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ISSN: 2365-9793

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ABSTRACT

The Impact of a Rising Wage Floor on Labour Mobility across Firms*

In April 2016, a National Living Wage replaced the National Minimum Wage for employees in the UK aged 25 and above, raising their statutory wage floor by 50 pence per hour. This uprating was almost double any in the previous decade and expanded the share of jobs covered by the wage floor by around 50%. Using linked employer-employee data, we examine the effect of this policy on the propensity for minimum-wage employees to change firms. We find no evidence that the substantial compression at the bottom of the wage distribution affected the average rates of year-to-year cross-firm mobility among low-paid workers. While past studies have suggested relatively benign effects of UK minimum wage policy on employment levels, our findings suggest that this also applies to employment dynamics and the aggregate reallocation of workers across firms.

JEL Classification: J23, J38, J68, J88

Keywords: national minimum wage, on-the-job search, low pay, living wage, UK labour

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* We gratefully acknowledge funding from ADR UK (administrative Data Research UK) and the Economic and Social Research Council (Grant No. ES/T013877/1). The work is based on analysis of the research-ready datasets from the Annual Survey of Hours and Earnings (ASHE), accessed through the ONS Secure Research Service. The use of the ONS data in this work does not imply the endorsement of the ONS or data owners in relations to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates. National Statistics follow consistent statistical conventions over time and cannot be compared to these findings. We thank Rachel Scarfe, Eduin Latimer, and participants at the 2023 Work and Pensions Economics Group Conference, 2023 Low Pay Commission Research Symposium, and 2024 Colloquium on Personnel Economics, for valuable comments.

1. Introduction

A vast literature has evaluated the impacts of minimum wage policies on labour markets (for reviews of the recent evidence, see Cengiz et al., 2019; Dube, 2019; Neumark, 2018; Neumark & Shirley, 2022). One potential impact, subject to relatively limited research so far, is on job-to-job mobility, which affects labour market efficiency, and provides for job ladders that allow firms to grow and workers to escape low pay (e.g., Bagger & Lentz, 2019; Haltiwanger et al., 2018; Lise et al., 2016; Moscarini & Postel-Vinay, 2018). However, the theoretical impact of a rising wage floor on aggregate labour mobility between firms is unclear. On the one hand, mobility could increase through a reallocation effect, whereby low-paid employees move to more efficient or profitable firms that are better placed to absorb the higher labour costs. There is evidence in this direction from Germany, where the introduction of a national minimum wage led to economically significant job upgrading among the affected employees, from smaller to larger and less to more productive employers (Dustmann et al., 2022). On the other hand, mobility could decrease if the floor increases the wages of workers who would otherwise have felt underpaid and been searching for a new job. The expected value of on-the-job search is diminished when the wage floor covers an increasing proportion of all jobs; employees reduce their search effort and, furthermore, if assuming random search, any offers they receive are less likely to improve on their current terms. Studies from the United States have shown that higher minimum wages increase low-wage job tenure, suggesting that this latter channel can dominate in some settings (Dube et al., 2007, 2016; Jardim et al., 2018).

In this paper, we use linked employer-employee data and difference-in-differences estimation to evaluate the impact of a large hike in the UK wage floor on labour mobility across firms. There is already evidence that this hike in 2016, known as the National Living Wage (NLW), substantially raised the earnings of low-paid employees, with significant spillovers up the wage distribution and little negative impact on employment, except for among women working part-time (e.g., Aitken et al., 2019; Avram & Harkness, 2019; Giupponi et al., 2024). Despite the substantial upwards compression of wage differences between jobs, here we find no evidence that the NLW on average altered the extent of firm-to-firm switching among low-paid workers.

As well as other evaluations of the NLW, we contribute to the vast literature that has studied the aggregate impacts of UK minimum wage policy since its introduction in 1999. Policy makers may already have felt somewhat reassured that the majority of studies have found modest or no evidence of negative employment effects from the subsequent uprating of the UK wage floor over the following decades (e.g., Dickens et al., 2014, Dickens et al., 2015; Dolton et al., 2012; Fidrmuc & Tena, 2018; and for a meta-regression analysis see de Linde et al., 2013). Our findings may add to that reassurance, since there appears to be no evidence that the largest ever hike in the UK wage floor affected the mobility of workers across

firms, which is an important metric for the health of the labour market, particularly in its ability to reallocate resources and drive aggregate productivity growth (e.g., Foster et al., 2008; Fujita et al., 2024).

2. Context, Data and Methodology

The UK government introduced a National Minimum Wage (NMW) in 1999, set at £3.60 per hour for all employees aged 18+. After substantial initial increases, the decade from 2004 saw the wage floor raised at a modest average 3% per year, from £4.85 in 2005 to £6.70 in 2015. In July 2015, the UK announced a new National Living Wage (NLW) only for workers aged 25+. The NLW and was set at £7.20 per hour in April 2016, 50 pence (7.5%) higher than the previous £6.70 rate of October 2015, and 70 pence (10.2%) higher than the £6.50 rate of April 2015 (see Figure 1a). This was the largest annual increase in the UK wage floor since its introduction, raising the bite from around 55% to 60% of median hourly wages for all employees aged 25+. The share of employee jobs among those aged 25+, paid at or below the minimum wage, rose from 5.1% in April 2015 to 7.8% in April 2016 (Figure 1b).[1]

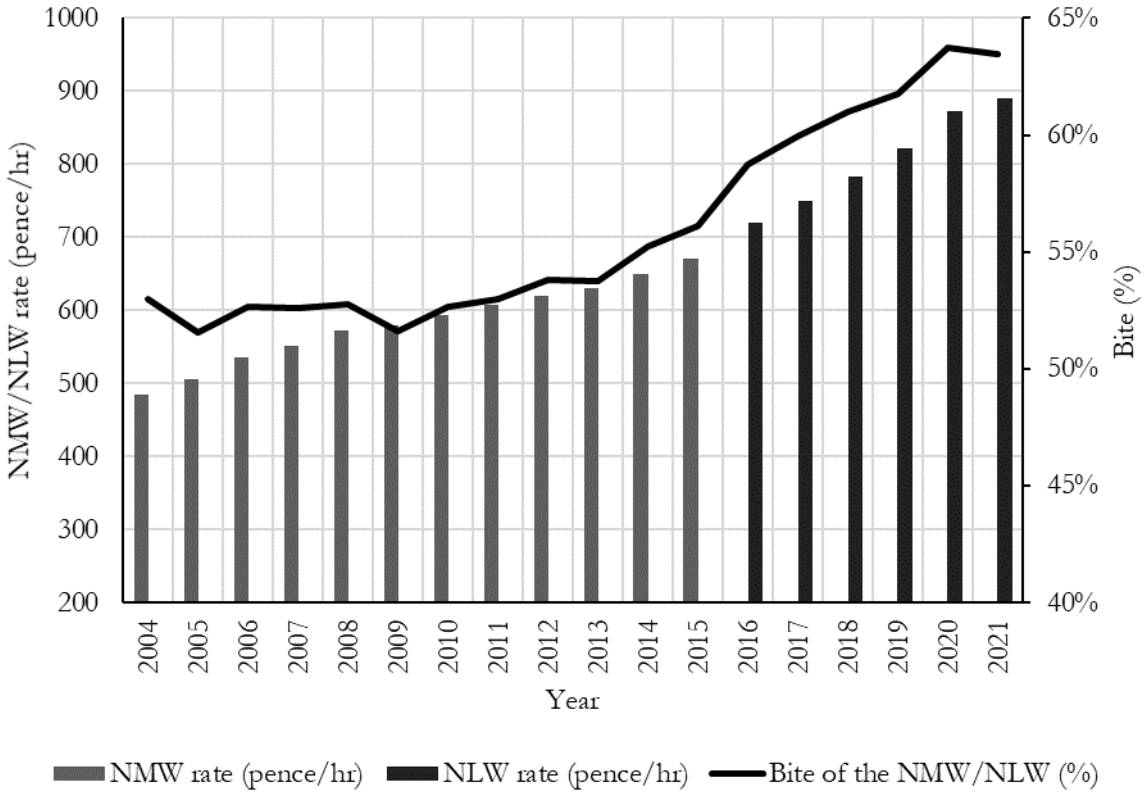
Our data are from the research-ready version of the Annual Survey of Hours and Earnings (ASHE) (Office for National Statistics, 2022; Ritchie et al., 2023). The ASHE is based on a 1% sample of employee jobs, taken from administrative records. Employees are selected by the last two digits of their social security number and appear in the issued sample every year that they hold an employee job. Their employer reports for a specific reference date in April, and responses are typically obtained for around two-thirds of the issued sample each year. Personal and employer identifiers allow the linking of workers and jobs over time. In general, the ASHE tends to under-represent jobs in smaller private sector employers. Weights are available to address employer-level response biases in each annual sample and the panel attrition across consecutive years (see Forth et al., 2022, 2023).

The ASHE data cover Great Britain and provide around 150,000 annual observations. The pay and hours data in ASHE are high quality, coming directly from payrolls. We follow the Low Pay Commission, the independent public body that advises the government on the UK wage floor, in using a measure of gross hourly earnings which includes basic pay, bonus or incentive pay and pay received for other reasons, but excludes overtime and shift premium pay, and use this to identify employees affected by the increasing wage floor.[2] We focus our analysis on workers employed from one year to the next and use the employer identifiers in ASHE to indicate whether an employee moved jobs between years. In ASHE, it is not possible to distinguish exits to non-employment from panel non-response, and the dis-employment effects of the NLW have been investigated elsewhere (e.g., Aitken et al., 2019; Giupponi et al., 2024).

