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The bedroom tax

Stephen Gibbons[a], Maria Sanchez-Vidal[b,c,*], Olmo Silva[a]

[a] Department of Geography and Environment & Centre for Economic Performance, London School of Economics and Political Science, Houghton Street, London, WC2A 2AE, UK
[b] Centre for Economic Performance, London School of Economics and Political Science, UK
[c] Institut d'Economia de Barcelona (IEB), Spain

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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 cost 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 using a strategy that compares the observed changes in behaviour of the treated households to those of a control group. The treatment and control groups are defined by the detail of the policy rules. We find that – as expected – the treated group loses housing benefits and overall income. Although the policy was not successful in encouraging residential moves (despite efforts to make mobility within the social sector easier), it did incentivise people who moved to downsize – suggesting some success in terms of one of the policy goals, namely reducing under-occupancy. The policy did not incentivise people to work more and we find no statistically significant effects on households' food consumption or saving behaviour. The implication of our findings is that this type of policy has limited power to change housing consumption or employment in the short run. While it might reduce the costs of housing subsidies to the taxpayer, it does so by imposing a direct financial cost to social tenants unable or unwilling to downsize.

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 – officially named the ‘under-occupancy penalty’ – 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 was euphemistically labelled the ‘removal of the spare room subsidy’ by its advocates, but it was more commonly known as the ‘bedroom tax’. 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 – but 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

* Corresponding author. Centre for Economic Performance, London School of Economics and Political Science, UK.
E-mail address: m.sanchez-vidal@lse.ac.uk (M. Sanchez-Vidal).

[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.

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small sample of controls (Clarke et al., 2015) – this provides no explicit treatment-control comparisons, no pre-policy survey and no adjustments for differential 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 2015. 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 later in 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 level data, with 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 around 200 treated households and 640 control households – observed multiple times before and after the policy intervention. Admittedly, the small sample size means some of our results are imprecisely estimated. However, the likely 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 – as well as to labour income. However, the policy was unsuccessful in encouraging residential moves – though tenants who moved did downsize. We are unable to exactly determine how the tenants who did not move adjusted to these income reductions: 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). Furthermore, point estimates of the effects of the policy on wellbeing and material deprivation are in line with what was predicted by its critics – i.e., that the policy caused affected tenants some hardship. However, the estimates are too imprecisely measured to be conclusive.

The rest of the paper is organized as follows. In the next section, we provide some institutional context and explain the hypotheses we intend to test In Section 3, we discuss the data we use and in Section 4 we present our empirical methods. In Section 5, we present descriptive statistics and regression results. Section 6 provides some concluding remarks.

2. Policy background and expected outcomes

2.1. Institutional context

In 2013 – at the time of the enforcement of the bedroom tax – approximately 5 million individuals received housing benefits. Of these, around 3.4 million were accommodated in units provided by Local Authorities (LAs or councils) and registered social landlords and housing associations (HA, not-for-profit organisations that provide homes for people in housing need), while 1.6 million were housed in privately owned accommodation. Given the number of households in the UK (approximately, 26 million in 2011), this means that roughly 13% of families in the UK occupy social housing – with this figure increasing to 19% if households in private accommodations but on housing benefits are included. This corresponds to approximately 45%–50% of all households renting in the UK.

The percentage of social renters has remained relatively stable since the early 2000s. However, the stock of social housing has shrunk dramatically since the 1980s as councils sold off large proportions of their stock and built no new housing. This decline was driven by a policy to promote homeownership and reduce the role of the state in housing provision – specifically the 'right-to-buy' scheme introduced by Margaret Thatcher in 1980 – as well as changing perceptions of the role of social housing, which came to be seen as a safety net for individuals in need (see Hills, 2007 and Holmans, 2005 for a more detailed historical account). Notwithstanding, the incidence of (relative) poverty climbed during this same period – especially during the 1980s – and has remained stable since then (see Belfield et al., 2014).

As a result of these opposing trends, publicly provided housing today mainly accommodates poor households and demand for social housing greatly outstrips supply – with long waiting lists to gain access to it (1.16 million households were on the waiting list for 1.59 million council houses in England in 2017).[2] In order to ration social housing provision, LAs and HAs have regulations that vary in their exact detail. These specify who is eligible – although evidence of needs is paramount, as well as how long an individual or a household has resided in the area. Furthermore, some LAs grant an element of choice to households on their waiting list – offering them the opportunity to apply for specific dwellings – while others simply operate a direct offer system with no choice component. Different LAs have different regulations regarding the size of the property for which a household will be considered. Some attention is given to the number of household members, their age, their gender and their relationship. However, central government guidelines dictate that priority should be given to the homeless, to those living in unsanitary or overcrowded conditions, and to those who need to live in a given location because of special medical/welfare reasons. Therefore, when allocating social housing, the precise alignment of accommodation type and size to family circumstances is second order to issues of basic household need.

Another important aspect to bear in mind is that social tenants are offered a right to occupy their property indefinitely – i.e., they do not need to move if their household composition and characteristics change. LAs and HAs try to promote tenants' mobility, especially when a household's accommodation is 'too big' for its needs, so that other families in overcrowded conditions can move in. However, historically there has been no compulsion or explicit sanction for under-occupancy.

As a result of this imperfect matching of new tenants to homes and of existing tenants' immobility, many households were occupying homes deemed 'too big' for them at the time the bedroom tax reform was introduced (we will clarify what 'too big' means later in this section). This situation will have arisen either because they were initially allocated to over-size accommodation due to a shortage of properties that better suited them, or because of subsequent changes in their household composition (e.g., a child ageing and leaving home).

