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

John Forth[1,6,7], Carl Singleton[2,7], Alex Bryson[3,6,7,8],
Van Phan[4], Felix Ritchie[4], Lucy Stokes[5] and Damian Whittard[4]

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Abstract

In April 2016, the National Living Wage (NLW) raised the statutory wage floor for employees in the UK aged 25 and above 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 per cent. Using a difference-in-differences approach with linked employer-employee data from the UK's Annual Survey of Hours and Earnings, we examine how the introduction and uprating of the NLW affected the likelihood of minimum-wage employees changing firms. We find some evidence that the NLW reduced the rate of job-to-job transitions among such workers, consistent with predictions that an increase in the wage floor discourages job search. However, we find no evidence that the NLW affected differences in job mobility between minimum wage workers and their co-workers in the same firm. Together, these findings suggest that the increased wage floor made quits less attractive to minimum-wage workers in firms with limited opportunities for progression.

Keywords: National Living Wage, On-the-job search, Low pay, UK labour market
JEL Codes: J23, J38, J68, J88

[1] Corresponding author. Email: john.forth@city.ac.uk. Faculty of Management, Bayes Business School, City St Georges, University of London, 106 Bunhill Row, London, EC1Y 8TZ, United Kingdom.
[2] University of Stirling, UK.
[3] University College London, UK.
[4] University of the West of England, UK.
[5] Competition and Markets Authority, UK.
[6] Research Associate, Economic Statistics Centre of Excellence (ESCoE), UK.
[7] Research Fellow, Institute of Labor Economics (IZA), Germany.
[8] Faculty Fellow, Rutgers University, USA.

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). Such reviews typically conclude that minimum wage policies have improved wages with little or no impact on employment. In the UK specifically, the majority of impact evaluations have found modest or no evidence of negative employment effects from the introduction of the National Minimum Wage (NMW) in 1999 and its subsequent uprating 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). The introduction of the National Living Wage (NLW) in 2016, which significantly uprated the wage floor for those aged 25 or more, similarly raised the earnings of low-paid employees, with significant spillovers up the wage distribution and little negative impact on employment, except possibly among women working part-time (e.g., Aitken et al., 2019; Giupponi et al., 2024).

Whilst the focus of most of the existing research on the employment effects of minimum wages has been on employment rates, it has been argued elsewhere (Dube et al., 2016) that minimum wages could have a much larger effect on employment transitions. These have been subject to less investigation. As such, a focus on transitions provides the opportunity to gain additional insights into the impact of minimum wages on the labour market and its dynamics. In this paper, we use linked employer-employee data and difference-in-differences (DiD) estimation to evaluate the impact of a large hike in the UK wage floor on labour mobility across firms.

An increase in the wage floor could have differing effects on labour mobility. On the one hand, a higher minimum wage may induce layoffs, since fewer job matches will be profitable from the firm's perspective. This is an adverse outcome for the worker, since layoffs typically lead to periods of unemployment (Simmons, 2024), even if reallocation effects may eventually entail low-paid employees moving to more efficient or profitable firms that are better placed to absorb the higher labour costs.[1]

On the other hand, a higher wage floor could reduce quits if it increases the wages of workers who would otherwise have felt underpaid and been searching for a new job. In a range of search and matching models of the labour market, 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

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[1] 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).

and, furthermore, if assuming random search, any offers they receive are less likely to improve on their current wage.[2] Where there is imperfect information on the non-pecuniary aspects of the new job – as in learning models which treat jobs as experience goods (Jovanovic, 1979) – the equalisation of wage offers thereby aids the worker in avoiding risky moves.[3]

Studies from the US show that higher minimum wages are associated with reductions in low-wage job separations and increases in low-wage job tenure in some settings (Dube et al., 2007, 2016; Jardim et al., 2018), consistent with reduced search. But there is limited evidence in this regard for the UK. Avram and Harkness (2025) find no significant evidence that the introduction of the NLW affected the probability of transitioning from employment to non-employment. However, they did not undertake any detailed investigation of transitions between jobs due to the limitations of their dataset.

We address this gap in the literature by investigating the impact of the introduction and uprating of the NLW on job-to-job mobility. In doing so, we answer a call for more focus on the impact of minimum wages on job-to-job transitions (Dube et al., 2016: 700). We find negative effects of the NLW on job-to-job mobility within a given local labour market, consistent with predictions that an increase in the wage floor will reduce job search, leading to a reduction in quits. Our results are sensitive to the inclusion of occupational controls, but the negative treatment effect is stronger and more-often statistically significant across specifications when we account for the possibility of spillovers just above the new wage floor. Nevertheless, we find no evidence in any specification that the NLW affected differences in job mobility between minimum wage workers and their co-workers in the same firm, consistent with a muted treatment effect on within-firm pay structures. In combination, our findings suggest that the introduction of the NLW may have aided workers by delivering wage growth without the need to engage in potentially risky moves to other jobs. However, they also suggest that, by primarily compressing the wage distribution across firms, the uprating of the wage floor made quits less attractive principally to minimum wage workers in firms with limited opportunities for internal wage progression.

2. Context

The UK government introduced the NMW in 1999, with an adult rate set at £3.60 per hour for all employees aged 22+, and a youth rate set at £3.00 per hour for those aged 18-21. The policy was one of the flagship elements of New Labour's labour market program and introduced the first

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[2] See Caldwell et al. (2024) for evidence that workers direct their search to firms where they believe they will earn higher pay. Melo et al. (2025) provide evidence that higher minimum wages reduce job search.
[3] See Van Huizen and Alessie (2019) for evidence on risk aversion and job mobility.

statutory, national wage floor in the UK.[4] After substantial initial upratings, the decade from 2004 saw the adult rate rise at an average 3 per cent per year, from £4.85 in 2005 to £6.70 in 2015 (Figure 1), by which time the age threshold had been reduced to 21+. Such modest increases arguably reflected concerns about the potential labour market impacts of the NMW, particularly around 2008-9 when the UK economy was in recession. Nevertheless, the latter part of this period saw the bite of the NMW increasing as median wages stagnated.

[FIGURE 1A HERE]
[FIGURE 1B HERE]

In July 2015, the government announced that the NLW would replace the NMW for workers aged 25+ the following April. The policy was introduced against the backdrop of an improving economy and as part of a broader "plan for working people" (HM Treasury and Osborne, 2015).[5] The NLW was set at £7.20 per hour in April 2016. This was 50 pence (7.5 per cent) higher than the NMW rate of October 2015 (£6.70), and 70 pence (10.2 per cent) higher than the rate of April 2015 (£6.50) (see Figure 1a). It was the largest annual increase in the UK wage floor since its introduction. It raised the real value of the minimum wage by 6.7 per cent for all employees aged 25+, at a time when real median wage growth was 2.0 per cent. The share of employee jobs among those aged 25+, paid at or below the minimum wage, rose from 5.1 per cent in April 2015 to 7.8 per cent in April 2016 (Figure 1b): the single largest increase in coverage since the introduction of the NMW. The government continued to raise the NLW thereafter, aiming for it to reach 60% of median hourly wages by 2020, but the annual increases were smaller after 2015 (30-38 pence per year). Coverage did not increase again until an increase of 49 pence in April 2020 coincided with a temporary reduction in many employees' earnings arising from the Coronavirus Job Retention Scheme (furlough).[6]

Evaluations of the impact of the NLW introduction – such as that by Giupponi et al. (2024) – have been consistent in finding a substantial treatment effect on wages, with some evidence of spillovers to employees earning just above the new wage floor. These wage increases appear to have been achieved without an adverse impact on overall employment. Indeed, Avram and

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[4] Wages Councils had set minimum wages for specific industries until their abolition 1993. The Agricultural Wages Board survived, but its minimum wage powers were superseded by the NMW.

[5] The shift from October to April brought the annual uprating into line with the financial year for most firms.