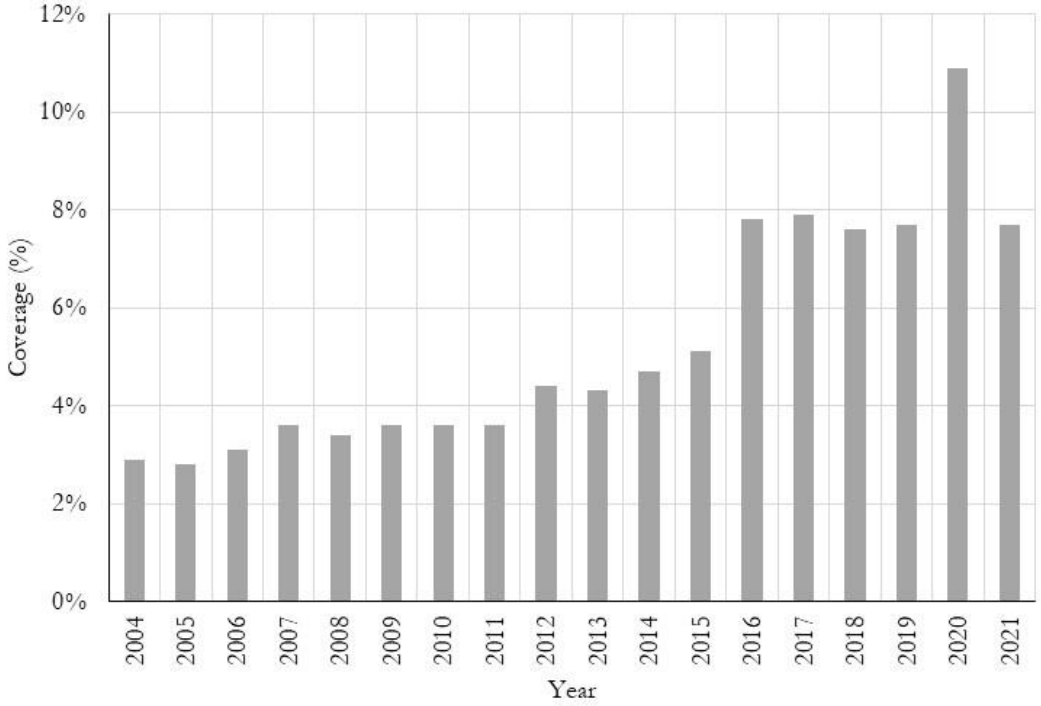
[1] In Figure 1b, the spike in 2020 reflects a temporary reduction in many employees' earnings arising from the Coronavirus Job Retention Scheme (furlough).
[2] Employees whose pay was affected by absence during the reference period are excluded.

FIGURE 1: Nominal rates, bite, and coverage of the UK National Minimum Wage (NMW) and National Living Wage (NLW), by year

a. Nominal hourly rates and bite



b. Coverage of employee jobs



Source: ASHE

Notes: Bite and coverage of the NMW/NLW are estimated using wages for all employees aged 25+, main job, with no loss of pay (except furlough), adult rates, using the revised ASHE weights developed by Forth et al. (2022). 95% confidences intervals around the estimates shown in Figure 1b are all within +/- 0.2 percentage points.

To estimate the employment impact of a hike in the wage floor due to the introduction of the NLW, we deploy a difference-in-differences (DiD) estimator, as in earlier studies examining the introduction and upratings of the NMW (e.g., Aitken et al., 2019). This estimates the policy impact (the wage floor hike due to the NLW) by comparing a treated group, directly affected due to wages below the new floor, with a control group earning just above the new floor. The difference in mobility rates between treated and control groups is assumed stable in the absence of the treatment (the parallel trends assumption). The average treatment effect (ATE) is identified by comparing the differences between the two groups' cross-firm mobility rates before and after the policy change. Since the policy is national, there is no geographical variation in the level or timing of the hike that we can exploit at the worker level; workers may also move home when switching jobs. There is some variation in the treatment across worker ages that could be used, since those aged 21-24 at the time of the NLW introduction had no hike in their wage floor in April 2016; their wage floor remained at £6.70 as per the previous main rate of the NMW for all workers aged 21 and over, before the new 21-24 rate rose to £6.95 in October 2016 when the NLW for 25+ was not further uprated. However, assuming workers are forward-looking and make decisions based on the expected present value of different opportunities, then standard theory implies that younger workers in minimum wage jobs were also directly treated to some extent by the NLW hike in April 2016. Thus, comparing cross-firm mobility between worker groups on either side of the age 25 threshold, before and after the policy, is not an especially attractive identification strategy. Even so, we come back to this later as a robustness check. Until that point, our estimation sample excludes workers aged below 25.

Until 2016, the wage floor was uprated annually in October, six months after the preceding ASHE and six months prior to the next. From 2016 onwards, the uprating was in April and broadly coincided with the ASHE fieldwork. As the NLW was announced in July 2015 and came into force in April 2016, we define the policy as starting in the year from April 2015 to April 2016 (2015/16 hereafter). This is conventional in the literature (see Aitken et al., 2019); it accounts for anticipation effects from October 2015 and any immediate effects of the rising floor in April 2016. We include 2016/17 as a second policy period, since any effect on mobility may take longer to work through than for wages. We thus compare the rates of labour mobility in 2015/16 ($t = 3$) and 2016/17 ($t = 4$) (the policy periods) with those in 2013/14 ($t = 1$) and 2014/15 ($t = 2$) (the base periods). As the wage floor was uprated to some extent in each period, we are looking to identify the impact of the particularly large NLW uprating in 2015/16.

We estimate the following using least squares:

$$Y_i = \alpha_0 + \beta D_i + \sum_{t=\{1,3,4\}} \gamma_t Z_{ti} + \sum_{t=\{1,3,4\}} \delta_t (D_i \cdot Z_{ti}) + \boldsymbol{\vartheta} \boldsymbol{X}_i + \varepsilon_i \quad (1)$$

where Y_i is the outcome of interest for worker observation i. Z_{ti} is a set of period dummy variables corresponding to when the worker is observed, where 2014/15 ($t = 2$) is the omitted category. $D_i = 1$

if the worker belongs to a treated group and is zero otherwise. The vector $\boldsymbol{X_i}$ includes controls, which vary across specifications. These are omitted from our initial specification. All other specifications include dummy variables capturing the three-way interaction of employee gender (male/female), age (25-34, 35-44, 45-54, 55-64, 65+), and hours worked (full-time/part-time), as well as dummy variables for tenure in the job at the beginning of the period (in years: [0-0.5), [0.5,1), [1-2), [2-5), [5,10), [10,20), 20+). This is extended with fixed effects for the {region×period}, {region×period×occupation}, and {firm×period} of a job, where region is the employee's home Government Office Region (e.g., Scotland, London, West Midlands) and occupation is classified at the 2 or 3-digit level of the Standard Occupational Classification 2010 (SOC). The parameters δ_t give the regression-adjusted differences in Y_i between the treated and control groups across periods. These establish whether the treated and control groups exhibit parallel trends in the base period, δ_1, and whether the difference in Y_i between these two groups changes, compared to the base period difference, β, for two periods after the policy, δ_3 and δ_4. We compute standard errors robust to clustering at both worker and {firm×period} levels, where the former is possible because the same person can be observed in ASHE across multiple periods.

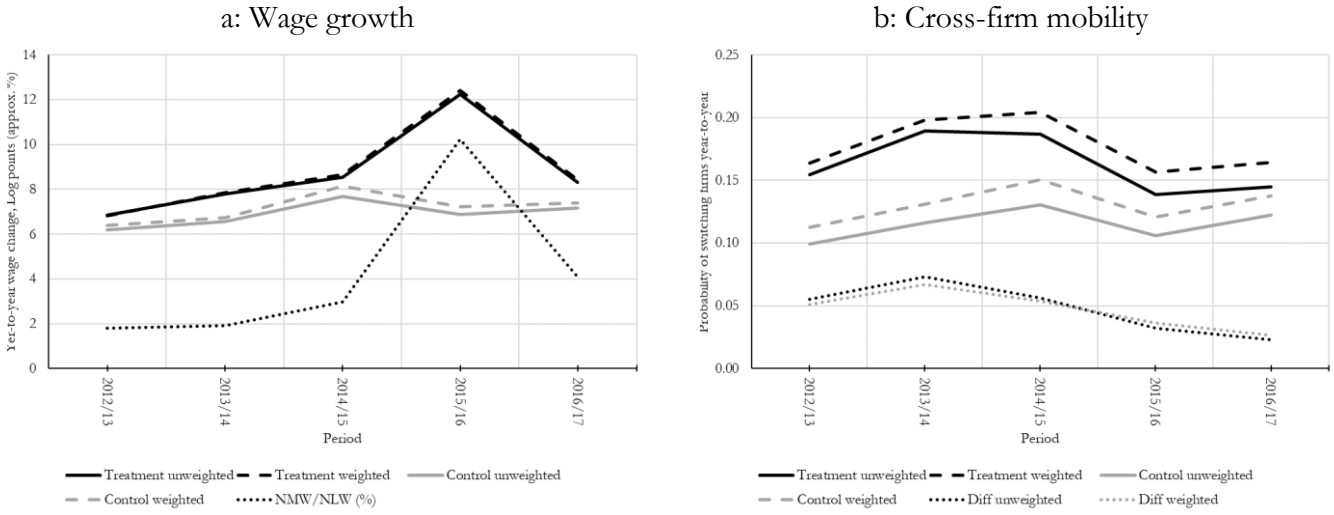
We first estimate the ATE on wage growth. We then estimate the effect on firm-to-firm mobility. In our base specification, the treated group is employees with earnings in the first year of each two-year period, t, that are at or above the wage floor applying in that year but below the floor that will apply in the second year of each t; these employees are directly affected by the policy change. The control group is all employees with earnings in the first year of each period, t, that are either at the incoming wage floor or up to 10% above it. We check the sensitivity of our results to the definition of the treated and control groups, by allowing the former to extend 10 pence and 50 pence above the level of the incoming wage floor, re-defining the control group accordingly as employees earning within 10% of that new threshold. This allows the increase in the wage floor to have spillover effects on the employees paid just above it, as employers limit the erosion of internal pay structures.[3] We also redefine the control group to include all workers above the NLW. We check the sensitivity to using sample weights addressing response biases and panel attrition in ASHE. We run a placebo test, looking for a treatment effect higher up the wage distribution than where we would expect any impact.[4] Finally, we will also consider results from an age-based definition of the treatment and control groups.