Housing benefits in Britain are calculated on the basis of local social housing rents with some deductions if the household income is above a certain threshold. In turn, social housing rents at the time of the bedroom tax policy were calculated from a formula, which aimed to bring LA rents in line with housing association rents and provide consistency in rents across different areas. This formula starts from a baseline figure anchored to mean national housing association rents in 2000 (£53.50 per week, at a time when private sector rents were around £85). Rents are then updated over time and across space using a formula that adjusts this baseline by: *a*-multiplying it by ratio of the average 'manual' (unskilled) earnings of individuals in the county relative to the national average; *b*-considering a 'bedroom factor' that reduces rents for smaller accommodation; *c*-modifying them to reflect average housing prices in the LA relative to national ones in 1999; and *d*-adjusting for inflation using the retail price

[2] Source: Ministry of Housing Communities and Local Government online tables: https://www.gov.uk/government/statistical-data-sets/live-tables-on-rents-lettings-and-tenancies.

index.[3] This approach gives rise to wide variations in terms of rents – and thus housing benefits offered to individuals – across the UK. For example, in 2013–2014 on average, the weekly benefit for households in homes provided by LAs and HAs were approximately £80. However, families living in the most expensive areas of the UK – for example the boroughs of Westminster, Camden, Hackney and Kensington – would receive up to £85 more, while families in the cheapest area of the country – Moray, a Scottish council – would receive around £25 less. Similarly, families in London would receive higher benefits, while households in other large urban conurbations – e.g., Manchester, Liverpool and Newcastle – would receive about the average amount of housing benefits.[4]

Note also that most individuals living in housing provided by councils do not actually pay any rents or receive any benefits – as these are transferred directly from the government to the 'housing account' of the LA. Similar arrangements are often set up for tenants of HAs and other social landlords.[5]

Against this backdrop, the UK Government passed legislation in April 2012 taking effect in April 2013 that would reduce housing benefits for social tenants deemed to have a 'spare' bedroom. The aim of the legislation was twofold. On the one hand, this was an attempt to curb increases in social housing expenditure, which had been steadily rising since the 1990s. On the other hand, the Government hoped to promote mobility and the reallocation of the social housing stock to better match households' size and needs. As discussed above, families occupying more space than needed coexisted in the sector with households living in overcrowding conditions for historical and institutional reasons.

In particular, the 'bedroom tax' legislation dictated that one bedroom would be allowed for the following 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; *e*-any other child (where, for example, there are three children under 10). Anyone deemed as having one spare bedroom would face a 14% cut in the benefits, while households with two under-occupied rooms would face cuts of 25%.

According to official figures, approximately 660,000 – or nearly 20% of social housing tenants on benefits – were liable for the 'bedroom tax' in 2013. This would imply average shortfalls of £11 and £20 for one and two under-occupied rooms respectively – and up to £23-£40 in the most expensive parts of the country. Note that these shortfalls had to be paid in cash directly from the tenants to the LA or HA – unlike rents which (as discussed) were usually directly offset against benefits – making this a salient and significant cut in individuals' finances.

To help social tenants deal with the bedroom tax, councils, housing associations and housing charities (such as Shelter) published a number of advisory guidelines. These included recommending that social tenants take on additional work – as well as providing discussion of the potential implications in terms of deductions from housing benefits for earnings above certain thresholds – and taking on lodgers (legally allowed if agreed with the owner of the property). Furthermore, LAs and HAs created or improved their 'housing swap' portals and websites with the aim of coordinating the mobility of tenants on housing benefits to more suitable accommodations within the social sector – thus avoiding payment of the bedroom tax. There was thus a considerable effort by the involved authorities to publicise the policy, suggest potential responses, and facilitate moving. However, anecdotal evidence and discussions with LA/HA 'welfare reform managers' suggests that portals (and other mobility channels) were not widely used.

More drastic measures were taken by the Scottish Government which between 2013/2014 and 2016/2017 injected £125 million of additional funding for Discretionary Housing Payments to mitigate the effects of the bedroom tax,[6] and by Northern Ireland where the bedroom tax was not put in place till February 2017 – and even then came with a large associated mitigation fund.[7] Some discretionary payments were also made by LAs in England and Wales, although the scope and extent of these payments was much more limited. Furthermore, affected households in England and Wales had to re-apply for these payments on a quarterly basis limiting ease of access and actual take-up. For these reasons, we limit our empirical analysis to England and Wales.

2.2. Expected outcomes of the policy

The two main goals of the policy were to: *i*-cut public expenditure on housing benefits; and *ii*-incentivise tenants in over-sized homes to relocate to smaller accommodation. We thus first expect to see effects on housing benefits received by households. Furthermore, if the policy worked as intended, we would also expect to see that it encouraged affected tenants to move home – and to move to a smaller accommodation.

However, there are many reasons why tenants may have found it difficult or impossible to move or may not have wanted to – some of which we already discussed in Section 2.1. First, even if tenants want to move, doing so within the social housing sector is not fast or straightforward because of lack of available and suitable space – something which was not addressed or facilitated in the aftermath of the policy. Indeed, according to one survey of LAs, the number of small homes available at the time the policy was introduced would have re-housed just under 4% of the affected households.[8] Second, even if housing was available, households may have preferred not to move or had constraints – such as local jobs, schooling, and family ties – that meant that moving was very costly.

These non-moving tenants would have suffered a reduction in benefits income and would have had to adjust their behaviour in ways other than by reducing housing consumption. We therefore expect to see effects along one or more of the following margins: *i*-number of people in employment and labour/overall income; *ii*-savings and/or consumption (as a function of changes in overall/labour/benefit income); *iii*-changes in indicators of standards of living, deprivation and hardship; *iv*-changes in levels of satisfaction (stemming from changes in labour participation, income, consumption and standards of living).

Standard economic theories of labour supply would imply that a cut in benefits should lead to an increase in labour supply – through the income effect on consumption of leisure. However, the reality in our context is not so straightforward because benefits are means-tested and withdrawal of benefits, including housing benefit, can imply marginal tax rates of 100%. Furthermore, housing benefits are calculated at the household level, so household members' labour supply responses are

[3] Note that factors *a*- and *b*-had a weight of 70%, while *c*-had a weight of 30% in this calculation. Our figure for private sector rents is taken from Udagawa and Tang (2008). Details of the rent formula are available in HCA (2015).

[4] These figures and considerations refer to individuals in accommodations owned by LAs and HAs. Individuals eligible for housing support but renting privately owned accommodations received rents in proportion to a Local Housing Allowance (LHA) calculated on the basis of an adjusted private sector rent that varies according to the number of available rooms in the property. As these individuals were not subject to the bedroom tax, we do not discuss this part of the publicly-supported housing market in detail.

[5] Once again, the arrangements are different for individuals receiving housing benefits but occupying privately owned accommodations. These receive benefits in their accounts and are responsible for paying rents to the private landlord.

[6] See Section 7.3 of the Welfare Reform official Scottish documentation available at: http://www.gov.scot/Publications/2017/06/6808/8#s73.

[7] See official documentation available at: https://www.nihe.gov.uk/index/advice/advice_for_housing_executive_tenants/benefits-social-sector-size-criteria-bedroom-tax.htm.

