

CEP Discussion Paper No 1537

April 2018

The Bedroom Tax

**Stephen Gibbons
Maria Sánchez-Vidal
Olmo Silva**

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 from 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 – suggesting some success in terms of one of the policy goals, namely reducing 'under-occupancy' in the long run. 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.

Key words: social housing, social rents, bedroom tax, housing benefits
JEL: H55; H2; R21; R28

This paper was produced as part of the Centre's Urban and Spatial Programme. The Centre for Economic Performance is financed by the Economic and Social Research Council.

We would like to thank participants at the CEP Labour Workshop and at the 2017 Workshop on "Public Policies, Cities and Regions" (Lyon) for their comments and suggestions. We are responsible for any errors and omissions. Funding for this research was provided under ESRC grant ES/M010341/1.

Stephen Gibbons, London School of Economics and Centre for Economic Performance, London School of Economics. Maria Sánchez-Vidal, Centre for Economic Performance, London School of Economics. Olmo Silva, London School of Economics and Centre for Economic Performance, London School of Economics.

Published by
Centre for Economic Performance
London School of Economics and Political Science
Houghton Street
London WC2A 2AE

All rights reserved. No part of this publication may be reproduced, stored in a retrieval system or transmitted in any form or by any means without the prior permission in writing of the publisher nor be issued to the public or circulated in any form other than that in which it is published.

Requests for permission to reproduce any article or part of the Working Paper should be sent to the editor at the above address.

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 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 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 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

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

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 or individual level data, with individual or 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 198 treated households and 620 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 some derived measure of material deprivation and 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. Finally, we study whether our results are heterogeneous along several geographical dimensions. Broadly speaking, we find that the ‘bedroom tax’ had a stronger impact on households’ benefits, income levels and deprivation in more urban areas, in London and in parts of England and Wales with higher housing costs. However, once again, none of these patterns is statistically significant.

The rest of the paper is organized as follows. In the next section, we provide some institutional context. 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, while in Section 6 and 7 we present our household and individual level findings, respectively. Finally, Section 8 provides some concluding remarks.

2. 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 landlord and housing associations (i.e., not-for-profit organisations that provide homes for people in housing need), while 1.6 million were housed in privately owned accommodations. 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. Furthermore, over the past two decades, LAs and housing associations have continued to accommodate the largest share of needy individuals on housing benefits – with a split of approximately 40% and 60% respectively. However, relative to the 1970s and 1980s, the stock of social housing has significantly shrunk with no new supply brought to the market as well as councils and housing associations selling part of their estates. This decline was partly driven by policy decisions aimed at promoting homeownership – such as the “right-to-buy” scheme introduced by Margaret Thatcher in 1980 – as well as a change in the perceived aims of publicly provided housing which came to be seen as a tool to provide accommodations to individuals in extreme needs (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 in order to obtain publicly provided housing (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 housing associations have regulations that vary in their exact details. These specify who is eligible – although evidence of ‘needs’ is paramount – as well as how long an individual or a household need to have resided in the area in order to be considered. Furthermore, some LAs grant an element of choice to households on their waiting list – offering them the opportunity to bid for specific accommodations – while others simply operate a direct offer system with no choice components. Finally, 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 to family circumstances is second order to issues of basic household need and the availability of different types of local accommodation.

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 housing associations try to promote tenants’ mobility – especially when these occupy accommodations that are ‘too large’ so that other families in overcrowded conditions can be moved in. However, historically there has been no compulsion or explicit sanction for under-occupancy.

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>

Taken together, these aspects suggest that when the ‘bedroom tax’ reform was introduced, a number of households were occupying accommodations deemed ‘too big’ for them (we will clarify what this means later in this section). This situation will have arisen either because they were initially allocated to an 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 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; d- adjusting for inflation using the retail price 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 accommodations provided by LAs and housing associations 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 individuals occupying housing provided by social landlords and housing associations – though the arrangements in this part of the sector can vary.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

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 housing associations. 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 any more details.

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

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 was hoping to promote mobility and the reallocation of the social housing stock to better match households' size and needs given that – as discussed above – for historical and institutional reasons some families occupying more space than needed coexisted in the sector with households living in over-crowding conditions.

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 600,000 – 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 housing association – 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 social tenants to take on some additional work – as well as providing some discussions 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, councils and LAs 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'.

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 tax⁶, 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

Given these considerations, in the analysis that follows, we focus on England and Wales – while excluding Scotland and Northern Ireland – and study whether the 'bedroom tax' had an impact on a

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

broad range of households' and individuals' outcomes, such as mobility, household size and composition as well as labour force participation and earnings.

3. 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 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 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 place measures that likely neutralized the effects or delayed the implementation of the 'bedroom tax'.⁸ Given the policy did not apply to families containing a retired person, we only keep households in the sample if they include a male aged below 60 or a woman aged below 55 – i.e., five years before retirement age. As already mentioned, the policy affected families with a 'spare room'. The

⁸ 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.

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. 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.

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.⁹ 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. 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 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/repared; 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; 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.

⁹ 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.

We also look at a number of individual-level outcomes. These are useful in assessing whether the policy had differential impacts on different household members depending on their characteristics. A dummy indicator captures whether or not the respondent is in paid employment at the survey date. We also look at 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. Life satisfaction is measured on four dimensions: 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 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 and individual level 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/individuals who had a spare room according to the policy rules with the pre-post-policy change in outcomes for comparable households/individuals who did not. We do this using the following fixed effects, difference-in-difference regression specification, which we describe first for the household case:

$$y_{itl} = \alpha_i + \delta Treat_i \times I(t \geq PolicyOn) + \sum X_i^{pre} \times I(t \geq PolicyOn) \Lambda' + \theta_{tl} + \varepsilon_{itl} \quad (1)$$

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 $t \geq PolicyOn$ 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). 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 post-policy dummy with the mover indicator.

Our specification further controls for differential time trends using interactions between the policy-on indicator and household characteristics (X_i^{pre}) measured prior to the policy announcement (specifically, average age, length of tenure in current accommodation and number of bedrooms per person), and with LA-by-year of survey 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. Parameter δ is the parameter of interest.¹⁰

This equation is estimated on the sample of social housing tenants, receiving housing benefits, extracted from the US data from 2009-2014 and described in Section 3. Estimation is by Ordinary Least Squares (OLS) even when the dependent variable is a binary outcome. 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.¹¹ Standard errors are clustered at the household level, to allow for heteroscedasticity and serial correlation within households.