[6] The Low Pay Commission (LPC) has calculated the bite of the NMW in April 2015 at 52.5% of median hourly wages (LPC, 2022). Their figures indicate that the 60% target was achieved by April 2020. The bite shown in Figure 1a is calculated using the revised ASHE weights developed by Author et al. (2024). These lead to a higher bite throughout the series, rising from 56.1% in April 2015 to 61.0% in 2018 and 63.7% in 2020.

Harkness (2025) find no statistically significant effect of the introduction of the NLW on the probability of transitioning to non-employment. However, as noted earlier, they were unable to look at job-to-job transitions due to "data sparseness" (ibid, p.10). We utilise a large linked-employer dataset to examine this issue, providing new evidence on the impact of the rising wage floor.

3. Data and Methodology

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 per cent 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 is asked to report on the employee's gross earnings and working hours over a specific reference period in April, and responses are typically obtained for around two-thirds of the issued sample each year.[7] 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.[8]

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.[9] We focus our analysis on workers employed in consecutive years, using stacked two-year panels, and use the employer identifiers in ASHE to indicate whether an employee moved jobs between years; such firm identifiers are not available in the other employee datasets typically used to evaluate labour market policy changes in the UK (the UK Labour Force Survey, or the UK Household Longitudinal Study utilised by Avram & Harkness

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[7] The survey is mandatory, but the ONS have limited resources to pursue employers who do not respond after the standard two reminders. As data is supplied by employers, the information is limited to what can be supplied from payroll records: detailed information on wages and paid hours, employer pension contributions, occupation, industry and location; the only personal characteristics observed for the employee are age and gender.
[8] The revised ASHE weights that we use in the paper build on the standard cross-sectional weights derived by ONS. The revised weights first use control totals from the UK's official business register to address ASHE's under-representation of jobs in small, young, private-sector organisations. They then adjust for longitudinal attrition by calibrating patterns of sample exit in ASHE against the probability of an employee moving out of scope to ASHE, estimated from the Annual Population Survey. See Author (2024) for further details.
[9] Employees whose pay was affected by absence during the reference period are excluded.

(2025)), which must instead rely on employee self-reports. We also use the employer identifiers to examine the differential rates of mobility among co-workers within the same firm: a unique capability of ASHE. It is not possible in ASHE 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; Avram & Harkness, 2025).

To estimate the employment impact of a hike in the wage floor due to the introduction of the NLW, we deploy a 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 because their wages are 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. We can use variation in the treatment across worker ages, 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, 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.[10]

As noted earlier, until 2016, the wage floor was uprated annually in October, six months after the preceding ASHE and six months prior to the next, whilst 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

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[10] Another strategy for identification would be to use a grouping estimator, exploiting regional variation in the bite of the NLW. We do not adopt such an approach here, focusing instead on applying a wide range of robustness checks to our individual-worker-level DiD methodology. Studies using both approaches to look at other minimum wage effects (e.g. Dickens et al., 2009) typically find that results are consistent between the two. See also de Linde Leonard et al. (2014) on the limited impact of choice of estimator for employment effects.

accounts for anticipation effects from October 2015 and any immediate effects of the rising floor in April 2016. We include 2016/17, 2017/18 and 2018/19 as additional policy periods, since any effect on mobility may take longer to work through the labour market than for wages, through any ensuing compression of the wage distribution facing workers. We thus compare the rates of labour mobility in 2015/16 ($t = 5$), 2016/17 ($t = 6$), 2017/18 ($t = 7$) and 2018/19 ($t = 8$) (termed the policy periods) with those in 2011/12 ($t = 1$), 2012/13 ($t = 2$), 2013/14 ($t = 3$) and 2014/15 ($t = 4$) (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:

$$Y_i = \alpha_0 + \beta D_i + \sum_{t=\{1,2,3,5,6,7,8\}} \gamma_t Z_{ti} + \sum_{t=\{1,2,3,5,6,7,8\}} \delta_t (D_i \cdot Z_{ti}) + \boldsymbol{\vartheta X}_i + \varepsilon_i \qquad (1)$$

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 ($t = 4$) 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, all measured at the start of the period in question and which vary across specifications. Controls 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 set is extended with fixed effects for the {area×period}, {region×period×occupation}, and {firm×period} of a job. An area is the employee's Travel to Work Area (TTWA), based on their home address, and proxies for the local labour market; a 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).[11] TTWAs are generally preferred over regions for estimating the aggregate labour market level effects of minimum wages and other labour market interventions (see, for example, Giupponi et al., 2024). However, the occupational mix of an area may change over time in ways that are associated with the uprating of the NLW, and so it is valuable also to look within occupations within areas. Regions are used instead of TTWAs in this case, as any interaction between TTWA and occupation places too-heavy demands on the data given our sample size of workers. The estimation sample includes

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[11] Travel to Work Areas (TTWAs) are geographic areas designed to approximate self-contained labour markets, used by UK government bodies for policy and planning. They are defined by Office for National Statistics based on commuting patterns observed from the census, aiming to reflect where most people both live and work within a given region.

observations from 233 distinct TTWAs, 13 Government Office Regions, 25 two-digit occupations, and 88 three-digit occupations.

The parameters δ_t in Equation 1 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 periods, δ_1, δ_2 and δ_3, and whether the difference in Y_i between treated and control groups changes, compared to the base period difference, β, for four periods after the policy, $\delta_5, \delta_6, \delta_7$ and δ_8. 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 per cent above it.[12] In the period before the introduction of the NLW, the wage floor is the prevailing rate of the NMW. Our estimation sample offers a minimum of 3,000 observations in the treated group and 6,000 observations in the control group, across a minimum of 800 firms, in each year (see Table A1). The two groups are similar in terms of personal, job and employer characteristics (see Table A2). The main differences are that treated workers are more likely than those in the control group to work in smaller, private sector firms.

We check the sensitivity of our results to the definition of the treated and control groups, by allowing the former to extend 10 pence, 25 pence and 50 pence above the level of the incoming wage floor, re-defining the control group accordingly as employees earning within 10 per cent of that new threshold.[13] Redefining the treatment and control groups in these ways allows the increase in the wage floor to have spillover effects on the employees paid just above it, as employers potentially limit the erosion of internal pay structures. For instance, 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. Extending the treated and control groups in these ways also allows for any rounding of actual wage rates by employers to the nearest 10 pence, 50 pence, or £1 (Lam et al, 2006). We also present results where

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[12] Employees earning more than 10 per cent above the wage floor are excluded from the analysis.
[13] We also present results where we compare these redefined treatment groups with a consistent control group.

we redefine the control group to include all workers above the NLW. We check the sensitivity of results to using sample weights addressing response biases and panel attrition in ASHE. Further, we run a placebo test, looking for a treatment effect higher up the wage distribution than where we would expect any impact.[14] Finally, we also consider results from an age-based definition of the treatment and control groups.

4. Main Results

Figure 2a shows the raw trends in average annual wage growth for three mutually exclusive groups of employees, namely the treated and control groups, as well as for all other employees (i.e., those with higher hourly wages than the control group).[15] There is a clear increase among the treated group in 2015/16; this coincides with the hike in the wage floor, suggesting that the policy had a material impact on wage setting. It is notable that there is also a visible dip in nominal wage growth for the control group in 2015/16 compared with the preceding period. Reassuringly though, a similar, but slightly larger dip, is also seen among the other workers in ASHE.

Figure 2b shows the trends in the average probabilities of year-to-year employee switching between firms for the three groups of workers in ASHE. This switching is higher in the treated group than in the control group across the whole observation period, reflecting a general negative correlation between labour mobility and wages; the incidence of year-to-year job switching for workers in neither the treatment nor control group is approximately 7-9 per cent over the sample period. As in Figure 2a, the incidences of job-to-job switching for the treatment and control groups move approximately in parallel from 2007/8; there is a small increase in the difference between the groups in 2013/14, but this is reversed in 2014/15. This difference between the groups then falls from around 6 percentage points in 2014/15 to around 3 in 2015/16, and to around 2 in 2016/17.