[8] See "'Big lie' behind the bedroom tax: Families trapped with nowhere to move face penalty for having spare room", *Independent*, 5th August 2013: https://www.independent.co.uk/news/uk/politics/big-lie-behind-the-bedroom-tax-families-trapped-with-nowhere-to-move-face-penalty-for-having-spare-8745597.html.

interdependent – possibly implying that the policy incentivised some individuals more than others to react by changing their employment status depending on the number of hours worked and wages at baseline. Lastly, as noted by Chetty et al. (2009), individual responses to complex policy changes are not always fully rational.

With these theoretical channels in mind, our empirical analysis focuses on three outcomes that are fundamental targets of the policy – i.e., housing benefit, residential mobility, accommodation size/occupancy – and a number of incidental outcomes reflecting other margins on which households may have adjusted or experienced changes – namely, overall income, labour income, employment, savings, food expenditure, indicators of deprivation and indicators of wellbeing.

3. Data construction and sample selection criteria

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 each subsequent wave spans overlapping 3-year periods. 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 in the subsequent waves.

The survey covers a number of topics including family structure, educational attainment, 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 2015 – taken from six waves of US data. 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 3 years and 8 months after it. Furthermore, since the reform targeted families already in social housing and receiving housing benefits, we only keep households who are social tenants on housing benefits in the pre-policy period (more precisely, in the last observation before the policy announcement). 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 private rental accommodation as these families were not targeted by the policy. As discussed in Section 2, we also exclude Scotland and Northern Ireland as these put in places measures that likely neutralized the effects or delayed the implementation of the ‘bedroom tax’.[9] Finally, given the policy did not apply to families containing a retired person, we drop households from the sample if they include a male aged above 60 or a woman aged above 55 – i.e., five years before retirement age. 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 consider the age, the gender and the relationship between household members. These criteria are discussed in detail in the next section, when we define ‘treated’ and ‘control’ households – i.e., those with and without a spare room.

Some important aspects of our sample selection criteria are worth mentioning. First, we do not use the latest available years of the US data (2016 and part of 2017), because other benefits-related policies – especially Universal Credit (UC) – came into play and it is increasingly unlikely that we can reliably attribute changes in behaviour to the effects of the bedroom tax policy. However, it is important to note that, while the UC was passed in the same bill as the bedroom tax, at first it only affected new benefit claimants – which are not included in our sample – and was only slowly rolled out to existing claimants. Official figures show that by May 2015 the number of benefit claimants on UC was 65000 – or approximately 2% of the total benefit caseload.[10] Furthermore, UC did not affect tenants with/without spare bedrooms differentially, making it very unlikely that this reform has any bearing on our findings.

Second, we drop from our analysis private renters on benefits – and do *not* consider them as a possible control group – for the following reasons: *i*-while they were not targeted by the bedroom tax, they were affected by changes to the Local Housing Allowance – i.e., the amount that private tenants can claim in housing benefit (which instead did not affect individuals in social housing); and *ii*-as a group they are unlikely to be comparable to social renters in terms of income, housing benefits, rents, mobility and a range of other relevant characteristics (we confirmed this intuition by comparing their pre-policy characteristics).

As set out in Section 2.2, we investigate a wide range of relevant outcome variables at the household level. 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.[11] 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. Employment responses are captured by counting the number of people working in the household, which is constructed by aggregating an individual-level dummy indicator that captures whether or not the respondent is in paid employment at the survey date. Saving behaviour is represented by 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.

The approach of McFall and Garrington (2011) is further used to 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 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;

[9] We considered using Scotland as ‘control’ country to investigate the impact of the ‘bedroom tax’ by comparing individuals just north/south of the border with England. Unfortunately, the US does not sample a sufficient number of households to give enough geographical density to properly implement this research design.

[10] https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/435409/universal-credit-statistics-to-28-may-2015.pdf.

[11] 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.

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. An overall material deprivation index is obtained 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. Lastly, to measure household well-being, we average individual responses to questions on life satisfaction. This is measured along four dimensions: health; income; the amount of leisure time; and 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 and variables are provided in Section 5 – after we discuss our empirical methods and the definition of affected and unaffected households in the next section.

4. Empirical methods

The aim of our analysis is to estimate the causal effect of the bedroom tax policy on a number of household outcomes for people in social housing and on housing benefits. 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, 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.

Our strategy for estimating this impact is to compare the pre-post-policy change (before and after April 2013) in outcomes for treated households who had a spare room according to the policy rules with the pre-post-policy change in outcomes for comparable households who did not. We do this using the following fixed effects, difference-in-difference regression specification:

$$y_{itl} = \alpha_i + \delta Treat_i * Post_t + \sum_k \Lambda_k X_{ki}^{pre} + \theta_{tl} + \varepsilon_{itl} \tag{1}$$

In this specification, y_{itl} denotes outcome for household i at time t and living in LA l; $Treat_i$ identifies whether the household is subject to the bedroom tax; and *Post* is a dummy variable indicating 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). In some versions of the specification, we look at differences between the effects of the policy on households who moved home at some point between the policy announcement and the end of our observation window and those who did not by interacting the treatment and post-policy dummy with the mover indicator.

Our specification further allows households with different characteristics to have different time trends, using interactions between the post indicator and household characteristics (X_{ki}^{pre}) measured prior to the policy announcement (the k indexes the characteristic, specifically, average age, length of tenure in current accommodation and number of bedrooms per person). We also allow for differential geographical time trends and policy shocks using LA-by-year fixed effects (θ_{tl}). The unobservable α_i is a household-level fixed effect, potentially correlated with treatment, while ε_{itl} is an error term assumed to be uncorrelated with the other variables in our empirical model. Finally, we include survey wave dummies and quarter-by-year dummies in the regressions – hence there is no un-interacted post dummy in equation (1) – but for simplicity suppress these in the notation. Parameter δ is the parameter of interest.

This equation is estimated on the sample of social housing tenants, receiving housing benefits, extracted from the US data from 2009 to 2015 and described in Section 3. Estimation is by Ordinary Least Squares (OLS) even when the dependent variable is a binary outcome.[12] 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. Standard errors are clustered at the household level, to allow for heteroscedasticity and serial correlation within households over time.

We categorise households in our data as treated or control by inferring whether they would have been affected by the policy according to its rules. This is done by comparing the information on household composition and accommodation size, with the detailed and specific policy criteria used by the government to establish whether families had a spare room. Household and accommodation characteristics are fixed to those recorded in the last household observation prior to April 2012 when the policy was announced. As discussed, 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; *e*-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. In order to determine households with a 'spare room', we only consider the main categories as we are not able to identify individuals subject to 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.[13]

Based on these guidelines, we consider a household as treated if it occupies more rooms 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 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.

