

THE BEDROOM TAX

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Abstract: Housing subsidies for low income households are a central pillar of many welfare systems, but an expensive one. This paper investigates the consequences of an unusual policy aimed at reducing the burden of these subsidies by rationing tenants' use of space. Specifically, we study a policy introduced by the UK Government in 2013 which substantially cut housing benefits for tenants deemed to have a 'spare' bedroom – based on specific criteria related to household composition. Our study is the first to evaluate the impacts of the policy on its target group considering a range of outcomes. To do so, we use a difference-in-difference methodology that compares the observed behaviour of the treated households relative to a control group determined by exploiting the details of the policy rules. We find that – as expected – the treated group experienced losses to housing benefit and overall income. Although the policy was not successful in encouraging residential moves, it did incentivise people who moved to downsize. We find no statistically significant effects on households' food consumption, savings or employment outcomes, despite the associated income reductions. Finally, we find some evidence of a reduction in self-reported satisfaction though this effect is not precisely estimated.

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1. Introduction

Housing subsidies for low income households are a central pillar of many welfare systems, but an expensive one. In Britain in 2015/16, housing benefit expenditure was £24.2 billion, amounting to 14% of total expenditure on benefits – an increase of 43% in real terms in the 20 years since 1996/7.¹ Attempts have been made to control these expenditures, in Britain and in similar schemes world-wide, spawning a small academic literature on the effect of these reforms on rents (Susin, 2002, Gibbons and Manning, 2006, Fack, 2006, Kangasharju, 2010, Viren, 2013, Brewer et al 2014). This paper investigates the consequences of an unusual policy aimed at reducing the burden of these housing subsidies by rationing tenants' use of space and reallocating the social housing stock.

In April 2012, the UK Government voted for a policy which took effect in April 2013 to reduce subsidy payments (housing benefit) for social tenants deemed to have a 'spare' bedroom on the basis of specific criteria related to household composition. This policy, euphemistically labelled the 'removal of the spare room subsidy' but colloquially known as the 'bedroom tax', was much criticised for its draconian regulation of low income tenants' entitlement to space and for its potential adverse impacts on their welfare (Shelter 2013). The policy targeted new and existing social tenants – that is, tenants in Local Authority (LA) provided housing or accommodation provided by housing associations and other Registered Social Landlords – and did not directly affect those in or entering private rental accommodation, even if they were claiming housing benefits.

Our study is the first to evaluate the impacts of the policy on its target group considering a range of outcomes and using a difference-in-difference methodology that compares the observed behaviour of treated families with the outcomes of a suitable control group. Although an official evaluation exists – using a survey of affected tenants and a small sample of controls (Clarke et al., 2015) – this provides no explicit treatment-control comparisons, no pre-policy survey, nor adjustments for household characteristics.

In order to carry out our investigation, we use a large panel survey of individuals and households – the Understanding Society (US) survey – covering the period 2009 to 2014. We carefully define our treatment and control groups according to the policy rules. Specifically, this means we first take the subset of the survey respondents who are social tenants in receipt of housing benefits and who have no retired household members. We then define the treated group as the set of households that are 'under-occupying' their accommodation using the official criteria – while the control group comprises of households who do not 'under-occupy' their residence. The details on these under-occupancy criteria are described in the empirical section of the paper, but broadly depend on the number of adults and on the number of children – with the number of rooms 'available' for the latter depending on their age and gender. We estimate the effect of the policy using regression analysis on household or individual level data, with individual or

¹ Source: Department of Work and Pensions, Benefit expenditure and caseload tables, 2016
https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/554069/benefit-expenditure-by-country-and-region-2015-16.ods

household fixed effects and controls for household composition and characteristics. This design means that we compare the pre-post policy changes in outcomes for social tenant households who under-occupy their accommodation with the pre-post policy changes for comparable households who do not.

The sample restrictions dictated by this research design imply that we end up with a relatively small sample of 176 treated households and 588 control households. However, we observe these households multiple times before and after the policy intervention date. Moreover, the relative persistence over time of the outcomes we study in the absence of any policy treatment effects imparts our study with more statistical power than these sample sizes might at first suggest.

In a nutshell, we find that, as expected, the treated group experienced losses to housing benefits and overall income. However, the policy was unsuccessful in encouraging residential moves – though tenants who moved did downsize. We are unable to find the margins on which tenants adjusted to these income reductions, apart from a small increase in youth employment. We find no adjustment to food consumption or savings, although changes in consumption featured in studies based on self-reported behaviour in Clarke et al. (2015) and Bragg et al (2015). Somewhat in line with what was predicted by its critics, the policy reduced well-being, at least in so far as this is captured by the self-reported measures of life-satisfaction – as well as satisfaction with health, income and amount of leisure time. However, most of these effects are not statistically significant or large.

The rest of the paper is organized as follows. In the next section, we discuss the data we use. In Section 3, we present our empirical methods. In Section 4, we present descriptive statistics, while in Section 5 and 6 we present our household and individual level findings, respectively. Finally, Section 7 provides some concluding remarks.

2. Data construction

We use household and individual level data from the Understanding Society (US) survey. US is a longitudinal annual survey conducted by the Institute for Social and Economic Research (ISER) at Essex University. The sample was selected to be nationally representative of households in the UK and every adult member of the sampled households (age 16 or above) is interviewed using a computer assisted personal interview (CAPI) software. One individual per household – usually the household head – answers the household questionnaire. Younger individuals (age 10-15) respond to a shorter, self-completion questionnaire.

The first wave was collected during the time window covering January 2009 and 2011, and at present five data waves are available. The same households and individuals sampled in the first wave are re-interviewed in subsequent waves – approximately 12 months after the first survey. Households and individuals who move within the UK are followed to their new address, and new individuals joining the sampled households are also interviewed. The first two waves included approximately 30,000 individuals though the number progressively declined towards 25,000 in the subsequent waves.

The survey covers a number of topics including individual background information, family structure, educational attainments, labour market outcomes, financial resources, tenure and housing conditions, and benefit eligibility and claims. As such, it is well designed to study the impact of policy interventions as well as general trends in socio-economic outcomes in the UK population.

For our analysis, we retain data covering the period 2009 to 2014. Given that the ‘bedroom tax’ policy was announced in April 2012 and enforced in April 2013, this gives 3 years and 4 months of data before the announcement of the reform and 2 years and 8 months after it. Since the reform targeted families in/entering social housing and receiving housing benefits, we only keep households identified as such. This group includes tenants in LA provided housing as well as households in accommodation provided by housing associations and other registered social landlords. On the other hand, we exclude families on social benefits but in/entering private rental accommodation as these were not targeted by the policy. As already mentioned, the policy affected families with a ‘spare room’. The notion of spare room was based on very detailed criteria about room occupancy that would take into account the age, gender and the relationship between household members. We discuss these in detail in the next section, where we lay out our empirical methods and define ‘treated’ and ‘control’ households – i.e., those with and without a spare room. Furthermore, we only keep households that include male individuals aged below 60 and women aged below 55 – i.e., five years before retirement age – because the criteria used to determine who has/does not have a spare room were levied for families with retired people.

We investigate outcomes at both the household and the individual level. At household-level, we extract a wide ranging set of outcome variables. To capture possible effects on household size and space we use number of bedrooms, number of household members and number of rooms per person – taken directly from the household-level questionnaire. For residential mobility, we construct a dummy indicating whether the households’ spatial location (geographical coordinates) has changed between one survey and the next. Household overall income, labour income and social benefits income variables are derived by aggregating the individual-level data across household members.² Given that the reform entailed a cut in housing subsidies for the affected households, we also single out the amount of housing subsidies received by the household from the total income obtained from benefits. Next, we define a dummy variable identifying whether the household makes some savings at the end of the month, and a continuous variable that measures the weekly amount of money spent on food. Lastly, we follow the approach used in McFall and Garrington (2011) and construct three proxies for households’ material deprivation. The first one is an indicator of ‘lifestyle changes’ that considers answers to the following questions: whether the occupied house is in decent state of repair; whether the household takes holidays at least once per year; whether worn out furniture can be replaced; whether the household has insurance; whether major electrical goods can be replaced/repaired; and whether individuals in the household have money for their selves. The second indicator gathers information about ‘financial stress’ by considering whether the household is able to: keep

² Note that since income questions refer to monthly amounts, we obtain weekly figures by multiplying all numbers by 12 and dividing it by 52. This follows standard practice.

up with bills; keep up with council tax payments; keep up with rents; and overall up to date with all bills. The third indicator, instead, relates to ‘durable good purchase’ and considers the following items: colour TV; video/DVD; satellite; cable TV; deep freezer; washing machine; tumble drier; dish washer; microwave; home PC; compact disc player; landline telephone; mobile phone. In order to analyse whether the policy had an impact on levels of deprivation, we construct three separate indicators by summing the various items in the three different groups – i.e. ‘lifestyle’, ‘financial stress’ and ‘durable goods’ – and then standardizing the resulting numbers in the full sample (i.e., prior to only focussing on households on benefits and in council/housing association provided accommodations). Given the way in which answers are coded in US, larger values of the ‘lifestyle’ and ‘financial stress’ variables correspond to worse outcomes, while smaller values of the ‘durable goods’ variable correspond to worsening standards of living. Furthermore, we obtain an overall material deprivation index by summing all the answers in the ‘lifestyle’ and ‘financial stress’ categories and subtracting the ‘durable good’ answers – and standardizing the resulting figures in the full sample. Larger values of this indicator correspond to worse material standards of living.

