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THE IMPACT OF HOUSING SUBSIDY CUTS ON THE LABOUR MARKET OUTCOMES OF CLAIMANTS: EVIDENCE FROM ENGLAND

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The Impact of Housing Subsidy Cuts on the Labour Market Outcomes of Claimants: Evidence from England

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Abstract

Housing subsidies are aimed at helping low-income individuals afford appropriate housing, but are costly to offer and, in the view of some experts and policy makers, reduce incentives for claimants to participate in the labour market. This paper investigates the labour market impacts of recent housing subsidy cuts in England that were aimed at encouraging labour market participation and increased work effort among claimants. My identification strategy relies on the fact that, within the time period investigated, the subsidy cuts were only implemented for claimants renting from private landlords while claimants renting from other segments of the rental market were unaffected. I utilise this variation in exposure to the subsidy cuts within a difference-in-differences framework and find no evidence of a change in labour market outcomes for those affected by subsidy cuts. My findings indicate that, at least on aggregate, the subsidy cuts did not succeed in encouraging employment among claimants. These null findings suggest that as a policy instrument, cuts to housing subsidies may not be effective in generating efficiency gains through increased labour market participation or work effort.

JEL classification numbers: H31, H42, H53
Keywords: housing subsidies, welfare programs, labour market behaviour

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1 Introduction

Housing subsidies assist low-income households with their rent obligations to help them afford appropriate housing. Critics note that these subsidies might depress work incentives among claimants leading to a reduction in labour supply. Following this line of reasoning, cuts to housing – and other in-kind – subsidies are often justified by governments as measures to encourage work or search effort by claimants (Taylor-Gooby, 2012). In England, recent housing subsidy cuts were justified in a similar manner, the main government objective being the creation of a 'fair' system where claimants are encouraged to work in order to afford quality housing[1].

Despite the common justification, economic theory yields largely ambiguous predictions on the sign (and size) of labour supply impacts from housing subsidies (Murray, 1980; Schone, 1992; Moffitt, 2002; Shroder, 2002). The standard neoclassical model of labour supply predicts that housing subsidy provision will lead to a reduction in labour supply through both substitution and income effects (Moffitt, 2002). Conversely, alternative models predict that labour supply effects may vary depending on whether housing is a substitute or a complement to leisure (Murray, 1980); whether housing is a complement to other consumption goods (Schone, 1992); or could even be positive if subsidies lead to reduced housing uncertainty and allow claimants to spend more time seeking employment (Collinson et al., 2015).

While theoretical predictions of housing subsidy effects on labour supply are ambiguous, empirical evidence mostly confirms the stipulations of the neoclassical model. Recent quasi-experimental studies from the U.S. find conclusive evidence that housing subsidy provision has a negative effect on labour supply, although this effect diminishes with time and is mostly rather small (Mills et al., 2006; Jacob and Ludwig, 2012; Carl-

[1] See House of Commons, Work and Pensions Committee (2010) for an overview of the policy consultation.

son et al., 2012). In contrast, empirical evidence from outside the U.S. is inconclusive and rather sparse (Shroder, 2010). Furthermore, the case studies in the literature tend to focus on the effects of housing subsidy provision, and very few studies focus on the effects of a reduction or withdrawal of subsidy entitlements from existing claimants. More specifically, when housing subsidies are withdrawn or reduced, does this induce claimants to increase labour supply either along the extensive (having a job) or intensive (hours of work) margin? In this paper, I aim to answer this question through an analysis of recent housing subsidy cuts in England.

In England, through the Housing Benefit (HB) system, housing subsidies constitute a significant share of welfare expenditures. In the first decade of the 21^{st} century, expenditures on housing subsidies increased by 46%, with annual expenditures in 2010/11 totalling at £21.4 billion (Wilson et al., 2016). Along with a range of austerity measures aimed at reducing the public deficit, the 2010 Coalition Government introduced several changes to the English housing subsidy system[2]. Specifically, the June 2010 Budget announced changes to the way Local Housing Allowance (LHA) rates, which determine subsidy entitlements for claimants in the private rental sector (PRS), are set[3]. These changes, rolled out in 2011-12, resulted in substantial cuts to the housing subsidy entitlements of PRS claimants.

The government's justification for the LHA cuts relied on two underlying objectives. First, they intended to curb expenditures from the housing subsidy system. Second, they wanted to ensure that households on subsidies do not occupy more expensive housing than low-income working families. This second objective was an attempt to induce claimants to seek employment, and to create the means for housing through labour and

[2] The reforms were rolled out UK wide, however the changes discussed in this paper concerned the English subsidy system to the greatest extent, as devolved administrations in other parts of the UK had some discretion over related housing policies and in some cases decided to mitigate the impacts of housing subsidy reform. For this reason, my analysis of the recent reforms to the housing subsidy system only concerns England.

[3] Claimants in the PRS are renting accommodation from private landlords.

not benefit income (see Wilson et al., 2016).

The LHA cuts came under significant scrutiny upon their implementation. Critics of the new system commented on the hardships the subsidy cuts created for claimants by reducing rent affordability leading to increased risk of eviction by landlords. A briefing report released by the Shelter Foundation claimed that while the LHA cuts led to a deterioration in housing conditions for PRS claimants, they did not lead to a noticeable increase in employment (Shelter, 2015). There is also evidence from the academic literature that the subsidy cuts led to a substantial reduction in rent affordability (Brewer et al., 2014). On the other hand, evidence on the reforms' impacts on claimant mobility is mixed (Brewer et al., 2014; Braakmann and McDonald, 2018) and no analysis to date has focused explicitly on their effect on labour market outcomes.

My analysis draws on individual-level panel data on housing subsidy claimants in England from the Understanding Society (US) longitudinal survey for the period 2009-2015. The empirical strategy makes use of a panel difference-in-differences (diff-in-diff) approach that examines the effects of the subsidy cuts through a comparison, over time, of groups affected (treated) and unaffected (control) by the policy. As the control group, I use a cohort of social rental sector (SRS) claimants. Subsidy claimants in the control group are renting accommodation from local authorities and housing associations (as opposed to private landlords) and are therefore unaffected by the housing subsidy cuts. Estimates are displayed using event-study plots, whereby the reform's impacts can be assessed at different points in time and long-run impacts can be estimated.

My results provide no evidence of a significant (and lasting) impact from the subsidy cuts on labour market outcomes either along the extensive or the intensive margin. My findings therefore provide no evidence that the government's objective to get claimants back to work (or even get them to seek work) was accomplished, at least not at the aggregate level.

The remainder of this paper is organised as follows. Section 2 outlines the recent changes to the UK housing subsidy system. Section 3 describes the data. Section 4 outlines the identification strategy. Section 5 presents and discusses the results. Section 6 concludes.