The identifying assumption in this difference-in-difference/fixed-effects panel design is that treatment – having a spare room, as defined by these arbitrary and nuanced policy rules – is effectively random, conditional on the fixed effects and the control variables included in the

[12] We also estimated by conditional logit. Unfortunately, this estimator did not converge when using the full set of LA-by-year fixed effects. If we drop these fixed effects and estimate conditional logits (absorbing family unobservables α_i), the results are similar in their implications to the OLS estimates.

[13] Our approach defines 'treatment' using household composition before the policy announcement and holding it fixed over time. However, households might switch status because of changes to their composition that can be considered exogenous – e.g., the ageing of a child that makes him/her entitled to a room. In some extensions, we explored whether our results differed if we allowed the treatment status to change on the basis of characteristics that households cannot manipulate (i.e., mainly the gender and age of the children), but they remained the same.

regression. This assumption seems justifiable given that having a spare room or not under these rules was largely the outcome of historical accidents related to the kind of accommodation the family was originally given and subsequent changes in household age and composition (see discussion in Section 2). We discuss the empirical validity of this assumption in the next section where we look at the 'balancing' of baseline (pre-policy announcement) characteristics in the treatment and control groups.

Notwithstanding these considerations, our treatment is essentially an indicator of a very specific set of interactions between household size, number of bedrooms and the age of household members. It is thus important to control for general trends related to these factors, which might drive the pre-post policy change in outcomes. The interaction terms between pre-determined household characteristics (age, length of tenure and bedrooms per person) and the post dummy included in our empirical model (1) play precisely this role: they capture the impact of time trends in outcomes that could be related to family structure and accommodation size. Furthermore, LA-by-year dummies 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 changes – that might be related to both treatment and outcomes. We also tried controlling for linear time trends interacted with a treatment-group status indicator. This approach did not change the results presented below in any meaningful way.[14]

5. Results

5.1. Descriptive statistics

We present descriptive evidence in Table 1. This reports statistics (unweighted) at the household 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 and control groups. Alongside the means and standard deviations, we report t-tests for the difference in means between the two groups.

There are overall 203 treated households and 641 controls, meaning that around 24% of households in our base sample of social tenants on housing benefits are in the treated group.[15] As we expect given the definition of treatment, treated households differ significantly from the controls in terms of size of household (2.2 for treated, 3.0 for controls), dwelling size (2.7 bedrooms for treated, 2.1 for controls) and bedrooms per person (1.6 for treated, 0.8 for controls). The two groups also differ significantly in terms of average age (35.6 years for treatment, 25.9 years for controls). This likely reflects the fact that the definition of treatment depends on the age of children and that control households will have younger children. Treated households also have significantly longer tenure (over 3 years longer) in their current accommodation.

Looking at the pre-policy magnitudes of the outcome variables we investigate, we see no statistically significant difference in mobility rates between treatment (6.4%) and controls (8.8%). A concern for our analysis would be if the US data we use cannot track households who move – meaning we fail to detect moves in the data. However, comparing the overall one-year mobility rates in our data with the corresponding mobility rates found in the Survey of English Housing – at 7.4% for social tenants in 2014/15 – suggests this is not an issue. Housing benefit income is also similar in the two groups (at around £80), as is labour income (around £43) though overall income is significantly lower in the treatment group (£314, lower by £34) due to lower social benefits from other sources. Once again, this reflects the fact that the control group has more household members, while income is similar in the two groups if we look at the individual level data (results not tabulated). Housing benefit makes up 27–30% of benefits income. In both groups, labour income is a small share of the total, in line with the fact that the average number of working age people working in the household is under 0.4 in both groups. Variables measuring other financial aspects are similar across the two groups. When dissimilar, this largely reflects the fact that the treated households are smaller than the controls and have fewer younger children. Only around 15% of both groups save money; average weekly food expenditure is £48 in the treated group and £61 in the control group.[16] Finally, pre-policy indicators of financial distress and material hardship are similar across groups (these variables are standardised indices).

As discussed in Section 4, our most stringent specification controls for interaction between average age, length of tenure in current accommodation and number of bedrooms per person, and a dummy identifying the post-enforcement periods. This allows for differential pre/post-policy trends along these dimensions. Any time-fixed components of these household characteristics – as well as other time-fixed unobservable characteristics – is instead absorbed by the household fixed effects we use throughout our analysis.

Note that in our estimation sample that there are 639 pre-policy observations and 345 post-policy observations for the 203 treated households. There are also 1988 post-bedroom tax observations and 1176 pre-policy observations for the 641 control households. However, the precise numbers of data points vary according to the specification being estimated and the outcome analysed.

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

We start by studying the first key policy target – housing benefit – investigating whether the bedroom tax reduced the amount of housing benefit treated households receive relative to controls. Table 2, Columns (1) to (3) show the coefficients and standard errors for the corresponding regression estimates of equation (1) for all households in our sample. Columns (4)–(6) separate out the impact of the policy for families who stay or move home by interacting the treatment variable and the post-policy dummy indicator with a dummy identifying households that relocate between surveys. Lastly, Columns (7)–(9) restrict the sample to households that do not move. Columns (1), (4) and (7) control only for household, LA-by-year, year-by-quarter and wave fixed effects. Columns (2), (5) and (8) control additionally for interactions between the post indicator, and pre-policy average age and tenure length. Columns (3), (6) and (9) add an interaction between the number of bedrooms per person (measured pre-policy announcement) and the post dummy.

The first three columns show that the policy did reduce housing benefit by approximately £7-£10 per week per household on a baseline of around £80. This is not too far from the expected 14% reduction mandated by the policy for anyone with one spare room. Columns (4) to (9) reveal that all of the effect is concentrated on the households who do not move home for whom the effect is the same size as that found on average across the mover and stayer groups. The implied effect for movers is negligible. Note that our preferred specification is the one used in Columns (2), (5) and (8). This controls for household and LA-by-year unobservable effects – as well as for the possibility that the imbalances in

[14] Braackman and McDonald (2018) investigate the impact of changes in the Local Housing Allowance (LHA) on property prices. As already discussed, while this reform occurred at the same time as the bedroom tax we analyse, it only affected private renters on housing benefits – which are excluded from our analysis, Furthermore, 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. Lastly, 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.

[15] Further statistics not reported show that approximately 57% of household that rent the property they inhabit are social tenants. These figures line up with the numbers discussed in Section 2 for the whole of the UK.

[16] We find similar imbalances if we focus on food expenditure per person. This suggests that differences in household size between treated and control units do fully not explain the differences visible in Table 1.