[3] Giupponi et al. (2024) provide evidence that the introduction of the NLW led to statistically significant spillover effects on wages up to £1.50 above the wage floor, although most effects seem to lie within 25 pence.
[4] Here, we set the boundary between the treated and control groups £4 above the wage floor. This is approximately where Giupponi et al.'s (2024) estimates of the distributional impact of the NLW reduce to zero.

3. Main Results

Figure 2a shows the raw trends in average annual wage growth for the treated and control groups. There is a clear increase among the treated group in 2015/16, coinciding with the hike in the wage floor. Figure 2b shows the trends in the average probabilities of year-to-year employee switching between firms. This switching is higher in the treated group across the whole observation period, reflecting a general negative correlation between labour mobility and wages. As in Figure 2a, the groups move approximately in parallel between 2013/14 and 2014/15. However, the difference between the groups falls from around 6 percentage points in 2014/15 to around 3 in 2015/16, and to around 2 in 2016/17.

FIGURE 2: Average annual wage growth and rate of cross-firm mobility for treated and control groups (unweighted and weighted), and the annual growth in the NMW/NLW, by period



Source: ASHE. "Diff" refers to the difference between the treated and control groups' average rates of switching.

The main results of estimating Equation (1) for rates of cross-firm mobility are presented in Table 1 and Figure 4, with equivalent wage growth estimates shown in Online Appendix Table A2 and Figure 3. We do not comment at length on the wage growth estimates in Table A2 but, for all specifications, we find that the wages of the treated group rose significantly and substantially relative to the control group in 2015/16.[5] The wage impact of the NLW notably attenuates when controlling for {firm×period} fixed effects (Table A2, column VI), i.e., when comparing among coworkers; this suggests spillover effects within firms, consistent with employers limiting the impact of the rising wage floor on internal pay structures. Our findings on wage effects are consistent with previous studies (e.g., Giupponi et al., 2024).

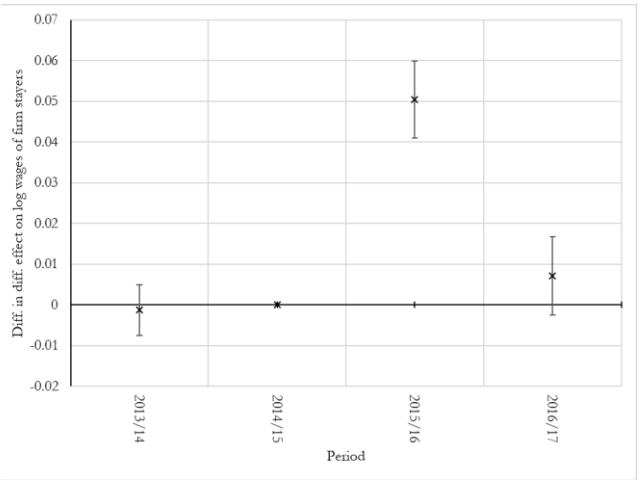
Column (I) of Table 1 presents the results for cross-firm mobility without control variables, matching the raw trends in Figure 2b. The reference period in the regressions is 2014/15 (prior to the introduction

[5] The wage growth estimates shown in Table A2 are conditioned on employees who remain in the same firm ('firm stayers') so as to focus on wage growth within continuing jobs.

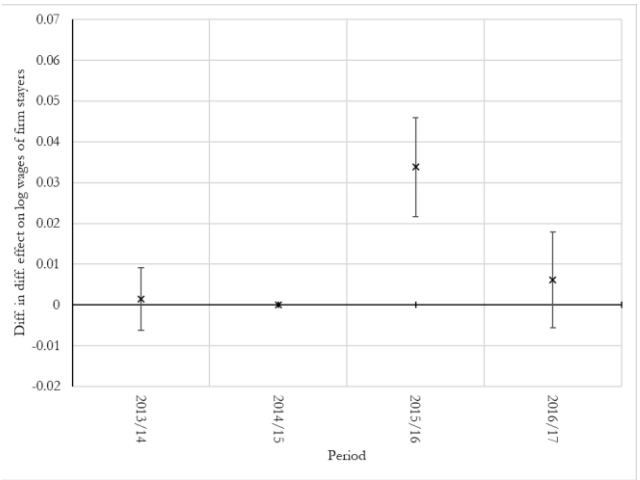
of the NLW). Thus, the coefficient in the second row of column (I), β, shows a 5.6 percentage-point difference in firm-to-firm mobility rates in 2014/15. The coefficient δ_1, in the first row of column (I), indicates that the difference between treated and control was slightly larger in 2013/14 than 2014/15, but is not statistically significant from zero, supporting the identifying assumption of parallel trends prior to treatment. The negative coefficients δ_3 and δ_4, in the third and fourth rows, respectively, show the narrowing of the gap between treated and control groups in 2015/16 and 2016/17, though only δ_4 is statistically significant at the 5 per cent level.

FIGURE 3: Differences-in-differences estimates of the effects of the National Living Wage on year-to-year log wage changes for firm stayers (columns IV & VI, Online Appendix Table A2)

a: column (IV), Table A2



b: column (VI), Table A2



Source: ASHE
Notes: The figure shows point estimates and 95% confidence intervals. For other notes, see appendix Table A2.

Column (II) of Table 1 adds gender, age, part-time, and job tenure controls to the DiD specification, and column (III) further adds period-by-period region-specific time trends. Column (IV) then allows for occupation-specific regional time trends at the two-digit level, and column (V) allows for the equivalent at the three-digit level. Column (IV) is our preferred specification, showing that the difference in cross-firm mobility rates between the treated and control groups was around 1.1 percentage points smaller in 2015/16 than in 2014/15, but not statistically significant from zero. The difference was around 1.5 percentage points smaller in 2016/17 but, again, not statistically significant. These effects are even smaller in column (V) with finer occupational controls.

In column (VI), we use the firm identifiers in ASHE to focus on differential rates of mobility among workers within the same firms.[6] This suggests whether unobserved firm heterogeneity may be biasing the

[6] Such firm identifiers are not available in the UK Labour Force Survey or UK Household Longitudinal Study – the other two datasets typically used to evaluate the impacts of labour market policy changes in the UK.

coefficients discussed above. The sample size is reduced because we require at least two employees in each {firm×period} cell; the analysis is biased towards larger firms as a result. Thus, column (VII) of Table 1 directly replicates column (IV) on the reduced sample and, here, we see a larger, but still statistically non-significant, treatment effect. The coefficient attenuates in column (VI) with the introduction of the {firm×period} fixed effects. Figure 3 demonstrates the key results, from columns (IV) and (VI), plotting the estimated δ_t coefficients and their confidence intervals. There is no significant evidence that the introduction of the National Living Wage affected rates of labour mobility across firms.