We categorise families in our data as treated or non-treated 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 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 – 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 rooms than it is entitled to. Conversely, we label a household as a control if it resides in an accommodation with the

¹⁰ Note that we also include wave and month-of-interview dummies; however, we do not index these in our equation to simplify notation.

¹¹ 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.

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.¹²

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 regression. This assumption seems justifiable given that having a spare room or not under these rules was largely an outcome of ‘historical accident’ 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 ‘balancing’ of characteristics in the treatment and control groups. Notwithstanding these considerations, 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 therefore important to control for other more general trends related to these factors which might drive the pre-post policy change in outcomes. The interactions terms between pre-determined household characteristics (age, length of tenure and bedrooms per person) and the policy-on dummy included in our empirical model (1) play precisely this role, and 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.¹³

Some outcomes – employment, income, wellbeing – can be studied at the individual rather than household level. An individual-level analysis also 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. When we study individual level outcomes, we replace household fixed effects in equation (1) with individual fixed effects and cluster our standard level at this level.¹⁴ 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 – and the specification is identical in all other ways. We continue to control for pre-determined household level characteristics interacted with the

¹² This 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.

¹³ 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.

¹⁴ We also tried clustering at the household level and it did not alter our main conclusions.

policy-on variable and for the LA-by-year shocks. To investigate heterogeneity, 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.¹⁵

5. 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 and control groups.

As shown in the top panel, there are overall 198 treated households and 620 controls, meaning that around 32% of households in our base sample of social tenants on housing benefits are in the treated group.¹⁶ Treated households include approximately 2.1 members and occupy accommodations with approximately 2.7 rooms – giving rise to an average bedroom/person ratio of around 1.6. Note that this variable is not particularly skewed – with a median value of 1.5 and the bottom/top quartiles at 1 and 2, respectively. However, the top 10% of the households report to have three rooms per person, while the household with the smallest occupancy ratio has four rooms per member of the family (coming from one person in an accommodation with four rooms). Treated household’s average rate of mobility is 6% while the overall weekly income is approximately £317. Most of this comes from social benefits (£269) with only a small contribution from labour income (£41). Note that housing subsidies account for nearly 30% of the social benefits. Only around 16% of the treated households report that they manage to save money, and the average weekly food expenditure is £48.2. 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 in England and Wales).

The second panel presents data at the individual level. We have 293 treated working age adults with non-missing basic labour market outcomes (i.e., work or not) in our sample and nearly 1,000 control individuals – though the exact number of observations varies depending on the variable considered.

¹⁵ 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.

¹⁶ Further statistics not reported show that approximately 57% of household that rent the property they inhabit are ‘social tenants’ and that – of these – approximately 18% are subject to the ‘bedroom tax’. These figures line up with the numbers discussed in Section 2 for the whole of the UK.

Working age adults are defined as males aged 16 to 60, and women aged 16 to 55. Approximately 22% 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 1 also presents some descriptive statistics for households and individuals in the control group and compares them with the households and individuals in the treatment group. As expected, 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.¹⁷ Treated households also have a significantly higher average age (approx. 9.43 years or 35% older than the control group) and longer tenure (3 years or around 38% 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 9% – and that almost 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. 20%) but significant difference in weekly food expenditure.¹⁸ Similarly, we find no differences in the levels of deprivation for all the items we consider.

At the individual level we find less evidence of discrepancies in income levels, though once again the observed differences 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 0.19 and 0.91 points on the 1-7 scale, depending on the satisfaction measure – with many of these differences being significant.

Appendix Tables 1 to 3 provide more descriptive information about our sample. There are approximately 620 observations before and 280 observations after the policy was implemented for the 198 treated households. The corresponding figures for the 620 families in the control groups are approximately 1,950 and 940. Furthermore, 31 and 119 households in the treated and control groups

¹⁷ Note that largest number of rooms per person in the control group is one, the minimum is zero (studio flats) and the top/bottom quartiles are 0.6 and 1, respectively.

¹⁸ We find similar imbalances (similarly significant) if we focus on food expenditure per person. This suggests that different household sizes between treated and control units do fully not explain the differences visible in Table 1.

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 412 in the treatment group and 1,810 in the control group, giving 1,806 survey observations in the treated group (1,233/573 before/after treatment) and 8,071 observations for the controls (5,358/2,713 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 men, 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. Additional 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 slightly 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.

6. Household level analysis

6.1 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 2. Columns (1) to (3) show results for all households in our sample. Columns (4) to (6) 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 6% (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 1 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, 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 3, 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 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 14.5% – i.e., almost 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 4 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.

In some extensions (not tabulated), we considered the latest wave of data – with information up to 2016 – to investigate whether the ‘bedroom tax’ might have had an impact on mobility over a longer horizon – but failed to find any such evidence. We therefore decided to maintain our main sampling framework for most of the analysis as this ensures that our results capture the causal impact of the policy reform by looking in close time proximity to its announcement and enforcement. We also investigated whether the effects on housing benefit and on mobility differed between types of social landlord. As discussed in Section 2, there are two general categories of landlord providing social housing in Britain – Local Authorities and private housing associations or registered social landlords – each with different types of housing stock and different organisational arrangements. We found no significant differences between tenants in these two groups (the results are not tabulated but available from the authors).

Tables 5 to 7 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 pre-policy 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 5 show that the policy had an overall small, negative effect on the number of bedrooms per person. As might be expected, Columns (3) and (4) show that 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 6 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 to 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. Taken together with Table 4, these results imply that although the policy did not encourage people to move, it did encourage people to downsize when they did move. 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).

Lastly, Table 7 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.

6.2 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 8. 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 2.

Columns (1) to (3) show that overall weekly income falls after the policy by between £16 and £21. The largest point estimate (Column 1) is significant, while the one from our favourite specification (Column 2) is borderline significant (with a t-stat of approximately 1.4). The documented coefficients are twice as large as the drop we found in housing benefits (documented in Tables 2 and 3). 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 £13 using our preferred specification in Column (5). Conversely, Columns (7) to (9) suggest that the drop in housing benefit was the main driver of the reductions in income derived from benefits. We return to these issues below where we analyse individual level data to identify more precisely what behavioural responses might determine these labour income shortfalls.

Next, we study whether these income reductions affect savings, consumption behaviour and material deprivation. Our first set of results is reported in Table 9. Columns (1) to (3) show that the policy is associated with a positive impact on the households’ propensity to save – of approximately 30% (using Column 2). 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 £0.5 per week – or 1%. 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 10 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 13.7% of a standard deviation 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. The policy was however successful in some its goals: housing benefits were reduced for movers and stayers, and under-occupying tenants who moved – and would have moved without the policy – shifted to smaller accommodation.