Table 1
Descriptive statistics and balancing tests before the reform.

Variables	Treatments			Controls			Difference	Difference (conditional on fixed effects)
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.		
Number of people	203	2.152	1.202	641	3.048	1.668	−0.895***	−0.967***
Number of bedrooms	203	2.748	0.675	641	2.134	0.844	0.614***	0.578***
Bedrooms per person	203	1.589	0.723	641	0.797	0.293	0.791***	0.824***
Average age	203	35.60	13.11	641	25.89	12.52	9.709***	11.72***
Tenure length	181	11.01	9.712	569	7.741	6.962	3.274***	3.555***
Housing benefits amount	203	78.84	34.28	641	82.07	36.21	−3.229	5.602
Mobility	203	0.064	0.245	641	0.088	0.284	−0.024	−0.027
Overall income	203	314.2	159.7	641	348.5	159.0	−34.27***	−13.57
Labour income	192	42.00	85.92	608	43.82	87.53	−1.820	9.970
Social benefits	203	265.6	122.6	641	296.7	135.9	−31.12***	−24.87
Number of people working	203	0.374	0.595	641	0.396	0.634	−0.021	−0.009
Household saves money	203	0.152	0.360	641	0.152	0.360	−0.000	0.042
Food expenditure	202	48.15	28.24	638	61.36	40.63	−13.20***	−9.255**
Lifestyle changes	91	1.298	1.185	325	1.431	1.052	−0.132	−0.089
Financial stress	203	0.971	1.961	641	1.100	2.193	−0.128	−0.178
Durable goods purchase	203	−0.480	0.921	639	−0.529	0.974	0.049	−0.168
Overall material deprivation	91	1.199	1.142	324	1.285	1.059	−0.085	0.030
Satisfaction with health	183	3.46	1.769	582	4.085	1.696	−0.622***	−0.798***
Satisfaction with income	183	2.966	1.576	581	3.246	1.560	−0.280**	−0.233
Satisfaction with amount of leisure	182	3.943	1.596	581	4.165	1.477	−0.222*	−0.156
Overall life satisfaction	183	3.793	1.705	582	4.146	1.647	−0.353**	−0.527**

Note: Number of household (HH) observations and variables measured prior to the policy announcement in April 2012 and using information gathered from the interview prior to this date and closest in time. Overall number of household observations (without missing) as follow. Treated households: 639 observations before treatment date and 345 observations after treatment date. Control households: 1988 before treatment date and 1176 observations after treatment date. Of these, 131 observations represent movers in the treated group before the policy and 67 after the policy. For the controls, these figures are of 463 observations of movers before the policy and of 285 and after the policy. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 2
Housing benefits (£/week).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treat	−6.967**	−9.900***	−8.146**	−6.391*	−8.453**	−6.297	−7.211*	−9.824**	−10.49**
	(3.253)	(3.630)	(4.139)	(3.559)	(4.012)	(4.578)	(3.813)	(4.338)	(5.186)
Post*Treat*Movers				−1.149	−7.620	−7.361			
				(8.168)	(8.827)	(8.688)			
Post*Movers				4.981	9.023*	9.309*			
				(4.550)	(5.159)	(5.184)			
Sample	All	All	All	All	All	All	Stayers	Stayers	Stayers
Beds./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
Quarter*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	4148	3764	3764	4148	3764	3764	3202	2903	2903
R-squared	0.764	0.767	0.767	0.765	0.768	0.768	0.786	0.788	0.788

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. 'Treat' is an indicator that household is affected by the 'bedroom tax' policy in that they would be deemed to have had a spare room at the time of the policy announcement in April 2012. 'Post' is an indicator for the pre-post April 2013 period when policy enacted. 'Beds./person' is number of bedrooms per person. 'Movers' is an indicator that the household moved residential address at some point over the post policy enforcement period. 'Age' refers to average age of household members. 'Tenure' is length of tenure in current accommodation pre-policy. 'Household FE' refers to household fixed effects. 'Wave FE' refers to dummies for US survey waves. 'LA x Year FE' are interactions between LA and Year identifiers. 'Quarter*Year FE' are interactions of quarter and year dummies. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

average age and tenure length between treated and control households documented in Table 1 affect their outcomes post-treatment. We consider the specification reported in Column (3), (6) and (9) a robustness check that controls 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 comes from the more nuanced aspects of the treatment group definition – rather than simply from the pre-policy number of bedrooms per person. Nevertheless, all the results provide similar conclusions so only we report both specifications for comparison in some of the remaining tables in this section, and then focus on our preferred model.

So far, we have focussed on the enforcement date – i.e., we have defined as 'post' all time periods from April 2013. In Table 3, we study whether housing benefits 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 analogous to Columns (7)–(9) of Table 2. 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 be around £13 – while the announcement effect is positive but insignificant. 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

Table 3
Housing benefits – Announcement and Enforcement.

	(1)	(2)	(3)
Enforcement*Treat	−10.41**	−12.85**	−13.58**
	(4.687)	(5.329)	(6.026)
Announcement*Treat	4.879	4.763	5.238
	(3.905)	(4.610)	(6.006)
Sample	Stayers	Stayers	Stayers
Beds./person*Enforcement	N	N	Y
Age*Enforcement	N	Y	Y
Tenure*Enforcement	N	Y	Y
Bedrooms/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
Quarter*Year FE	Y	Y	Y
Observations	3202	2903	2903
R-squared	0.786	0.788	0.788

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. 'Enforcement' is an indicator of pre-post policy enforcement (April 2013). Announcement is an indicator of pre-post policy announcement (April 2012). For other notes, see Table 2. *: significant at 10% level. **: significant at 5% level. ***: significant at 1% level.

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.

A more refined event-study analysis of the effects on housing benefit is presented in Fig. 1, which is more revealing about the timing of the impacts. The figure shows point estimates and associated 90% confidence intervals of the effect of the bedroom tax spanning ten quarters before and after its enforcement (i.e., $t = 0$ is the post date of April 2013). The omitted period against which all outcomes are benchmarked is centred on five quarters before the policy came into force – i.e., one quarter prior to the policy announcement in April 2012 when we might start to see effects (this is also the mid-point in our pre-policy period). The aim of the figure is twofold. First, it allows us to investigate whether there are significant pre-trends in housing benefit receipt. Second, it allows us to investigate when the reduction in housing benefit starts, how long it takes to fully materialise and how long it lasts. We find that the point estimates are negative in every quarter post-enforcement, relative to the pre-announcement baseline, though only significantly so for four out of eleven quarters. There is little evidence of effects in the period between announcement and enforcement. The precision of the estimates decreases as we move away from the 'post' date. This is expected as multiple other factors start to come into play obscuring the impact of the policy. Overall, this evidence confirms the insights gathered from Table 3 where we compared the impact of enforcement and announcement on changes in housing benefits – and found that only the former mattered.