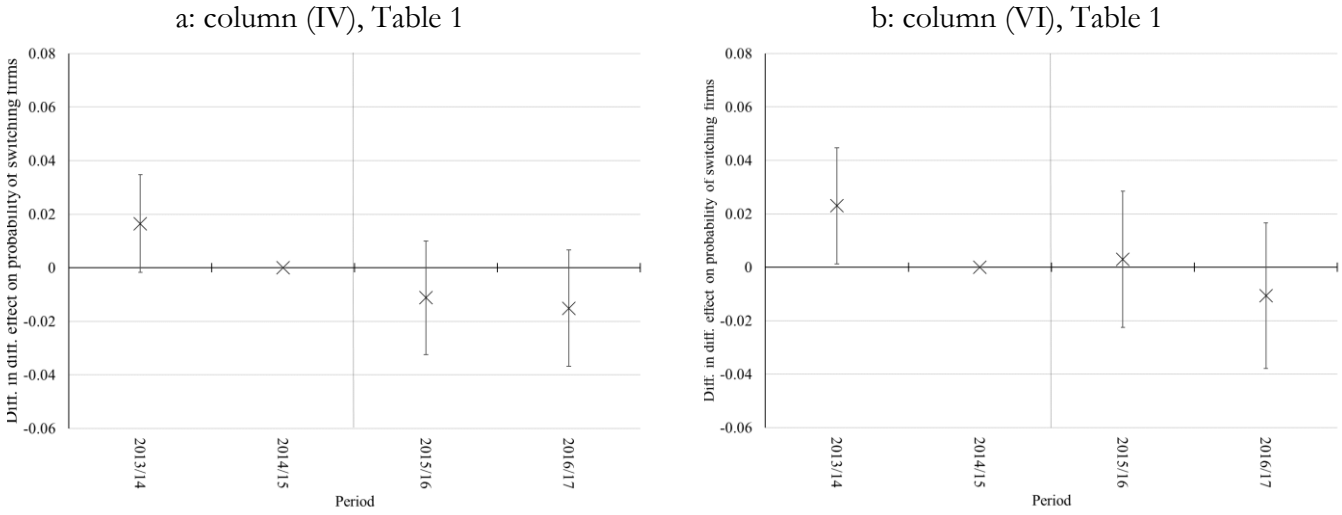
TABLE 1: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, t=2013/14}; δ_1	0.0167	0.0138	0.0148	0.0165	0.0227	0.0230	0.0266
	(0.0112)	(0.0098)	(0.0097)	(0.0093)	(0.0089)	(0.0111)	(0.0135)
	[0.2900]	[0.3223]	[0.2802]	[0.1977]	[0.0625]	[0.1665]	[0.1528]
Treated, {Control period, t=2014/15}; β	0.0560	0.0341	0.0338	0.0289	0.0195	-0.0042	0.0399
	(0.0158)	(0.0139)	(0.0137)	(0.0128)	(0.0122)	(0.0166)	(0.0186)
	[0.0000]	[0.0005]	[0.0005]	[0.0019]	[0.0295]	[0.7082]	[0.0032]
NLW period - DiD effects:							
Treated ×{Policy period, t=2015/16}; δ_3	-0.0234	-0.0186	-0.0173	-0.0112	-0.0022	0.0030	-0.0239
	(0.0128)	(0.0113)	(0.0112)	(0.0108)	(0.0105)	(0.0130)	(0.0153)
	[0.0677]	[0.1002]	[0.1247]	[0.3012]	[0.8333]	[0.8171]	[0.1188]
Treated ×{Policy period, t=2016/17}; δ_4	-0.0329	-0.0229	-0.0214	-0.0151	-0.0051	-0.0106	-0.0248
	(0.0137)	(0.0118)	(0.0117)	(0.0111)	(0.0108)	(0.0139)	(0.0160)
	[0.0161]	[0.0527]	[0.0682]	[0.1729]	[0.6339]	[0.4453]	[0.1201]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0051	0.0548	0.0569	0.0782	0.0989	0.3791	0.0936
N	58,032	58,032	58,032	58,032	57,587	34,706	34,591

Source: ASHE

Notes: γ_t omitted for brevity. Standard errors in parentheses are robust to person and firm-year clusters. Square brackets show p-values for significance from zero, two-sided tests. Sample size in (VI) is smaller as singletons are dropped. Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI). Numbers of observations by {Treated×Period} are shown in Online Appendix Table A1 for columns (IV) and (VI).

FIGURE 4: Differences-in-differences estimates of the effects of the National Living Wage on the probability of year-to-year firm switching (columns IV & VI, Table 1)



Source: ASHE
Notes: The figure shows point estimates and 95% confidence intervals. For other notes, see Table 1.

4. Robustness Checks

ASHE suffers from panel attrition when an individual continues to hold employee status but their employer ceases to respond to the survey, as well as when eligible individuals cease to hold the status of employees (e.g., due to retirement) (see Forth et al, 2023). The possibility that control and treatment groups could differ in their probability of year-to-year exit from ASHE is a threat to identification. To check this, we estimate Equation (1) changing the dependent variable to a dummy variable equal to one if a person exited the ASHE panel (i.e., for the period 2015/16 ($t = 3$), the dummy variable is equal to one if a person is observed in 2015 but not 2016. It is equal to zero if a person is observed in both years). The results are shown in Online Appendix Table A3 for the equivalent model specifications as in Table 1. There is evidence that the treatment group was significantly more likely to exit from ASHE between years in the 2015/16 policy period, by as much as 2.9 percentage points compared with the control group in our preferred specification, controlling for occupation-specific regional time trends at the two-digit level (column IV). However, we cannot disentangle using ASHE whether this is due to genuine sample attrition (employer non-response) within employment or due to a potential treatment effect on a person remaining in employment. It is reassuring though that our main results are robust to using the longitudinal sample weights (see Online Appendix Tables A4 & A5, and Figure A1), which were specifically designed to address the non-random attrition of employees from ASHE. In addition, we can use the ASHE panel to impute missing information about whether employees made year-to-year switches. For example, if we observe an employee in 2015 and 2017 but not 2016, but their 2017 record tells us that they are working at the same employer as in 2015, then we impute a value of zero for the firm-switch dummy. But if we observe an employee in 2015 and 2017 but not 2016, and their 2017 record tells us they are working at a different employer with tenure between 1 and 2 years, then we impute a value of one for the firm-switch

dummy. Using this approximately 10% larger estimation sample, the treatment effects are generally larger and more negative, but in our preferred specifications (columns IV & VI; controlling for occupation-specific regional time trends and firm-year fixed effects), they remain statistically insignificant (Online Appendix Table A6).

We also consider robustness to the three changes in the definition of treated and control groups described in Section 2 (Online Appendix Tables A7-A9.) In the first of these sensitivity checks, which extends the treated group by 10p (Table A7), and the third, which uses all workers above the NLW as the control (Table A9), the main specifications (column IV) reveal statistically significant negative treatment effects of around 3 percentage points, which strengthen to between 4 and 5 percentage points on moving to the reduced sample of larger firms (column VII). But compared to our main results in Table 1, these sensitivity checks show substantially larger differences between the groups in the average conditional likelihood of job-switching in the base period (β), suggesting that the groups are much less alike prior to the policy change. Aligning with this notion, we find that the treatment effects in these checks fall to 1 percentage point and are non-significant with the introduction of firm fixed effects (column VI and subsequent attenuation of β). The second check in Table A8, which extends the treated group by 50p and well above the incoming wage floor, aligns closely with Table 1, showing no statistically significant treatment effect in any specification.

Online Appendix Table A10 shows the results of our placebo test. The treatment coefficients for 2015/16 in columns (IV) and (VI) are very small (less than one percentage point) and statistically non-significant. The same applies to the treatment coefficient for 2016/17 in column (VI). We obtain a statistically significant negative treatment coefficient of around -2 percentage points for 2016/17 in column (IV). Such placebo tests are mostly valuable in cases where the main specifications show statistically significant treatment effects; this is not the case in our analysis. Nevertheless, it is reassuring that most of the treatment effects observed in this placebo test are close to zero.

We also test a revised or more particular definition of mobility, where we focus on instances when an employee changes firm *and* occupation, defined using the full four digits of the SOC. This measure of mobility is necessarily more restrictive than elsewhere in the paper and focuses on those forms of job change that are the most likely to be associated with significant wage hikes (e.g., Frederiksen et al., 2016). The results are presented in Online Appendix Table A11 and Figure A2. They are qualitatively like those in Table 1 and Figure 3, showing no statistically significant treatment effects on cross-firm mobility of the NLW.

Although we find no average treatment effects, it is plausible that some low-wage occupations are more sensitive to the rising wage floor than others.[7] In our main estimation sample (Table 1, columns I-IV), there are three occupation sub-major groups (SOC 2-digit) with over 5,000 employee observations. These are "Caring personal services" (SOC61, N=8,387), "Sales" (SOC71, N=12,542), and "Elementary administration and services" (SOC92, N=18,324). Using each of these sub-samples of jobs, we re-estimate Equation (1) controlling for region-specific trends (specification III in Table 1). For the 2015/16 policy period, Table 2 shows no significant effects on cross-firm mobility for employees holding jobs in any of these three occupation groups. For the 2016/17 policy period, SOC61 and SOC92 show no significant effects. But there is evidence of a negative effect for SOC71 at 3.9 percentage points, which is significant at the 5% level. Our main results showed no evidence of an average treatment effect from the NLW on year-to-year firm switching, robust to occupation-region-specific time trends. But within one of the most common low-paid occupations, where presumably there are continuously a good number of vacancies in local labour markets, there is some evidence of effects of the policy on firm-switching.

[7] For example, Machin et al. (2003) showed that large numbers of workers were affected by the NMW in the UK residential care homes industry, with effects on hours and employment but not home closure. Giupponi et al., (2016) found a similarly large bite of the NLW on this sector. Aitken et al (2019) report evidence of a negative effect of the NLW on job retention in retail, but their results may be biased by the conflation of employment exit and panel attrition in ASHE.