We also look at a number of individual-level outcomes. These are useful in assessing whether the policy had differential impacts on individuals’ outcomes depending on the characteristics of the different household members. Firstly, we look at employment, using a dummy indicator capturing whether or not the respondent is in paid employment at the survey date. We also consider the amounts of income received overall, from labour activities and in the form of benefits – i.e., we analyse the individual-level variables we used to construct the household-level income information discussed above. Finally, we collect information about level of satisfaction with the following four items: their health; their income; the amount of leisure they can enjoy; and with their life overall. The answers to these questions range from “Completely dissatisfied” – coded to 1 – to “Completely satisfied” – coded to 7. We treat this variable as ordinal with larger values corresponding to higher levels of satisfaction – though in some checks we investigated whether our findings are robust to dichotomising these indicators.

Descriptive statistics and number of observations for the retained households, individuals and variables are provided in Section 4 – after we discuss our empirical methods and the definition of affected and unaffected households in the next section.

3. Empirical methods

The aim of our analysis is to estimate the causal effect of the policy on a number of household and individual level outcomes. The nature of the policy implies treatment was not randomly allocated – rather, it was determined on the basis of information about household size and composition, and in relation to the number of rooms in the occupied accommodation. The main concern is that the same household and individual characteristics that determine treatment might be correlated with the outcomes under analysis –

either directly or through other unobservable individual/household level attributes – preventing us from estimating the causal impact of the ‘bedroom tax’.

To formalize our approach, we aim to identify the policy impact δ by estimating the following difference-in-difference equation:

$$y_{itl} = \alpha_i + \delta Treat_i \times I(t \geq PolicyOn) + \sum X_i^{pre} \times I(t \geq PolicyOn)\Lambda' + \theta_{tl} + \varepsilon_{itl}$$

Where y_{itl} denotes outcome for household i at time t and living in LA l ; α_i is a household-level unobservable; $Treat_i$ identifies whether the household is subject to the bedroom tax; and $t \geq PolicyOn$ identifies the observations following the enforcement of the policy (i.e., April 2013; in some robustness checks we use the policy announcement in April 2012 as the cut-off date). We further add to our empirical framework: a) interactions between household characteristics measured prior to the policy announcement (specifically, average age, housing tenure and number of bedrooms per person) and the policy-on indicator; and b) LA-by-year of survey effects (θ_{tl}). Last, ε_{itl} is an error term assumed to be uncorrelated with the other variables in our empirical model.³ We estimate this equation using Ordinary Least Square (OLS) even in instances where the dependent variable is a binary outcome. Moreover, to take into account any possible correlation in outcomes at the household level (over time and/or across individuals), we cluster standard error at this level.

In order to classify families as treated or non-treated, we use household demographics and characteristics of the accommodation measured in April 2012 – when the policy was announced – to reconstruct the very detailed policy criteria that determine households with a spare room. In particular, the legislation dictated that one bedroom would be allowed for the following main groups: *a)* every adult couple; *b)* any other adult aged 16 or over – including any son, daughter, stepson/stepdaughter; *c)* two children under 10; *d)* two children under 16 of the same sex; *v-* any other child (where, for example, there are three children under 10). Although these were the main features of the legislation, specific guidelines were provided for individuals with disabilities and their carers – for example, two adults forming a couple could occupy different rooms if one or both individuals had disabilities making it more appropriate to have separate spaces within the house. Other exceptions were also made for individuals serving in the armed forces and/or for students residing away from home – depending on whether their residence had been permanently moved or they could still be considered as residing with the original household. In order to determine households with a ‘spare room’, we only consider the main categories as we are not able to identify individuals that fall into these exceptions. This omission is unlikely to substantially affect our treatment and control group variables as these groups should only involve small numbers of individuals.

Based on these guidelines, we consider a household as ‘treated’ if it occupies more room than it is entitled to. Conversely, we label a household as a control if it resides in an accommodation with the correct number of rooms given its demographic structure. Note that the policy dictated that social tenants with one

³ Note that we also include wave and month-of-interview dummies; however, we do not index these in our equation to simplify notation.

under-occupied room would face a 14% cut in the benefits; while households on benefits with two under-occupied rooms would face cuts of 25%. In our data, most treated households have one under-occupied room so we do not consider this distinction.

Our approach to estimating the causal impact of the bedroom-tax policy compares the outcomes of treated and control households before and after the policy was enforced in April 2013. This difference-in-difference approach exploits the longitudinal dimension of the data to control for household unobservable characteristics α_i that might be correlated with both the treatment and the outcomes of interest. Since we do not have a balanced number of observations per household before/after the policy and given that we do not expect any effects to result in one-year changes in the considered outcomes, we control for fixed effects by within group differencing – rather than first differencing.⁴ Further, the interactions terms between pre-determined household characteristics and the policy-on dummy included in our empirical models capture any possible impact of time trends in outcomes related to family structure. Lastly, the LA-by-year effects control for unobserved time-varying shocks at the LA level – e.g., changes in the affordability of housing or other possible housing-related local policy changes⁵ – that might be related to both treatment and outcomes. The assumption underpinning our identification approach is that – conditional on these controls – policy assignment is ‘as good as random’. We discuss the validity of this assumption in the next section.

Note that when we study individual level outcomes, we replace household fixed effects with individual fixed effects and cluster our standard level at this level.⁶ Treatment is still defined at the household level and based on the households where individuals resided at April 2012 – i.e., at the point of the policy announcement. Furthermore, we continue to control for pre-determined household level characteristics interacted with the policy-on variable and for the LA-by-year shocks.

The individual level analysis allows us to study whether the impact of the policy was heterogeneous according to background characteristics of household members – for example, their age in April 2012 or gender. To do so, we augment our models with interactions between the term $Treat_i \times I(t \geq PolicyOn)$ and the relevant individual attribute Z_i . We further control for possible changes in the effect of this attribute following the policy reform that are common among treated and untreated individuals – i.e., we include in our models the term $I(t \geq PolicyOn) \times Z_i$. On the other hand, since all the characteristics we consider are time-fixed, their direct impact on the outcome is absorbed by the individual fixed effect.⁷

⁴ Not all households are interviewed at the same time because the US survey period covers up to two years per wave. This means that not all households will be interviewed the same number of months before/after the policy further making it difficult to work with first-differenced data.

⁵ Braakman and McDonald (2015) investigate the impact of changes in the Local Housing Allowance (LHA) on property prices. Although this reform occurred at the same time as the ‘bedroom tax’ we analyse, the implied variation in the LHA changes was at the LA-by-year level – so any potential confounding effects are controlled for in our specifications. It should also be noticed that relative to the potential subsidy cuts implied by the ‘bedroom tax’ policy, reforms to the LHA amounted to relatively small reductions in housing benefits.

⁶ We also tried clustering at the household level and it did not alter our main conclusions.

⁷ This ‘interaction’ approach assumes that all other controls have the same impact on the outcome under analysis irrespective on the individual’s characteristics. An alternative split-sample approach is more flexible in this respect but at the cost of being inefficient (i.e., it does not impose restrictions on the data to improve precision). Given that we have a limited number of observations, we have decided not to follow this route. However, we checked that the main patterns survive even when we use a split-sample approach.

4. Descriptive statistics

We present the first set of descriptive statistics in Table 1. This reports information at the household (top panel) and individual (bottom panel) level for the variables used in our analysis. These are measured in the last observation period prior to the policy announcement and tabulated for the treated group only.

As shown in the top panel, there are 176 treated households with approximately 2.2 members and occupying accommodations with approximately 2.7 rooms – giving rise to a bedroom/person ratio of around 1.6. The household’s average rate of mobility is 6.8% while the overall weekly income is approximately £322. Most of this comes from social benefits (£271.5) with only a small contribution from labour income (£43.9). Note that housing subsidies account for nearly 30% of the social benefits. Only around 16% of the households report that they manage to save money, and the average weekly food expenditure is £48.4. We also find that treated households have higher level of deprivation along all the margins we consider than the average household in the US (recall that these variables are standardized in the full sample – i.e., prior to focussing only on treated/control households only).

The second panel presents data at the individual level. We have 260 working age adults with non-missing basic labour market outcomes (i.e., work or not) in our sample. Working age adults are defined as males aged 16 to 60, and women aged 16 to 55. Approximately 23% of these are working. As expected, the individual weekly income levels are smaller than the household level amounts but the relative importance of benefits vis-à-vis employment income also emerges when focussing on individual data. Finally, the average level of satisfaction ranges from 3 for income – below the middle of the 1 to 7 scale – to 4 for leisure – i.e., at its mean level.

Table 2 compares treated households and individuals to the control group. Panel A of the table focuses on household level information. There are overall 588 control households. Expectedly, given the nature of the bedroom tax, treated households have significantly more bedrooms per person. This difference is driven by a smaller number of individuals in treated families and a larger number of rooms – both discrepancies being significant. Treated households also have a significantly higher average age (approx. 9.8 years or 37% older than the control group) and longer tenure (3.3 years or around 43% more years than the control families). As discussed – and given the possibility that these pre-determined demographic characteristics could affect households’ (and individuals’) outcomes following treatment – in our specification we control for an interaction between these attributes and the dummy identifying the policy-on periods. However, any time-fixed effect of these attributes – and related unobservable characteristics – on our outcomes of interest is absorbed by the household fixed effects we use throughout our analysis.