[FIGURE 2A HERE]
[FIGURE 2B HERE]

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 A3 and Figure 3. We do not comment at length on the wage growth estimates in Figure 3 but, for

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[14] Here, we define the treated group as all employees earning below the incoming rate of the NLW plus £4.00 and the control group as all employees earning at or above that threshold. This point, £4.00 per hour above the NLW, is approximately where Giupponi et al.'s (2024) estimates of the distributional impact of the NLW reduce to zero.

[15] The wage growth estimates shown in Figure 2a, and later, are conditioned on employees who remain in the same firm ('firm stayers'), to focus on wage growth within continuing jobs. This provides a more robust indication of whether employers adjusted the wages of their workers in response to the policy than if one were also to include movers when computing these averages.

specification (III), which controls for employee characteristics and {area×period} fixed effects, and specification (IV) which replaces {area×period} with {region×period×occupation} fixed effects, we find that the wages of the treated group rose significantly and substantially relative to the control group in 2015/16. Our findings on these wage effects are consistent with previous studies (e.g., Giupponi et al., 2024). The estimated wage impact of the NLW on wages notably attenuates when controlling for {firm×period} fixed effects (specification VI, Figure 3c), i.e. when comparing among coworkers; this suggests some spillover effects within firms, consistent with employers limiting the impact of the rising wage floor on their internal pay structures.

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 of the NLW). Thus, the coefficient in the fourth row of column (I), β, shows a 5.6 percentage-point difference in firm-to-firm mobility rates between the treated and control groups in 2014/15. The DiD coefficients δ_1-δ_3, in the first, second and third rows of column (I), indicate that the differences between treated and control groups were slightly smaller in 2012/13, and slightly larger in 2011/12 and 2013/14, than in 2014/15, but none of these differences are statistically significant from zero, supporting the identifying assumption of parallel trends prior to treatment by the NLW. The negative coefficients δ_5-δ_8, in the fifth to eighth rows, respectively, show the significant narrowing of the gap between treated and control groups from 2015/16 onwards, relative to that seen in 2014/15.

[TABLE 1 HERE]

Column (II) of Table 1 adds gender, age, part-time, and job tenure controls to the DiD specification, and column (III) further controls for period-by-period TTWA-specific effects. Column (IV) then allows for occupation-specific regional effects at the two-digit level, and column (V) allows for the equivalent at the three-digit level. The DiD coefficients attenuate progressively with the addition of more detailed controls. Column (III) shows a negative average treatment effect within local labour market areas after controlling for differences in gender, age, hours, and tenure. The difference in cross-firm mobility rates between the treated and control groups is around 1.9 percentage points smaller in 2015/16 than in 2014/15, although the estimate does not reach our preferred 5 per cent level of statistical significance (p=0.098). This difference is greater and statistically significant in 2016/17 (δ_5=-0.023; p=0.046) and 2017/18 (δ_6=-0.028; p=0.019). These effects are substantial and economically significant when viewed against the baseline job switching rates shown in Figure 2b. However, these effects are also much reduced when we instead control for occupation-specific regional trends in columns (IV) and (V). With two-digit

occupations (column IV), the coefficients reduce in size by around one-third and are no longer statistically significant in any of the NLW periods. The coefficients reduce almost to zero in column (V) with even 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. This indicates whether unobserved firm heterogeneity may be biasing the 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, columns (VII) and (VIII) of Table 1 directly replicate columns (III) and (IV) on the reduced sample. These replications yield larger treatment effects but, notably, the coefficients on δ_5-δ_8 all attenuate and become non-significant in column (VI) with the introduction of the {firm×period} fixed effects.

Figure 4 demonstrates the selected key results, from columns (III), (IV) and (VI), plotting the estimated δ_t coefficients and their confidence intervals. The evidence thus far is somewhat mixed. The introduction of the National Living Wage appears to be associated with reduced rates of labour mobility across firms, on average, within a given local labour market (column III). However, some part of this reduction appears to be related to differential trends in mobility rates across occupations and firms. When we compare workers within region*occupation cells (column IV), there is no statistically significant evidence of a treatment effect from the NLW, and the estimated effects are still closer to zero when we compare workers within firms (column VI).

[FIGURE 3 HERE]
[FIGURE 4 HERE]

5. Robustness Checks

ASHE suffers from panel attrition when an individual continues to be an employee but their employer ceases to respond to the survey, as well as when eligible individuals cease to be employees (e.g., due to retirement) (see Forth et al., 2024). 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 (e.g., for the period 2015/16 ($t = 5$), 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 A4 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 3.0

percentage points compared with the control group when 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 notable though that, when we use the longitudinal sample weights, which were specifically designed to address the non-random attrition of employees from ASHE, the treatment effects on mobility all attenuate and, with the exception of δ_7 in columns (II) and (III), become non-significant (see Online Appendix Table A5 and Figure A1). In contrast, the treatment effects on wages are virtually unchanged when these weights are applied (Online Appendix Table A6).

In addition, we can use the ASHE panel to impute some 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 per cent larger estimation sample, the treatment effects are generally larger and more negative (Online Appendix Table A7). However, the main change of note, when compared with Table 1, is that δ_6 and δ_7 are now statistically significant at the 10 per cent level in column (IV) – controlling for region*occupation time trends. Otherwise, the main patterns seen in Table 1 across our highlighted specifications (columns III, IV & VI) remain unchanged.[16]

We also consider robustness to the four changes in the definition of treated and control groups described in Section 2, thereby allowing the increase in the wage floor to have spillover effects on employees paid just above it. The first three changes extend the scope of the treated group by 10, 25 and 50 pence, respectively. The results are summarised in Figure 5 and presented fully in Online Appendix Tables A8-A10. In the first and second of these sensitivity checks, the specifications shown in columns (III) and (IV) reveal statistically significant negative treatment effects of around 3 percentage points in each of the policy periods. However, in the third check, which extends the treated group by 50 pence, the treatment effects are around 1-2 percentage points and only statistically significant in 2015/16 (δ_5). One concern is that there is some evidence that the parallel trends assumption is violated in 2012/13 (δ_2 is statistically significant at the 5 per cent level in column (III) with the 25 pence extension; it is also significant at 10 per cent in columns (III) and

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[16] We do not use this imputation approach in Table 1 because we can only impute if people re-appear in ASHE. The imputation may thus be skewed towards those with high tenure, and hence low mobility.

(IV) with the 10 pence extension, and in column (IV) with the 25 pence extension. However, δ_3 is never statistically significant, suggesting that the treatment and control groups behave similarly just prior to the policy change. Turning to the sample used to control for firm fixed effects, we find that (except in the case of the 50 pence extension) the treatment effects strengthen to between 4 and 5 percentage points on moving to the reduced sample of larger firms (columns VII & VIII) But as in Table 1, these treatment effects fall close to zero and are non-significant with the introduction of firm fixed effects (column VI). There is no violation of parallel trends in these models. Results are similar under weighting (see Tables A11-A13 and Figure A2 in the Online Appendix).

[FIGURE 5A HERE]
[FIGURE 5B HERE]

Our fourth sensitivity check defines the control group as comprising all those paid above the NLW (see Online Appendix Table A14). The results are qualitatively similar to those described above in respect of the 10 and 25 pence treatment groups; they show statistically significant negative treatment effects of around 3 percentage points in each of the policy periods under specifications (III) and (IV), albeit with a concern about non-parallel pre-trends in 2012/13, and with no evidence of a treatment effect within firms.

The robustness checks which vary the definition of the treatment group to 10p, 25p and 50p above the NLW also redefine the control group in each case. This has the feature that the estimates are generated across different samples as the control and treatment groups expand. It also means that the control group for the results presented in Table 1 (up to 10% or 72p above the NLW in 2015/16) in large part becomes the treatment group for these subsequent robustness checks. So as a final robustness check, we compare different treatment groups against the control group that has been used when treatment is defined in the range of the NLW plus 50p. These results are presented in Figure 6. Here, we see a negative treatment effect on mobility, even for the base treatment group. This indicates that the estimated treatment effect for this group, shown in Table 1, Figure 4 and Figure 5, was being depressed by spillovers into the control group. Here, in Figure 6, the estimated treatment effect for this group is very similar to the +10p and +25p groups. In

sum, we find evidence of spillover effects on job-to-job mobility up to 25p above the NLW, aligning somewhat with Giupponi et al.'s (2024) evidence on wages, discussed earlier.