TABLE 2: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching: Selected Sub-major occupation groups

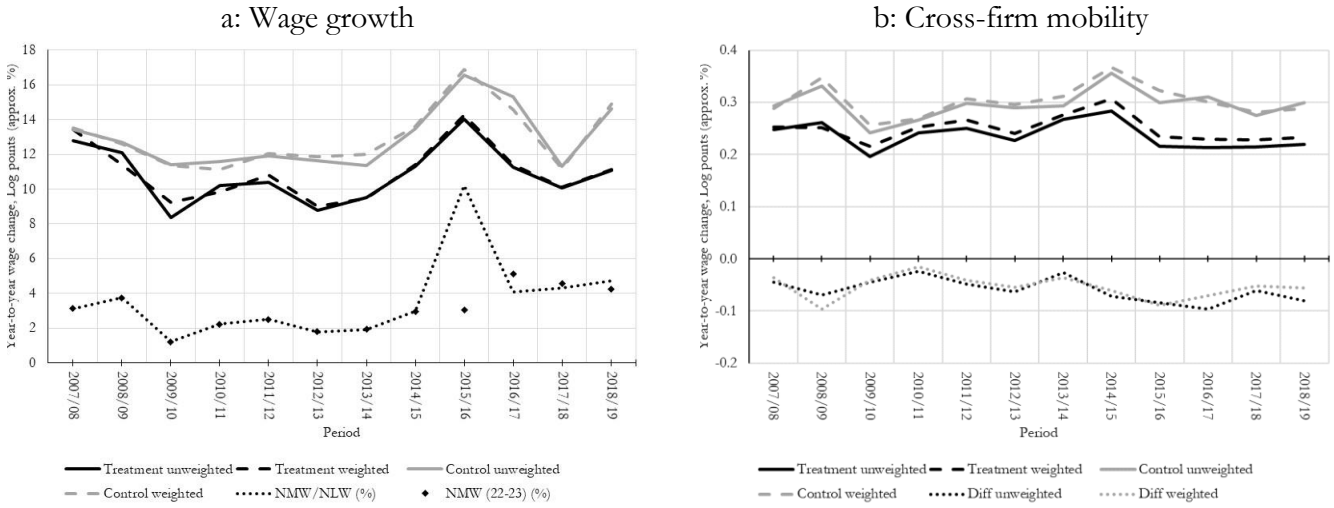
	SOC61	SOC71	SOC92
Treated × {Control period, t=2013/14}; δ_1	-0.0022	0.0107	0.0490
	(0.0190)	(0.0126)	(0.0122)
	[0.9425]	[0.5516]	[0.0057]
Treated, {Control period, t=2014/15}; β	0.0305	0.0326	0.0211
	(0.0300)	(0.0180)	(0.0177)
	[0.1086]	[0.0099]	[0.0839]
NLW period - DiD effects:			
Treated × {Policy period, t=2015/16}; δ_3	0.0045	0.0015	-0.0112
	(0.0228)	(0.0156)	(0.0153)
	[0.8421]	[0.9258]	[0.4654]
Treated × {Policy period, t=2016/17}; δ_4	-0.0025	-0.0385	0.0033
	(0.0241)	(0.0156)	(0.0160)
	[0.9170]	[0.0139]	[0.8386]
Controls	Yes	Yes	Yes
Period FEs	No	No	No
Region × Period FEs	Yes	Yes	Yes
Region × Period × Occ 2-digit FEs	No	No	No
Region × Period × Occ 3-digit FEs	No	No	No
Firm × Period FEs	No	No	No
R^2	0.0437	0.0544	0.0674
N	8,387	12,542	18,324

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters. Square brackets show p-values for significance from zero, two-sided tests.

Finally, we consider an alternative identification strategy, using the age-based nature of the NLW, which only applied to workers aged 25 or older. We retain the same definition for the treatment group as before but only consisting of workers aged 25-30. As our control group, we consider employees aged 22-23 with earnings in the first year of each period, t, that are either at the incoming wage floor or up to 10% above it. In this case, the treatment is demonstrated by Figure 5 below, which plots the wage floor applying to each age group as well as the observed average year-to-year wage changes and rates of cross-firm mobility for these new treatment and control groups. Identification comes from the greater uprating of the wage floor for those aged 25 or older due to the NLW, whereas beforehand the same wage floor applied to all employees aged 21 or older. However, the prospects of observing a treatment effect of the policy via this route appear limited because the sample averages in Figure 5a show that, although wage growth for the younger control group is generally greater than for the older treatment group, both saw a notable spike with the NLW, perhaps because employers passed on the wage rise to these younger workers to maintain consistency in their internal pay structures. As Figure 5b shows, the younger control group tend to move between firms more often than the older treatment group, but there is no obvious change in this difference when the NLW was introduced.

FIGURE 5: Average annual wage growth and rate of cross-firm mobility for treated and control groups (unweighted and weighted), and the annual growth in the NMW/NLW, by period: age-based strategy



Source: ASHE. "Diff" refers to the difference between the treated and control groups' average rates of switching.

Using these new age-based treatment and control groups, we estimate the equivalent specifications of Equation (1) as for our preferred empirical strategy above, except for those with 3-digit occupational controls and firm-year fixed effects, due to much smaller sample sizes. The results in Online Appendix Tables A12 & A13 show no evidence of a treatment effect from the NLW, either on year-to-year firm switching or wage growth. As mentioned earlier, the latter result should not be overly surprising given the likelihood that the NLW treated workers just under the age of 25 through firm-specific pay structures, or just because employers decided to pay their employees equally or fairly regardless of their age, consistent with theories such as the efficiency gains of fair wages or equal treatment wage contracts (Akerlof, 1982; Snell & Thomas, 2010). It is also consistent with specific evidence from the residential care homes sector of significant spillovers of the NLW policy to workers aged under 25 at both the market and firm level (Giupponi & Machin, 2022).

5. Conclusion

Previous studies found that the introduction of the National Living Wage in 2016 raised the earnings of low-paid employees in the UK, with little evidence of negative employment effects (e.g., Aitken et al., 2019; Giupponi et al., 2024). No attention in those studies was given to the impact on cross-firm mobility among those who remained employees. Theoretical labour market search models provide no clear indication for the direction of any such effects; job displacement and positive re-allocation effects could be offset by reduced on-the-job search and a compressed wage-offer distribution. Studies evaluating increased minimum wages in other countries have fallen on either side, with evidence of increased beneficial cross-firm mobility in Germany (Dustmann et al., 2022) but increased firm-specific tenure in the United States (Dube et al., 2007, 2016; Jardim et al., 2018).

In this study, we used linked employer-employee data and a difference-in-differences estimator to provide the first UK evaluation of the impact of a rising wage floor on the propensity for minimum-wage employees to switch firms. We find no evidence that the substantial equalisation of wages caused by the NLW affected the average rate of cross-firm mobility among the low-paid. This suggests generally benign effects of minimum wage policy on aggregate employment dynamics in the UK. However, there is some evidence that workers in sales occupations reduced their rate of switching between employers one year after the NLW wage rise. This is a generally female-dominated sector, with a high proportion of part-time work, and there are suggestions in some previous studies that part-time female employment and hours have been relatively more sensitive to the UK wage floor (see Aitken et al, 2019).

Further research could explore whether the two theoretical mechanisms described above on average cancelled each other out, or whether neither were affected. Cancelling out would imply that recent UK minimum wage policy, despite its apparently benign effects on aggregate employment and job mobility, is still affecting the efficiency of some parts of the labour market, with knock-on implications for productivity growth.

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The Impact of a Rising Wage Floor on Labour Mobility across Firms

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Online Appendix

A. Additional Tables & Figures

TABLE A1: Numbers of observations by period and group in the main estimation samples (i.e., Table 1 columns I-IV & columns VI-VII)

	Main			Within firm-periods		
	Control	Treated	Total	Control	Treated	Total
2013/14	7,027	3,793	10,820	4,563	2,059	6,622
2014/15	7,624	4,655	12,279	4,655	2,565	7,220
2015/16	7,811	10,353	18,164	4,778	6,208	10,986
2016/17	9,185	7,584	16,769	5,500	4,378	9,878
Total	31,647	26,385	58,032	19,496	15,210	34,706

TABLE A2: Estimated effects of the National Living Wage relative to other NMW rises on log wage changes for firm stayers - UNWEIGHTED

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, 2013/14}; δ_1	0.0002	0.0000	0.0001	-0.0013	-0.0020	0.0014	0.0057
	(0.0034)	(0.0034)	(0.0033)	(0.0032)	(0.0031)	(0.0039)	(0.0047)
	[0.9678]	[0.9996]	[0.9902]	[0.8037]	[0.7113]	[0.8446]	[0.4725]
Treated, {Control period, t=2014/15}; β	0.0027	0.0024	0.0023	0.0049	0.0055	0.0190	0.0043
	(0.0055)	(0.0055)	(0.0054)	(0.0052)	(0.0053)	(0.0071)	(0.0079)
	[0.4308]	[0.4738]	[0.4835]	[0.1223]	[0.0790]	[0.0000]	[0.3582]
NLW period - DiD effects:							
Treated ×{Policy period, 2015/16}; δ_3	0.0485	0.0491	0.0501	0.0504	0.0502	0.0338	0.0551
	(0.0051)	(0.0050)	(0.0050)	(0.0048)	(0.0049)	(0.0062)	(0.0070)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Treated ×{Policy period, 2016/17}; δ_4	0.0056	0.0064	0.0067	0.0071	0.0066	0.0062	0.0103
	(0.0049)	(0.0048)	(0.0048)	(0.0049)	(0.0050)	(0.0060)	(0.0075)
	[0.2547]	[0.1852]	[0.1662]	[0.1473]	[0.1832]	[0.3076]	[0.1737]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0185	0.0235	0.0254	0.064	0.0889	0.2896	0.0586
N	50,151	50,151	50,151	50,123	49,670	29,843	29,721

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

Sample sizes in (V) and (VI) are smaller as singletons are dropped.

Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI).

TABLE A3: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year exit from ASHE.