6.3 ‘Event studies’ policy estimates

In this section, we present 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 two, three 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 3 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 8, 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 9.¹⁹

6.4 Geographical heterogeneity

To conclude this section, we study whether our results are heterogeneous along several geographical dimensions. In particular, we investigate the following margins. First, we consider households living in urban areas as opposed to rural ones (168 and 30 treated households, respectively).²⁰ Next, we study households living in London vs. the rest of the country (29 vs. 160 treated units, respectively). Finally, we consider households living in areas where housing costs are above/below the median of the sample. We proxy for these using local reference rents (LRR) for broad rental market areas (BMRAs) in England (not available in Wales) measured in March 2012.²¹ LRR are used in conjunctions with a person's BMRA of residence to calculate local housing allowances (LHA) that anchor the subsidies that can be claimed by households on benefits renting private accommodations. As discussed in Section 2, these households are not subject to the 'bedroom tax' – which only affects benefit claimants who rent accommodations from LAs and social landlords whose benefits/rents are calculated using a different approach (also detailed in Section 2). Unfortunately, detailed and consistent information on the levels of social housing rents in different areas of the country is not available for the years around the announcement and enforcement of the 'bedroom tax'. However, the two measures are likely to be very highly correlated as they are both benchmarked against the cost of private housing.

¹⁹ The last plot in Figure 1 refers to the probability of having a job. We discuss to this graph in the next section.

²⁰ In order to classify areas as urban/rural, we use the standard Office for National Statistics (ONS) definition. This identifies those settlements with a population of 10,000 or more as urban areas and rural areas otherwise. We use this to classify whether a household lives in an urban/rural area using the exact coordinate of a family's address.

²¹ There are 154 BRMA in England. For each BRMA, reference rents are posted for accommodations with different room numbers (one to five, and then six or above). We consider rents prevailing in the BMRA averaged across all accommodation types.

Broadly speaking, we find no evidence of striking and significant heterogeneity. However, some patterns are worth discussing.²² Starting with the urban/rural divide, we find that the negative effect on housing benefits is mainly concentrated in urban areas – at around £8 – while the effect in rural areas is as low as £2. Note however the two estimates are not statistically different, and the rural sample size is small. Furthermore, we find no evidence of differential impact on mobility but some heterogeneity in the effect of the policy on overall income: while this drops by up to £50 in rural areas, it only decreases by £15 in urban areas. This difference seems to be mainly driven by a more significant drop in labour income in rural areas (approx. £31) than in urban ones (less than £10) – still implying an overall negative (and somewhat puzzling) negative labour supply response to the policy. Once again, while interesting, these differences are not statistically significant. Lastly, we find that overall levels of deprivation increase more substantially in rural areas (0.26 of a standard deviation) than in urban ones (0.12 of a standard deviation), but this heterogeneity is not significant.

When we explore the London vs. the rest of the country, we reproduce the patterns found for the urban/rural divide: benefits drop by more substantial amounts in London (£15 vs. £5) but mobility is still unaffected across the board. We also find that overall income drops more substantially out of London than in the capital, and that this differential response is driven by heterogeneity in the effect of the ‘bedroom tax’ on labour income (down by £20 and £5 out of London and in the capital, respectively). Finally, we still find an overall negative (insignificant) impact on deprivation though this is smaller in London. Once again, while these patterns are interesting, none of the differences is significant and the London sample size is small.²³

Next, we study heterogeneity in areas with above/below median housing costs as captured by the LLRs. The median LLR in our sample is £135 while the average is £170 – substantially higher than the average housing benefit (at £80) in the sample. As discussed, LLR are used to calculate LHA that anchor the benefits of recipients renting from private landlords. Rents are higher in the private sector than in social housing – so this difference is expected. However, relative to national figures, our descriptive statistics are skewed towards higher LLRs for two reasons: *a-* we are considering the average LLR in the BRMA across all accommodation types – irrespective of their number of rooms and without weighting more common (smaller) properties more than other less frequent (bigger) property types; and *b-* given the US sampling and our data restrictions, we are using a set of households skewed towards urban, more expensive area. In relation to the latter, the median/average LLR in urban areas is £140/£175 – further climbing to £240/£260 in London – while in rural areas the corresponding figures are both £130.

²² All the findings summarised above come from our favourite specification that includes all layers of fixed effects, LA-by-year dummies and interactions between household characteristics and the post-policy period – except for the interaction with the number of bedrooms per person.

²³ We also study whether there are heterogeneous effects along the city size dimension. We proxy for the latter by considering the 2011 population of the built-up urban area in which a household resides. Built-up areas are defined as land which is ‘irreversibly urban in character’, meaning that they are characteristic of a town or city. While we fail to find any significant heterogeneity, the overall patterns are similar to those documented along the urban/rural and London-vs.-the rest dimensions.

Our results show that the negative effect of the ‘bedroom tax’ on housing benefits is concentrated in areas with LLRs above median – at £13 – whereas BRMAs with below median housing costs experience a reduction in housing benefits of only £3. However, this heterogeneity is not statistically significant. We still find no heterogeneity in the mobility response and some evidence that overall and labour income fall more in areas with below median LLRs. For example, we find that labour earnings are £2.5 lower in BMAs with above median housing costs and £16 in areas with below-median LLRs. Correspondingly, overall deprivation is higher in areas with lower LLRs. Once again, none of these differences is statistically significant. However, these patterns mirror those we found for the urban/rural divide and London/rest-of-the-country margin.

We also studied whether there is some *within-city* variation. To do so, we characterised every household’s residential location by the distance to the closest city centre. The latter is measured as (log of) metres away from the population centroid of the nearest 10,000 inhabitants’ build-up area and is meant to capture the most accessible employment cluster. Our results show that the ‘bedroom tax’ had a positive impact on housing benefits and overall/labour income in areas closest to the city centre with a fairly steep spatial decline. For example, the impact on housing benefits is positive at £35 at log-distance zero (i.e., one meter from the centre) – but declines rapidly by approximately £5 for one standard deviation increase in the log-distance. At the average distance level, the impact on housing benefits is negative at £7.75 – very close to the headline figures in Table 2. It should once again be noticed that neither the main effect of the ‘bedroom tax’ nor its interaction with log-distance are significant – so our results are not conclusive. On the other hand, we find no consistent patterns of heterogeneity on mobility and some evidence that material deprivation increases closer to city centres than in more peripheral areas – though once again these patterns are not significant.