2 Policy Background

In England, all individuals can apply for Housing Benefit (henceforth HB), a means-tested subsidy that provides assistance with the rental costs of housing, as long as claimants: 1) live in rental accommodation 2) are on a low-income or are claiming benefits and 3) possess savings lower than £16,000. The amount of HB received by each individual/household is determined as follows:

$HB = \min\{rent,\ HB_{max}\}\ \ if\ Y \leq Y^T$ or

$HB = \min\{rent,\ HB_{max}\}\ - 0.65\left(Y - Y^T\right)\ if\ Y > Y^T$

where HB_{max} is the maximum eligible housing subsidy amount, Y is household income, and Y^T is the threshold income for HB eligibility. Above the threshold income, housing subsidies are withdrawn at the taper rate of 65%, i.e. the subsidy amount is reduced by 65 pence for every £1 increase in income. Note, that increased subsidies will lead to higher marginal tax rates by increasing the opportunity cost of labour (and increasing earnings in the absence of labour). In the UK case, due to the high taper rate (65%) the income increases of claimants from increased work effort are 'punished' by a particularly high marginal tax rate. Increased housing subsidies therefore make labour relatively 'costly' in comparison to leisure, inducing a substitution effect towards the latter. Conversely, a reduction in housing subsidies should have the opposite effect: a reduction in the opportunity cost of labour and increased incentives to provide work effort.

In the PRS, low-income households can access housing benefits through the Local Housing Allowance (henceforth LHA) system. The LHA system was introduced on April 7, 2008 and provides a set of rules that determine the amount of housing subsidy low-income households are eligible to claim (see Wilson et al., 2016). The extent to which LHA will cover rental costs is determined by the private market rent distribution in the specific geographical area (referred to as Broad Rental Market Area); the size of the household; and the earnings and income from other benefits claimed by household members. Upon their introduction, LHA rates were set to cover housing costs for properties with values below the local median house price (the cheapest half of local properties). Rates were then adjusted (monthly) to reflect inflation in rents.

Starting in 2011, the UK Government introduced (as part of the Welfare Reform Act 2012) several subsidy cuts and eligibility rule changes to the LHA system:

- LHA rates were set so that they only cover the bottom 30^{th} percentile of local rental properties instead of the bottom 50^{th} percentile;
- the Shared Accommodation Rate (a lower rate for claimants not living in shared accommodation) was extended to cover a wider age group;
- LHA rates were capped;
- the £15 per week excess, the amount claimants could keep when their rent was below the LHA rate, was removed;
- LHA rates no longer adjust to the inflation of rental costs – they are currently uprated in line with CPI inflation[4]

The introduction of the reform package was staggered: it was announced through the June 2010 budget and rolled out starting from April 2011, however, due to transitional

[4] This is important because according to analysis by Shelter (2015) rents have been rising more sharply than LHA rates, particularly in London.

protection periods some claimants were not rolled in until late 2012 (see Brewer et al., 2014). Transitional protection also meant that the earliest enrolment date for existing claimants was January 2012, and only new claimants were rolled in in the months before.

In the social rental sector (SRS), where accommodation is rented from local authorities or housing associations, there were plans to introduce the same LHA rules as in the PRS, but this policy was first deferred and then scrapped altogether by the UK Government (see Wilson et al., 2016). Instead, a tax on 'spare' bedrooms was introduced to encourage downsizing among claimants (Gibbons et al., 2018). This policy, often referred to as the 'bedroom tax', was introduced in April 2013, and constituted a small monetary 'punishment' for households occupying properties with more rooms than they are entitled to based on policy rules. The 'bedroom tax' therefore targeted a different policy base in comparison to the LHA cuts: it only led to a subsidy cut for those in the SRS not adhering to the specific subsidy eligibility rules, whereas the LHA cuts applied to all subsidy claimants in the PRS.

The objectives of the government with the LHA cuts were related to two concerning aspects of the housing subsidy system: its cost, and its effects on the housing and labour markets (Tunstall et al., 2015). First, a concern was that spending on housing subsidies constituted a large share of total welfare expenditures in the pre-reform period and yet did not contribute to an investment in the housing stock. This was because the subsidies were paid to PRS landlords for existing property. Second, as housing subsidies help pay rent costs for those at low wages, they effectively subsidise low wage (labour) income.

Considering these concerns, on one hand, the government's objective was to reduce expenditures from the housing benefit system and make the system simpler. On the other hand, they wanted to encourage labour market participation amongst claimants by providing more incentive to withdraw from benefits and seek work. As the Department for

Work and Pensions (DWP) argued:

> Providing some customers, mainly in London, with the ability to live in very high cost rented properties makes it extremely unlikely they would ever move completely off Housing Benefit because of the very high income levels required. Moving to more affordable accommodation could therefore encourage households to take up employment and move completely off benefits[5].

During the consultation period, the main government justification of the subsidy cuts was based on the assertion that housing subsidy claimants occupy more expensive housing than working individuals not in receipt of benefits. The Minister of Pensions put it this way:

> Low-income households rent at about 90% of what the Housing Benefit recipients are renting at. So they are renting at a lower level. [...] The facts are that low-income people who are not taking Housing Benefit are having to live in cheaper housing[6].

The government argued that housing subsidy cuts were necessary to eliminate an 'un-level' playing field, so that subsidy recipients would not enjoy higher quality housing than low-income working families, and would be encouraged to seek work. Some experts found this justification unsatisfactory. For example, a study by the Cambridge Centre for Housing and Planning Research (Fenton, 2010) claimed that the LHA cuts were likely to have no impact on the labour market activity of claimants as most subsidy recipients who were able to work were already in employment before the reforms. The author also claimed that the reason unemployment levels were high in some areas was due to structural weaknesses in regional economies, and not due to individuals' lack of willingness to work. Qualitative assessment of the recent reform by Shelter (2015) also

[5] See House of Commons, Work and Pensions Committee (2010).
[6] See House of Commons, Work and Pensions Committee (2010).

found that it had no noticeable impact on the labour market activity of claimants. Later sections of this paper will aim to identify these labour market impacts empirically.

3 Data

To assess the labour supply impacts of recent PRS housing subsidy cuts in England, I use data on housing benefit claimants from the Understanding Society (US) survey, covering the time period 2009 to 2015. The short sample period is due to the fact that the US survey only started in 2009. The US survey, also known as the UK Household Longitudinal Study (UKHLS), is a longitudinal survey of 40,000 UK households. In Petersen et al. (2013), the respondents surveyed in the General Population Survey (which includes most of the households surveyed in US) were found to be representative of the census population at the neighbourhood level.