We also studied event-study graphs in the style of Fig. 1 for some of other outcomes discussed below. This analysis confirmed that there were no differential pre-trends, but otherwise revealed no more interesting patterns than the simple pre/post policy difference in difference that we capture with estimates of the empirical model in equation (1). For brevity, we do not report them in the paper.[17]

Table 4 looks at the second of the key intended policy targets: residential mobility. The table clearly shows that the policy was not successful in incentivising moves. If anything, the point estimates suggest that the policy discouraged moves – relative to a baseline mobility 6.4% in the treatment group – though the coefficients are not significant.[18]

Table 5 investigates the third intended policy outcome, namely use of space in social housing. The reported regressions estimate whether the policy affected the number of bedrooms per person (Columns 1-3), the number of bedrooms in the household (Columns 4–6) or the number of people in the household (Columns 7–9). These are all margins along which we might expect to see some adjustment – if the policy worked as expected and increased efficient occupancy of space. In this table, we separate out the effects for movers and stayers (Columns 2, 5, 7) as we did in Table 2. Furthermore, we focus on our favourite specification that does not control for 'post' periods interacted with the pre-policy number of bedrooms per person. The first column of Table 5 shows that the policy led to a small reduction in the number of bedrooms per person in the full sample (−0.1 rooms per person). As might be expected, Column (2) and (3) show that all of this effect is concentrated among movers, who experience a 0.4 reduction in the number of rooms per person following relocation (Column 2, row 2), whereas we find no change for stayers (first row of Column 3).

From the rest of the table it is evident that the change in bedrooms per person comes primarily from movers choosing smaller accommodation. We find a small overall effect on number of bedrooms (−0.16 rooms, Column 4) that is completely explained by changes in accommodation size for movers. Treated movers experience almost a one-room reduction (−0.8) relative to control households when they relocate – in line with what is expected if individuals comply with the policy and avoid the under-occupancy penalty. In contrast, movers unaffected by the policy tended to move to bigger housing (an additional 0.3 bedrooms). Taken together, the results of Tables 4 and 5 imply that although the policy did not encourage people to move, it did encourage people to downsize relative to the control group when they did so. Evidently, the policy has the potential to shift the equilibrium use of social housing space in the long run – as tenants move out of homes for other reasons – but these changes depend on the natural turnover rate and will occur at a slower rate than might have been intended (i.e., had the policy had an impact on mobility).

The final columns of Table 5 look at whether household size changes in response to the policy. We find this is not the case: while household size did increase on average post-policy for movers (by 0.15 people), the treatment and control groups are no different in this respect. We further investigated whether there were changes in the number of very young babies, young people (age 16–21), working age adults, near retirees, and adults who are not family members (potential '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.

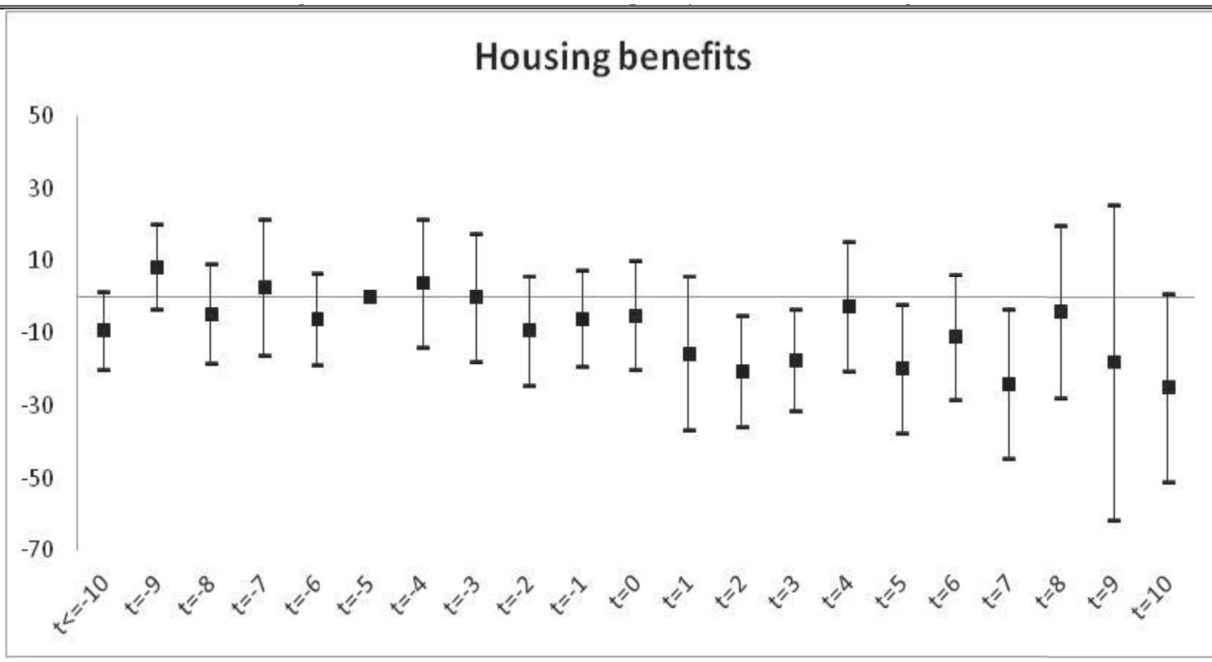
5.3. *Effects on other outcomes: income, employment, savings, food expenditure, deprivation and satisfaction*

In this section, we look the impact of the bedroom tax on the other outcomes related to changes in household behaviour and experience that we discussed in Section 2.2. Our findings are presented in Tables 6 and 7. We report results for the full sample that pools stayers and movers, though the results for the stayers' sample are similar.

The first column of Table 6 suggests that treated households experienced a £27 per week fall in total income relative to the control households, significant at the 10% level. This reduction is much more substantial than the £10 reduction in housing benefit reported in Table 2. The reason for this drop becomes evident in Columns (2) and (3), which look at labour income and total benefit income. Total labour income in the household falls by nearly £20, while total benefits fall by nearly £8 – in line with the fall in housing benefit documented above although the effect here is non-significant (note that total benefits include housing as well as unemployment, disability, child and other benefits). The fall in labour income is surprising and the precise reasons are not clear from our

[17] Results are however available in the working paper version of this paper (Gibbons et al., 2018).