7. Individual level analysis

7.1 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 11. For simplicity, we only present the coefficients that come from our preferred specification (i.e., the one including LA-by-year effects and post-policy interaction with household characteristics except bedroom per person). The first column pools all individuals, whereas the subsequent columns single out the impact of the policy on the employment of the following subgroups: heads of household, female members of the households, individuals aged 16 to 21 and nearly retirees, i.e. males age 55-60 and women aged 50-55.

Starting from the first column, we find no evidence that the policy changed individuals' propensity to work.²⁴ 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. Similarly, we find no discernible effects after the policy announcement a year earlier.

Some patterns emerge when we study the variation in the policy effect among different household members. The point estimate in Column (2) shows that heads of household have a lower probability of working following the policy enforcement. Although this estimate is not significant at conventional level, the magnitude is quite sizeable – implying a 15%-20% reduction in the likelihood of having a job. Column (3) further documents a negative and significant effect on the probability that female members of the households have a job – corresponding to approximately a 40% decrease in the likelihood of working. Finally, we find a reduction in the probability of holding a job for young individuals aged 16-to-21 and a positive but small 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 approximately 0.25. This gives a variance of 0.187 (i.e., $(0.25) \times (1-0.25)$) and a standard deviation of 0.43. Second, consider we are working with changes in employment given our difference-in-difference methodology and that we have approximately a ratio of treated-to-control of 0.29. 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 0.29 (i.e., a variance of 0.083) 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 0.147. By further dividing this expression by the variance of $Work^{Pre}$ (at 0.187), 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

²⁴ 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.

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 imply a reduction to almost zero in the labour market participation of 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.

7.2 Effects on income from different sources

Table 13 studies the effect of the policy on individuals' weekly income by focussing on overall income – Columns (1) to (5) – labour income – Columns (6) to (10) – and income derived from social benefits – Columns (11) to (15). The table has the same structure as Table 11 and investigates 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 Columns (1) to (5), we find a £12 reduction in average weekly income, increasing to £20 when focussing on the head-of-household. Although this point estimate is not significant, its size is consistent with the evidence provided in Table 8: overall household income decreases by around £16-£21. 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 up to £12; not significant). This is in part compensated by a positive effect on the income of young household members of £11 (not significant). Furthermore, the income of near retirees also increases by a similar extent (£9).

We investigate these issues in more detail in Columns (6) to (10) and (11) to (15) where we focus on labour and benefits income, respectively. As in Columns (1) to (5), none of our estimates is significant – but the patterns are instructive and relatively consistent with the findings discussed above. Starting from Columns (6) to (10), we find both heads of household and women experience relatively sizeable labour income drops (of up to £8 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 up to £14. We also find that near retirees earn less from labour income – although previous findings suggested that the policy modestly increase their work probability.

Columns (11) to (15) further show that head-of-households experience a loss in income from benefits of approximately £12. This is consistent with the drop reported in Table 8 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 increase in the amount of benefits received by women and a fairly 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.

7.3 Effects on satisfaction

We present our results in Table 14. This focuses on four different outcomes. The first column studies the impact of the policy on satisfaction with an individual's own health. Column (2) focuses on satisfaction with income, while Column (3) investigates possible effects on satisfaction with the amount of leisure time. Finally, the last column focuses on overall life satisfaction. Across all columns smaller values correspond to lower levels of satisfaction.

Column (1) shows that the policy may have lowered satisfaction with health, although this estimate is not statistically significant and the implied magnitude is small. Column (2) similarly shows that the

policy negatively affected satisfaction with income. However, in this case the coefficient shows a relatively sizeable effect – although still not statistically significant – implying up to a 10% decrease from the mean income satisfaction. Column (3) conversely shows that satisfaction with leisure time increased following the enforcement of the bedroom tax reform – but once again the point estimate is far from being precisely estimated. Lastly, Column (4) shows that overall life satisfaction was not clearly 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.²⁵

8. Concluding remarks

We have studied the impact of an under-occupancy penalty 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

²⁵ 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.

people to move – the initiative 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) 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 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 because of problems with (un-)coordinated allocation and mobility costs. Although the policy didn’t encourage moves, it did encourage movers to downsize, so in the long run under-occupancy of social housing might be reduced. This change will however only occur in conjunction with natural turnover occupants of social housing

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 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 cruder interpretation would suggest that the policy was a success. According to official estimates it saved around £500 million in housing-benefit spending per year in the first three years following its implementation.²⁶ Given our findings, this happened without significantly (from a statistical point of view) affecting people’s behaviour and livelihood.

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 puzzling is the lack of evidence on any other significant margins of adjustment in response to this income shortfall. 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.

²⁶ Figures on the projected savings were published by the Government following a ‘Freedom of Information’ request. These show estimated savings of £490 million, £525 million and £560 million in 2013/14, 2014/15 and 2015/16, respectively. See document posted at the following address:

https://www.whatdotheyknow.com/request/how_much_has_the_government_save

References

- Belfield, C., J. Cribb, A. Hood and R. Joyce (2014): “Living Standards, Poverty and Inequality in the UK: 2014”, Institute for Fiscal Studies Report N. 96, London.
- Braackman, N and S. McDonald (2016): “Housing Subsidies and Property Prices: Evidence from England”, mimeo, Newcastle University.
- Bragg, Joanna, Erica Burman, Anat Greenstein, Terry Hanley, Afroditi Kalambouka, Ruth Lupton, Lauren McCoy, Kate Sapin and Laura Anne Winter (2015): “The Impacts of the ‘Bedroom Tax’ on Children and Their Education: A Study in the City of Manchester”, University of Manchester
- Brewer, M., C. Emmerson, A. Hood, and R. Joyce (2014): “Econometric Analysis of the impacts of Local Housing Allowance reforms on existing claimants”, Department for Work and Pensions Research Report no. 871.
- Clarke, A., L. Hill, B. Marshall, M. Oxley, I. Pereira, E. Thomson, and P. Williams, (2015): “Evaluation of Removal of the Spare Room Subsidy: Final Report”, Department for Work and Pensions.
- Fack, G. (2006): “Are housing benefit an effective way to redistribute income? Evidence from a natural experiment in France”, *Labour Economics*, vol. 13(6), pp. 747-771
- Gibbons, S. and A. Manning (2006): “The incidence of UK Housing Benefit: evidence from the 1990s reforms”, *Journal of Public Economics*, vol. 90 (4-5), pp. 799-822.
- Gibbons, S., O. Silva and F. Weinhardt (2017): “Neighbourhood Turnover and Teenage Attainment”, *Journal of the European Economic Association*, 15(4), pp. 746–783
- HCA (2015) *Rent Standard Guidance*, Homes and Communities Agency, London
<https://www.gov.uk/government/publications/rent-standard-guidance> (accessed March 2018)
- Hills, J. (2007): “Ends and Means: The Future Roles of Social Housing in England”, CASE-LSE Report N. 34.
- Holmans, A. (2005): “Housing and Housing Policy in England 1975–2002”, Report to the Office of the Deputy Prime Minister, London.
- Kangasharju, A. (2010): “Housing Allowance and the Rent of Low-income Households”, *Scandinavian Journal of Economics*, vol. 112(3), pp. 595-617.
- McFall, S. L. and Garrington, C. (Eds.) (2011): “*Early findings from the first wave of the UK’s household longitudinal study*”, Institute for Social and Economic Research, University of Essex, Colchester (UK).
- Moffatt, S, S. Lawson, R. Patterson, E. Holding, A. Dennison, S. Sowden, and J. Brown (2016): “A qualitative study of the impact of the UK ‘bedroom tax’”, *Journal of Public Health*, vo.. 38(2), 197-205.
- Shelter (2013): “What’s wrong with the bedroom tax?” Policy briefing, Shelter, London
https://england.shelter.org.uk/professional_resources/policy_and_research/policy_library/policy_library_folder/briefing_whats_wrong_with_the_bedroom_tax (accessed November 2017)
- Susin, S. (2002): “Rent vouchers and the price of low-income housing”, *Journal of Public Economics*, vol. 83(1), pp. 109-152.
- Udagawa, C. and C. P.Y. Tang (2008) *Private rents and rental rates of return, 1996/97 to 2006/07*, Cambridge Centre for Housing and Planning Research, Cambridge
<https://www.cchpr.landecon.cam.ac.uk/Projects/Start-Year/2007/Comparative-analysis-of-private-and-social-sectors-rates-of-return/Rents-rates-of-return-1998-99-to-2006-07/PRS-Report> (accessed March 2018)
- Viren, M. (2013): “Is the housing allowance shifted to rental prices?”, *Empirical Economics*, vol. 44(3), pp. 1497-1518.

Tables

Table 1. Descriptive statistics and balancing tests before the reform

	Treatments			Controls			Difference	Difference (conditional on fixed effects)
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.		
<i>Household</i>								
Average age	198	35.49	13.20	620	26.16	12.58	9.331***	11.69***
Tenure length	177	10.75	9.568	552	7.786	6.920	2.972***	3.330***
Bedrooms per person	198	1.590	0.720	620	0.805	0.295	0.784***	0.838***
Number of bedrooms	198	2.752	0.679	620	2.119	0.843	0.633***	0.587***
Number of people	198	2.151	1.203	620	3.003	1.669	-0.851***	-0.977***
Mobility	198	0.060	0.239	620	0.072	0.260	-0.011	-0.034
Overall income	198	316.57	160.64	620	347.02	158.79	-30.44**	-15.48
Labour income	187	40.84	85.15	588	43.74	87.18	-2.90	6.366
Social benefits	198	268.76	123.04	620	295.44	136.75	-26.68**	-23.13
Housing benefits amount	198	79.36	34.45	620	82.27	36.59	-2.909	6.491
Household saves money	198	0.161	0.369	620	0.151	0.357	0.010	0.052
Food expenditure	196	48.21	28.09	615	60.83	40.70	-12.62***	-9.834**
Lifestyle changes	88	1.282	1.193	311	1.430	1.055	-0.147	-0.096
Financial stress	197	0.969	1.969	616	1.109	2.197	-0.139	-0.154
Durable goods purchase	197	-0.468	0.919	616	-0.549	0.975	0.081	-0.127
Overall material deprivation	88	1.174	1.142	310	1.304	1.063	-0.129	-0.008
<i>Individual</i>								
Working	293	0.225	0.418	994	0.233	0.423	-0.008	-0.030
Overall income	293	209.40	143.89	994	213.30	166.59	-3.897	-4.551
Labour income	280	24.69	57.37	937	26.61	66.59	-1.923	2.946
Social benefits	293	179.80	135.33	994	182.27	156.18	-2.462	-10.59
Satisfaction with health	233	3.480	1.918	793	4.137	1.866	-0.656***	-0.906***
Satisfaction with income	233	3.012	1.685	792	3.358	1.702	-0.345***	-0.346*
Satisfaction with amount of leisure	232	4.012	1.701	790	4.208	1.629	-0.195	-0.300
Overall life satisfaction	233	3.836	1.831	792	4.248	1.772	-0.411***	-0.622***

Note: *: 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	-6.233**	-7.525**	-4.753	-6.323*	-7.392**	-4.239	-7.645**	-9.164**	-8.054*
	(2.994)	(3.427)	(4.025)	(3.243)	(3.724)	(4.318)	(3.364)	(3.954)	(4.740)
Post	0.809	-3.952	-2.004	0.441	-5.420	-3.310	1.729	-5.782	-5.069
	(3.885)	(5.488)	(5.619)	(3.880)	(5.530)	(5.616)	(4.314)	(5.854)	(5.953)
Post*Treat*Movers				1.234	-0.969	-0.378			
				(8.194)	(8.864)	(8.625)			
Post*Movers				2.184	4.941	5.545			
				(4.948)	(5.393)	(5.401)			
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
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,781	3,431	3,431	3,781	3,431	3,431	3,088	2,793	2,793
R-squared	0.767	0.769	0.770	0.767	0.770	0.770	0.775	0.777	0.777

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% 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 policy-on 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. 'Month FE' is a series of dummies for the year-month the household is surveyed.

Table 3. Housing benefits – Announcement and Enforcement

	(1)	(2)	(3)
Enforcement*Treat	-11.06*** (4.265)	-11.81** (4.869)	-11.11** (5.640)
Announcement *Treat	5.313 (3.900)	5.850 (4.512)	7.731 (6.098)
Enforcement	2.194 (4.527)	-8.953 (6.681)	-8.403 (6.666)
Announcement	-4.456 (4.711)	-0.709 (6.478)	0.634 (6.789)
Sample	Stayers	Stayers	Stayers
Beds./person* Enforcement	N	N	Y
Age* Enforcement	N	Y	Y
Tenure* Enforcement	N	Y	Y
Beds./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	3,088	2,756	2,756
R-squared	0.776	0.782	0.782

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% 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.

