we use data from the first three waves of Understanding Society, a large annual panel survey of 40,000 households with three waves between January 2009 and May 2013. The sample is restricted to individuals living in England at wave 1 and to individuals renting their accommodation, which gives a sample of 35,210 observations from 17,252 individuals. Three outcomes are considered: whether individuals would prefer to move house in the next year, whether they expect to move in the next year, and whether they have moved in the respective period. Descriptive statistics for this sample are presented in Table 6. It is

Table 5
Interactions with share of vacant dwellings in local authority, dependent variable: Ln(price).

	Preferred sample from February 2010, only February to June transactions
Specification 1: Vacancy rate above median	
Base estimates (vacancy rate below median), scaled by 1000	
07/2010 to 03/2011 * % recipients in May 2010	0.020 (0.158)
04/2011 to 12/2011 * % recipients in May 2010	−0.020 (0.180)
2012 * % recipients in May 2010	0.022 (0.320)
2013 * % recipients in May 2010	0.048 (0.460)
Interactions with vacancy rate above median, scaled by 1000	
07/2010 to 03/2011 * % recipients in May 2010 * vacancy rate above median (dummy)	−0.006 (0.050)
04/2011 to 12/2011 * % recipients in May 2010 * vacancy rate above median (dummy)	−0.080** (0.040)
2012 * % recipients in May 2010 * vacancy rate above median (dummy)	−0.158*** (0.040)
2013 * % recipients in May 2010 * vacancy rate above median (dummy)	−0.285*** (0.074)
Specification 2: Interactions with centred vacancy rate	
Base estimates (scaled by 1000)	
07/2010 to 03/2011 * % recipients in May 2010	0.005 (0.169)
04/2011 to 12/2011 * % recipients in May 2010	−0.100 (0.000179)
2012 * % recipients in May 2010	−0.110 (0.321)
2013 * % recipients in May 2010	−0.212 (0.426)
Interactions with share of vacant dwellings (scaled by 1000)	
07/2010 to 03/2011 * % recipients in May 2010 * share of vacant buildings in year (centred)	−0.038 (0.036)
04/2011 to 12/2011 * % recipients in May 2010 * share of vacant buildings in year (centred)	−0.061** (0.029)
2012 * % recipients in May 2010 * share of vacant buildings in year (centred)	−0.088** (0.044)
2013 * % recipients in May 2010 * share of vacant buildings in year (centred)	−0.143** (0.060)
Observations	954,374

Notes: Coefficients, standard errors adjusted for clustering on the local authority level in parentheses. */**/***denote statistical significance on the 10%, 5% and 1% level respectively. All estimations also include the share of vacant buildings in a year and local authority (centred at mean). Estimates based on specification (viii) from Table 2.

important to be clear that the three periods in Table 6 do not correspond to waves and that the last wave is in fact spread over the periods "April 2011 to December 2011" and "after January 2012", i.e., the drop in observations in the final period is not due to panel attrition.

Estimation is based on a simple difference-in-differences estimator of the general form

$$Y_{it} = \alpha_i + \phi_t + \sum_{P=1}^{3} \tau_P * HB_i * T_P + \varepsilon_{it}, \tag{3}$$

where i indexes individuals and t time. We estimate four specifications of (3). We always include month-year fixed effects (ϕ_t) and estimate versions of (3) with and without individual fixed effects (α_i) and with and without interactions with dummies indicating the receipt of other benefits, specifically incapacity benefits, job seeker's allowance, employment and support allowance, carer's allowance and pension credits. We consider individuals to be treated when they received HB in wave 1 (marked by HB_i).[7] We also use the information on other benefits from wave 1. As our preferred control group we use other renters not in receipt of HB. We also tried using all non-recipients (i.e. including owner-occupiers) as an alternative control group. While including owner-occupiers leads arguably to a worse control group, as homeowners are less mobile than renters (e.g., Dielemann et al., 2000), the results were essentially unchanged. There are three treatment periods (denoted by T_P). We again distinguish between the announcement period and the remainder of 2011 to look at short run effects, but 2012 and 2013 are combined into a longer post-reform period due to the low number of observations in 2013. The estimation is carried out for two samples, London and the rest of the country.

Table 6
Descriptive statistics, individual sample.

Variable	Mean	Std. dev.	Min	Max
Housing benefit recipient	0.27	0.44	0	1
Observed 07/2010 to 03/2011	0.27	0.44	0	1
Observed 04/2011 to 12/2011	0.23	0.42	0	1
Observed after 01/2012	0.16	0.37	0	1
Would like to move next year	0.44	0.50	0	1
Expects to move next year	0.24	0.42	0	1
Moved during observation period	0.14	0.34	0	1
Observations	35,210			
Individuals	17,252			

Source: Understanding Society, Waves 1 to 3.