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, 2013/14}; δ_1	0.0347	0.0307	0.0323	0.0291	0.0264	-0.0087	0.0275
	(0.0150)	(0.0144)	(0.0142)	(0.0115)	(0.0114)	(0.0132)	(0.0169)
	[0.0806]	[0.1087]	[0.0877]	[0.0811]	[0.1046]	[0.6362]	[0.2701]
Treated, {Control period, t=2014/15}; β	0.0450	0.0179	0.0160	0.0187	0.0189	0.0017	0.0079
	(0.0199)	(0.0192)	(0.0189)	(0.0167)	(0.0162)	(0.0185)	(0.0250)
	[0.0026]	[0.2145]	[0.2600]	[0.1053]	[0.0958]	[0.8950]	[0.6407]
NLW period - DiD effects:							
Treated ×{Policy period, 2015/16}; δ_3	0.0200	0.0269	0.0303	0.0294	0.0254	-0.0081	0.0221
	(0.0172)	(0.0165)	(0.0163)	(0.0141)	(0.0141)	(0.0160)	(0.0204)
	[0.2461]	[0.1038]	[0.0621]	[0.0376]	[0.0707]	[0.6098]	[0.2792]
Treated ×{Policy period, 2016/17}; δ_4	0.0057	0.0148	0.0179	0.0148	0.0139	-0.0061	0.0206
	(0.0175)	(0.0167)	(0.0163)	(0.0145)	(0.0145)	(0.0157)	(0.0214)
	[0.7456]	[0.3774]	[0.2716]	[0.3075]	[0.3360]	[0.6965]	[0.3350]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0044	0.0359	0.0404	0.0559	0.0734	0.2842	0.0604
N	89,716	89,716	89,716	89,716	89,254	54,120	54,008

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

Sample sizes in (V) and (VI) are smaller as singletons are dropped.

Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI).

TABLE A4: Estimated effects of the National Living Wage relative to other NMW rises on log wage changes for firm stayers - WEIGHTED

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, 2013/14}; δ_1	0.0020	0.0015	0.0017	0.0011	0.0010	0.0010	0.0064
	(0.0035)	(0.0035)	(0.0035)	(0.0031)	(0.0031)	(0.0040)	(0.0044)
	[0.7162]	[0.7787]	[0.7440]	[0.8318]	[0.8519]	[0.8960]	[0.3935]
Treated, {Control period, t=2014/15}; β	0.0001	0.0004	0.0003	0.0037	0.0044	0.0192	0.0049
	(0.0054)	(0.0053)	(0.0053)	(0.0050)	(0.0051)	(0.0073)	(0.0075)
	[0.9673]	[0.9028]	[0.9259]	[0.2354]	[0.1559]	[0.0000]	[0.2726]
NLW period - DiD effects:							
Treated ×{Policy period, 2015/16}; δ_3	0.0487	0.0494	0.0505	0.0504	0.0502	0.0327	0.0531
	(0.0049)	(0.0049)	(0.0049)	(0.0047)	(0.0048)	(0.0061)	(0.0065)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Treated ×{Policy period, 2016/17}; δ_4	0.0059	0.0066	0.0069	0.0068	0.0060	0.0064	0.0085
	(0.0049)	(0.0048)	(0.0048)	(0.0047)	(0.0048)	(0.0062)	(0.0070)
	[0.2250]	[0.1707]	[0.1510]	[0.1466]	[0.2194]	[0.3013]	[0.2248]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0169	0.0225	0.0249	0.078	0.108	0.2961	0.0646
N	50,139	50,139	50,139	50,111	49,659	29,833	29,711

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

Sample sizes in (V) and (VI) are smaller as singletons are dropped.

Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI).

TABLE A5: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching - WEIGHTED

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, 2013/14}; δ_1	0.0134	0.0110	0.0116	0.0148	0.0214	0.0194	0.0250
	(0.0110)	(0.0098)	(0.0098)	(0.0096)	(0.0095)	(0.0126)	(0.0141)
	[0.3817]	[0.4261]	[0.4020]	[0.2603]	[0.0984]	[0.2910]	[0.1910]
Treated, {Control period, t=2014/15}; β	0.0529	0.0294	0.0296	0.0241	0.0163	-0.0033	0.0384
	(0.0154)	(0.0139)	(0.0138)	(0.0131)	(0.0130)	(0.0184)	(0.0191)
	[0.0000]	[0.0029]	[0.0026]	[0.0122]	[0.0863]	[0.7910]	[0.0064]
NLW period - DiD effects:							
Treated ×{Policy period, 2015/16}; δ_3	-0.0163	-0.0131	-0.0125	-0.0063	0.0009	0.0021	-0.0226
	(0.0128)	(0.0116)	(0.0116)	(0.0114)	(0.0114)	(0.0148)	(0.0162)
	[0.2030]	[0.2618]	[0.2816]	[0.5801]	[0.9398]	[0.8896]	[0.1620]
Treated ×{Policy period, 2016/17}; δ_4	-0.0258	-0.0175	-0.0168	-0.0095	-0.0017	-0.0171	-0.0220
	(0.0135)	(0.0121)	(0.0121)	(0.0117)	(0.0117)	(0.0159)	(0.0168)
	[0.0555]	[0.1474]	[0.1637]	[0.4185]	[0.8858]	[0.2818]	[0.1909]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0047	0.0572	0.0588	0.0859	0.1093	0.3964	0.106
N	58,017	58,017	58,017	58,017	57,573	34,696	34,581

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

Sample sizes in (V) and (VI) are smaller as singletons are dropped.

Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI).

TABLE A6: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching – With IMPUTED firm-switching information

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, 2013/14}; δ_1	0.0155	0.0129	0.0140	0.0152	0.0211	0.0173	0.0273
	(0.0115)	(0.0100)	(0.0100)	(0.0095)	(0.0088)	(0.0108)	(0.0137)
	[0.3373]	[0.3616]	[0.3108]	[0.2359]	[0.0770]	[0.2825]	[0.1379]
Treated, {Control period, t=2014/15}; β	0.0584	0.0364	0.0362	0.0310	0.0214	0.0037	0.0461
	(0.0162)	(0.0141)	(0.0139)	(0.0129)	(0.0119)	(0.0161)	(0.0184)
	[0.0000]	[0.0003]	[0.0003]	[0.0011]	[0.0155]	[0.7306]	[0.0008]
NLW period - DiD effects:							
Treated ×{Policy period, 2015/16}; δ_3	-0.0263	-0.0205	-0.0193	-0.0128	-0.0042	-0.0038	-0.0294
	(0.0128)	(0.0113)	(0.0112)	(0.0108)	(0.0103)	(0.0124)	(0.0153)
	[0.0404]	[0.0697]	[0.0865]	[0.2391]	[0.6841]	[0.7579]	[0.0536]
Treated ×{Policy period, 2016/17}; δ_4	-0.0370	-0.0273	-0.0258	-0.0192	-0.0098	-0.0211	-0.0339
	(0.0141)	(0.0120)	(0.0119)	(0.0112)	(0.0105)	(0.0131)	(0.0161)
	[0.0086]	[0.0228]	[0.0307]	[0.0858]	[0.3505]	[0.1090]	[0.0349]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0051	0.0561	0.0581	0.0788	0.0994	0.3761	0.0934
N	64,151	64,151	64,151	64,151	63,723	38,957	38,836

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

Sample sizes in (V) and (VI) are smaller as singletons are dropped.

Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI).

TABLE A7: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching - Sensitivity Check, plus 10p

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, 2013/14}; δ_1	-0.0067	-0.0035	-0.0019	0.0004	0.0037	0.0123	0.0022
	(0.0099)	(0.0082)	(0.0081)	(0.0077)	(0.0072)	(0.0108)	(0.0109)
	[0.6346]	[0.7711]	[0.8751]	[0.9684]	[0.7105]	[0.4024]	[0.8867]
Treated, {Control period, t=2014/15}; β	0.0704	0.0468	0.0465	0.0432	0.0351	0.0039	0.0521
	(0.0141)	(0.0121)	(0.0118)	(0.0109)	(0.0101)	(0.0146)	(0.0152)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.7185]	[0.0000]
NLW period - DiD effects:							
Treated ×{Policy period, 2015/16}; δ_3	-0.0454	-0.0364	-0.0351	-0.0302	-0.0221	-0.0042	-0.0373
	(0.0114)	(0.0098)	(0.0096)	(0.0094)	(0.0090)	(0.0127)	(0.0130)
	[0.0001]	[0.0002]	[0.0003]	[0.0013]	[0.0142]	[0.7407]	[0.0040]
Treated ×{Policy period, 2016/17}; δ_4	-0.0445	-0.0330	-0.0317	-0.0268	-0.0175	-0.0153	-0.0313
	(0.0123)	(0.0103)	(0.0101)	(0.0097)	(0.0092)	(0.0133)	(0.0136)
	[0.0003]	[0.0014]	[0.0017]	[0.0054]	[0.0579]	[0.2502]	[0.0217]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0054	0.0544	0.0565	0.0774	0.0974	0.3784	0.0935
N	62,930	62,930	62,930	62,930	62,476	37,726	37,611

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

Sample sizes in (V) and (VI) are smaller as singletons are dropped.

Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI).

TABLE A8: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching - Sensitivity Check, plus 50p

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, 2013/14}; δ_1	0.0088	0.0089	0.0097	0.0128	0.0089	0.0041	0.0107
	(0.0060)	(0.0057)	(0.0056)	(0.0054)	(0.0055)	(0.0077)	(0.0072)
	[0.3767]	[0.3340]	[0.2920]	[0.1570]	[0.3195]	[0.6844]	[0.4140]
Treated, {Control period, t=2014/15}; β	0.0325	0.0180	0.0195	0.0173	0.0170	-0.0039	0.0214
	(0.0100)	(0.0092)	(0.0092)	(0.0090)	(0.0090)	(0.0100)	(0.0131)
	[0.0000]	[0.0016]	[0.0005]	[0.0012]	[0.0019]	[0.6138]	[0.0029]
NLW period - DiD effects:							
Treated ×{Policy period, 2015/16}; δ_3	-0.0126	-0.0119	-0.0131	-0.0095	-0.0068	-0.0021	-0.0132
	(0.0082)	(0.0077)	(0.0076)	(0.0074)	(0.0074)	(0.0100)	(0.0100)
	[0.1271]	[0.1205]	[0.0863]	[0.2004]	[0.3593]	[0.8307]	[0.1855]
Treated ×{Policy period, 2016/17}; δ_4	-0.0136	-0.0091	-0.0094	-0.0072	-0.0056	0.0055	-0.0027
	(0.0090)	(0.0081)	(0.0080)	(0.0077)	(0.0076)	(0.0102)	(0.0106)
	[0.1301]	[0.2656]	[0.2402]	[0.3526]	[0.4665]	[0.5873]	[0.8020]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0021	0.0515	0.0533	0.0716	0.091	0.3729	0.0832
N	80,750	80,750	80,750	80,750	80,277	49,612	49,506

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

Sample sizes in (V) and (VI) are smaller as singletons are dropped.

Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI).

TABLE A9: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching - Sensitivity Check, all above NLW as control

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, 2013/14}; δ_1	0.0099	0.0094	0.0092	0.0120	0.0124	0.0148	0.0177
	(0.0075)	(0.0070)	(0.0070)	(0.0074)	(0.0067)	(0.0078)	(0.0100)
	[0.4175]	[0.4182]	[0.4276]	[0.3119]	[0.2465]	[0.2226]	[0.2993]
Treated, {Control period, t=2014/15}; β	0.1028	0.0562	0.0576	0.0550	0.0457	0.0131	0.0687
	(0.0122)	(0.0116)	(0.0116)	(0.0119)	(0.0107)	(0.0122)	(0.0170)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0937]	[0.0000]
NLW period - DiD effects:							
Treated ×{Policy period, 2015/16}; δ_3	-0.0423	-0.0330	-0.0334	-0.0302	-0.0229	-0.0139	-0.0468
	(0.0097)	(0.0089)	(0.0089)	(0.0093)	(0.0082)	(0.0096)	(0.0124)
	[0.0000]	[0.0002]	[0.0002]	[0.0012]	[0.0053]	[0.1464]	[0.0002]
Treated ×{Policy period, 2016/17}; δ_4	-0.0412	-0.0280	-0.0282	-0.0274	-0.0202	-0.0143	-0.0339
	(0.0113)	(0.0097)	(0.0097)	(0.0098)	(0.0086)	(0.0100)	(0.0134)
	[0.0003]	[0.0038]	[0.0036]	[0.0053]	[0.0195]	[0.1543]	[0.0114]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0049	0.0461	0.0468	0.0523	0.0619	0.3343	0.0511
N	419,604	419,604	419,604	419,604	419,510	320,801	320,798

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

Sample sizes in (V) and (VI) are smaller as singletons are dropped.

Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI).

TABLE A10: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching - Placebo Check

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, 2013/14}; δ_1	-0.0111	-0.0122	-0.0130	-0.0128	-0.0166	0.0089	-0.0007
	(0.0101)	(0.0087)	(0.0086)	(0.0087)	(0.0092)	(0.0105)	(0.0099)
	[0.4366]	[0.3408]	[0.3069]	[0.3164]	[0.2130]	[0.5755]	[0.9604]
Treated, {Control period, t=2014/15}; β	0.0140	0.0152	0.0158	0.0158	0.0180	0.0082	0.0082
	(0.0143)	(0.0128)	(0.0127)	(0.0128)	(0.0133)	(0.0159)	(0.0151)
	[0.1684]	[0.0818]	[0.0675]	[0.0679]	[0.0496]	[0.4351]	[0.4047]
NLW period - DiD effects:							
Treated ×{Policy period, 2015/16}; δ_3	-0.0092	-0.0128	-0.0124	-0.0093	-0.0106	-0.0074	-0.0016
	(0.0113)	(0.0100)	(0.0099)	(0.0100)	(0.0105)	(0.0118)	(0.0114)
	[0.4150]	[0.2002]	[0.2111]	[0.3515]	[0.3141]	[0.5272]	[0.8876]
Treated ×{Policy period, 2016/17}; δ_4	-0.0160	-0.0204	-0.0217	-0.0227	-0.0280	-0.0129	-0.0122
	(0.0123)	(0.0110)	(0.0110)	(0.0110)	(0.0116)	(0.0131)	(0.0127)
	[0.1913]	[0.0646]	[0.0479]	[0.0394]	[0.0161]	[0.3253]	[0.3367]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0003	0.0353	0.0379	0.0698	0.1245	0.3912	0.0941
N	39,019	39,019	39,019	39,019	38,504	22,780	22,727

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

Sample sizes in (V) and (VI) are smaller as singletons are dropped.

Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI).

TABLE A11: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm & occupation switching

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Treated ×{Control period, 2013/14}; δ_1	0.0174	0.0156	0.0162	0.0179	0.0189	0.0239	0.0234
	(0.0075)	(0.0067)	(0.0066)	(0.0063)	(0.0064)	(0.0100)	(0.0084)
	[0.1031]	[0.1032]	[0.0882]	[0.0505]	[0.0394]	[0.1048]	[0.0576]
Treated, {Control period, t=2014/15}; β	0.0245	0.0085	0.0086	0.0064	0.0050	-0.0080	0.0108
	(0.0107)	(0.0096)	(0.0095)	(0.0091)	(0.0092)	(0.0147)	(0.0123)
	[0.0011]	[0.2034]	[0.1920]	[0.3145]	[0.4375]	[0.4223]	[0.1988]
NLW period - DiD effects:							
Treated ×{Policy period, 2015/16}; δ_3	-0.0003	0.0031	0.0041	0.0079	0.0080	0.0056	-0.0035
	(0.0088)	(0.0080)	(0.0079)	(0.0078)	(0.0079)	(0.0117)	(0.0102)
	[0.9747]	[0.6956]	[0.6088]	[0.3101]	[0.3059]	[0.6332]	[0.7310]
Treated ×{Policy period, 2016/17}; δ_4	-0.0099	-0.0024	-0.0014	0.0026	0.0042	-0.0015	0.0006
	(0.0092)	(0.0081)	(0.0081)	(0.0078)	(0.0079)	(0.0122)	(0.0103)
	[0.2821]	[0.7707]	[0.8599]	[0.7419]	[0.5947]	[0.9005]	[0.9513]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No
Region × Period FEs	No	No	Yes	No	No	No	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No
R^2	0.0025	0.0463	0.048	0.067	0.0877	0.2697	0.0806
N	58,040	58,040	58,040	58,040	57,595	34,709	34,594

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

Sample sizes in (V) and (VI) are smaller as singletons are dropped.

Sample size in (VII) drops again because it estimates model (IV) starting with the sample in (VI).

TABLE A12: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching: AGE-Based model

	(I)	(II)	(III)	(IV)
Treated ×{Control period, 2013/14}; δ_1	0.0405	0.0345	0.0347	0.0369
	(0.0238)	(0.0251)	(0.0251)	(0.0250)
	[0.2535]	[0.3189]	[0.3137]	[0.2902]
Treated, {Control period, t=2014/15}; β	-0.0708	-0.0493	-0.0522	-0.0607
	(0.0354)	(0.0346)	(0.0344)	(0.0349)
	[0.0030]	[0.0493]	[0.0374]	[0.0153]
NLW period - DiD effects:				
Treated ×{Policy period, 2015/16}; δ_3	-0.0121	0.0142	0.0157	0.0329
	(0.0320)	(0.0308)	(0.0310)	(0.0318)
	[0.7052]	[0.6449]	[0.6132]	[0.3006]
Treated ×{Policy period, 2016/17}; δ_4	-0.0251	-0.0144	-0.0093	0.0008
	(0.0350)	(0.0335)	(0.0334)	(0.0339)
	[0.4729]	[0.6669]	[0.7812]	[0.9814]
Controls	No	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No
Region × Period FEs	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No
Firm × Period FEs	No	No	No	No
R^2	0.0104	0.0597	0.0673	0.1218
N	9,030	9,030	9,030	9,030

Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

Square brackets show p-values for significance from zero, two-sided tests.

TABLE A13: Estimated effects of the National Living Wage relative to other NMW rises on log wage changes for firm stayers: AGE-Based model

	(I)	(II)	(III)	(IV)
Treated ×{Control period, 2013/14}; δ_1	-0.0066	-0.0083	-0.0088	-0.0116
	(0.0091)	(0.0104)	(0.0105)	(0.0108)
	[0.6232]	[0.5364]	[0.5173]	[0.4041]
Treated, {Control period, t=2014/15}; β	-0.0025	0.0033	0.0042	0.0050
	(0.0135)	(0.0135)	(0.0135)	(0.0139)
	[0.7791]	[0.7513]	[0.6929]	[0.6416]
NLW period - DiD effects:				
Treated ×{Policy period, 2015/16}; δ_3	-0.0056	-0.0048	-0.0040	-0.0037
	(0.0160)	(0.0161)	(0.0162)	(0.0167)
	[0.7265]	[0.7660]	[0.8051]	[0.8265]
Treated ×{Policy period, 2016/17}; δ_4	-0.0172	-0.0160	-0.0166	-0.0199
	(0.0163)	(0.0161)	(0.0163)	(0.0168)
	[0.2902]	[0.3215]	[0.3073]	[0.2358]
Controls	No	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No
Region × Period FEs	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No
Firm × Period FEs	No	No	No	No
R^2	0.0151	0.0212	0.0266	0.0898
N	6,533	6,533	6,533	6,533