Table 4. Mobility

	(1)	(2)	(3)
Post*Treat	0.007 (0.033)	0.005 (0.037)	-0.033 (0.048)
Post	0.038 (0.048)	0.055 (0.065)	0.028 (0.067)
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
Month FE	Y	Y	Y
Observations	3,781	3,431	3,431
R-squared	0.559	0.579	0.580

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

Table 5. Bedrooms per person

	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treat	-0.102** (0.051)	-0.098* (0.052)	-0.026 (0.044)	-0.024 (0.052)	-0.019 (0.046)	-0.023 (0.055)
Post	0.057 (0.036)	0.036 (0.065)	0.053 (0.036)	0.023 (0.063)	0.056* (0.032)	0.004 (0.051)
Post*Treat*Movers			-0.484*** (0.163)	-0.477*** (0.163)		
Post*Movers			0.016 (0.040)	0.024 (0.045)		
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	3,781	3,431	3,781	3,431	3,088	2,793
R-squared	0.925	0.919	0.929	0.923	0.942	0.937

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

Table 6. Number of bedrooms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treat	-0.181*** (0.047)	-0.139*** (0.041)	-0.091* (0.053)	-0.018 (0.022)	0.004 (0.028)	0.051 (0.045)	0.005 (0.010)	0.014 (0.015)	0.009 (0.022)
Post	0.011 (0.044)	0.118 (0.078)	0.151* (0.091)	-0.050 (0.043)	0.001 (0.070)	0.032 (0.078)	-0.013 (0.010)	0.001 (0.017)	-0.001 (0.019)
Post*Treat*Movers				-0.955*** (0.202)	-0.935*** (0.207)	-0.927*** (0.199)			
Post*Movers				0.351*** (0.079)	0.357*** (0.088)	0.366*** (0.089)			
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
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,781	3,431	3,431	3,781	3,431	3,431	3,088	2,793	2,793
R-squared	0.971	0.971	0.971	0.975	0.975	0.975	0.995	0.995	0.995

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

Table 7. Number of people

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treat	-0.054 (0.065)	0.061 (0.075)	0.093 (0.097)	0.001 (0.068)	0.086 (0.080)	0.132 (0.099)	0.015 (0.068)	0.085 (0.083)	0.127 (0.112)
Post	-0.107 (0.065)	0.127 (0.094)	0.149 (0.097)	-0.155** (0.065)	0.058 (0.094)	0.088 (0.098)	-0.121* (0.069)	0.078 (0.109)	0.106 (0.115)
Post*Treat*Movers				-0.284 (0.205)	-0.163 (0.208)	-0.155 (0.205)			
Post*Movers				0.277*** (0.094)	0.225** (0.093)	0.234** (0.093)			
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
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,781	3,431	3,431	3,781	3,431	3,431	3,088	2,793	2,793
R-squared	0.967	0.969	0.969	0.968	0.970	0.970	0.972	0.974	0.974

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

Table 8. 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	-21.22*	-19.95	-16.48	-15.73	-13.21	-4.299	-8.532	-9.731	-7.796
	(12.84)	(14.59)	(17.67)	(10.16)	(11.61)	(14.45)	(11.08)	(12.89)	(16.34)
Post	13.61	30.41	32.84	10.12	12.73	18.85	12.96	30.14*	31.49*
	(17.02)	(21.77)	(22.84)	(9.367)	(12.46)	(14.00)	(12.84)	(17.08)	(17.79)
Sample	All	All	All	All	All	All	All	All	All
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
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,781	3,431	3,431	3,598	3,255	3,255	3,781	3,431	3,431
R-squared	0.795	0.800	0.800	0.787	0.798	0.798	0.814	0.816	0.816

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

Table 9. Saving and expenditure patterns – overall sample

	Household saves			Food expenditure (£/week)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treat	0.014	0.046	0.042	-1.083	0.583	2.991
	(0.039)	(0.046)	(0.052)	(2.796)	(3.364)	(4.333)
Post	-0.104	-0.066	-0.069	-2.606	2.178	3.909
	(0.057)	(0.077)	(0.078)	(3.192)	(5.047)	(5.372)
Sample	All	All	All	All	All	All
Beds./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	3,781	3,431	3,431	3,752	3,405	3,405
R-squared	0.632	0.637	0.637	0.823	0.829	0.829

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

Table 10. Material deprivation

	Lifestyle changes			Financial stress			Durable goods purchase			Overall		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post*Treat	0.143	0.031	-0.172	0.167	0.159	-0.129	-0.102	-0.056	-0.098	0.204	0.137	0.067
	(0.228)	(0.257)	(0.307)	(0.234)	(0.269)	(0.338)	(0.079)	(0.088)	(0.105)	(0.176)	(0.205)	(0.258)
Post	0.093	-0.139	-0.283	-0.061	-0.222	-0.425	-0.051	0.039	0.009	0.037	-0.123	-0.172
	(0.334)	(0.416)	(0.437)	(0.312)	(0.429)	(0.444)	(0.106)	(0.142)	(0.148)	(0.308)	(0.384)	(0.403)
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
Beds./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
Observations	2,318	2,124	2,124	3,779	3,429	3,429	3,768	3,420	3,420	2,310	2,118	2,118
R-squared	0.790	0.793	0.794	0.662	0.666	0.667	0.835	0.838	0.838	0.857	0.860	0.860

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

Table 11. Have a job – Overall sample and by individual background

	(1)	(2)	(3)	(4)	(5)
Post*Treat	-0.008 (0.033)	0.006 (0.057)	0.047 (0.044)	0.0000 (0.034)	-0.009 (0.037)
Post	0.012 (0.044)	0.026 (0.050)	0.009 (0.053)	-0.011 (0.046)	0.0007 (0.046)
Post*Treat*Head		-0.020 (0.066)			
Post*Head		-0.023 (0.032)			
Post*Treat*Female			-0.097* (0.056)		
Post*Female			0.013 (0.032)		
Post*Treat*21years				-0.011 (0.096)	
Post*21years				0.102** (0.044)	
Post*Treat* 50/55 years					0.023 (0.095)
Post*50/55 years					-0.009 (0.037)
Sample	All	All	All	All	All
Bedrooms/person*Post	N	N	N	N	N
Age*Post	Y	Y	Y	Y	Y
Tenure*Post	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Observations	5,326	5,326	5,326	5,326	5,326
R-squared	0.751	0.752	0.752	0.753	0.752

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level. ‘Head’ is an indicator that individual is head of household. ‘Female’ is an indicator that the individual is female. ‘21 years’ is an indicator that the individual is 16-21 years of age. ‘50/55’ is an indicator that a male individual is aged 55 plus and a female individual is aged 50 plus. For other notes see Table 2.