Further down the table, we see that the average rate of mobility is lower for treated households – though the difference is not statistically significant. Furthermore, there are some differences in the levels of income – though most of these are not statistically significant, especially when controlling for LA-by-year effects. These differences show that treated households have lower levels of overall weekly income than the control – by approximately 7% – and that all of the difference comes from social benefit payments. There are instead no differences in the percentage of households able to save some money, and a small

(approx. 16%) but significant difference in weekly food expenditure. Similarly, we find no differences in the levels of deprivation for all the items we consider.

Panel B of Table 2 reports statistics for the variables at the individual level. There are nearly 950 control individuals – though the exact number of observations varies depending on the variable considered. At the individual level we find less evidence of discrepancies in income levels, though once again the observed differences (conditional on LA-by-year effects) suggest that treated households earn slightly less – with social benefits explaining this gap. Note also that there are no substantial differences in the probability of working, and that individuals in treated households are less satisfied – between 0.34 and 0.96 points on the 1-7 scale, depending on the satisfaction measure – with these differences being significant at conventional levels.

Appendix Tables 1 to 3 provide more descriptive information about our sample. There are approximately 560 observations before and 250 observations after the policy was implemented for the 176 treated households. The corresponding figures for the 588 families in the control groups are nearly 1,850 and 900. Furthermore, 29 and 115 households in the treated and control groups respectively can be identified as movers – with roughly equal mobility instances in the two groups before and after the policy change (proportionally). The underlying number of individuals within these households is 367 in the treatment group and 1712 in the control group, giving 1,605 survey observations in the treated group (1097/508 before/after treatment) and 7,730 observations for the controls (5144/2586 before/after treatment). There are slightly more females in the sampled households as well as smaller groups of individuals aged below 21 and close to retirement age – i.e., between 55 and 60 for man, and between 50 and 55 for women.

Note that when we focus on individual level variables we restrict our attention to working adults only with non-missing data on employment status (see Section 2). This reduces sample sizes considerably – though the proportions remain similar. Some figures are provided in Appendix Table 2 for individuals in the treated group with non-missing information in either their labour market outcomes or their satisfaction levels. Once again, we observe more females than males – and a large number of individuals in the working age bracket. There are again small groups of young individuals (16-21) and nearly retirees (above 50/55 for women/men respectively). Appendix Table 3 tabulates the mean of the outcome variables for individuals in these groups. We find that males are more likely to work than females, and that non-head of household and 16-to-21 year-old individuals are the two groups most likely to be in employment. The head of the household is the individual reporting the highest level of overall income – with this largely explained by the amounts of benefits claimed. On the other hand, the highest level of labour income is found among the non-head of households and young individuals (who have the highest probability of being employed). We also find that non-head of household and 16-to-21 year-old individuals are the two groups reporting the highest levels of overall satisfaction, as well as satisfaction with health and income. On the other hand, the highest level of satisfaction with leisure is reported by nearly retired females.

5. Household level analysis

5.1 Effects on the targeted outcomes: housing benefits, mobility and household structure

We start our investigation by studying whether the policy impacted the amount that households receive in the form of housing benefits. Our evidence is presented in Table 3. Columns (1) to (3) consider all households. Columns (4) to (6) still pool all households, but single out the impact of the policy for families who stay/move by interacting the treatment variable (and the policy-on control) with a dummy identifying households that relocate. Finally, Columns (7) to (9) focus on households that do not move. Columns (1), (4) and (7) only include household and LA-by-year effects alongside wave and month dummies. Columns (2), (5) and (8) instead add interactions between household average age and tenure length measured before the policy was announced and the policy-on indicator. Finally, Columns (3), (6) and (9) further append an interaction between the number of bedrooms per person (measured pre-policy announcement) and the policy-on dummy.

The first two columns show that the policy did reduce benefits by approximately £6-£7 per week. Given a baseline amount of housing subsidies among treated household of around £80, this is not far off from the expected 14% reduction mandated by the policy for anyone with one spare room. This result weakens somewhat in Column (3) where we add the interaction terms between the policy-on dummy and the number of bedrooms-per-person. However, the coefficient still reveals a negative and sizeable reduction of about 5% (though not significant).

Columns (4) to (6) reveal that all of the effect is concentrated on the households who do not relocate. On the other hand, households that move experience an increase in housing benefits though this is not statistically significant. These results are further strengthened when we move to the last three columns that only consider households who do not relocate. In this case, we find an effect of £7-£9 pounds – even closer to the expected reductions implied by the policy. Note that we consider the specification of Column (8) as our favourite. This model controls for household and LA-by-year unobservable characteristics – as well as for the possibility that the imbalances in terms of average age and tenure length of the treated households documented in Table 2 affect their outcomes post-treatment. The specification reported in Column (9) is a robustness check for possible post-policy effects of baseline differences between treated and control households in terms of bedrooms-per-person. In this regression, identification of the policy effect now comes from the more nuanced aspects of the treatment group definition, rather than simply pre-policy bedrooms per person. Clearly, this interaction term partly captures the impact of the policy itself so may amount to ‘over controlling’. Nevertheless, broadly speaking, the results in Columns (8) and (9) provide similar conclusions, and we will continue to report both specifications for comparison in subsequent tables.

So far we have focussed on the enforcement date – i.e., we have defined as ‘policy on’ all time periods from April 2013. In Table 4, we study whether housing benefits actually do change in response to the enforcement of the policy as opposed to its announcement in April 2012. The table focuses on stayers

where we expect to observe an effect and presents the three specifications reported in the various columns of Table 3. We find that the enforcement date has a slightly larger effect when controlling for any possible effect of the policy announcement on the treated households' housing benefits. Using our favourite specification (Column 2), this impact is found to 14% – i.e., identical to the expected reduction for households with one spare room. On the other hand, we find a positive but non-significant effect of the policy announcement. This evidence is reassuring about our empirical design: we find a policy effect on the variable most directly impacted by the bedroom tax reform *where we expect to find it* and no effect where none should be documented. This suggests that our method enables us to isolate the impact of the policy – while netting out other unobservable characteristics that might contaminate our causal inference.

Table 5 examines another of the targeted outcomes of the policy: mobility. Indeed, the aim of the policy was at the same time to reduce the overall cost of housing welfare policies and force relocation of individuals with 'too much space' to smaller dwellings – releasing space for households in need of a larger home. The table clearly shows that the policy was not successful in this respect. If anything two of the three point estimates suggest that the policy reduced mobility – though the numbers are very close to zero and not significant.

We also investigated whether these effects – as well as those referring to the amount of housing benefits received – are heterogeneous for households living in council housing (i.e., Local Authority provided) as opposed to those living in social housing (i.e., provided by Housing Associations or Registered Social Landlords) given that the type of properties as well as the way the housing stock is managed are different between these two entities. However, we found no significant differences between council and other social tenants in terms of housing benefit reductions or mobility (the results are not tabulated but available from the authors).

Tables 6 to 8 investigate whether the policy affected the structure of the household or the type of accommodation occupied by individuals. The first table focuses on the number of bedrooms per person. Note that in this case we do not estimate specifications that control for the 'bedroom-per-person' \times 'policy-on' interaction. The second table instead concentrates on the number of rooms, while the last table studies the impact on the number of people in the household.

The first two columns of Table 6 show that the policy had an overall small, negative effect on the number of bedrooms per person. However, as made clear in Columns (3) and (4) all of this effect is concentrated among movers – who experience a 0.5 reduction in the number of rooms per household member following relocation. On the other hand, we find no changes for stayers (see Columns 5 and 6).

Table 7 sheds further light on this finding by looking at the number of rooms in the accommodation. Once again, we find a small overall effect (Columns 1 and 2) that is completely explained by changes in accommodation size for movers. These experience almost a one-room reduction after relocating – in line with what expected if individuals move with the aim of complying with the policy.

Lastly, Table 8 looks at whether household size changes in response to the policy. We find this is not the case – irrespective of whether we pool all individuals or consider movers and stayers separately. We

further investigated whether we find changes in household size when focussing only on young members (age 16-21), working age adults, near retirees and very young babies – i.e., those below one which might have been conceived to ‘neutralize’ the policy. We also studied whether we see any change in the number of adults residing with the household who are not family members – and could instead be ‘lodgers’. Irrespective of the specific groups we consider, we never detect any sizeable and significant effect. This suggests that anecdotal evidence and ‘media rumours’ about social tenants subletting their space or having children to by-pass the ‘bedroom tax’ find no empirical support in our data.

5.2 Effects on the other outcomes: income, savings, food expenditure and deprivation

Besides affecting housing benefit receipt, the policy could have impacted households’ income level by changing their labour market behaviour and/or by affecting other benefit claims. We provide some evidence in Table 9. The table presents results for overall weekly income (Columns 1 to 3); labour income (Columns 4 to 6) and benefit income (Columns 7 to 9). The latter category includes housing benefits but also incorporate amounts received, for example, in unemployment benefits, disability claims and child benefits. The sample used in the analysis considers both stayers and movers – although the results on stayers only are very similar (not tabulated, but available upon request). Lastly, the different specifications presented in the table follow the structure of the empirical models used in Table 3.

Columns (1) to (3) show that overall weekly income falls after the policy by between £20 and £26. The largest point estimate (Column 1) is clearly significant, while the one from our favourite specification (Column 2) is borderline significant at the 10% level with a t-statistic of 1.63. The documented coefficients are 2-2.5 times as big as the drop we found in housing benefits (documented in Tables 3 and 4). This is surprising and warrants further investigation.