The data can be described as an unbalanced panel where individuals are observed in waves (these need not overlap with years)[7]. I use data from the first six waves of the US survey. Some individuals are not measured in consecutive waves. I track individuals using the cross-wave person identifier (‘*pidp*’). Adult individuals that share the same household all receive claimant status even if only one of them is indicated as a housing subsidy claimant – whilst from an administrative point of view only one person claims the subsidies, those accrue to the entire household. Under these circumstances, household outcomes are considered, with two individuals forming a benefit unit. When a single person occupies a household, she is the only benefit unit. The sample includes

[7] The panel is unbalanced due to attrition present in the US survey. For example, in the extended sample there is a total of 10,536 observations for 2,734 individuals and 6 waves of the US survey (see Table 1, Appendix A). This indicates that in this sample, a specific individual is surveyed approximately 3.85 times in 6 waves. It is therefore possible that individuals are only measured once or twice in the period before or after the policy change. This is a limitation because ideally, the entire time path for the outcome variables should be observable for all individuals so that aggregate level information would reflect annual data on the entire sample.

housing subsidy claimants from the rental market, and only from England. I exclude SRS tenants with a 'spare room' so that the impacts of the 'bedroom tax' (introduced in 2013) do not bias the estimates (see Section 4 below). Overall, the sample contains 10,536 claimants, 31.4% of whom resided in the PRS before the reform and the remaining 68.6% resided in the SRS. Summary statistics on key variables are provided in Appendix A.

4 Identification Strategy

In this section, I use individual level panel data from the Understanding Society (US) longitudinal survey to examine the extent to which the LHA cuts impacted the labour market outcomes of housing subsidy claimants living in England.

I estimate the impacts of the housing subsidy cuts using a panel difference-in-differences (diff-in-diff) model. This approach compares changes in outcomes for private rental sector (PRS) claimants to changes in outcomes for social rental sector (SRS) claimants, over time. The model is an extension of the specification in Braakmann and McDonald (2018), who estimate the impact of LHA cuts on mobility outcomes. The baseline specification takes the following form:

$$\mathrm{y}_{\mathrm{it}} = \alpha_0 + \theta_i + \theta_t + \theta_w + \theta_{rt} + \sum_{\rho=1}^{7} \gamma_\rho * T_\rho * PRS_i + \sum Z_i^{'} * T_p + \epsilon_{it} \tag{1}$$

where:

- y_{it} are indicators of labour market outcomes (for individual 'i' at time 't') from the US survey, namely: 1) whether an individual is employed full-time; 2) whether an individual is employed part-time; 3) whether an individual participates in the labour market either by working or by actively looking for work; 4) whether an individual is unemployed; 5) the (log) hours of work by those in employment; 6) whether an individual would like to work or not and; 7) whether an individual has a second job or not;

- θ_i, θ_t , θ_w, θ_{rt} are individual, month-year, wave, and region-year fixed effects, respectively;
- PRS_i is an indicator of whether a claimant is in the PRS just before the reforms – this dummy indicates exposure to the reform;
- T_ρ are dummies for each specific sample year where $\rho = [2009,\ 2010,\ \ldots,\ 2015]$;
- γ_ρ are the coefficients of interest – they aim to estimate the extent to which outcomes were impacted by the LHA cuts in a given year ρ;
- γ_ρ is estimated for each year ρ;
- The term $\sum Z_i' * T_\rho$ includes individual specific control variables (measured each year) interacted with the treatment period;

To control for the impact of time-fixed individual characteristics on labour market outcomes, I include individual fixed effects. I also include month-year fixed effects to control for cyclical fluctuations in outcomes; wave fixed effects to account for survey wave specific trends and shocks; and region-year fixed effects to control for unobservable time-varying changes at the regional level. Standard errors are clustered at the individual level to account for within-individual correlation of error terms over time.

The sample is restricted to individuals and members of households who claimed housing benefits in the pre-reform (before 2012) period in the social (SRS) or private rental sectors (PRS). The LHA cuts only applied to those renting from the PRS, claimants from the PRS therefore constitute the treatment group: the group 'treated' with the policy change. SRS claimants are untreated by the reform and are therefore included in the control group[8]. The identifying assumption of the diff-in-diff estimation is that, in the absence of the LHA cuts, PRS claimants and SRS claimants would have followed parallel trends in outcome variables (see Angrist and Pischke, 2008). If trends in outcomes change between the two groups after the LHA cuts, I associate these changes with the housing subsidy reductions. I test this assumption using event study plots later in this paper.

A concern related to my identification strategy is that of endogenous selection into (or out of) the treatment group, due to, for example, individuals moving out of the PRS in response to the reform. Moreover, labour market responses to the reform, which partly determine claimant status, could also remove individuals from the treatment group putting a downward bias on estimates of labour market effects. To address this, I fix the selection into treatment and control groups in the pre-reform period.

Summary statistics for the treatment and control groups, before the reform, can be found in Table 1. The table summarises mean values for relevant covariates and outcome variables in the pre-announcement period. Balancing tests are also carried out to see whether there are significant pre-treatment differences in some variables. I include other sources of benefit income (for example jobseeker's allowance or employment support allowance) as potential covariates to control for the impact of changes in other benefits on outcomes (see Braakmann and McDonald, 2018).

In general, the pre-treatment differences between the treatment and control groups observed in Table 1 are not a threat to the diff-in-diff identification assumptions (Daw and Hatfield, 2018). On the other hand, confounders that are correlated with both treatment assignment and post-treatment trends in the outcome variable could lead to biased es-

[8] Not all SRS claimants were untreated during the sample period due to the introduction of the so-called 'bedroom tax', which applied to SRS claimants with a 'spare room' as of April 2013 (see Gibbons et al., 2018). So that the impact of the bedroom tax does not interfere with the results, individuals/households eligible for this tax are dropped from the sample (it is possible to identify these claimants based on the policy criteria).

Table 1: *Summary Statistics – Before LHA Reform Announcement*

Variable	Treatment	Control	Difference-in-means (t-test)
Covariates			
Age	38.84	45.53	6.69***
% student	0.03	0.03	0.00
Children in HH	1.13	0.88	-0.27***
Female	0.66	0.67	0.01
JSA	0.14	0.16	0.01*
ESA	0.02	0.01	0.00
CA	0.03	0.04	0.01*
IBA	0.08	0.14	0.06***
Outcomes			
% would like a job	0.22	0.25	0.03
% full-time	0.12	0.05	-0.07***
% part-time	0.17	0.08	-0.08***
% unemployed	0.24	0.23	-0.01
% participating	0.53	0.34	-0.18***
% having a second job	0.02	0.01	-0.01**
Hours worked	24.74	21.41	-3.33***
Observations	897	2,290	