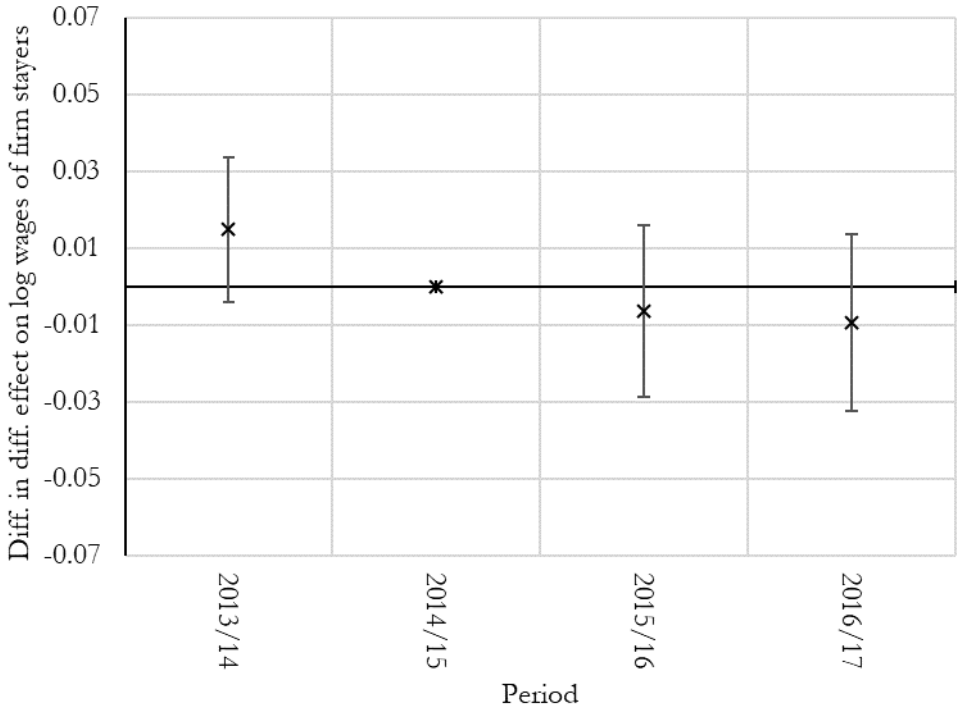
Source: ASHE

Notes: Standard errors in parentheses are robust to person and firm-year clusters.

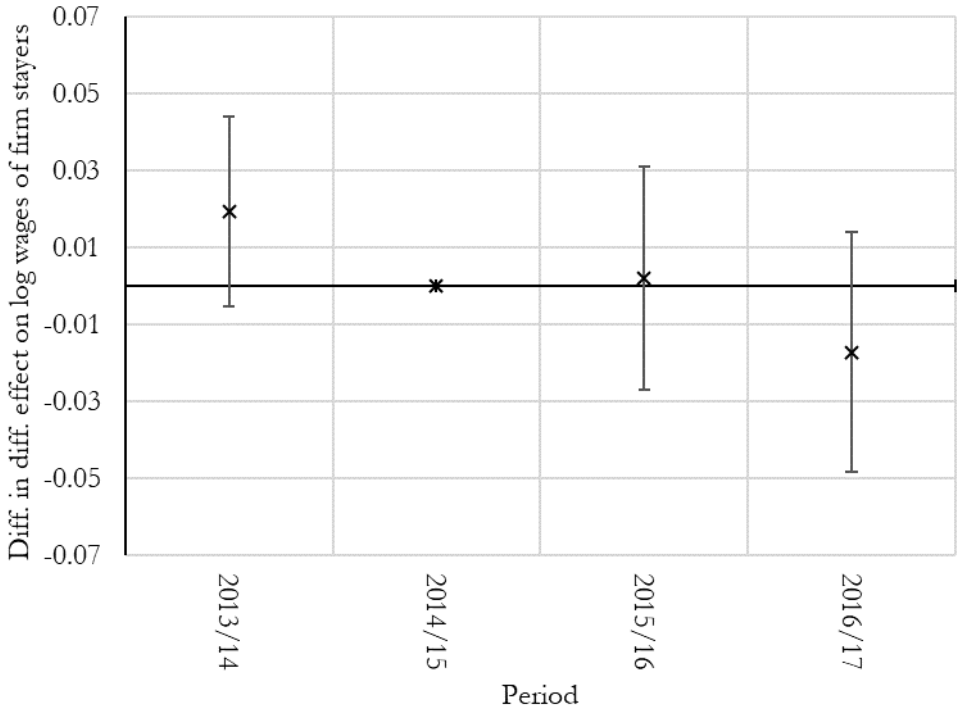
Square brackets show p-values for significance from zero, two-sided tests.

FIGURE A1: Differences-in-differences estimates of the effects of the National Living Wage on the probability of year-to-year firm switching – WEIGHTED (columns IV & VI, Table A5)

a: Column (IV), Table A5



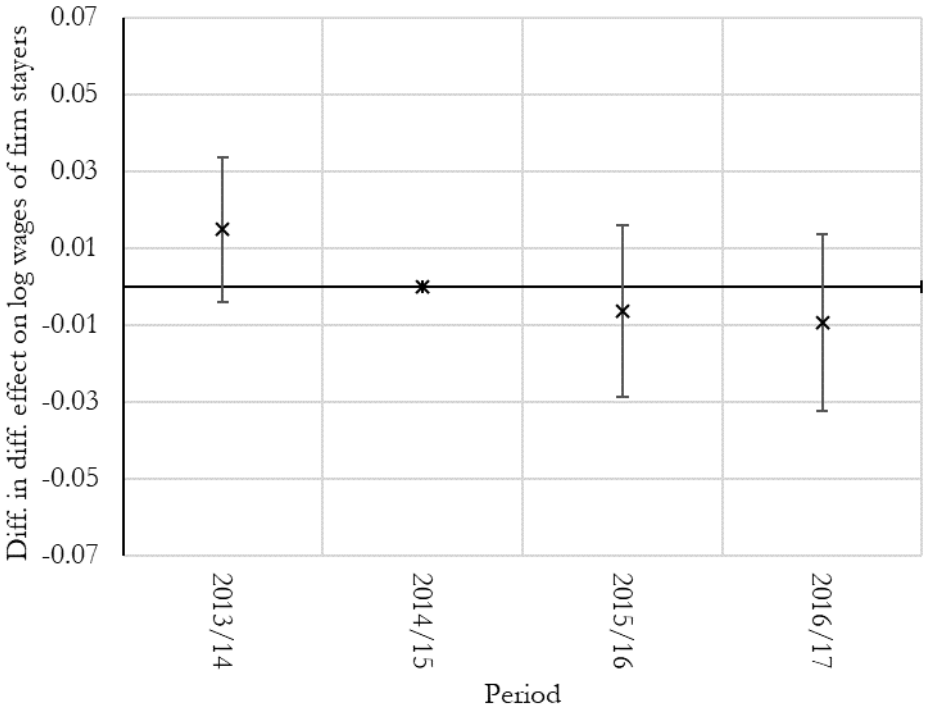
b: Column (VI), Table A5



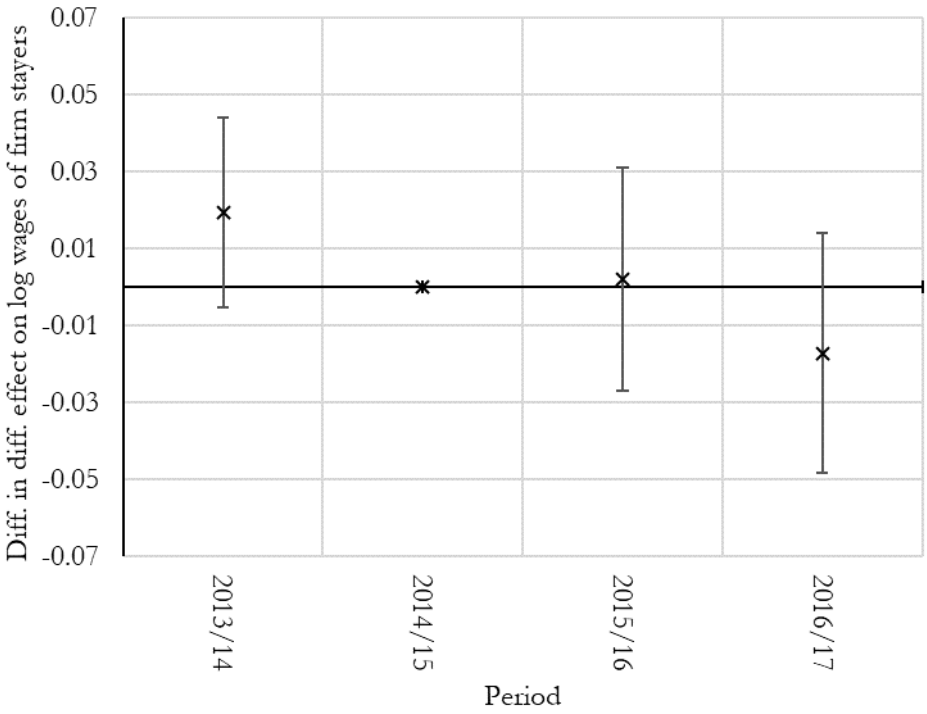
Source: ASHE
Notes: The figure shows point estimates and 95% confidence intervals. For other notes, see Table A5.

FIGURE A2: Differences-in-differences estimates of the effects of the National Living Wage on the probability of year-to-year firm & occupation switching (columns IV & VI, Table A11)

a: Column (IV), Table A11



b: Column (VI), Table A11



Source: ASHE
Notes: The figure shows point estimates and 95% confidence intervals. For other notes, see Table A11.