To shed light on this issue, Columns (4) to (6) study changes in labour income. Although our results do not yield any significant effects, we find consistently negative results – of up to £10 using our preferred specification in Column (5). Furthermore, Columns (7) to (9) suggest that the drop in housing benefit was accompanied by further reductions in income derived from other benefits. Once again, although the estimates are not statistically significant, the £17 reduction in Column (8) is almost twice as large as the drop in housing subsidies measured in Table 3. We return to these issues below where we analyse individual level data to identify more precisely what behavioural responses might determine these income shortfalls.

Before concluding, we study whether these income reductions affect savings, consumption behaviour and material deprivation. Our first set of results is reported in Table 10. Columns (1) to (3) show that the policy is associated with a small positive impact on the households’ propensity to save – of approximately 16%-20%. However, none of the estimates is close to significance at conventional levels. Columns (4) to (6) instead show mixed patterns in terms of food expenditure. Using our favourite specification (Column 5), we find that food consumption increased by £1.45 per week – or 3%. Besides being not significant, the

sign of this estimate is counterintuitive and its size is very small compared to the income reductions documented above.

Table 11 focuses on the three measures of material deprivation – lifestyle changes, financial stress and durable good purchase – discussed above and the composite indicator that combines the information from these three categories. Most of the estimates point in the direction of an increase in material deprivation – although none of the coefficients is significant at conventional levels. Focussing for example on the overall deprivation indicator and using our favourite specification reported in Column (11), the ‘bedroom tax’ is associated with a 18.7% increase in material deprivation – though the standard error of this point estimate is 0.208 (giving rise to a t-ratio of 0.89), making it far from significant.

All in all, the evidence from this section suggests that the policy implied a reduction in income for targeted households. This reduction was more than proportional to the housing benefit cut mandated for families with a spare room. On the other hand, the policy did not succeed in incentivising mobility – nor did it affect individuals’ consumption and saving patterns. As a result, the policy might have worsened households’ standard of living – although any such impact cannot be precisely estimated using the material deprivation proxies we consider. Given the evidence collected so far, we can only conclude that affected households made do with what they had and set in place other coping strategies that we cannot measure given the data at hand.

5.3 ‘Event studies’ policy estimates

We wrap up this section by presenting some ‘event studies’ policy estimates focussing on the following household level outcomes: housing benefits; mobility; overall income; labour income; income from benefits; savings; and food expenditure. Our results are presented in Figure 1. These show point estimates and associated 90% confidence intervals of the effect of the ‘bedroom tax’ spanning several quarters before and after its enforcement. The omitted period against which all outcomes are benchmarked is centred on five quarters before the policy came into force – in April 2013. This also corresponds to one quarter prior to the policy announcement in April 2012. The aim of the figure is twofold. First, it allows us to investigate whether there are significant pre-trends in most of the outcomes we have considered. Second, it allows us to investigate whether any significant impact on our variables of interest takes time to materialise and/or occurs at a specific time/quarter.

Starting from the top-left corner, we find that the policy had a significant effect on housing benefits in three out of the six quarters following its enforcement – i.e., quarter one, two and five. Two of the other three quarters are negative – though not significant – while the remaining one is very close to zero. Interestingly, we also observe an inflection one and two quarters before the enforcement – i.e., three and four quarters after the announcement – although these estimates are not significant. Overall, this evidence confirms the insights gathered from Table 4 where we compared the impact of enforcement and announcement on changes in housing benefits – and found that only the former mattered. The top right

corner instead focuses on mobility and confirms our previous evidence: the policy did not significantly affected households' propensity to move.

The next three plots focus on income. Consistently with the evidence presented in Table 9, we find an overall income reduction – which is predominantly linked to a decrease in the amount of benefits but further amplified by a decline in labour income. The most negative estimates occur two and three quarters after the policy enforcement – although none of them is statistically significant. We also find some evidence of a drop in income from benefits – matched by a corresponding drop in overall income – one quarter before the policy was enforced. This estimate is not significant and could simply be driven by random fluctuations in the outcome variable over time.

Lastly we focus on savings and food expenditure. Once again we find no evidence of any significant effect following the policy or of any substantial pre-trends in these variables. These results are consistent with the findings in Table 10.⁸

6. Individual level analysis

6.1 Effects on the probability of working

We begin our investigation of individual level outcomes by studying whether the policy had an effect on the probability that individuals have a job. Our first set of results is reported in Table 12. The first three columns pool all individuals, whereas the subsequent columns single out the impact of the policy on the employment of the following subgroups: *i*- heads of household (Columns 4 to 6); *ii*- female members of the households (Columns 7 to 9); *iii*- individuals aged 16 to 21 (Columns 10 to 12); *iv*- nearly retirees, i.e. males age 55-60 and women aged 50-55 (Columns 13-15).

Starting from the first three columns, we find no evidence that the policy changed individuals' propensity to work.⁹ This pattern is confirmed in the bottom-right plot of Figure 1 where we present 'event studies' estimates of the policy on this outcome: most of the estimates of the policy effect are small and close to zero following its enforcement in April 2013. If anything, we find a relatively sizeable impact of the policy two quarters after its announcement, although this estimate is barely significant.

Some patterns emerge when we study the variation in the policy effect among different household members. The point estimates in Columns (4) to (6) show that heads of household have a lower probability of working following the policy enforcement. Although these estimates are not significant at conventional level, the magnitudes are quite sizeable – implying a 30%-35% reduction in the likelihood of having a job. Columns (7)-(9) further document a negative and significant effect on the probability that female members of the households have a job – corresponding to approximately a 25% decrease in the likelihood of working.

⁸ The last plot in Figure 1 refers to the probability of having a job. We discuss to this graph in the next section.

⁹ We also looked at the distinction between full-time vs. part-time employment, and the 'intensive' margin – i.e., the number of hours worked, conditional on working but found no evidence that these outcomes are affected by the policy.

Conversely, we find a positive impact on the employment probability of young individuals aged 16-to-21 – at around 7%-10% of the baseline rate – and a similarly positive but smaller impact on near retirees. None of the effects is statistically significant.

Given the small sample sizes involved in our analysis, it is legitimate to ask whether we lack statistical power to detect any significant effect. To shed light on this issue, one can carry out the following calculations. First, assume that treatment is as ‘good as random’ and that the two groups have identical pre-policy mean outcomes – i.e., for both individuals in treated and non-treated households the baseline probability of having a job is 0.25. This gives a variance of 0.1875 (i.e., $(0.25) \times (1-0.25)$) and a standard deviation of 0.43. Second, consider we are essentially working with changes in employment given our difference-in-difference methodology and that we have approximately a ratio of treated-to-control of 0.275 (i.e., 260/946). The variance of the change in outcome can be written as $Var(Work^{Post} - Work^{Pre}) = Var(Work^{Post}) + Var(Work^{Pre}) - 2Cov(Work^{Post}, Work^{Pre})$. Given our sample size and assuming a power of 0.80 (as standard in the practice), this means we need the standard deviation of the change in employment pre/post policy to be 0.288 (i.e., a variance of 0.0829) if we want to detect an effect of 0.05 with 10% significance level. Rearranging the formula above, we can reformulate this in terms of covariance of outcomes before and after the bedroom tax, i.e.:

$$Cov(Work^{Post}, Work^{Pre}) = [(Var(Work^{Post}) + Var(Work^{Pre}) - Var(Work^{Post} - Work^{Pre}))]/2$$

Using the numbers discussed above, this means $Cov(Work^{Post}, Work^{Pre})$ needs to be 0.1469. By further dividing this expression by the variance of $Work^{Pre}$ (at 0.1875), this can be interpreted as the required regression coefficient of post-policy employment probability on the pre-policy likelihood of working that would allow us to estimate a 10% significant 0.05 employment effect given sample size. This approach yields a ‘target’ regression coefficient of 0.78, which we are able to confront with the actual coefficients we find when regressing post ‘bedroom tax’ employment outcomes on the corresponding pre-policy variables. Our investigations provide reassuring evidence: the coefficient on the lagged-dependent variable is always in the region of 0.65-0.70 (depending on the controls we include in our analysis and on whether we focus on non-treated individuals or also include members of treated households), with 95% confidence intervals confidence level clearly covering the 0.75-0.78 area. This is true for both the employment variable discussed in this section, as well as the income variables which we study next. In short, this suggests that – despite the fairly limited sample sizes – our analysis has relatively strong power given the persistence of outcomes across the policy enforcement date.

We continue our investigation in Table 12 where we study whether the impact of the policy varies depending on the household’s income prior to the ‘bedroom tax’ announcement. To do so, we create three dummy variables that identify whether the family had earnings (i.e., labour market income) below the sample median; in the bottom quartile of the sample distribution; and in the top quartile of the sample distribution. Our evidence shows that the broadly negative effects documents in Table 11 are all concentrated among families in the bottom half or bottom quartile of the earning distribution. While not

significant, these estimates are sizeable and basically imply a reduction to zero in the labour market participation individuals in these households.

Although these findings need to be interpreted with caution – especially given the small sample sizes – they are consistent with some perverse incentives created by the ‘bedroom tax’ in interaction with other aspects of the welfare system. In particular, earnings beyond a certain threshold trigger a reduction in the amount of housing benefits that a family is entitled to. This occurs because of deductions made to the housing benefits when the claimants earn more than a given weekly personal allowance (e.g., £160 approximately for a single under 65) or because of deductions made when non-dependant members of the household have income in varying earning brackets (e.g., a non-dependent household member aged above 18, working at least 16 hours per week and earning less than £136 entails a benefit reduction of nearly £15 per week; figures updated to April 2017). It is thus possible that individuals in the low-to-middling part of the labour income distribution – where these incentives are likely to be the strongest – might have reduced their labour market activities to neutralize, at least in part, the negative effect of the ‘bedroom tax’ policy.