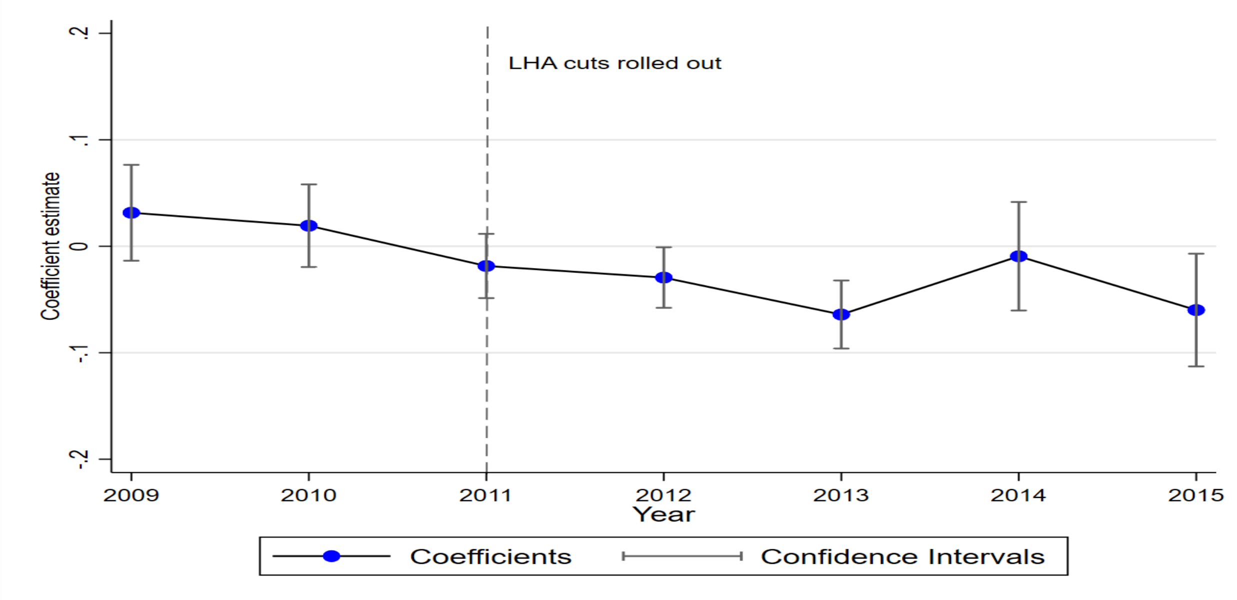
Notes: JSA stands for jobseeker's allowance, ESA is employment support allowance, CA is carer's allowance and IBA is incapacity benefit allowance.

timates. For example, the SRS control group is on average older in comparison to the treatment group (see Table 1). If age specific trends in outcomes coincide with the LHA cuts, the impact of this would be likely (wrongly) contributed to the policy change. To control for this, a common suggestion in the diff-in-diff literature is to interact covariates with the treatment period (see for example Kahn-Lang and Lang, 2019). I do this by picking the covariates that significantly differ between the treatment and control groups (see Table 1) and interacting them with the time indicators (T_p) in the estimations[9]. In the empirical model, the covariates used are age, number of children, and a dummy indicating whether the claimant is a recipient of incapacity benefit allowance (IBA). All other possible controls are fixed over time and their effects on the outcome variables should be captured by the individual fixed effects (see Gibbons et al., 2018).

Another important assumption for identification is that, in comparison to the control group, the LHA cuts have resulted in an 'effective' housing subsidy cut for the treatment group. In the presence of rent adjustments, rent affordability might not decline even after a reduction in housing subsidies, prompting no change in individual/household outcomes (see Brewer et al., 2014). To statistically assess whether the LHA reforms can be associated with an effective housing subsidy cut for the treatment group, I estimate Equation 1 using the share of rent covered by housing subsidies (subsidy coverage) as the outcome variable. Point estimates corresponding to different sample year 'treatment' effects are plotted in Figure 1. The negative coefficients shown after the LHA reform roll-out are indicative of an overall negative impact of the cuts on housing subsidy coverage in the PRS (relative to the SRS). Significant point estimates of -0.064 (for 2013) and -0.059 (for 2015) indicate that for these two years, the LHA cuts have led to (on average) a roughly 6 percentage point decrease in housing subsidy coverage for claimants in the PRS.

[9] I estimated various versions of the empirical model including/excluding interaction terms with control variables. Results are robust to the set of covariates used.

Figure 1: *Housing subsidy coverage of rents - event study plot to test impact of LHA cuts on PRS claimants*



Notes: Confidence intervals are drawn for 95% confidence – significance at the 5% level is indicated for each coefficient by the vertical bars (confidence intervals) not spanning zero.

Similarly to the results presented in Figure 1, in the following I employ an event study design to identify the labour market effects of the subsidy cuts. In this, I estimate 'treatment' effects ($\gamma_p * T_p * PRS_i$ in equation Equation 1) for each year of the sample and then plot the corresponding point estimates. Using the event study design, it is possible to track how each of the reform stages affected the outcome variables:

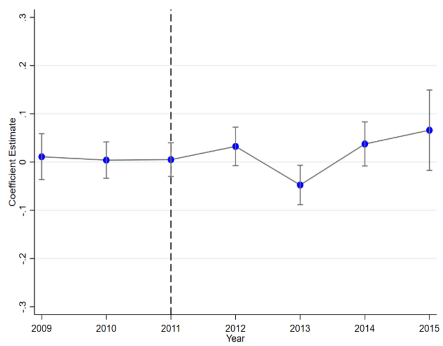
- Assuming no anticipation effects, the first three years are placebo treatments: since no actual change to the LHA system takes place, large and significant coefficients indicate diverging trends between treatment and control groups. The pre-reform period is therefore used to evaluate the parallel trends assumption: that values for the outcome variables would have followed parallel trends in the absence of a treatment (Angrist and Pischke, 2008). If we find diverging trends pre-reform, we cannot validate the parallel trends assumption[10].
- All subsequent years correspond to point estimates of treatment effects in all subsequent post-reform periods.

I use the event study plots in Panels (a)-(g) of Figure 2 to assess the impact of LHA cuts on claimants' labour market outcomes. In the event study plots, the y-axis shows point estimates for treatment impacts from the LHA cuts in each year of the sample. To find evidence of a significant impact: 1) coefficients should be zero close to zero and there shoud be no significant treatment effects in the first three years and 2) coefficients should significantly different from zero in subsequent (post-reform) years[11].

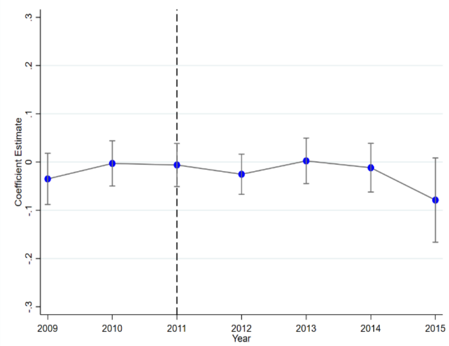
[10] Naturally, this assumption concerns whether trends are parallel post-treatment – as we cannot evaluate this (we do not observe the true counterfactual), we use pre-treatment trends to assess the likelihood that the assumption is going to be valid.