[FIGURE 6A HERE]

[FIGURE 6B HERE]

Online Appendix Table A18 shows the results of our placebo test. The treatment coefficients for 2015/16 and 2017/18 in columns (III) and (IV) are small (one percentage point or less) and statistically non-significant. Those for 2016/17 are larger (around 2 percentage points) but only statistically significant at the 10 per cent level. The treatment coefficients in column (VI) are around one percentage point, but all are non-significant. It is therefore 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 A19 and Figure A3. They show no statistically significant treatment effects on cross-firm and occupation mobility due to the NLW in any period.

It is plausible that employees working in some low-wage occupations are more sensitive to the rising wage floor than others and so our next sensitivity check tests for heterogeneity in the treatment effect across occupations.[17] In our main estimation sample (Table 1, columns I-IV), there are three occupation sub-major groups (SOC 2-digit) with over 1,000 employee observations in each year. These are "Caring personal services" (SOC61, N=16,612), "Sales" (SOC71, N=24,108), and "Elementary administration and services" (SOC92, N=35,106). Using each of these sub-samples of jobs, we re-estimate Equation (1) controlling for region-specific trends (specification III in Table 1). Results are shown in Table 2. 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 and 2018/19 policy periods, SOC61 and SOC92 show no significant effects. But there is evidence of a negative effect for SOC71 at 4.2 percentage points

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[17] 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.

in 2016/17 and 5.1 percentage points in 2018/19, both of which are significant at the 1 per cent level. Hence, adding to the average treatment effect on year-to-year firm switching in column (III) of Table 1, we find some evidence of effects from the policy within one of the most common low-paid occupations, where presumably there are continuously a good number of vacancies in local labour markets.

[TABLE 2 HERE]

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 (i.e., workers aged 25 or above with earnings in the first year of each period, t, that are below the incoming wage floor) but focus only on workers aged 25-30. As our control group, we consider employees aged 22-23 in the same wage interval. 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.

Using these new age-based treatment and control groups, we estimate the equivalent specifications of Equation (1) as previously discussed, except that specification (III) replaces {TTWA×period} fixed effects with {region×period} fixed effects, and the specifications with 3-digit occupational controls and firm-year fixed effects are omitted, due to much smaller age-based sample sizes. The results in Online Appendix Tables A20 & A21 show no evidence of a treatment effect from the NLW, either on year-to-year firm switching or wage growth. The latter of these two results indicates that the NLW treated workers just under the age of 25 through the maintenance of firm-specific pay structures, or that employers decided to pay their employees equally or fairly, regardless of their age, consistent with theories on 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).

6. 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). Little attention in those studies was given to the impact on cross-firm mobility among those who remained in employment. Theoretical labour market search models provide no clear indication for the direction of any such effects; job displacement 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 displacement 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 DiD 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. In doing so, we contribute to a small, but growing literature on the impact of minimum wages on labour mobility. We find no evidence that the introduction of the NLW increased job-to-job transitions among minimum wage workers, as might be expected if the dominant effect were to induce layoffs. Instead, we find some evidence that the introduction of NLW reduced job-to-job transitions, consistent with a reduction in voluntary quits. The impact is stronger, and more robust to occupation controls, if we account for the possibility that the wage effects of the NLW have spilled over to workers up to 25 pence above the wage floor (as suggested by Giupponi et al., 2024). However, we find no evidence that the NLW affected differences in job mobility between minimum wage workers and their co-workers in the same firm, consistent with the more-limited treatment effect on within-firm pay structures.

Our findings suggest that, in the short run, the NLW may have aided low wage workers by delivering wage growth without them otherwise needing to engage in potentially risky moves to other firms and jobs. The findings also suggest that, by primarily compressing the wage distribution across firms, the uprating of the wage floor made quits less attractive principally to low-wage wage workers facing limited opportunities for internal wage progression within their current firm. However, this could have implications for the overall labour market and economy in the long run, if growing and more productive firms are thus finding it more difficult to fill low-wage vacancies, because the bite of the wage floor is curtailing workers' incentives to search and move across firms (for theory and evidence on the importance of so-called 'job-ladders' for efficiency see, e.g., Bagger & Lentz, 2019; Haltiwanger et al., 2018; Lise et al., 2016; Moscarini & Postel-Vinay, 2018). In this way, the rate of job-to-job mobility is an important metric for the health of the aggregate labour

market, particularly in its ability to reallocate resources and help drive or maintain aggregate productivity growth (e.g., Foster et al., 2008; Fujita et al., 2024).

Further research could attempt to explore whether the average negative effect of the NLW on mobility might, in some instances, represent a partial cancelling out of impacts on quits and layoffs, as well as exploring the implications for overall labour market efficiency and productivity. Any partial cancelling out of quits and layoffs would imply that recent UK minimum wage policy is having differential effects on particular segments of the labour market. However, such an analysis would require large-scale linked employer-employee data where the reasons for job mobility are recorded, to distinguish between layoffs and quits; these data are not currently available for the UK.

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FIGURE 1: Nominal rates, bite, and coverage of the UK National Minimum Wage (NMW) and National Living Wage (NLW) among employees aged 25 or more, by year

a.   Nominal hourly rates and bite
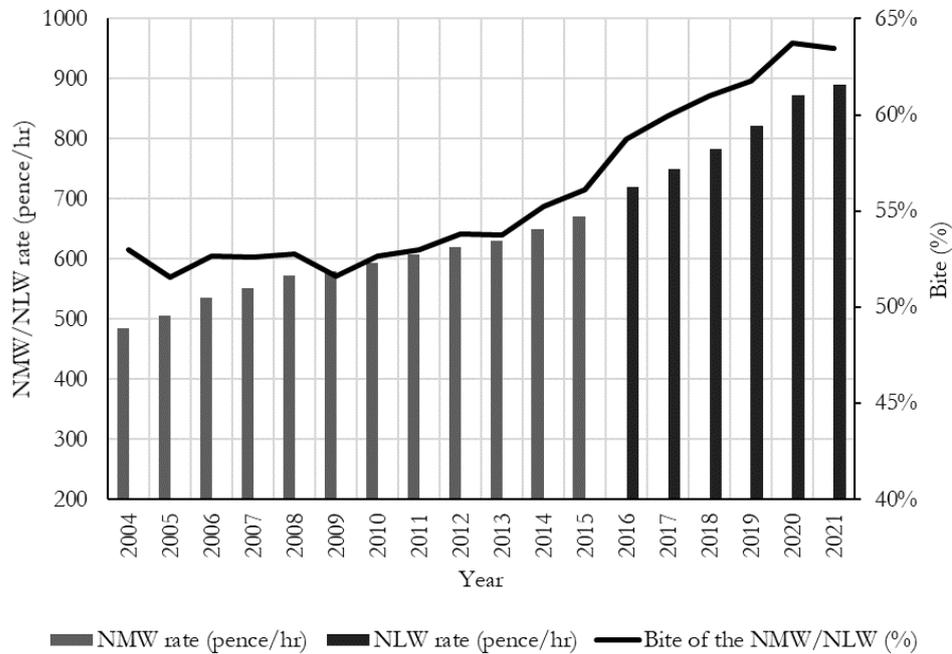
b.   Coverage of employee jobs
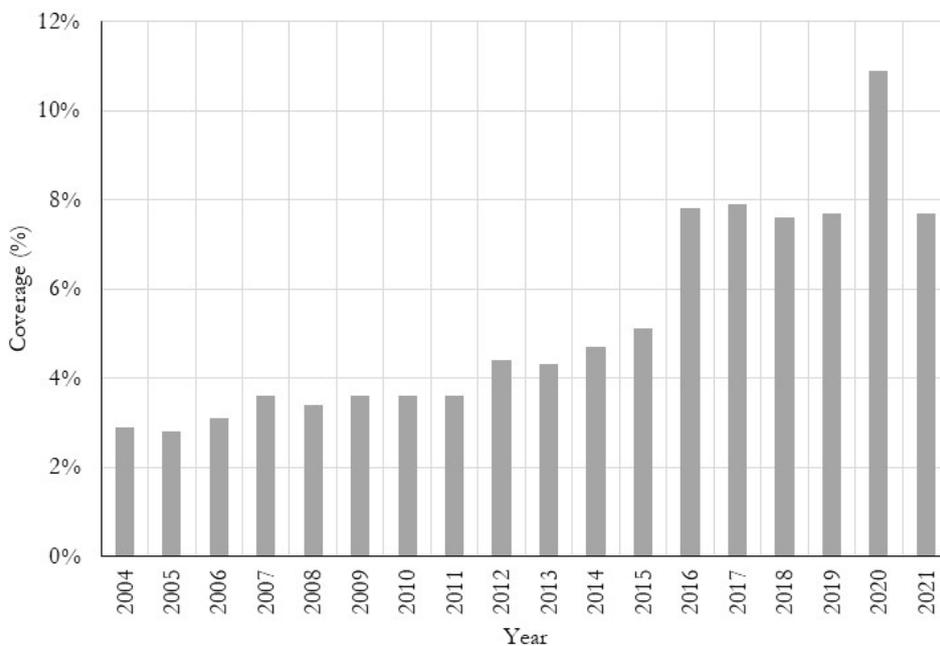
Source: ASHE