Unfortunately, given the limited sample size of the US data, we are not in a position to investigate this issue further by exploiting the non-linearities in the incentives provided by the ‘spare room’ policy in interaction with these other aspects of the housing welfare programme.

6.2 Effects on income from different sources

Tables 14 to 16 study the effect of the policy on individuals’ weekly income by focussing on overall income (Table 14), labour income (Table 15) and income derived from social benefits (Table 16). The tables have the same structure as Table 11 and investigate the effect of the policy by first pooling all individuals and then by singling out the impact for household members with difference characteristics.

Starting with Table 14, we find a £15-£17 reduction in average weekly income, increasing to £26-£30 when focussing on the head-of-household. Although this point estimate is not significant, its size is consistent with the evidence provided in Table 9: overall household income decreases by around £25. This is likely due to a loss of housing benefits – mainly claimed by the head-of-household – coupled with female household members also reducing their workforce activities. Consistently, we find that the overall income of women in the treated households decreases (by approximately £12; not significant). This is in part compensated by a positive employment response among young and nearly retirees – and consistently their overall income increases by around £15 (though not significantly). Furthermore, the income of near retirees also increases – though by a smaller extent (£10).

We investigate these issues in more detail in Table 15 and 16 where we focus on labour and benefits income, respectively. As in Table 13, none of our estimates is significant – but the patterns are instructive and relatively consistent with the findings discussed above. Starting from Table 14, we find both heads of household and women experience relatively sizeable labour income drops (of £10 and £11 approximately). This is consistent with our previous findings showing that they are the two groups most likely to reduce

their labour supply following the policy. Conversely, the young increase their weekly labour income by approximately £19 – consistent with the results showing that they are more likely to work following the policy enforcement. We also find that near retirees earn less from labour income – although previous findings suggested that the policy modestly increase their work probability.

Table 16 further shows that head-of-households experience a loss in income from benefits of approximately £17. This is consistent with the drop reported in Table 9 and justified by the fact that household heads are most likely the family members claiming household-level benefits – especially the housing benefits targeted by the policy, but also other benefits disbursed at the household level (e.g., child benefits). We also see a small reduction in the amount of benefits received by women and a more substantial drop in the benefits received by young individuals. Finally, near retirees seem to increase the amount of weekly income derived from benefits. Coupled with the result documented on the labour income and employment participation for this group, this pattern suggest that near retirees might be at the same time anticipating retirement – and thus claiming a pension – and possibly finding some small jobs – paying less than their previous employment. Unfortunately, given our small sample size, this conjecture remains speculative and we cannot dig deeper into this finding.

6.3 Effects on satisfaction

We present our results in Table 17. This focuses on four different outcomes. The first three columns study the impact of the policy on satisfaction with an individual's own health. Columns (4) to (6) focus on satisfaction with income, while Columns (7) to (9) investigate possible effects on satisfaction with the amount of leisure time. Finally, Columns (10) to (12) focus on overall life satisfaction. Across all columns smaller values correspond to lower levels of satisfaction.

Columns (1)-(3) show that the policy lowered satisfaction with health although none of these estimates is statistically significant and the implied magnitudes are small. Columns (4) to (6) similarly show that the policy negatively affected satisfaction with income. However, in this case the coefficients show a relatively sizeable effect – significant at the 10% level for one of the three specifications – implying up to a 10.8% decrease from the mean income satisfaction. Columns (7) to (9) conversely show that satisfaction with leisure time increased following the enforcement of the bedroom tax reform – but once again the point estimates are far from being precisely estimated. Lastly, Columns (10) to (12) show that overall life satisfaction was not altered following the policy change. This suggests that the decrease in satisfaction with income was evened out by the small increase in satisfaction with leisure time..

In some extensions to this analysis, we further studied whether the impact of the policy on overall life satisfaction is heterogeneous among the different household members considered above. Our results are not tabulated for space reasons. Overall, we failed to find systematic and significant patterns. However, our estimates pointed in the direction of more negative effects on the life satisfaction of young family members, and slightly positive effects for heads of households. Though highly speculative, a possible interpretation of these results is that they are mediated by individuals' labour market participations: the

group with improving well-being – household heads – is the one more likely to reduce work activities following the policy implementation. Conversely, the group more likely to increase labour supply – i.e., young individuals – is also the one experiencing the largest drop in satisfaction. Unfortunately, given the data at hand, we are not in a position to study this possible link more directly.¹⁰

7. Concluding remarks

We have studied the impact of a policy – nick-named the ‘bedroom tax’ – aimed at reducing the burden of housing subsidies in the UK by rationing social tenants’ use of space. Our study is the first to present an evaluation of this reform using a difference-in-difference methodology and considering a broad range of outcomes.

In a nutshell, we find that the policy reduced housing benefits among treated households as expected. These households experienced further losses in overall income, stemming from changes to income from labour and other benefit sources, and from small changes in labour market participation of some household members. Although the policy was not successful in encouraging residential moves, it did incentivise people who moved to downsize. We also find that the policy did not significantly affect households’ food consumption, savings. Similarly, although the reform worsened households’ levels of material deprivation, this effect was not statistically significant or sizeable. Finally, we find a negative impact on individuals’ overall life satisfaction – mainly stemming from reduced satisfaction with income levels – which is however imprecisely estimated.

Although not very conclusive, our results allow us to comment on a number of issues raised in relation to the ‘bedroom tax’. First and foremost, one concern with this policy was that – by forcing people to move – the initiative would hollow out communities, increase neighbourhood turnover, deprive poor children of a stable learning environment and push individuals already at the risk of being detached from the labour market to areas with even fewer employment opportunities. Our findings that the policy did not significantly affect individuals’ mobility allay this concern. This reluctance to move, due to family and community ties, is also documented in a small scale qualitative study of families in Manchester (Bragg et al 2015). Clearly, however, this evidence also suggests that the policy failed on one of its stated aims – namely, rationalising the use of publicly-funded housing and addressing the problem of over-occupied and under-occupied dwellings co-existing in the system because of problems with (un-)coordinated allocation and mobility costs.

Our evidence also gives some credence to critics of the policy who argued that it would further strain the finances and standards of living of already worse-off individuals – without generating any benefits besides a reduction in the amount of public spending devoted to housing subsidies. While our estimates

¹⁰ We also studied heterogeneity in satisfaction along the three other specific domains (i.e., health, income and leisure time). Once again, we failed to find any significant and systematic pattern.

are too imprecise to be fully confident, their flavour is in line with the qualitative work by Moffatt et al. (2016) who argue that policy had adverse effects on households' poverty, wellbeing and health.

A more agnostic interpretation of our results is that the only sizeable and significant effect was a reduction of individuals' housing benefits roughly corresponding to the policy-mandated reduction for individuals with a spare room. Given the low baseline income of households affected by the change, this represents a non-negligible amount – at approximately 3.5% of the weekly overall income.

What seems more puzzling then is the lack of evidence on any other significant margins of adjustment. Our results should then be taken as the first attempt at providing a causal evaluation of a peculiar policy whose effects have yet to be understood.

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Tables

Table 1. Descriptive statistics for the treated group before the reform

<i>Household</i>	Obs	Mean	Std. Dev.
Average age	176	35.97	13.22
Tenure length	163	11.01	9.781
Bedrooms per person	176	1.590	0.729
Number of bedrooms	176	2.738	0.692
Number of people	176	2.164	1.247
Mobility	176	0.068	0.252
Overall income	176	322.24	164.19
Labour income	167	43.86	89.16
Social benefits	176	271.51	123.71
Housing benefits amount	176	81.21	35.29
Household saves money	176	0.159	0.366
Food expenditure	175	48.44	28.53
Lifestyle changes	78	1.247	1.247
Financial stress	176	0.995	1.968
Durable goods purchase	176	-0.484	0.952
Overall material deprivation	78	1.197	1.198
<i>Individual</i>			
Working	260	0.234	0.424
Overall income	260	214.44	144.34
Labour income	248	26.63	59.91
Social benefits	260	182.99	135.53
Satisfaction with health	208	3.471	1.900
Satisfaction with income	208	3.000	1.682
Satisfaction with amount of leisure	208	4.000	1.682
Overall life satisfaction	208	3.822	1.831

Table 2a. Balancing test – household level controls and outcome variables

<i>Household level</i>	Treatments	Controls	Difference (t-test)	Difference (conditional on fixed effects)
Average age	35.97	26.18	9.78***	12.04***
Tenure length	11.01	7.705	3.306***	3.576***
Bedrooms per person	1.590	0.802	0.788***	0.845***
Number of bedrooms	2.738	2.105	0.633***	0.601***
Number of people	2.164	2.986	-0.821***	-0.935***
Mobility	0.068	0.073	-0.004	-0.027
Overall income	322.24	347.30	-25.05*	-17.50
Labour income	43.86	42.23	1.627	7.314
Social benefits	271.51	296.91	-25.39**	-25.88
Housing benefits amount	81.21	83.67	-2.45	6.318
Household saves money	0.159	0.154	0.004	0.039
Food expenditure	48.44	60.71	-12.26***	-10.04**
Lifestyle changes	1.247	1.404	-0.157	-0.043
Financial stress	0.995	1.110	-0.115	-0.152
Durable goods purchase	-0.483	-0.548	0.065	-0.157
Overall material deprivation	1.196	1.288	-0.091	0.082