[18] In results not tabulated we also investigated whether the effects on housing benefit and on mobility differed between types of social landlord, LAs ad HAs. but found no significant differences.



Note: The graphs present estimates of the effect of the policy on housing benefits 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. Number of observations: 3,764. Standard errors clustered at the household level. 90% confidence intervals displayed in the plots.

Fig. 1. Event studies of the policy effect on housing benefit.

Table 4
Mobility.

	(1)	(2)	(3)
Post*Treat	−0.036 (0.037)	−0.037 (0.040)	−0.054 (0.049)
Sample	All	All	All
Beds./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
Quarter*Year FE	Y	Y	Y
Observations	4148	3764	3764
R-squared	0.545	0.558	0.559

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. For other notes, see Table 2. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

analysis. When we look at the employment outcome (number of people working) in Column (4), the point estimate implies a sizeable reduction – 26% less employment in affected households – although this effect is non-significant (note that this effect becomes significant at the 5% and stays at a similar magnitude if we take out the interactions between the post indicator and pre-policy average age and tenure length from the regression).

In additional regressions (not reported), we found no significant effect on hours worked or part-time versus full time either – though the general pattern from these additional results confirmed that affected households worked less. For example, we found a 12%–17% reduction in total hours worked in the household (though this effect was not significant at conventional level).

As noted in Section 2.2, basic economics would predict an increase in labour supply due to the reduction in non-labour income. However, the incentives in our context are complex due to the benefit withdrawal rate as employment income increases above certain thresholds, and because of the interdependency in the employment decisions between household members. In a more detailed analysis, we used individual level data to see whether there were any specific effects on employment and labour income related to particular types of household members – namely, younger people, older people, males or females – but found no important nor statistically significant patterns (reported in the working paper version of this paper; Gibbons et al., 2018). Unfortunately, we are unable to shed more light on why individuals are observed to work less in response to the bedroom tax. As discussed in Chetty et al. (2009), individuals make systematic optimisation errors even when policies are relatively simple – so irrational behavioural changes in response to the bedroom tax are not implausible. Given the data at hand, this possibility can only remain a conjecture.

Lastly, we find that, despite the overall reduction in household income, households do not seem to be less likely to save, nor spending less on food (Columns 5 and 6). In fact, the point estimates suggest increases on both outcomes for treated households relative to controls, although neither coefficient is statistically significant.

To conclude, we look at potential consequences of the reductions in housing benefit and income documented so far in terms of households' wellbeing. Table 7 reports results from regressions with indicators of material deprivation and self-reported satisfaction as dependent variables. The basic message from this table is that life experience worsened for treated households in ways that might have been expected – with higher indices of material deprivation in terms of financial stress, fewer durable goods purchases and higher overall material deprivation as well as reductions in satisfaction with health, income and life overall. On the other hand, none of these estimates is significant and treated households report a relative improvement in their satisfaction with leisure time (perhaps commensurate with the reduction in employment and hours worked discussed in relation to Table 6).

Table 5
Bedrooms per person, number of bedrooms and number of people.

	Bedrooms per person			Number of bedrooms			Number of people		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treat	−0.108**	−0.029	−0.023	−0.158***	−0.009	0.011	0.052	0.061	0.065
	(0.051)	(0.053)	(0.057)	(0.040)	(0.029)	(0.014)	(0.076)	(0.083)	(0.088)
Post*Treat*Movers		−0.443***			−0.825***			−0.041	
		(0.138)			(0.167)			(0.184)	
Post*Movers		0.036			0.317***			0.149*	
		(0.041)			(0.074)			(0.090)	
Sample	All	All	Stayers	All	All	Stayers	All	All	Stayers
Age*Post	Y	Y	Y	Y	Y	Y	Y	Y	Y
Tenure*Post	Y	Y	Y	Y	Y	Y	Y	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
Quarter*Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3764	3764	2903	3764	3764	2903	3764	3764	2903
R-squared	0.919	0.922	0.937	0.969	0.972	0.996	0.968	0.968	0.973

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. For other notes, see Table 2. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 6
Income, employment, savings and expenditure.

	Overall Income (£/week)	Labour income (£/week)	Benefits income (£/week)	Number of people working	Household saves	Food expenditure (£/week)
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treat	−27.22*	−19.63*	−7.883	−0.093	0.031	1.422
	(14.92)	(11.19)	(12.18)	(0.062)	(0.044)	(3.095)
Sample	All	All	All	All	All	All
Age*Post	Y	Y	Y	Y	Y	Y
Tenure*Post	Y	Y	Y	Y	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
Quarter*Year FE	Y	Y	Y	Y	Y	Y
Observations	Y	Y	Y	Y	Y	Y
R-squared	3764	3563	3764	3764	3764	3734

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. For other notes, see Table 2. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

Table 7
Material deprivation and satisfaction.

	Material deprivation				Satisfaction			
	Lifestyle changes	Financial stress	Durable goods purchase	Overall	Health	Income	Leisure time	Life overall
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Treat	−0.015	0.143	−0.062	0.116	−0.257	−0.233	0.122	−0.103
	(0.214)	(0.251)	(0.083)	(0.171)	(0.245)	(0.219)	(0.221)	(0.203)
Sample	All	All	All	All	All	All	All	All
Age*Post	Y	Y	Y	Y	Y	Y	Y	Y
Tenure*Post	Y	Y	Y	Y	Y	Y	Y	Y
Household FE	Y	Y	Y	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Quarter*Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	2419	3761	3744	2407	3349	3343	3343	3346
R-squared	0.789	0.667	0.838	0.852	0.724	0.728	0.672	0.744

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. In all columns, the dependent variables have been standardised. 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. For other notes, see Table 2. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level.

To sum up, the evidence in this section suggests that households affected by the bedroom tax experience a loss in income relative to unaffected households. This reduction is more than the one implied by the housing benefit cut mandated for families with a spare room. However, the margins on which households adjust to this loss of income are obscure – with no changes in consumption and saving patterns. The policy might have worsened households' standard of living and life satisfaction – although our estimates are imprecise.

5.4. Geographical heterogeneity

The results so far have described the picture on average for the whole of England and Wales. An important question is to what extent the effects differ across cities, within cities and across other types of geography.

We investigated this issue by studying whether our results differ along the along following dimensions: urban versus rural; London versus rest of England; high rent versus low rent areas; and distance between place of residence and city centre. Broadly speaking, we find no evidence of striking and significant heterogeneity, but a few patterns are worth mentioning. A subset of results, along with explanations of how we define these geographical categories, are presented in Appendix Table A1.