[11] In the event study plots, significance at the 5% level is indicated by the confidence intervals (vertical spikes surrounding the coefficients) not spanning zero. I also present the corresponding point estimates in Table 2 of Appendix A.

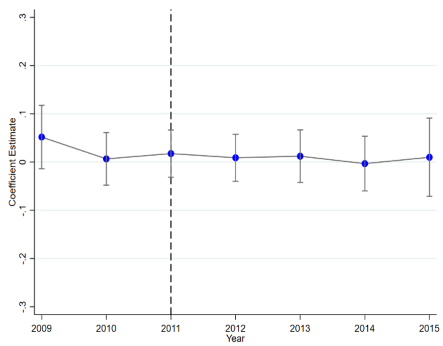
Figure 2: *Event Study Plots - Baseline*



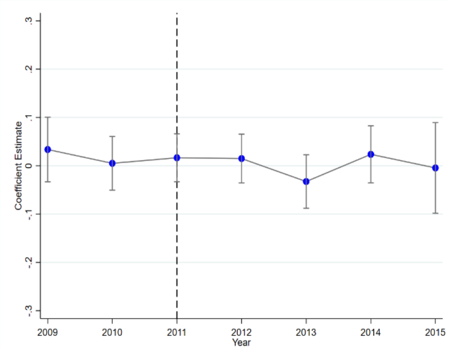
(a) Probability of being employed full-time



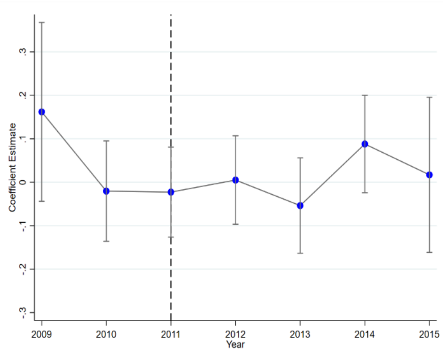
(b) Probability of being employed part-time



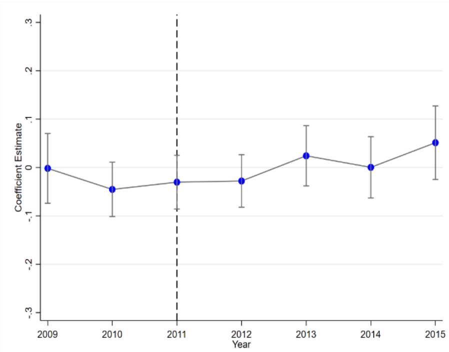
(c) Probability of being unemployed



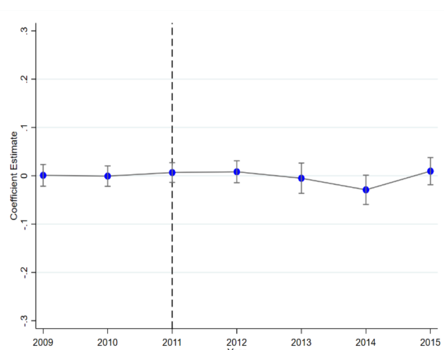
(d) Probability of participation



(e) Hours of work (ln)



(f) Would like to work



(g) Probability of having a second job

5 Results

The event study plots (see Figure 2) of the baseline estimates do not provide evidence of a significant impact from the LHA cuts on any of the labour market outcomes investigated. Nearly all point estimates are small and insignificant, and there is no discernible post-reform deviation (relative to the control group) in the labour market outcomes of PRS claimants. The only significant point estimate is found for full-time employment, for the year 2013 – the coefficient estimate of -0.0476 indicates that the introduction of LHA cuts in the PRS led to an on average 4.76 percentage point reduction in the probability of having a full-time job, relative to the control group. This result is not supported by point estimates for full-time employment in other post-reform years, as these are positive but imprecisely estimated (see Figure 2). Given that all other point estimates are small and random, it is entirely plausible that my finding for full-time employment is a consequence of random noise and is not related to the policy change investigated. To check for the robustness of my findings, and ensure that these are not biased by sample selection or modelling assumptions, I perform the following sensitivity checks.

5.1 Robustness checks

To evaluate the plausible robustness of the results, I make several changes to my baseline specification in Equation 1. More specifically, I test whether results change under: 1) a specification where disabled and/or retired individuals are excluded; 2) different regional sample specifications; 3) a specification where I restrict the sample to include only women; 4) and a specification where non-claimant PRS residents act as the control group.

5.1.1 Excluding disabled/retired individuals

The baseline sample includes subsidy claimants who are disabled or have already retired from the labour market. It is possible that these individuals are unable to return to the labour market as their personal circumstances might prohibit them from seeking (and engaging in) employment, thereby putting downward bias on the estimates of labour market impacts. I test the robustness of the results to the decision to include disabled/retired individuals by re-estimating the diff-in-diff model using a sample that does not include this group. Roughly 28% of the sample falls within this category, along with nearly 20% of PRS claimants. The results are summarised in the Appendix A, Table 3. The point estimates presented in Table 3 provide us with no credible evidence of a change in labour market outcomes in response to the housing subsidy cuts. While I do observe significant point estimates for full-time employment and part-time employment, for both variables the paths of the outcome variable consistently deviate from those of their respective control groups. For example, the positive post-reform deviations for part-time employment are predicated upon positive pre-reform differences (albeit imprecisely estimated) between control and treatment groups, lending no validity to the parallel trends assumption.

5.1.2 Regional specifications

Second, the diff-in-diff model from Section 4 is estimated for different regional samples in order to assess potential regional heterogeneity in the impacts of the subsidy cuts. Based on previous findings in the literature, I can expect the reform's impact to differ between London and the rest of England (see Brewer et al., 2014). The Shelter (2015) review of the LHA cuts also provides some evidence that London claimants were impacted more severely in comparison to claimants in the rest of England. For this reason, I estimate Equation 1 using separate regional samples for London; and for the rest of the England[12]. Results from the regional specifications are summarised in Table 4 to Table 5 in the Appendix A. For brevity, I do not show these results on event study plots. Nonetheless, the coefficient estimates in these tables correspond to event study point estimates for each sample year. In line with the baseline estimates, in the regional specifications I find no evidence that the LHA cuts affected the labour market outcomes of claimants.