Notes: Bite and coverage of the NMW/NLW are estimated for all employees aged 25+, main job, with no loss of pay (except furlough), adult rates, using the revised ASHE weights developed by Author (2024). 95% confidences intervals around the estimates shown in Figure 1b are all within +/- 0.2 percentage points.

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

a: Wage growth
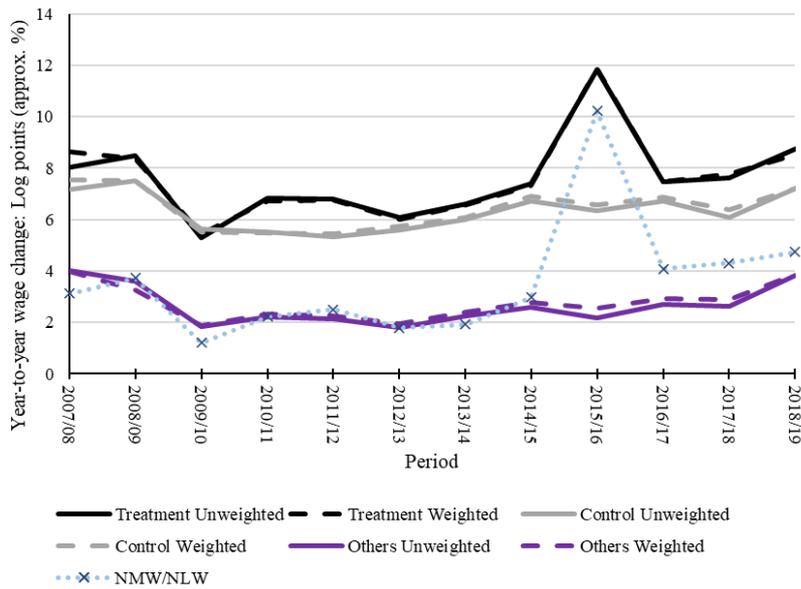
b: Cross-firm mobility
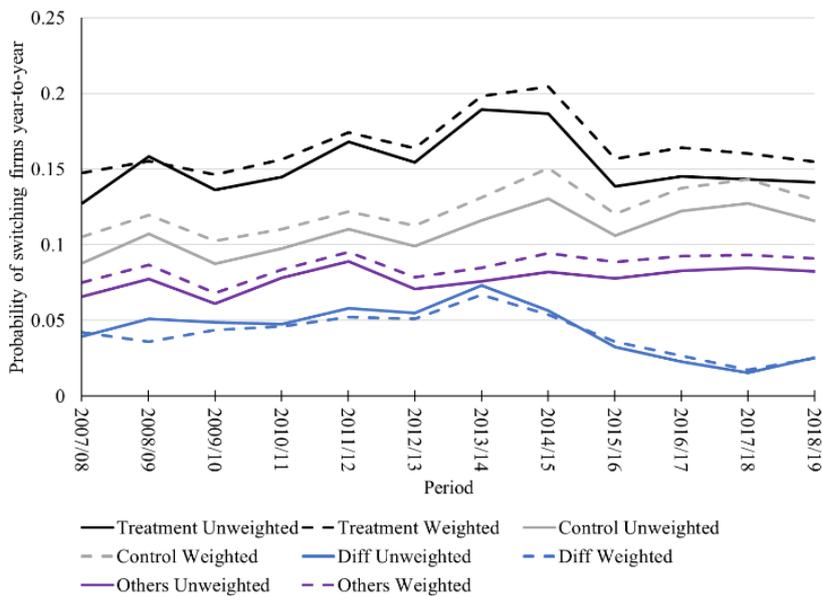
Source: ASHE. "Diff" refers to the difference between the treated and control groups' average rates of switching.

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 III, IV & VI, Online Appendix Table A3)
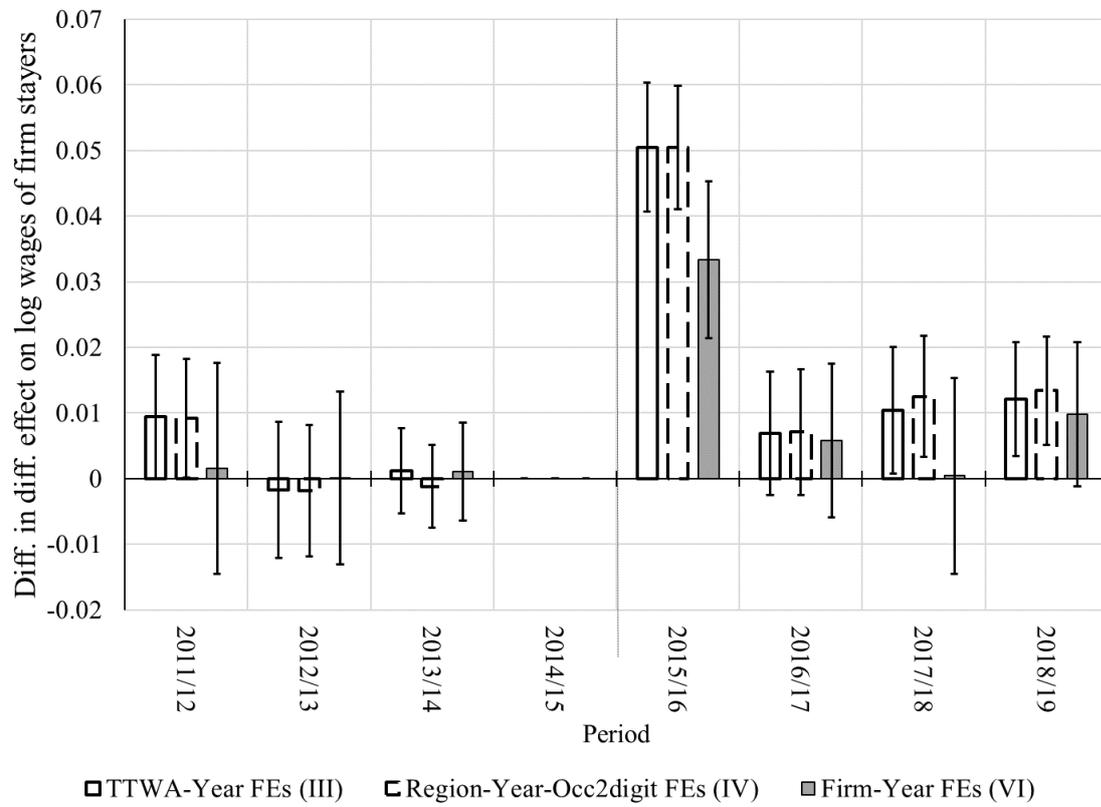
□ TTWA-Year FEs (III)　　Ϲ Region-Year-Occ2digit FEs (IV)　　▨ Firm-Year FEs (VI)

Source: ASHE

Notes: The figure shows point estimates and 95% confidence intervals. All specifications control for: gender × age (5 categories) × hours worked (full-time/part-time); and tenure in the job (7 categories). (III) adds TTWA × Year FEs; (IV) adds Region × Year × Occ 2-digit FEs; (VI) adds Firm × Year FEs. For other notes, see Online Appendix Table A3.