The most important differences relate to urban, high rent areas – especially London – versus other areas in England and Wales. The effect on benefits is generally larger in urban areas and high market rent areas, and particularly so in London. Conversely, the policy effect on total household income – driven by labour income – moves in the opposite direction with treated households in places outside of London being more negatively affected.

Results on *within-city* patterns imply that the adverse effects of the bedroom tax on benefits and income were worse for tenants living further out from the centre of cities, but the differences are not significant and as a result our findings are not conclusive.

In terms of the other outcomes, we find no clear patterns for mobility, material deprivation and life satisfaction. Given the imprecision in these results, it is hardly worth speculating over what drives them. The most important lesson from this geographical analysis is that there is nothing to suggest that the baseline results presented in Tables 2–7 are unrepresentative of the experience of the majority of the social tenant population in England and Wales.

6. Concluding remarks

We have studied the impact of an under-occupancy penalty policy – nick-named the bedroom tax – aimed at reducing the burden to the tax payer 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 range of outcomes on which the policy was expected to have some impact.

In a nutshell, we find that the policy reduced housing benefits for social tenant households with a spare bedroom, relative to those without a spare bedroom. The reduction, although small in absolute terms, is non-negligible given the low baseline income of the affected households (around 3.5% of their total income). These affected households experienced further losses in overall income, stemming from reductions in labour income and a tendency to work less relative to the control group.

The policy did not encourage residential moves, but it did incentivise people to downsize when they moved in the course of natural residential turnover. The implication is that the policy was only partly successful in 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. In the long run, under-occupancy of social housing might be reduced, but this change will only occur in conjunction with the natural turnover of tenants in social housing. In the short run, the affected groups simply suffered a loss of disposable income. The non-response in terms of mobility was unlikely due to a lack of information about the policy or a lack of salience (as in Chetty et al., 2009). The policy was widely publicised, and affected tenants would have either experienced direct cuts in their housing benefits or written demands for the shortfall in their rents – making it unlikely that the bedroom tax went unnoticed.

Our evidence 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 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.

Another concern with the policy was that it would hollow out communities, increase neighbourhood turnover, deprive poor children of a stable learning environment (with possible detrimental effects on their education, see Gibbons et al., 2017) by forcing people to move, 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 relocate due to family and community ties is also documented in a small-scale qualitative study of families in Manchester (Bragg et al., 2015).

An obvious question is whether this policy really saved tax payers in the UK any money. It was expected that the policy would affect 660,000 households at the time it was introduced. Based on the £10 per week benefits cut in our estimates, the direct savings would be around £350 million per year – seventy percent of the government's own estimates of total savings. These savings will also have been partly offset by the discretionary payments that the government boosted in order to help support families adversely affected by the bedroom tax – around £60 million per year up to 2015/16. The bottom line is that the policy seems to have saved some money – though not as much as expected – with the burden falling on the affected tenants.[19]

The general lesson from these findings is that this type of policy has limited power to change housing consumption or affect households' employment decisions in the short run. It might reduce the costs to the taxpayer of housing subsidies, but at a direct financial cost to social tenants who are unable or unwilling to downsize.

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[19] See official data and projections at the following links: https://www.gov.uk/government/news/housing-benefit-reform-removal-of-the-spare-room-subsidy-fact-sheet, https://www.whatdotheyknow.com/request/274781/response/671097/attach/3/FOI%202457%20fin%202016%206%2026.doc?cookie_passthrough=1, https://www.gov.uk/government/statistics/use-of-discretionary-housing-payments-financial-year-201516.

Appendix Material

Table A1
Geographical Heterogeneity.

	Benefits	Mobility	Income	N. of people working	Material deprivation	Life satisfaction
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Urban vs. rural						
Post*Treat	−2.443	0.0191	−38.68	−0.032	0.518	−0.439
	(9.694)	(0.093)	(43.01)	(0.216)	(0.452)	(0.517)
Post*Treat*Urban	−8.417	−0.058	15.21	−0.050	−0.457	0.389
	(10.31)	(0.104)	(46.19)	(0.226)	(0.479)	(0.564)
Observations	3764	3764	3764	3764	2407	3346
Panel B: London vs. the rest						
Post*Treat	−6.586*	−0.046	−30.61*	−0.092	0.114	−0.009
	(3.665)	(0.047)	(17.53)	(0.070)	(0.189)	(0.230)
Post*Treat*London	−13.75	0.028	8.211	−0.005	0.004	−0.423
	(9.728)	(0.093)	(31.22)	(0.128)	(0.414)	(0.456)
Observations	3764	3764	3764	3764	2407	3346
Panel C: High vs. low cost rental markets						
Post*Treat	−16.41**	−0.072	−24.71	−0.079	0.031	−0.290
	(6.675)	(0.061)	(21.38)	(0.086)	(0.266)	(0.315)
Post*Treat*LRR below median	11.70	0.054	−15.20	−0.027	0.255	0.236
	(7.597)	(0.083)	(26.31)	(0.113)	(0.322)	(0.403)
Observations	3558	3558	3558	3558	2280	3161
Panel D: Distance to city centre						
Post*Treat	36.35	0.079	57.37	−0.407	1.163	−0.006
	(29.83)	(0.402)	(152.1)	(0.511)	(1.396)	(1.676)
Post*Treat*Distance to city centre	−5.528	−0.013	−10.00	0.037	−0.122	−0.009
	(3.660)	(0.046)	(17.86)	(0.060)	(0.162)	(0.199)
Observations	3762	3762	3762	3762	2406	3344
Sample	All	All	All	All	All	All
Age*Post	Y	Y	Y	Y	Y	Y
Tenure*Post	Y	Y	Y	Y	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
Quarter*Year FE	Y	Y	Y	Y	Y	Y

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. Results in each cell come from different regressions. ‘Urban’ is an urban/rural indicator based on the ONS definition that classifies settlements of 10,000 inhabitants or more as urban and rural otherwise. ‘London’ is an indicator for London as opposed to the rest of England and Wales. ‘LRR below median’ is an indicator for below-above the median local reference rent in 2012 in the broad rental market area (BRMA) where the household is located. LRRs are used to calculate local housing allowances (LHA) that anchor the subsidies that can be claimed by households on benefits renting private accommodation. ‘Distance to the city centre’ measures distance to the closest city centre (population>10,000) in log meters. For other notes, see Table 2. *: significant at 10% level. **: significant at 5% level. ***: significant at 1% level.

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