Table 12. Have a job – Interactions with previous earnings

	(1)	(2)	(3)
Post*Treat	0.005 (0.139)	-0.047 (0.116)	-0.081 (0.139)
Post	-0.352 (0.230)	-0.281 (0.209)	-0.271 (0.208)
Post*Treat* Below median	-0.213 (0.220)		
Post*Below median	0.141 (0.103)		
Post*Treat* Bottom quartile		-0.206 (0.314)	
Post*Bottom quartile		-0.005 (0.143)	
Post*Treat* Top quartile			-0.030 (0.180)
Post*Top quartile			0.012 (0.097)
Sample	All	All	All
Beds./person*Post	N	N	N
Age*Post	Y	Y	Y
Tenure*Post	Y	Y	Y
Individual FE	Y	Y	Y
Wave FE	Y	Y	Y
LA*Year FE	Y	Y	Y
Month FE	Y	Y	Y
Observations	1,759	1,759	1,759
R-squared	0.792	0.791	0.791

Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level. Below median, Bottom quartile, Top quartile refers to distribution of individual earnings in the pre-policy period. For other notes see Table 2.

Table 13. Sources of income – Overall sample and by individual background

	Overall income					Labour income					Social benefits				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Post*Treat	-11.63 (9.305)	2.712 (20.51)	-6.086 (15.02)	-11.19 (9.904)	-12.81 (9.965)	-3.072 (5.037)	2.983 (11.02)	3.549 (7.200)	-3.241 (5.089)	-0.658 (5.745)	-9.456 (8.682)	-0.952 (17.29)	-12.34 (14.46)	-8.164 (9.216)	-12.69 (8.900)
Post	32.37** (13.72)	33.09** (15.58)	16.85 (18.62)	28.77** (14.14)	32.23** (14.45)	10.10 (6.929)	14.31* (8.566)	7.905 (8.599)	7.646 (6.969)	7.983 (7.096)	27.60** (11.34)	26.70** (13.30)	18.75 (15.26)	26.54** (11.70)	28.45** (12.11)
Post*Treat*Head		-20.12 (26.55)						-8.278 (11.65)				-11.99 (23.60)			
Post*Head		-1.546 (13.92)						-6.575 (6.158)				1.217 (12.45)			
Post*Treat*Female			-11.89 (24.18)					-11.44 (9.619)					3.673 (22.78)		
Post*Female			17.33 (14.45)					3.398 (6.078)					8.734 (12.84)		
Post*Treat*21years				11.20 (31.83)					14.56 (22.76)					-13.88 (23.66)	
Post*21years				14.46 (9.116)					9.757 (7.385)					5.510 (6.794)	
Post*Treat*50/55 years					8.660 (29.10)					-13.64 (13.41)					21.92 (27.12)
Post*50/55 years					-4.907 (16.64)					-3.996 (12.14)					-5.971 (15.27)
Sample	All	All	All	All	All	All	All	All	All	All	All	All	All	All	All
Beds./person*Post	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N
Age*Post	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Tenure*Post	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	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	5,326	5,326	5,326	5,326	5,326	4,989	4,989	4,989	4,989	4,989	5,326	5,326	5,326	5,326	5,326
R-squared	0.779	0.779	0.779	0.779	0.779	0.787	0.787	0.787	0.787	0.787	0.810	0.811	0.811	0.810	0.811

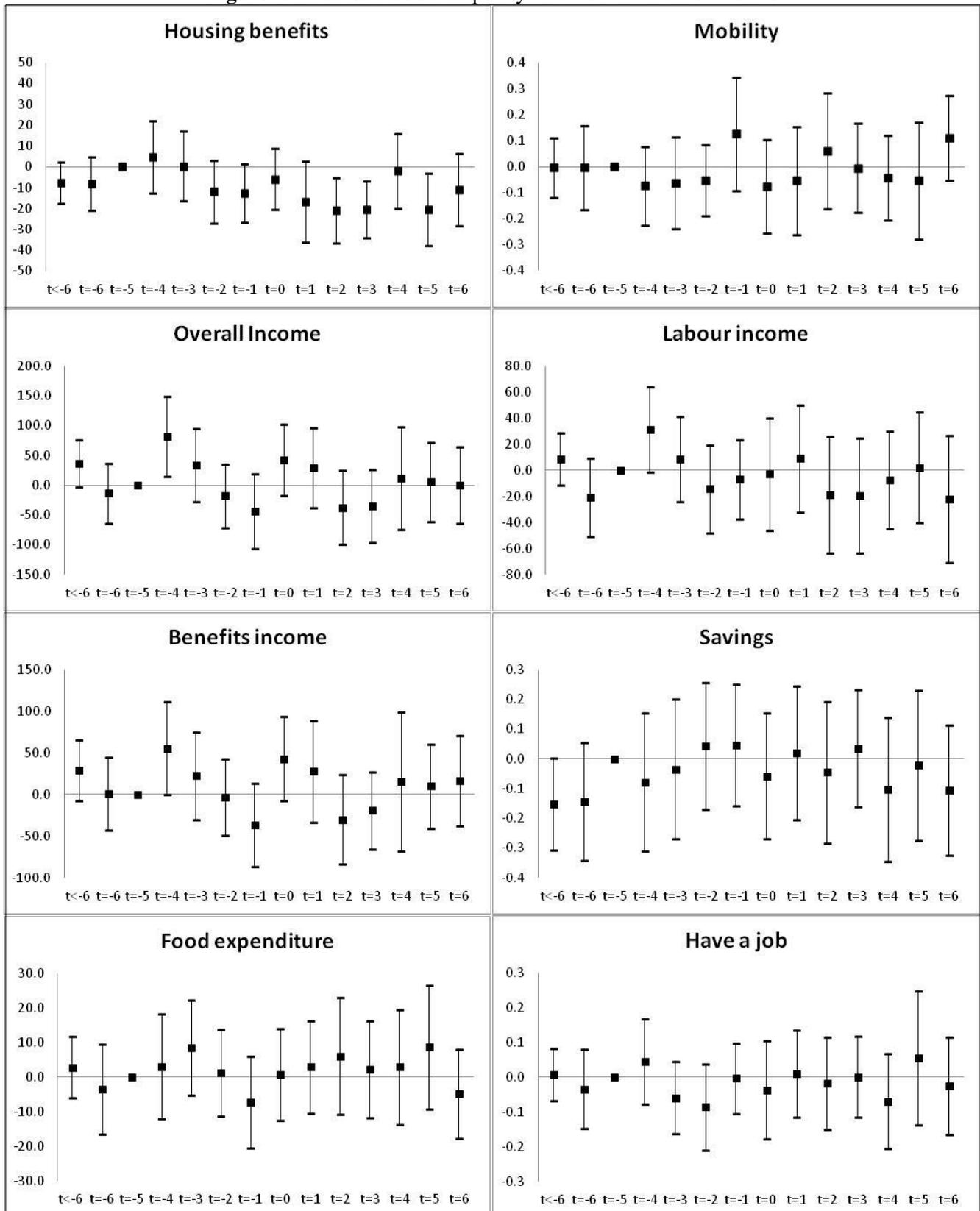
Note: Table reports regression coefficients and standard errors. Standard errors clustered at the household level. *: significant at 10% level; **: significant at 5% level; ***: significant at 1% level. For other notes see Table 2 and Table 11.