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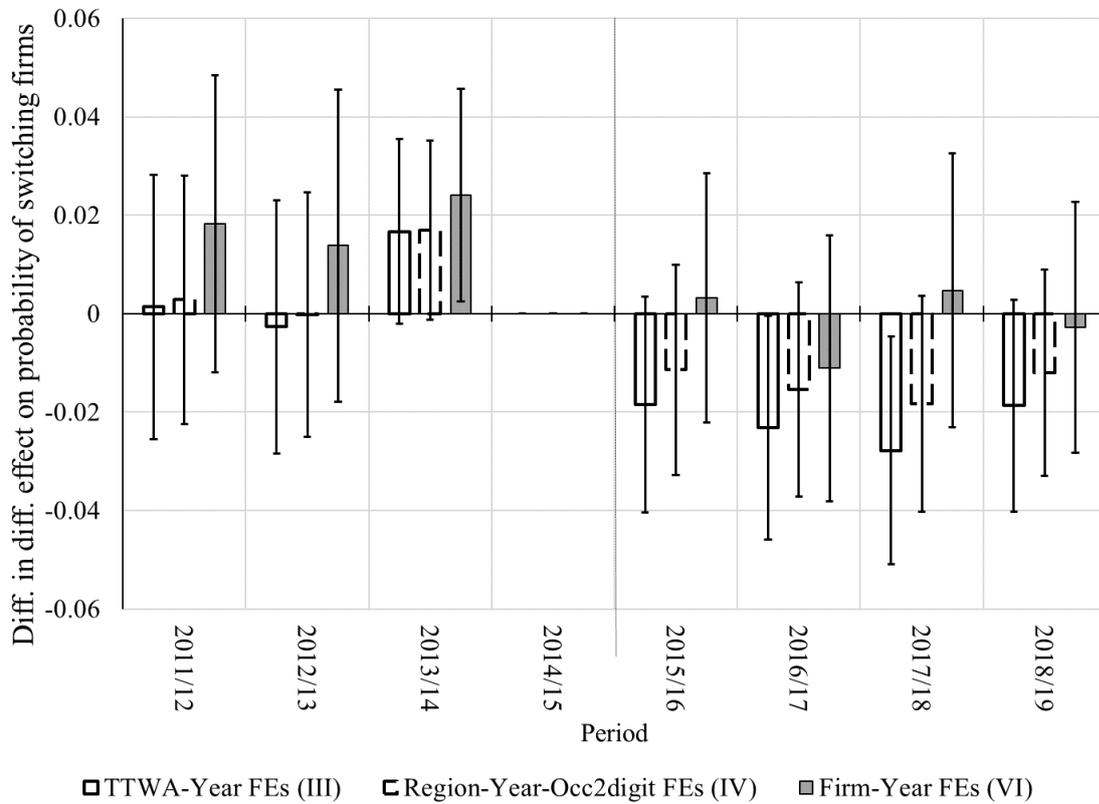
Source: ASHE

Notes: The figure shows point estimates and 95% confidence intervals. All specifications control for: gender × age (5 categories) × hours worked (full-time/part-time); and tenure in the job (7 categories). (III) adds TTWA × Year FEs; (IV) adds Region × Year × Occ 2-digit FEs; (VI) adds Firm × Year FEs. For other notes, see Table 1.

FIGURE 5: Differences-in-differences estimates of the effects of the National Living Wage on the probability of year-to-year firm switching: allowing for wage-rate rounding effects and spillovers (Tables 1 & A8-10)
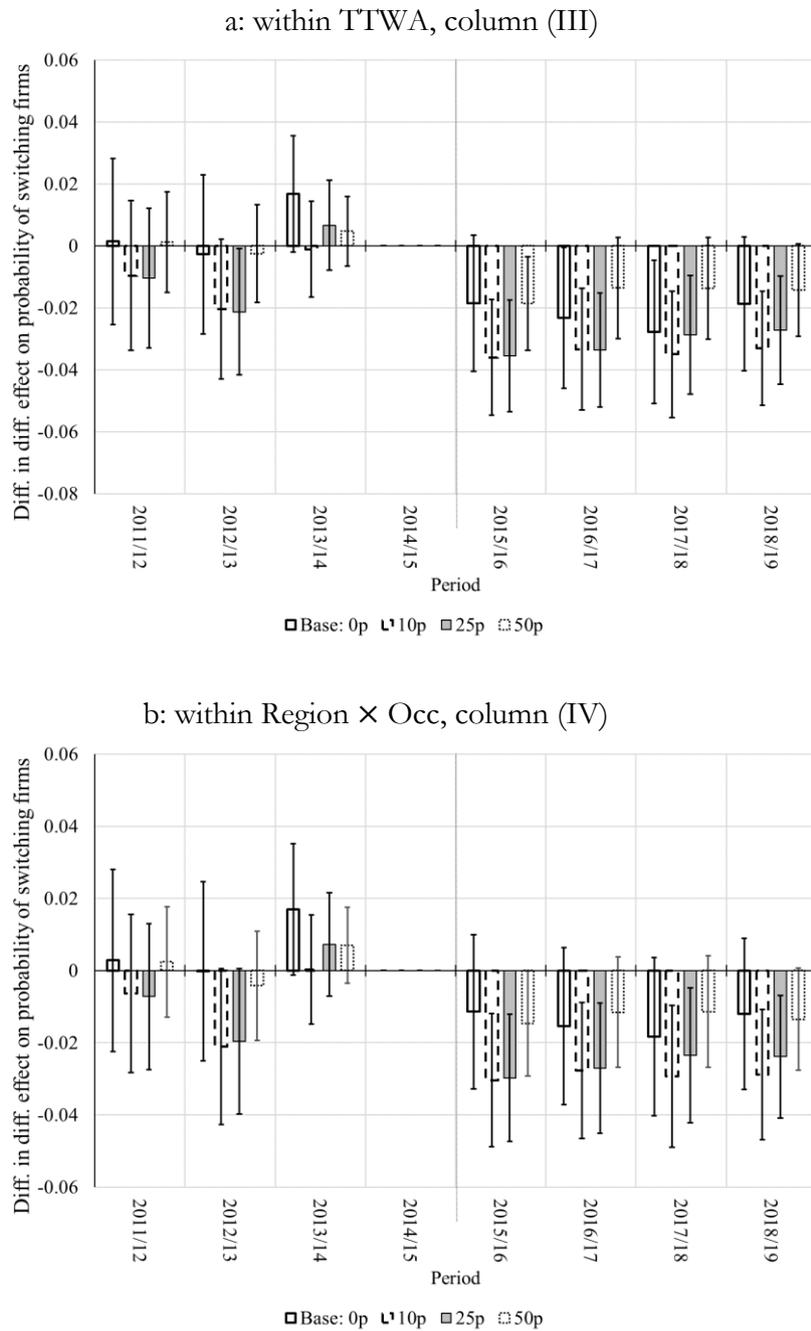
a: within TTWA, column (III)

b: within Region × Occ, column (IV)

Source: ASHE
Notes: The figure shows point estimates and 95% confidence intervals. Both specifications control for: gender × age (5 categories) × hours worked (full-time/part-time); and tenure in the job (7 categories). (III) adds TTWA × Year FEs; (IV) adds Region × Year × Occ 2-digit FEs. For other notes, see Tables 1 & Online Appendix Tables A8-A10.