Note: For mobility, bedrooms per person, number of bedrooms, number of people, average age, number of members working, overall income, social benefits, housing benefits amount and household savings there are 176 treatments and 588 controls. For tenure length there are 163 treatments and 527 controls. For labour income there are 167 treatments and 563 controls and for food expenditure there are 175 treatments and 585 controls. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 2b. Balancing test – individual level outcomes

<i>Individual level</i>	Treatments	Controls	Difference (t-test)	Difference (cond. on LA*year FE)
Working	0.234	0.231	0.003	-0.021
Total income	214.44	213.12	1.32	-5.586
Labour income	26.63	26.13	0.495	3.352
Social benefits	182.99	182.43	0.559	-11.41
Satisfaction with health	3.471	4.138	-0.666***	-0.964***
Satisfaction with income	3.000	3.354	-0.354***	-0.336*
Satisfaction with amount of leisure	4.000	4.219	-0.219*	-0.354*
Overall life satisfaction	3.822	4.252	-0.430***	-0.675***

Note: For working, overall income and social benefits there are 260 treatments and 946 controls. For labour income there are 248 treatments and 893 controls and for level of happiness there are 209 treatments and 746 controls. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 3. Housing benefits (£/week)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treat	-6.058*	-7.316**	-4.013	-6.462*	-7.396*	-3.740	-7.806**	-9.157**	-7.525
	(3.208)	(3.675)	(4.292)	(3.504)	(3.985)	(4.576)	(3.684)	(4.289)	(5.093)
Post	1.108	-3.398	-1.131	0.871	-4.565	-2.131	2.976	-4.339	-3.322
	(4.100)	(5.736)	(5.836)	(4.113)	(5.815)	(5.877)	(4.643)	(6.222)	(6.285)
Post*Movers				1.388	3.996	4.668			
				(4.881)	(5.353)	(5.351)			
Post*Treat*Movers				3.118	0.625	1.674			
				(8.565)	(9.337)	(9.049)			
Sample	All	All	All	All	All	All	Stayers	Stayers	Stayers
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	Y	Y	N	Y	Y	N	Y	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y
Household FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,552	3,254	3,254	3,552	3,254	3,254	2,880	2,637	2,637
R-squared	0.775	0.779	0.779	0.775	0.779	0.780	0.774	0.776	0.776

Note: standard error clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 4. Housing benefits – Announcement and Enforcement

	(1)	(2)	(3)
Enforcement*Treat	-11.91**	-11.34**	-10.14*
	(4.627)	(5.259)	(5.990)
Announcement *Treat	6.360	4.844	4.896
	(4.181)	(4.571)	(4.582)
Enforcement	3.723	-8.239	-7.430
	(4.871)	(7.466)	(8.035)
Announcement	-4.096	-3.892	-4.007
	(5.064)	(5.579)	(5.601)
Sample	Stayers	Stayers	Stayers
Bedrooms/person* Enforcement	N	N	Y
Age* Enforcement	N	Y	Y
Tenure* Enforcement	N	Y	Y
Bedrooms per person* Announcement	N	N	Y
Age* Announcement	N	Y	Y
Tenure* Announcement	N	Y	Y
Household FE	Y	Y	Y
Wave FE	Y	Y	Y
LA*Year FE	Y	Y	Y
Month FE	Y	Y	Y
Observations	2,880	2,603	2,603
R-squared	0.774	0.781	0.781

Note: standard error clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 5. Mobility

	(1)	(2)	(3)
Post*Treat	0.0001 (0.035)	-0.009 (0.038)	-0.050 (0.049)
Post	0.040 (0.050)	0.047 (0.067)	0.019 (0.069)
Sample	All	All	All
Bedrooms/person*Post	N	N	Y
Age*Post	N	Y	Y
Tenure*Post	N	Y	Y
Household FE	Y	Y	Y
Wave FE	Y	Y	Y
LA*Year FE	Y	Y	Y
Month FE	Y	Y	Y
Observations	3,552	3,254	3,254
R-squared	0.557	0.577	0.579

Note: standard error clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 6. Bedrooms per person

	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treat	-0.102* (0.054)	-0.094* (0.053)	-0.022 (0.046)	-0.021 (0.054)	-0.017 (0.048)	-0.021 (0.056)
Post	0.064* (0.035)	0.048 (0.067)	0.0613* (0.036)	0.032 (0.064)	0.069** (0.031)	0.021 (0.052)
Post*Movers			0.015 (0.040)	0.023 (0.045)		
Post*Treat*Movers			-0.508*** (0.174)	-0.502*** (0.174)		
Sample	All	All	All	All	Stayers	Stayers
Age*Post	N	Y	N	Y	N	Y
Tenure*Post	N	Y	N	Y	N	Y
Household FE	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Observations	3,552	3,254	3,552	3,254	2,880	2,637
R-squared	0.923	0.922	0.927	0.926	0.942	0.941

Note: standard error clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 7. Number of bedrooms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treat	-0.198***	-0.154***	-0.102*	-0.026	-0.010	0.035	0.002	0.007	0.005
	(0.050)	(0.043)	(0.056)	(0.023)	(0.029)	(0.047)	(0.011)	(0.015)	(0.023)
Post	0.007	0.105	0.141	-0.0521	-0.014	0.015	-0.011	-0.003	-0.005
	(0.045)	(0.081)	(0.093)	(0.045)	(0.073)	(0.080)	(0.008)	(0.012)	(0.015)
Post*Movers				0.339***	0.347***	0.355***			
				(0.079)	(0.089)	(0.089)			
Post*Treat*Movers				-0.989***	-0.974***	-0.961***			
				(0.212)	(0.219)	(0.212)			
Sample	All	All	All	All	All	All	Stayers	Stayers	Stayers
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	Y	Y	N	Y	Y	N	Y	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y
Household FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,552	3,254	3,254	3,552	3,254	3,254	2,880	2,637	2,637
R-squared	0.970	0.970	0.971	0.975	0.975	0.975	0.996	0.996	0.996

Note: standard error clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 8. Number of people

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treat	-0.077	0.035	0.073	-0.014	0.063	0.115	0.006	0.070	0.121
	(0.068)	(0.078)	(0.099)	(0.071)	(0.082)	(0.101)	(0.072)	(0.086)	(0.113)
Post	-0.114*	0.105	0.131	-0.163**	0.032	0.066	0.144**	0.036	0.068
	(0.065)	(0.097)	(0.101)	(0.065)	(0.097)	(0.102)	(0.071)	(0.114)	(0.120)
Post*Movers				0.281***	0.235**	0.244***			
				(0.094)	(0.094)	(0.094)			
Post*Treat*Movers				-0.309	-0.183	-0.169			
				(0.217)	(0.224)	(0.221)			
Sample	All	All	All	All	All	All	Stayers	Stayers	Stayers
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	Y	Y	N	Y	Y	N	Y	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y
Household FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,552	3,254	3,254	3,552	3,254	3,254	2,880	2,637	2,637
R-squared	0.967	0.969	0.969	0.968	0.969	0.970	0.972	0.974	0.974

Note: standard error clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 9. Sources of income – overall sample

	Overall Income (£/week)			Labour income (£/week)			Benefits income (£/week)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treat	-26.51**	-24.05	-20.10	-16.33	-10.49	-2.165	-13.26	-17.27	-15.02
	(13.13)	(14.77)	(17.76)	(10.55)	(11.90)	(14.85)	(11.43)	(13.10)	(16.73)
Post	11.43	27.30	30.01	12.73	18.06	23.64*	10.34	24.03	25.58
	(18.33)	(23.45)	(24.52)	(9.366)	(12.41)	(14.18)	(13.78)	(17.97)	(18.68)
Sample	All	All	All	All	All	All	All	All	All
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	Y	Y	N	Y	Y	N	Y	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y
Household FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,552	3,254	3,254	3,380	3,088	3,088	3,552	3,254	3,254
R-squared	0.787	0.791	0.791	0.782	0.794	0.794	0.807	0.807	0.807

Note: standard error clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 10. Saving and expenditure patterns – overall sample

	Household saves			Food expenditure (£/week)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treat	0.010	0.037	0.025	-0.409	1.449	4.389
	(0.041)	(0.047)	(0.052)	(2.891)	(3.453)	(4.350)
Post	-0.096	-0.068	-0.076	-3.636	2.617	4.684
	(0.059)	(0.080)	(0.080)	(3.261)	(5.309)	(5.594)
Sample	All	All	All	All	All	All
Bedrooms/person*Post	N	N	Y	N	N	Y
Age*Post	N	Y	Y	N	Y	Y
Tenure*Post	N	Y	Y	N	Y	Y
Household FE	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Observations	3,552	3,254	3,254	3,524	3,228	3,228
R-squared	0.628	0.635	0.635	0.820	0.827	0.828

Note: standard error clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 11. Material deprivation