5.1.3 Women only specification

The baseline estimates were based on a sample that included both male and female claimants. According to the literature on welfare programs, women are more likely to be the target of welfare schemes and are also more sensitive to changes in associated labour supply incentives than men (see Meghir and Phillips, 2010 or Blundell et al., 2016). To take this into account, I re-estimate the diff-in-diff model using a sub-sample that only includes female claimants. Results are summarised in Table 6 of the Appendix A. The point estimates for the women only sample are mostly small and insignificant providing little evidence that the LHA cuts can be associated with changed labour market outcomes for female claimants. For full-time employment we do observe an initial negative (in 2013) and subsequent positive (in 2014) impact from the subsidy cuts. These effects are difficult to reconcile as they nearly cancel each other out, suggesting that they are more likely to be a consequence of random fluctuations in full-time employment levels than a structural impact of subsidy reform. We can also observe a significant positive effect on claimants' willingness to work in 2015, but once again this effect is not supported by the path of the outcome variable post-reform. I conjecture that it is unlikely that it

[12] As a further robustness check, we also estimate a specification where the South East region of England, often thought to follow similar housing market trends as London, is included in the same regional sample with London. This robustness check has no noticeable impact on the results.

took four years for the subsidy cuts to start affecting individual work incentives, and a more likely explanation is that this finding is a consequence of random noise.

5.1.4 Alternative control group specification

Finally, to check the robustness of the results to the choice of control group, I re-estimate the original model using non-claimants residing in the PRS as the control group. In this specification, outcomes are compared between PRS claimants of housing subsidies and non-claimants, before and after the LHA cuts. Results are summarised in Table 7 of Appendix A. In Table 7, we can observe significant post-reform point estimates for both the 'hours of work' and 'would like to work' variables. Note however, that these estimates are predicated on significantly different pre-reform trends in both cases, violating the parallel trends assumption and lending no validity to associated findings. In the alternative control group specification, the parallel trends assumption is violated for four of the seven outcome variables, indicating that the non-claimant group is not a reliable comparison group in this case.

5.2 Discussion

The results presented in this section provide no credible evidence of an aggregate level impact from the LHA cuts on the labour market outcomes of claimants. Indeed, the null findings suggest that the policy was highly ineffective in its intended objective of encouraging labour supply amongst PRS claimants of housing benefits. For most outcomes, these null effects seem to be fairly precisely estimated, at least in the full sample model, where even the upper limits of confidence intervals would suggest reasonably small (between 4 and 7 percentage point) post-reform changes in labour market outcomes[13].

So what could explain the apparent lack of labour market responses to the subsidy cuts? One explanation is that most subsidy claimants who could (or wanted to) work were already in employment before the reforms to the subsidy system. According to Fenton (2010) the claimants that did not participate in the labour market before the reforms were mostly disabled, sick, or retired. Some claimants also reside in regions where unemployment is pervasive, and long-term structural issues in the labour market may prohibit them from finding employment. Moreover, it is possible that claimants on out-of-work benefits, such as income support or employment and support allowance, are not incentivised to seek employment as doing so would lead to a withdrawal of income from other sources (Shelter, 2015). Finally, while I do find evidence of a significant reduction (on average around six percentage points) in housing subsidy coverage for this sample of claimants (see Section 4), it is unclear whether this reduction is sizeable enough to trigger a change in aggregate level labour market behaviour. For example, consider a household whose monthly rent is £500. Holding other things constant, for this household my estimates of the reforms' effect on housing subsidy coverage would imply a roughly £30 monthly reduction in disposable income from reduced housing subsidies. Given the high marginal tax rates for claimants who decide to take up work (see Section 2), there might be easier ways for them to make up for this loss in disposable income, for example through reduced consumption or by moving to cheaper rental accommodation. For this reason, it is entirely plausible that similar housing subsidy cuts would have a larger effect on labour market outcomes in tax-benefit systems where marginal tax rates (taper rates) from taking up work are lower.

Another limitation of this study is that it does not look at responses to the LHA cuts that may have been overlapping with, or offsetting, labour market responses. For example,

[13] An exception is the hours of work outcome, where coefficients tend to be less precisely estimated, most likely due to the low sample size of claimants who are in employment.

if individuals moved to cheaper accommodation in response to the subsidy cuts and managed to increase their disposable income, this likely cancelled out the need for a substantial labour market response. Previous empirical studies on the LHA cuts find mixed evidence of a mobility response (Brewer et al., 2014; Braakmann and McDonald, 2018). In this case, measurement issues related to the US survey render any investigation of mobility outcomes potentially misleading. This is because the variables that can be used to indicate whether individuals moved or not in a given year are featured with very different frequencies in different survey waves, making the construction of time series problematic[14]. Nonetheless, since the explicit aim of the LHA reform was to increase labour supply among claimants, the finding of a null effect for these outcomes is important regardless of what the overlapping mobility response was. Whether the lack of a labour supply response is a consequence of mobility responses or other factors does not change the implication that the policy was ineffective in its main objective of bringing claimants back to work.

Overall, my results suggest that at the margin, labour market outcomes may not be highly dependent on changes in housing subsidies. From a policy perspective, this indicates that while the government is unable to accomplish their intended objective of bringing claimants back to work, they might force claimants, especially ones already in severe difficulty, under more vulnerable housing conditions. Nonetheless, the present analysis does not quantify these likely mechanisms behind the null effect observed. Consequently, the only inference that I can make based on the results is that, relative to the control group, the reforms have had no significant (and robust) aggregate level impact on the labour market outcomes of PRS claimants.

[14] These two variables are called 'mvyr' and 'plnowy4'. The latter seems to be used much more often in later survey waves, however using this variable results in a large drop in aggregate observations for mobility in later waves.

6 Conclusions

This paper looked at the labour supply impacts of a reform to the English housing subsidy system that has led to a substantial reduction in subsidy entitlements for private rental sector (PRS) claimants. These subsidy cuts were intended to encourage labour market participation and increased work effort by claimants. To estimate the effects of this policy on the labour market outcomes of claimants, I followed a difference-in-differences approach in which I compared PRS claimants to claimants renting from the social rental sector who were unaffected by the policy change. My findings indicate that the reforms had no significant and robust impact on the labour market outcomes of affected claimants, rendering the policy ineffective in its intended objective of bringing claimants back to work. These null findings are robust to a number of different specifications and sensitivity checks. Nonetheless, the precise mechanisms behind my findings are not identified in this paper, and more detailed analysis is needed to understand the factors that drive the labour market decisions of subsidy claimants.