FIGURE 6: Differences-in-differences estimates of the effects of the National Living Wage on the probability of year-to-year firm switching: allowing for wage-rate rounding effects and spillovers – with a consistent control group (Tables A15-A17)

a: within TTWA, column (III)
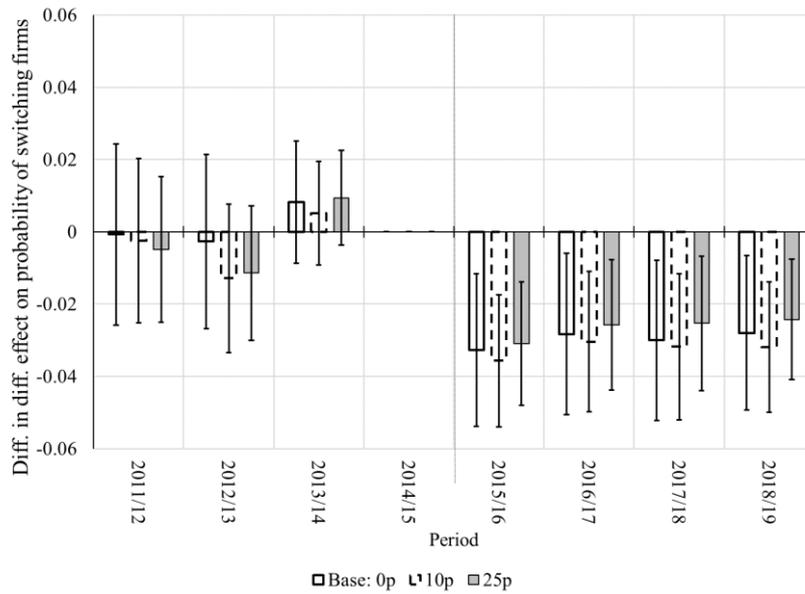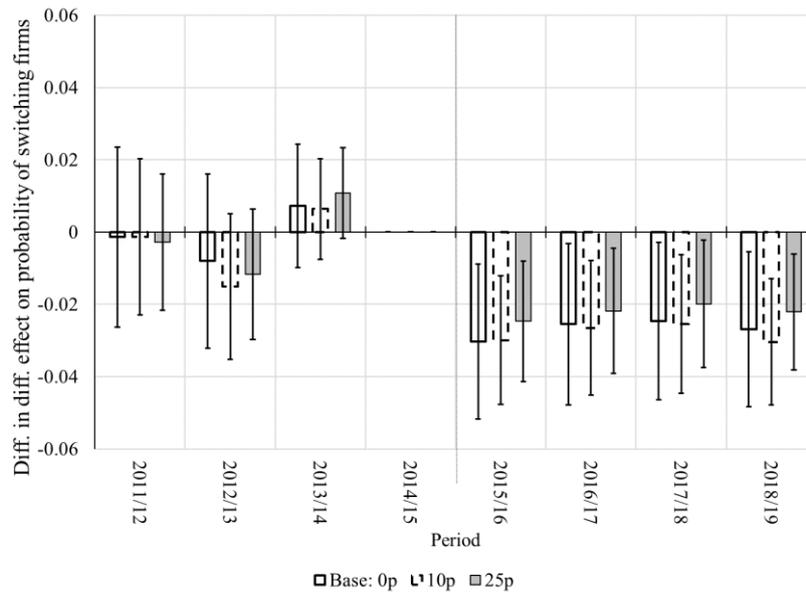
b: within Region × Occ, column (IV)

Source: ASHE

Notes: The figure shows point estimates and 95% confidence intervals. Both specifications control for: gender × age (5 categories) × hours worked (full-time/part-time); and tenure in the job (7 categories). (III) adds TTWA × Year FEs; (IV) adds Region × Year × Occ 2-digit FEs. For other notes, see Online Appendix A15-A17.

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)	(VIII)
Treated × {Control period, t=2011/12}; δ_1	0.0020	0.0027	0.0014	0.0028	0.0147	0.0182	-0.0001	-0.0050
	(0.0159)	(0.0141)	(0.0137)	(0.0129)	(0.0125)	(0.0154)	(0.0203)	(0.0188)
	[0.8979]	[0.8469]	[0.9173]	[0.8308]	[0.2415]	[0.2378]	[0.9952]	[0.7910]
Treated × {Control period, t=2012/13}; δ_2	-0.0014	-0.0037	-0.0027	-0.0002	0.0092	0.0138	-0.0063	-0.0074
	(0.0149)	(0.0133)	(0.0131)	(0.0127)	(0.0120)	(0.0162)	(0.0189)	(0.0183)
	[0.9232]	[0.7818]	[0.8350]	[0.9849]	[0.4425]	[0.3939]	[0.7381]	[0.6864]
Treated × {Control period, t=2013/14}; δ_3	0.0170	0.0143	0.0167	0.0169	0.0231	0.0241	0.0268	0.0265
	(0.0112)	(0.0098)	(0.0096)	(0.0093)	(0.0089)	(0.0110)	(0.0143)	(0.0136)
	[0.2809]	[0.3079]	[0.2188]	[0.1900]	[0.0585]	[0.1471]	[0.1753]	[0.1564]
Treated {Control period, t=2014/15}; β	0.0565	0.0355	0.0364	0.0295	0.0199	-0.0032	0.0487	0.0414
	(0.0158)	(0.0140)	(0.0136)	(0.0129)	(0.0122)	(0.0166)	(0.0198)	(0.0187)
	[0.0000]	[0.0003]	[0.0002]	[0.0016]	[0.0256]	[0.7703]	[0.0006]	[0.0023]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}; δ_5	-0.0242	-0.0197	-0.0185	-0.0114	-0.0024	0.0032	-0.0321	-0.0245
	(0.0128)	(0.0114)	(0.0112)	(0.0109)	(0.0106)	(0.0129)	(0.0161)	(0.0154)
	[0.0580]	[0.0834]	[0.0984]	[0.2954]	[0.8195]	[0.8066]	[0.0469]	[0.1119]
Treated × {Policy period, t=2016/17}; δ_6	-0.0337	-0.0241	-0.0232	-0.0154	-0.0054	-0.0111	-0.0341	-0.0257
	(0.0137)	(0.0119)	(0.0116)	(0.0111)	(0.0108)	(0.0138)	(0.0168)	(0.0161)
	[0.0136]	[0.0421]	[0.0453]	[0.1659]	[0.6175]	[0.4210]	[0.0426]	[0.1091]
Treated × {Policy period, t=2017/18}; δ_7	-0.0410	-0.0281	-0.0278	-0.0183	-0.0080	0.0047	-0.0351	-0.0209
	(0.0142)	(0.0121)	(0.0118)	(0.0112)	(0.0108)	(0.0142)	(0.0177)	(0.0166)
	[0.0040]	[0.0200]	[0.0185]	[0.1024]	[0.4594]	[0.7427]	[0.0471]	[0.2068]
Treated × {Policy period, t=2018/19}; δ_8	-0.0312	-0.0193	-0.0187	-0.0120	-0.0035	-0.0028	-0.0322	-0.0254
	(0.0130)	(0.0112)	(0.0110)	(0.0107)	(0.0104)	(0.0130)	(0.0159)	(0.0153)
	[0.0166]	[0.0844]	[0.0888]	[0.2594]	[0.7369]	[0.8264]	[0.0434]	[0.0966]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0043	0.0499	0.0665	0.0731	0.0941	0.3797	0.0785	0.086
N	115,946	115,946	115,946	115,739	114,826	68,646	68,600	68,343

Source: ASHE

Notes: γ_t omitted for brevity. Controls: gender × age (5 categories) × hours worked (full-time/part-time); and tenure in the job (7 categories). 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 sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI). Numbers of observations by {Treated×Period} are shown in Online Appendix Table A1 for columns (III) and (VI).