Overall, we find somewhat mixed results, see Table 7, largely dependent on whether individual fixed effects are included. The differences in the estimates across specifications point towards some compositional changes in either treatment or control group that are correlated with moving behaviour. It seems likely that this is driven by compositional changes among the general population of renters, for example, due to differences in who has to move house for economic reasons, and as a consequence we place greater weight on the specifications with individual fixed effects. Focussing on these specifications, the estimates suggest, if anything, an increase in the proportion of people who prefer or expect to move in the post treatment periods, but the results rarely reach conventional levels of statistical significance either for London or elsewhere. However, in terms of actual moving behaviour, we find strong evidence for areas outside of London of housing benefit recipients moving house at higher rates than other renters following the HB cuts. The smaller results in London might be due to housing demand regularly outstripping supply, which makes any property moves difficult to accomplish.

6. Conclusion

This paper analyses the effect of a major cut to housing rental subsidies on property prices in England. Estimations are carried out using difference-in-differences-type estimators that exploit the fact that local authorities are differently affected by these changes depending on the number of HB recipients that lived there prior to the reform. Overall, we find that the benefit cuts lowered house prices by an economically meaningful magnitude, but that these effects were concentrated in flats, which are more likely to be rented by those on housing benefits, and had stronger negative effects on house prices in areas where demand is lower relative to supply. We also find some evidence that the benefit cuts led to HB recipients being more likely to move home than other renters. In

[7] It is again important to fix this at a pre-reform value as whether an individual is in receipt of HB in later waves could be influenced by the benefit changes. Furthermore information on HB is missing from wave 3, making it both impossible and inadvisable to use a contemporaneous measure throughout.

Table 7
Individual-level estimates on moving behaviour.

	London			Outside London		
	Prefers to move within year	Expects to move within year	Moved within respective period	Prefers to move within year	Expects to move within year	Moved within respective period
Specification 1: Pooled OLS						
Recipient * 07/2010 to	0.008	0.036	−0.042**	−0.008	0.011	−0.002
03/2011	(0.027)	(0.024)	(0.021)	(0.016)	(0.013)	(0.012)
Recipient * 04/2011 to	0.060**	0.070***	−0.019	0.042**	0.036**	−0.023**
12/2011	(0.030)	(0.026)	(0.016)	(0.018)	(0.014)	(0.009)
Recipient * 01/2012 and	0.086**	0.121***	−0.034**	0.024	0.027*	−0.037***
later	(0.034)	(0.029)	(0.017)	(0.020)	(0.016)	(0.010)
Specification 2: Individual Fixed Effects						
Recipient * 07/2010 to	−0.016	0.012	−0.022	−0.035**	−0.006	0.034***
03/2011	(0.028)	(0.027)	(0.022)	(0.017)	(0.014)	(0.012)
Recipient * 04/2011 to	0.052*	0.043	0.021	0.019	−0.013	0.049***
12/2011	(0.031)	(0.028)	(0.021)	(0.018)	(0.015)	(0.012)
Recipient * 01/2012 and	−0.003	0.043	0.057**	0.001	−0.001	0.113***
later	(0.036)	(0.033)	(0.025)	(0.021)	(0.018)	(0.014)
Specification 3: Pooled OLS, additional interactions with other benefits						
Recipient * 07/2010 to	0.002	0.043*	−0.028	−0.028*	0.004	0.007
03/2011	(0.028)	(0.025)	(0.022)	(0.017)	(0.014)	(0.013)
Recipient * 04/2011 to	0.046	0.074***	−0.013	0.029	0.043***	−0.016*
12/2011	(0.032)	(0.028)	(0.016)	(0.019)	(0.015)	(0.009)
Recipient * 01/2012 and	0.074**	0.129***	−0.031*	0.011	0.038**	−0.027***
later	(0.036)	(0.030)	(0.017)	(0.022)	(0.017)	(0.010)
Specification 4: Individual Fixed Effects, additional interactions with other benefits						
Recipient * 07/2010 to	−0.008	0.013	−0.021	−0.036**	−0.003	0.045***
03/2011	(0.029)	(0.029)	(0.024)	(0.018)	(0.015)	(0.013)
Recipient * 04/2011 to	0.059*	0.042	0.005	0.025	−0.003	0.049***
12/2011	(0.033)	(0.031)	(0.024)	(0.019)	(0.016)	(0.013)
Recipient * 01/2012 and	−0.012	0.038	0.041	0.001	0.013	0.110***
later	(0.039)	(0.036)	(0.027)	(0.022)	(0.019)	(0.015)
Observations	9336	25,874				

Notes: Coefficients, standard errors adjusted for clustering on the individual level in parentheses. All estimates contain month dummies */**/***denote statistical significance on the 10%, 5% and 1% level respectively.

combination, these findings suggest a difficult public policy trade-off: Housing benefits enable recipients to rent certain properties that they otherwise could not afford, but at the same time they increase property prices, which affects the affordability of owner-occupied housing.

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