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A Appendix

Table 1: *Summary Statistics – Baseline Sample*

Variable	Mean	SD	Count
Assignment			
% PRS (pre-reform)	0.31	0.46	10,536
Covariates			
Age	46.12	15.46	10,536
% student	0.02	0.14	10,536
% retired	0.20	0.40	10,536
% disabled	0.18	0.39	10,536
Children in HH	0.92	1.20	10,536
Female	0.68	0.46	10,536
JSA	0.11	0.32	10,536
ESA	0.02	0.14	10,536
CA	0.04	0.20	10,536
IBA	0.12	0.33	10,536
Outcomes			
% would like a job	0.20	0.40	10,536
% full-time	0.09	0.29	10,536
% part-time	0.13	0.33	10,536
% unemployed	0.19	0.39	10,536
% participating	0.41	0.49	10,536
% having a second job	0.02	0.13	10,536
Hours worked	25.02	11.63	2,013

Table 2: OLS Results – Baseline sample

Variables	FT	PT	UN	PR	WL	SJ	HW
$\gamma_p * PRS_i * 2009$	0.0110	-0.0350	0.0518	0.0337	-0.0018	0.0009	0.1620
	(0.0243)	(0.0271)	(0.0335)	(0.0341)	(0.0368)	(0.0115)	(0.1050)
$\gamma_p * PRS_i * 2010$	0.0040	-0.0029	0.0066	0.0051	-0.0453	-0.0006	-0.0204
	(0.0192)	(0.0239)	(0.0278)	(0.0283)	(0.0287)	(0.0108)	(0.0589)
$\gamma_p * PRS_i * 2011$	0.0050	-0.0061	0.0175	0.0166	-0.0303	0.0068	-0.0227
	(0.0178)	(0.0228)	(0.0250)	(0.0253)	(0.0284)	(0.0104)	(0.0528)
$\gamma_p * PRS_i * 2012$	0.0324	-0.0253	0.0089	0.0150	-0.0279	0.0083	0.0051
	(0.0203)	(0.0213)	(0.0248)	(0.0257)	(0.0277)	(0.0116)	(0.0519)
$\gamma_p * PRS_i * 2013$	-0.0476**	0.0024	0.0121	-0.0326	0.0242	-0.0051	-0.0538
	(0.0209)	(0.0241)	(0.0279)	(0.0283)	(0.0318)	(0.0160)	(0.0560)
$\gamma_p * PRS_i * 2014$	0.0374	-0.0118	-0.0033	0.0237	0.0003	-0.0290*	0.0880
	(0.0233)	(0.0258)	(0.0290)	(0.0301)	(0.0323)	(0.0155)	(0.0572)
$\gamma_p * PRS_i * 2015$	0.0660	-0.0789*	0.0099	-0.0045	0.0513	0.0096	0.0170
	(0.0425)	(0.0446)	(0.0414)	(0.0478)	(0.0388)	(0.0144)	(0.0911)
Observations	10,536	10,536	10,536	10,536	10,536	10,536	2,013
R-squared	0.072	0.039	0.044	0.041	0.081	0.035	0.225
Nr. of individuals	2,734	2,734	2,734	2,734	2,734	2,734	716
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the individual level and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: OLS Results – Excluded disabled/retired sample

Variables	**FT**	**PT**	**UN**	**PR**	**WL**	**SJ**	**HW**
$\gamma_p * PRS_i * 2009$	-0.0557	0.0794	0.0186	0.0511	-0.0604	0.0125	0.139
	(0.0538)	(0.0600)	(0.0587)	(0.0601)	(0.0488)	(0.0198)	(0.1350)
$\gamma_p * PRS_i * 2010$	-0.0718	0.110*	-0.0130	0.0219	-0.108***	0.0099	-0.0568
	(0.0507)	(0.0571)	(0.0535)	(0.0554)	(0.0393)	(0.0195)	(0.0999)
$\gamma_p * PRS_i * 2011$	-0.0714	0.111*	-0.0027	0.0374	-0.0805**	0.0133	-0.0696
	(0.0507)	(0.0575)	(0.0533)	(0.0545)	(0.0404)	(0.0195)	(0.0978)
$\gamma_p * PRS_i * 2012$	-0.0749	0.115**	-0.0234	0.0167	-0.0406	0.0047	-0.0317
	(0.0510)	(0.0580)	(0.0532)	(0.0542)	(0.0405)	(0.0189)	(0.0927)
$\gamma_p * PRS_i * 2013$	-0.0436	0.0837	-0.0317	0.0084	-0.0732*	0.0185	-0.0452
	(0.0512)	(0.0563)	(0.0508)	(0.0532)	(0.0411)	(0.0201)	(0.0930)
$\gamma_p * PRS_i * 2014$	-0.109**	0.0928	-0.0044	-0.0192	-0.0404	0.0158	-0.105
	(0.0506)	(0.0576)	(0.0533)	(0.0565)	(0.0412)	(0.0219)	(0.0892)
$\gamma_p * PRS_i * 2015$	-0.0715	0.0695	0.0070	0.0069	0.0628	0.0191	0.0181
	(0.0509)	(0.0542)	(0.0487)	(0.0546)	(0.0417)	(0.0180)	(0.0895)
Observations	7,653	7,653	7,653	7,653	7,653	7,653	2,000
R-squared	0.091	0.052	0.061	0.070	0.081	0.049	0.231
Nr. of individuals	2,210	2,210	2,210	2,210	2,210	2,210	716
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the individual level and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: OLS Results – London specification

Variables	FT	PT	UN	PR	WL	SJ	HW
$\gamma_p * PRS_i * 2009$	-0.0316	0.0111	-0.0147	-0.0369	0.0935	0.0074	-0.109
	(0.0552)	(0.0614)	(0.0842)	(0.0802)	(0.0870)	(0.0149)	(0.150)
$\gamma_p * PRS_i * 2010$	-0.0416	-0.0001	-0.0288	-0.0717	0.0285	0.0198	-0.255*
	(0.0495)	(0.0553)	(0.0629)	(0.0592)	(0.0708)	(0.0182)	(0.131)
$\gamma_p * PRS_i * 2011$	-0.0147	-0.0306	-0.0075	-0.0535	0.102	0.0201	-0.136
	(0.0463)	(0.0549)	(0.0581)	(0.0559)	(0.0722)	(0.0189)	(0.148)
$\gamma_p * PRS_i * 2012$	-0.0197	-0.0065	-0.0160	-0.0426	0.0995	-0.0224	0.0307
	(0.0503)	(0.0465)	(0.0617)	(0.0618)	(0.0669)	(0.0137)	(0.145)
$\gamma_p * PRS_i * 2013$	-0.0611	-0.0203	0.0881	0.0122	0.0205	0.0274	-0.127
	(0.0475)	(0.0613)	(0.0724)	(0.0779)	(0.0852)	(0.0280)	(0.141)
$\gamma_p * PRS_i * 2014$	0.0361	-0.0323	-0.0439	-0.0436	-0.0958	0.0051	0.0542
	(0.0568)	(0.0695)	(0.0727)	(0.0826)	(0.0846)	(0.0287)	(0.191)
$\gamma_p * PRS_i * 2015$	0.0796	-0.0656	-0.0140	-0.0009	0.0382	0.0284	0.173
	(0.0808)	(0.0842)	(0.0818)	(0.113)	(0.0849)	(0.0249)	(0.251)
Observations	2,457	2,457	2,457	2,457	2,457	2,457	488
R-squared	0.107	0.084	0.087	0.076	0.115	0.081	0.446
Nr. of individuals	690	690	690	690	690	690	178
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the individual level and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: OLS Results – Rest of England specification