Table 14. Life satisfaction

	Health	Income	Amount of leisure time	Life overall
	(1)	(2)	(3)	(4)
Post*Treat	-0.127	-0.277	0.134	0.030
	(0.232)	(0.207)	(0.225)	(0.194)
Post	-0.367	-0.194	0.202	-0.033
	(0.346)	(0.326)	(0.338)	(0.316)
Sample	All	All	All	All
Beds./person*Post	N	N	N	N
Age*Post	Y	Y	Y	Y
Tenure*Post	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
Wave FE	Y	Y	Y	Y
LA*Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Observations	4,121	4,102	4,106	4,117
R-squared	0.732	0.717	0.651	0.734

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

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: 3,427. Job outcome measured at the individual level. Number of observations: 5,326. 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	198	624	281	620	1,935	941
Movers	31	105	44	119	364	180
Stayers	167	519	237	501	1,571	761
Individuals	412	1,233	573	1,810	5,385	2,713
Head of household	198	624	281	619	1,934	940
Non-head of household	214	609	292	1,191	3,451	1,773
Female	228	688	315	990	2,952	1,479
Male	184	545	258	820	2,433	1,237
16-21 years old	142	405	194	997	2,885	1,498
>21 years old	270	828	379	813	2,500	1,215
Working age	358	1,064	499	1,747	5,174	2,624
Nearly retired	54	169	74	63	211	89
Below median earnings – individuals	62	176	88	259	777	360
Bottom earnings quartile – individuals	27	74	37	131	410	183
Top earnings quartile – individuals	41	125	65	158	484	201

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	130	163	198	95	23	270	239	54
Overall income	130	163	198	95	23	270	239	54
Labour income	120	160	195	85	22	258	228	52
Social benefits	130	163	198	95	23	270	239	54
Satisfaction with health	95	138	171	62	19	214	187	46
Satisfaction with income	95	138	171	62	19	214	187	46
Satisfaction with amount of leisure	94	138	170	62	19	213	187	45
Overall life satisfaction	95	138	171	62	19	214	187	46

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	0.230	0.220	0.166	0.347	0.391	0.211	0.251	0.111
Overall income	179.08	233.58	253.43	117.63	68.36	221.41	205.07	228.57
Labour income	24.88	24.55	18.51	38.86	54.81	22.12	26.82	15.33
Social benefits	150.98	202.79	228.67	77.96	9.913	194.28	172.50	212.13
Satisfaction with health	3.242	3.644	3.239	4.145	5.000	3.345	3.625	2.891
Satisfaction with income	3.010	3.014	2.847	3.467	3.789	2.943	3.149	2.456
Satisfaction with amount of leisure	4.010	4.014	3.911	4.290	4.210	3.995	3.935	4.333
Overall life satisfaction	3.800	3.862	3.602	4.483	5.105	3.724	3.871	3.695

CENTRE FOR ECONOMIC PERFORMANCE
Recent Discussion Papers

1536	Pawel Adrjan Brian Bell	Pension Shocks and Wages
1535	Tom Kirchmaier Stephen Machin Matteo Sandi Robert Witt	Prices, Policing and Policy: The Dynamics of Crime Booms and Busts
1534	Laurent Bouton Paola Conconi Francisco Pino Maurizio Zanardi	Guns, Environment and Abortion: How Single-Minded Voters Shape Politicians Decisions
1533	Giulia Giupponi Stephen Machin	Changing the Structure of Minimum Wages: Firm Adjustment and Wage Spillovers
1532	Swati Dhingra Rebecca Freeman Eleonora Mavroeidi	Beyond Tariff Reductions: What Extra Boost From Trade Agreement Provisions?
1531	Doruk Cengiz Arindrajit Dube Attila Lindner Ben Zipperer	The Effect of Minimum Wages on Low-Wage Jobs: Evidence from the United States Using a Bunching Estimator
1530	Stephen Gibbons Vincenzo Scrutinio Shqiponja Telhaj	Teacher Turnover: Does it Matter for Pupil Achievement?
1529	Ghazala Azmat Stefania Simion	Higher Education Funding Reforms: A Comprehensive Analysis of Educational and Labour Market Outcomes in England
1528	Renata Lemos Daniela Scur	All in the Family? CEO Choice and Firm Organization
1527	Stephen Machin Matteo Sandi	Autonomous Schools and Strategic Pupil Exclusion

- | | | |
|------|--|--|
| 1526 | Stephan E. Maurer | Oil Discoveries and Education Spending in the Postbellum South |
| 1525 | Paola Conconi
Manuel García-Santana
Laura Puccio
Roberto Venturini | From Final Goods to Inputs: The Protectionist Effect of Rules of Origin |
| 1524 | Zack Cooper
Fiona Scott Morton
Nathan Shekita | Surprise! Out-of-Network Billing for Emergency Care in the United States |
| 1523 | Zack Cooper
Amanda Kowalski
Eleanor Neff Powell
Jennifer Wu | Politics, Hospital Behaviour and Health Care Spending |
| 1522 | Philippe Aghion
Ufuk Akcigit
Ari Hyytinen
Otto Toivanen | The Social Origins of Inventors |
| 1521 | Andrés Barrios F.
Giulia Bovini | It's Time to Learn: Understanding the Differences in Returns to Instruction Time |
| 1520 | Sara Calligaris
Massimo Del Gatto
Fadi Hassan
Gianmarco I.P. Ottaviano
Fabiano Schivardi | The Productivity Puzzle and Misallocation: An Italian Perspective |
| 1519 | Alex Bell
Raj Chetty
Xavier Jaravel
Neviana Petkova
John Van Reenen | Who Becomes an Inventor in America? The Importance of Exposure to Innovation |

The Centre for Economic Performance Publications Unit
 Tel: +44 (0)20 7955 7673 Email info@cep.lse.ac.uk
 Website: <http://cep.lse.ac.uk> Twitter: @CEP_LSE