	Lifestyle changes			Financial stress			Durable goods purchase			Overall		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post*Treat	0.199	0.107	-0.093	0.227	0.214	-0.048	-0.128	-0.068	-0.119	0.249	0.187	0.140
	(0.228)	(0.259)	(0.307)	(0.241)	(0.269)	(0.338)	(0.082)	(0.090)	(0.108)	(0.178)	(0.208)	(0.258)
Post	0.115	-0.133	-0.276	-0.022	-0.190	-0.370	-0.066	0.021	-0.014	0.0682	-0.0703	-0.104
	(0.334)	(0.423)	(0.444)	(0.322)	(0.435)	(0.448)	(0.109)	(0.146)	(0.151)	(0.308)	(0.387)	(0.406)
Sample	US-All	US-All	US-All	US-All	US-All	US-All	US-All	US-All	US-All	US-All	US-All	US-All
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Household FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs	2,184	2,018	2,018	3,550	3,252	3,252	3,540	3,244	3,244	2,176	2,012	2,012
R-sq	0.784	0.787	0.788	0.663	0.670	0.671	0.837	0.841	0.841	0.858	0.860	0.860

Note: In all columns, the dependent variables have been standardized. See main text for a description of the various indicators used to construct the material deprivation outcomes. More positive values of overall material deprivation, lifestyle changes and financial stress correspond to worse outcomes. More positive values of durable good purchase correspond to better outcomes. Standard error clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 12. Have a job – Overall sample and by individual background

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Post*Treat	-0.034	-0.0008	0.010	0.010	0.038	0.039	0.011	0.049	0.066	-0.029	0.004	0.015	-0.026	0.0002	0.012
	(0.029)	(0.035)	(0.040)	(0.047)	(0.053)	(0.055)	(0.038)	(0.044)	(0.049)	(0.029)	(0.035)	(0.041)	(0.032)	(0.038)	(0.043)
Post	-0.006	0.037	0.044	0.006	0.050	0.051	-0.020	0.029	0.038	-0.023	0.014	0.022	-0.001	0.023	0.032
	(0.033)	(0.049)	(0.052)	(0.040)	(0.054)	(0.056)	(0.035)	(0.056)	(0.057)	(0.033)	(0.050)	(0.052)	(0.033)	(0.051)	(0.053)
Post*Treat*Head				-0.060	-0.054	-0.053									
				(0.052)	(0.055)	(0.055)									
Post*Head				-0.021	-0.022	-0.022									
				(0.030)	(0.032)	(0.032)									
Post*Treat*Female							-0.083*	-0.090*	-0.094*						
							(0.049)	(0.052)	(0.052)						
Post*Female							0.024	0.017	0.020						
							(0.027)	(0.031)	(0.031)						
Post*Treat*21years										0.051	0.037	0.036			
										(0.091)	(0.092)	(0.093)			
Post*21years										0.086**	0.093**	0.093**			
										(0.043)	(0.045)	(0.045)			
Post*Treat* 50/55 years													0.009	0.020	0.021
													(0.085)	(0.091)	(0.091)
Post*50/55 years													-0.070	-0.085	-0.086
													(0.066)	(0.075)	(0.075)
Sample	All	All	All	All	All	All	All	All	All	All	All	All	All	All	All
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070
R-squared	0.755	0.754	0.754	0.756	0.755	0.755	0.756	0.755	0.755	0.757	0.757	0.757	0.756	0.755	0.755

Note: standard error clustered at the individual level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 13. Have a job – Interactions with previous earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treat	0.046	0.018	0.083	-0.006	-0.043	0.0006	-0.066	-0.101	-0.046
	(0.118)	(0.145)	(0.178)	(0.101)	(0.117)	(0.150)	(0.120)	(0.139)	(0.178)
Post	-0.160	-0.327	-0.307	-0.110	-0.282	-0.263	-0.113	-0.263	-0.241
	(0.102)	(0.225)	(0.221)	(0.091)	(0.205)	(0.207)	(0.0960)	(0.207)	(0.207)
Post*Treat* Below median	-0.230	-0.241	-0.230						
	(0.200)	(0.221)	(0.220)						
Post*Below median	0.106	0.133	0.151						
	(0.092)	(0.100)	(0.103)						
Post*Treat* Bottom quartile				-0.207	-0.250	-0.252			
				(0.277)	(0.319)	(0.312)			
Post*Bottom quartile				0.010	-0.007	0.003			
				(0.131)	(0.149)	(0.149)			
Post*Treat* Top quartile							0.001	0.006	-0.015
							(0.171)	(0.180)	(0.182)
Post*Top quartile							0.013	-0.006	-0.007
							(0.093)	(0.099)	(0.100)
Sample	All	All	All	All	All	All	All	All	US-All
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	N	Y	N	N	Y	N	N	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,771	1,672	1,672	1,771	1,672	1,672	1,771	1,672	1,672
R-squared	0.797	0.791	0.792	0.797	0.791	0.791	0.796	0.790	0.790

Note: standard error clustered at the individual level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 14. Overall income – Overall sample and by individual background

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Post*Treat	-16.58	-15.20	-17.04	2.436	4.911	1.631	-8.456	-9.820	-9.667	-16.07	-15.04	-17.05	-16.72	-16.54	-18.29
	(11.30)	(13.04)	(15.94)	(19.82)	(21.08)	(21.87)	(14.45)	(16.56)	(19.99)	(11.72)	(13.40)	(16.35)	(12.30)	(13.75)	(16.45)
Post	14.23	27.99	26.69	13.72	27.09	24.00	0.411	8.228	8.305	11.45	24.77	23.35	15.22	27.65	26.41
	(14.66)	(19.83)	(21.15)	(16.32)	(21.10)	(22.24)	(15.54)	(22.99)	(23.86)	(14.91)	(20.26)	(21.59)	(14.75)	(20.17)	(21.45)
Post*Treat*Head				-26.24	-28.40	-30.72									
				(21.71)	(22.45)	(23.01)									
Post*Head				0.615	0.866	0.177									
				(10.91)	(11.67)	(11.75)									
Post*Treat*Female							-11.05	-12.54	-12.58						
							(19.69)	(20.74)	(21.05)						
Post*Female							23.51**	21.52*	21.54*						
							(10.44)	(12.06)	(12.16)						
Post*Treat*21 years										14.91	15.35	15.59			
										(33.61)	(35.37)	(35.35)			
Post*21 years										13.92	12.74	12.75			
										(9.390)	(9.793)	(9.795)			
Post*Treat* 50/55 years													11.46	10.39	10.24
													(27.74)	(29.24)	(29.27)
Post*50/55 years													-14.54	-7.000	-6.872
													(15.51)	(17.90)	(17.91)
Sample	All	All	All	All	All	All	All	All	All	All	All	All	All	US-All	US-All
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070
R-squared	0.776	0.774	0.774	0.776	0.775	0.775	0.776	0.774	0.774	0.776	0.774	0.774	0.776	0.774	0.774

Note: standard error clustered at the individual level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 15. Labour income – Overall sample and by individual background

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Post*Treat	-5.426	-1.999	-2.294	3.036	5.573	3.887	-0.882	4.273	4.576	-5.439	-2.354	-2.899	-2.597	0.536	0.357
	(4.919)	(5.415)	(6.766)	(10.98)	(11.47)	(11.80)	(6.618)	(7.266)	(8.537)	(4.722)	(5.267)	(6.687)	(5.676)	(6.127)	(7.543)
Post	7.413	12.08	11.87	12.35*	16.03*	14.32	4.976	9.688	9.836	5.289	9.729	9.336	7.825	9.865	9.738
	(5.652)	(7.782)	(8.348)	(7.463)	(9.287)	(9.626)	(5.881)	(9.264)	(9.571)	(5.656)	(7.733)	(8.287)	(5.786)	(7.948)	(8.410)
Post*Treat*Head				-11.11	-10.38	-11.84									
				(11.01)	(11.48)	(11.45)									
Post*Head				-7.529	-6.303	-6.734									
				(5.662)	(6.013)	(6.005)									
Post*Treat*Female							-7.616	-11.08	-11.17						
							(8.663)	(9.084)	(9.123)						
Post*Female							4.199	3.578	3.623						
							(4.956)	(5.863)	(5.925)						
Post*Treat*21years										17.31	19.05	19.12			
										(23.35)	(24.52)	(24.50)			
Post*21years										10.56	9.266	9.280			
										(7.896)	(7.839)	(7.839)			
Post*Treat* 50/55 years													-9.593	-13.29	-13.30
													(12.79)	(14.35)	(14.31)
Post*50/55 years													-6.347	-5.445	-5.427
													(11.02)	(13.14)	(13.14)
Sample	All	All	All	All	All	All	All	All	All	All	All	All	All	All	All
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070
R-squared	0.728	0.723	0.723	0.729	0.723	0.723	0.729	0.723	0.723	0.730	0.724	0.724	0.729	0.723	0.723

Note: standard error clustered at the individual level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 16. Social benefits – Overall sample and by individual background