Variables	FT	PT	UN	PR	WL	SJ	HW
$\gamma_p * PRS_i * 2009$	0.0194	-0.0432	0.0712**	0.0551	-0.0223	-0.0017	0.152
	(0.0266)	(0.0305)	(0.0363)	(0.0373)	(0.0408)	(0.0141)	(0.129)
$\gamma_p * PRS_i * 2010$	0.0143	-0.0034	0.0182	0.0264	-0.0627**	-0.0075	0.0004
	(0.0207)	(0.0265)	(0.0309)	(0.0318)	(0.0315)	(0.0128)	(0.0680)
$\gamma_p * PRS_i * 2011$	0.0040	0.0057	0.0255	0.0356	-0.0640**	0.0027	-0.0065
	(0.0189)	(0.0248)	(0.0276)	(0.0279)	(0.0308)	(0.0121)	(0.0530)
$\gamma_p * PRS_i * 2012$	0.0434*	-0.0272	0.0188	0.0339	-0.0544*	0.0138	0.0356
	(0.0221)	(0.0242)	(0.0266)	(0.0278)	(0.0304)	(0.0142)	(0.0580)
$\gamma_p * PRS_i * 2013$	-0.0391*	0.0094	-0.0148	-0.0449	0.0244	-0.0124	-0.0282
	(0.0236)	(0.0256)	(0.0295)	(0.0289)	(0.0334)	(0.0191)	(0.0590)
$\gamma_p * PRS_i * 2014$	0.0266	-0.0024	0.0175	0.0425	0.0197	-0.0358*	0.0272
	(0.0257)	(0.0272)	(0.0312)	(0.0321)	(0.0346)	(0.0184)	(0.0548)
$\gamma_p * PRS_i * 2015$	0.0700	-0.0919	0.0227	0.0005	0.0671*	-0.0045	-0.0970
	(0.0512)	(0.0565)	(0.0486)	(0.0548)	(0.0402)	(0.0173)	(0.0990)
Observations	8,079	8,079	8,079	8,079	8,079	8,079	1,525
R-squared	0.078	0.047	0.054	0.050	0.089	0.037	0.298
Nr. of individuals	2,055	2,055	2,055	2,055	2,055	2,055	539
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the individual level and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: OLS Results – Women only specification

Variables	**FT**	**PT**	**UN**	**PR**	**WL**	**SJ**	**HW**
$\gamma_p * PRS_i * 2009$	0.0428	-0.0577	0.0262	0.0188	-0.0348	-0.0101	0.222
	(0.0284)	(0.0372)	(0.0415)	(0.0467)	(0.0478)	(0.0122)	(0.150)
$\gamma_p * PRS_i * 2010$	0.0123	-0.0133	0.0105	0.0085	-0.0617*	0.0049	0.0151
	(0.0221)	(0.0325)	(0.0343)	(0.0376)	(0.0366)	(0.0124)	(0.0668)
$\gamma_p * PRS_i * 2011$	0.0005	-0.0034	0.0208	0.0182	-0.0400	0.0015	-0.0465
	(0.0212)	(0.0301)	(0.0318)	(0.0350)	(0.0358)	(0.0115)	(0.0662)
$\gamma_p * PRS_i * 2012$	0.0343	-0.0375	0.0297	0.0261	-0.0365	0.0072	-0.0295
	(0.0238)	(0.0289)	(0.0305)	(0.0335)	(0.0358)	(0.0151)	(0.0634)
$\gamma_p * PRS_i * 2013$	-0.071***	0.0260	-0.0181	-0.0652*	0.0019	0.0079	-0.0360
	(0.0250)	(0.0326)	(0.0328)	(0.0363)	(0.0398)	(0.0205)	(0.0679)
$\gamma_p * PRS_i * 2014$	0.0539**	-0.0226	0.0231	0.0592	0.0006	-0.0343	0.0998
	(0.0272)	(0.0346)	(0.0358)	(0.0389)	(0.0403)	(0.0212)	(0.0655)
$\gamma_p * PRS_i * 2015$	0.0514	-0.0782	0.0330	0.0039	0.0927**	-0.0020	-0.0585
	(0.0576)	(0.0633)	(0.0496)	(0.0624)	(0.0452)	(0.0183)	(0.113)
Observations	7,164	7,164	7,164	7,164	7,164	7,164	1,468
R-squared	0.079	0.058	0.045	0.057	0.095	0.044	0.265
Nr. of individuals	1,847	1,847	1,847	1,847	1,847	1,847	519
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the individual level and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: OLS Results – Non-claimant control group

Variables	FT	PT	UN	PR	WL	SJ	HW
$\gamma_p * PRS_i * 2009$	-0.183***	0.0156	0.0648	-0.103*	0.152***	0.0104	-0.229**
	(0.0632)	(0.0710)	(0.0518)	(0.0607)	(0.0424)	(0.0243)	(0.0971)
$\gamma_p * PRS_i * 2010$	-0.214***	0.0582	0.0915*	-0.0622	0.0609*	0.0002	-0.271***
	(0.0601)	(0.0696)	(0.0490)	(0.0577)	(0.0347)	(0.0238)	(0.0836)
$\gamma_p * PRS_i * 2011$	-0.193***	0.0618	0.115**	-0.0164	0.0949***	-0.0082	-0.248***
	(0.0606)	(0.0694)	(0.0487)	(0.0564)	(0.0350)	(0.0237)	(0.0844)
$\gamma_p * PRS_i * 2012$	-0.158***	0.0786	0.0573	-0.0190	0.0799**	-0.0044	-0.185**
	(0.0601)	(0.0692)	(0.0488)	(0.0567)	(0.0344)	(0.0231)	(0.0823)
$\gamma_p * PRS_i * 2013$	-0.120*	0.0558	0.0523	-0.00935	0.0397	0.0313	-0.169**
	(0.0612)	(0.0672)	(0.0491)	(0.0584)	(0.0355)	(0.0239)	(0.0849)
$\gamma_p * PRS_i * 2014$	-0.130**	0.0971	0.0234	-0.0129	0.0632*	0.0285	-0.132
	(0.0615)	(0.0695)	(0.0487)	(0.0585)	(0.0351)	(0.0259)	(0.0810)
$\gamma_p * PRS_i * 2015$	-0.111*	0.0652	0.0271	-0.0184	-0.0313	0.0038	-0.138*
	(0.0612)	(0.0659)	(0.0458)	(0.0575)	(0.0357)	(0.0210)	(0.0812)
Observations	12,039	12,039	12,039	12,039	12,039	12,039	7,319
R-squared	0.059	0.030	0.080	0.057	0.046	0.033	0.090
Nr. of individuals	3,954	3,954	3,954	3,954	3,954	3,954	2,652
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the individual level and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.