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Post*Treat	-12.80	-14.99	-14.96	-0.569	-1.926	-2.858	-10.17	-16.57	-15.36	-11.71	-13.97	-13.92	-15.24	-18.38	-18.26
	(10.51)	(12.26)	(14.88)	(16.80)	(18.16)	(18.95)	(13.45)	(15.62)	(18.63)	(11.01)	(12.72)	(15.36)	(11.26)	(12.74)	(15.07)
Post	13.81	23.62	23.64	11.07	21.58	20.70	4.667	10.78	11.39	12.83	22.62	22.66	14.53	24.53	24.61
	(10.76)	(15.32)	(16.19)	(12.20)	(16.41)	(17.16)	(11.63)	(17.87)	(18.45)	(11.03)	(15.79)	(16.69)	(10.76)	(15.82)	(16.70)
Post*Treat*Head				-17.10	-18.55	-19.21									
				(19.04)	(19.85)	(20.49)									
Post*Head				4.179	2.902	2.707									
				(9.289)	(10.03)	(10.12)									
Post*Treat*Female							-2.084	0.794	0.505						
							(18.28)	(19.27)	(19.52)						
Post*Female							15.48*	12.97	13.15						
							(9.085)	(10.34)	(10.42)						
Post*Treat*21years										-10.12	-10.91	-10.91			
										(25.68)	(27.35)	(27.45)			
Post*21years										5.618	4.914	4.914			
										(6.721)	(7.185)	(7.189)			
Post*Treat* 50/55 years													20.20	22.58	22.59
													(13.31)	(15.75)	(15.74)
Post*50/55 years													-10.10	-5.752	-5.760
													(26.38)	(27.74)	(27.79)
Sample	All	All	All	All	All	All	All	All	All	All	All	All	All	All	All
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070	5,454	5,070	5,070
R-squared	0.805	0.804	0.804	0.806	0.804	0.804	0.806	0.805	0.805	0.806	0.804	0.804	0.806	0.804	0.804

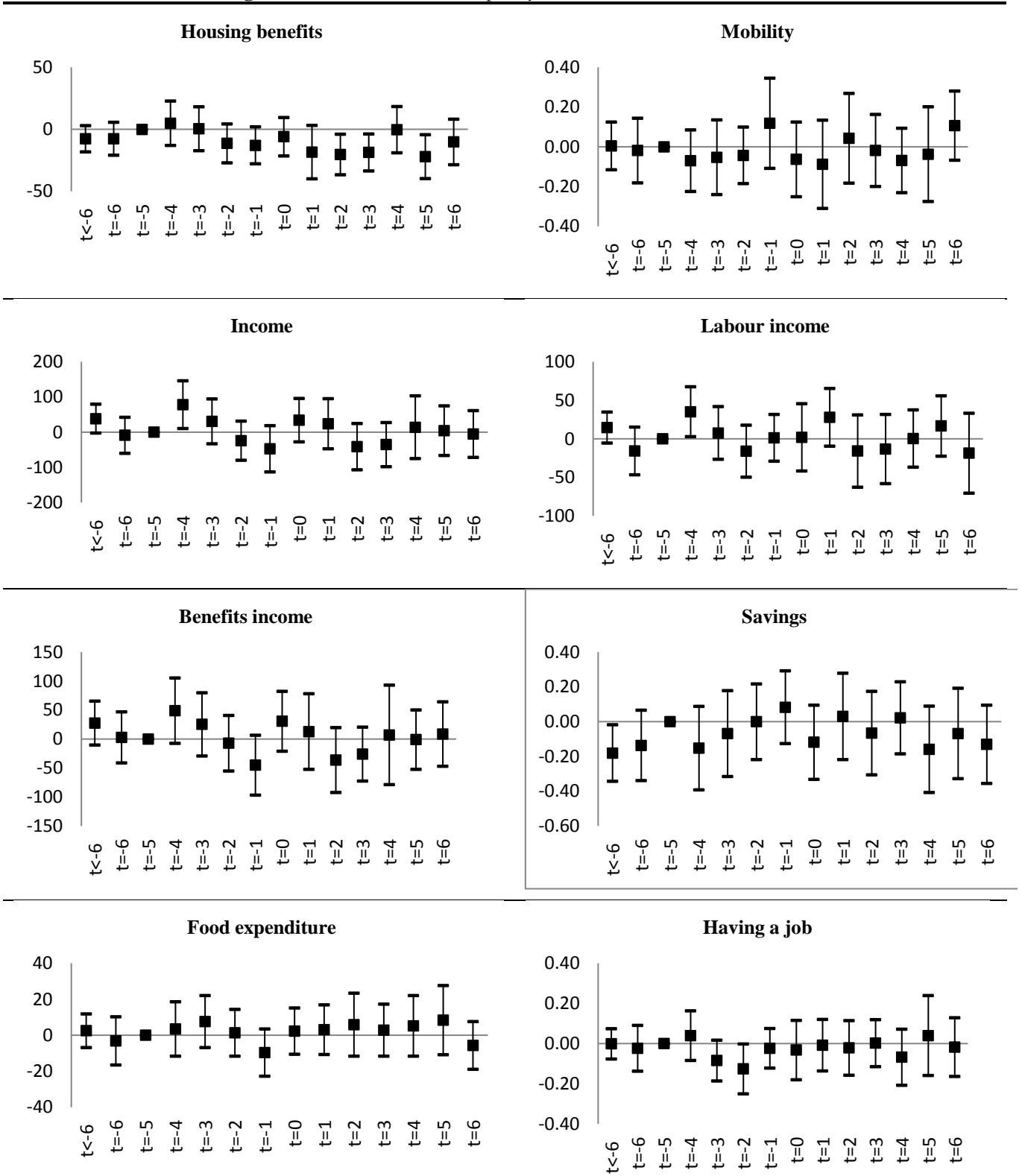
Note: standard error clustered at the individual level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 17. Life satisfaction

	Health			Income			Amount of leisure time			Life overall		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post*Treat	0.056	-0.086	-0.050	-0.324*	-0.290	-0.230	0.049	0.167	0.056	-0.028	0.040	-0.025
	(0.203)	(0.244)	(0.291)	(0.189)	(0.213)	(0.244)	(0.198)	(0.233)	(0.268)	(0.182)	(0.211)	(0.243)
Post	-0.257	-0.317	-0.292	-0.047	-0.010	0.031	0.073	0.272	0.195	-0.087	-0.047	-0.093
	(0.248)	(0.358)	(0.374)	(0.217)	(0.320)	(0.331)	(0.247)	(0.346)	(0.360)	(0.211)	(0.316)	(0.321)
Sample	US-All	US-All	US-All	US-All	US-All	US-All	US-All	US-All	US-All	US-All	US-All	US-All
Bedrooms/person*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Age*Post	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Tenure*Post	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs	4,240	3,903	3,903	4,219	3,885	3,885	4,225	3,889	3,889	4,237	3,901	3,901
R-sq	0.719	0.727	0.727	0.704	0.717	0.717	0.640	0.646	0.647	0.728	0.735	0.735

Note: standard error clustered at the individual level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Figure 1. Event studies of the policy effect on the main outcomes



Note: The graphs present estimates of the effect of the policy by quarters preceding and following the policy enforcement. Enforcement taking place in quarter 0. Announcement taking place in quarter -4. Event study centred on quarter -5 (the omitted group). This is the quarter just before the policy announcement. Benefits, income and mobility outcomes at the household level. Number of observations: 3,552. Job outcome measured at the individual level. Number of observations: 5,454. Standard errors clustered at the household level in all panels but the last where they are clustered at the individual level. 90% confidence intervals displayed in the plots.

Appendix Tables

Appendix Table 1. Treatment and control groups before and after the policy

	Treatments			Controls		
	Number	Observations Before	Observations After	Number	Observations Before	Observations After
Households	176	556	250	588	1,846	900
Movers	29	100	41	115	355	176
Stayers	147	456	209	473	1,491	724
Individuals	367	1,097	508	1,712	5,144	2,586
Head of household	176	556	250	587	1,845	899
Non-head of household	191	541	258	1,125	3,299	1,687
Female	201	603	278	932	2,806	1,398
Male	166	494	230	780	2,338	1,188
16-21 years old	126	355	170	937	2,745	1,418
>21 years old	243	742	338	775	2,399	1,168
Working age	318	943	441	1,655	4,952	2,504
Nearly retired	49	154	67	57	192	82
Below median earnings – individuals	57	160	77	259	780	363
Bottom earnings quartile – individuals	27	76	37	132	412	189
Top earnings quartile – individuals	41	125	65	153	465	202

Appendix Table 2. Number of observations underlying sub-groups used in the analysis

	Male	Female	Head of Household	Non-Head of household	16-21 years old	>21 Years old	Women 16-50 years old and men 16-55 years old	Women >50 years old and men >55 years old
Working	120	140	176	84	19	241	211	49
Overall income	120	140	176	84	19	241	211	49
Labour income	111	137	173	75	18	230	201	47
Social benefits	120	140	176	84	19	241	211	49
Satisfaction with health	89	119	152	56	17	191	167	41
Satisfaction with income	89	119	152	56	17	191	167	41
Satisfaction with amount of leisure	89	119	152	56	17	191	167	41
Overall life satisfaction	89	119	152	56	17	191	167	41

Appendix Table 3. Group averages of main outcomes for the treated individuals before the reform

	Male	Female	Head of Household	Non-Head of household	16-21 years old	>21 Years old	Women 16-50 years old and men 16-55 years old	Women >50 years old and men >55 years old
Working	0.241	0.228	0.170	0.369	0.473	0.215	0.265	0.102
Overall income	184.97	239.70	258.18	122.80	79.07	225.11	211.78	225.89
Labour income	26.89	26.41	19.29	43.54	66.99	23.47	29.24	15.46
Social benefits	154.53	207.38	232.92	78.38	8.315	196.76	176.90	209.22
Satisfaction with health	3.292	3.605	3.282	3.982	5.000	3.335	3.568	3.073
Satisfaction with income	3.011	2.991	2.848	3.410	3.823	2.926	3.113	2.536
Satisfaction with amount of leisure	3.988	4.008	3.907	4.250	4.294	3.973	3.904	4.390
Overall life satisfaction	3.820	3.823	3.598	4.428	5.176	3.701	3.802	3.902