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=2012/13}; δ_1	-0.0073	0.0202	0.0057
	(0.0328)	(0.0195)	(0.0188)
	[0.8244]	[0.3016]	[0.7610]
Treated × {Control period, t=2012/13}; δ_2	0.0157	-0.0032	0.0283
	(0.0328)	(0.0183)	(0.0180)
	[0.6329]	[0.8614]	[0.1148]
Treated × {Control period, t=2013/14}; δ_3	0.0113	0.0077	0.0557
	(0.0203)	(0.0127)	(0.0123)
	[0.7214]	[0.6733]	[0.0022]
Treated {Control period, t=2014/15}; β	0.0171	0.0369	0.0226
	(0.0316)	(0.0183)	(0.0182)
	[0.4002]	[0.0038]	[0.0676]
NLW period - DiD effects:			
Treated × {Policy period, t=2015/16}; δ_5	0.0192	-0.0024	-0.0112
	(0.0242)	(0.0160)	(0.0156)
	[0.4280]	[0.8822]	[0.4739]
Treated × {Policy period, t=2016/17}; δ_6	0.0108	-0.0424	0.0032
	(0.0257)	(0.0160)	(0.0164)
	[0.6737]	[0.0081]	[0.8428]
Treated × {Policy period, t=2017/18}; δ_7	-0.0157	-0.0508	0.0115
	(0.0253)	(0.0166)	(0.0161)
	[0.5352]	[0.0021]	[0.4762]
Treated × {Policy period, t=2018/19}; δ_8	0.0021	-0.0127	0.0032
	(0.0243)	(0.0164)	(0.0159)
	[0.9323]	[0.4394]	[0.8428]
Controls	Yes	Yes	Yes
TTWA × Period FEs	Yes	Yes	Yes
R^2	0.1165	0.1117	0.1059
N	16,612	24,108	35,106

Source: ASHE

Notes: Specification as per column (III) in Table 1, controlling for: gender × age (5 categories) × hours worked (full-time/part-time); tenure in the job (7 categories); and TTWA × Period FEs. Robust standard errors in parentheses. Square brackets show p-values for significance from zero, two-sided tests.

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

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-III & column VI)

	Main			Within firm-years			
	Control	Treatment	Total	Control	Treatment	Total	Firms
2011/12	6,399	3,186	9,585	4,038	1,722	5,760	816
2012/13	6,797	3,317	10,114	4,288	1,779	6,067	901
2013/14	7,056	3,801	10,857	4,576	2,065	6,641	971
2014/15	7,643	4,668	12,311	4,660	2,569	7,229	1,061
2015/16	7,826	10,363	18,189	4,784	6,210	10,994	1,580
2016/17	9,186	7,592	16,778	5,501	4,381	9,882	1,424
2017/18	9,501	8,953	18,454	5,639	5,062	10,701	1,648
2018/19	10,101	9,557	19,658	6,023	5,349	11,372	1,825
Total	64,509	51,437	115,946	39,509	29,137	68,646	

TABLE A2: Sample means and shares, by group in the main estimation sample selection, pooled 2011/12-2018/19

	Control	Treatment
Male	0.341	0.334
Age (Mean, years)	45.0	44.9
Male: 25-34	0.112	0.111
Male: 35-44	0.070	0.070
Male: 45-54	0.077	0.072
Male: 55-64	0.063	0.060
Male: 65+	0.018	0.022
Female: 25-34	0.135	0.149
Female: 35-44	0.150	0.149
Female: 45-54	0.213	0.203
Female: 55-64	0.134	0.136
Female: 65+	0.027	0.029
Full-time	0.492	0.403
Job tenure: Less than 6 months	0.064	0.095
Job tenure: 6-11 months	0.095	0.123
Job tenure: 12-23 months	0.136	0.157
Job tenure: 2-4 years	0.254	0.245
Job tenure: 5-9 years	0.224	0.196
Job tenure: 10-19 years	0.173	0.148
Job tenure: 20+ years	0.054	0.035
Basic hours (Mean)	28.7	26.6
Public Sector	0.180	0.068
Employer size (Median, rounded to nearest 10 employees)	2,500	710
Occupations - SOC2010 2-digit		
61: Caring personal services	0.164	0.119
71: Sales	0.198	0.231
92: Elementary administration and services	0.275	0.355
Other	0.363	0.295
N	64,509	51,437

Source: ASHE

TABLE A3: 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)	(VIII)
Treated × {Control period, t=2011/12}	0.0095	0.0095	0.0094	0.0092	0.0090	0.0016	0.0133	0.0118
	(0.0050)	(0.0049)	(0.0048)	(0.0046)	(0.0046)	(0.0082)	(0.0068)	(0.0067)
	[0.0573]	[0.0539]	[0.0508]	[0.0449]	[0.0495]	[0.8464]	[0.0509]	[0.0781]
Treated × {Control period, t=2012/13}	-0.0017	-0.0019	-0.0017	-0.0018	-0.0019	0.0001	0.0041	0.0051
	(0.0054)	(0.0053)	(0.0053)	(0.0051)	(0.0052)	(0.0067)	(0.0076)	(0.0078)
	[0.7474]	[0.7162]	[0.7453]	[0.7284]	[0.7161]	[0.9857]	[0.5915]	[0.5157]
Treated × {Control period, t=2013/14}	0.0007	0.0005	0.0012	-0.0012	-0.0019	0.0011	0.0087	0.0056
	(0.0034)	(0.0034)	(0.0033)	(0.0032)	(0.0031)	(0.0038)	(0.0046)	(0.0047)
	[0.8973]	[0.9260]	[0.8230]	[0.8188]	[0.7197]	[0.8709]	[0.2721]	[0.4726]
Treated {Control period, t=2014/15}	0.0023	0.0021	0.0020	0.0050	0.0056	0.0194	0.0027	0.0043
	(0.0055)	(0.0054)	(0.0053)	(0.0052)	(0.0053)	(0.0071)	(0.0079)	(0.0079)
	[0.5048]	[0.5250]	[0.5527]	[0.1167]	[0.0728]	[0.0000]	[0.5603]	[0.3549]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	0.0485	0.0491	0.0505	0.0504	0.0502	0.0333	0.0544	0.0551
	(0.0051)	(0.0050)	(0.0050)	(0.0048)	(0.0049)	(0.0061)	(0.0071)	(0.0070)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Treated × {Policy period, t=2016/17}	0.0060	0.0067	0.0069	0.0071	0.0066	0.0058	0.0097	0.0103
	(0.0049)	(0.0048)	(0.0048)	(0.0049)	(0.0050)	(0.0060)	(0.0071)	(0.0075)
	[0.2251]	[0.1642]	[0.1497]	[0.1471]	[0.1847]	[0.3314]	[0.1737]	[0.1696]
Treated × {Policy period, t=2017/18}	0.0089	0.0102	0.0104	0.0125	0.0115	0.0004	0.0105	0.0164
	(0.0050)	(0.0049)	(0.0049)	(0.0047)	(0.0047)	(0.0076)	(0.0071)	(0.0070)
	[0.0738]	[0.0361]	[0.0327]	[0.0073]	[0.0154]	[0.9549]	[0.1370]	[0.0184]
Treated × {Policy period, t=2018/19}	0.0105	0.0119	0.0121	0.0134	0.0130	0.0098	0.0136	0.0165
	(0.0046)	(0.0045)	(0.0044)	(0.0042)	(0.0042)	(0.0056)	(0.0064)	(0.0061)
	[0.0229]	[0.0074]	[0.0056]	[0.0015]	[0.0018]	[0.0831]	[0.0344]	[0.0070]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0121	0.0164	0.0339	0.0565	0.0773	0.2894	0.0487	0.0595
N	100,616	100,616	100,604	100,381	99,461	59,267	59,210	58,962

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI).

TABLE A4: 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)	(VIII)
Treated × {Control period, t=2011/12}	0.0357	0.0342	0.0347	0.0314	0.0277	0.0020	0.0254	0.0135
	(0.0246)	(0.0237)	(0.0231)	(0.0210)	(0.0210)	(0.0189)	(0.0352)	(0.0308)
	[0.1462]	[0.1483]	[0.1333]	[0.1347]	[0.1873]	[0.9164]	[0.4707]	[0.6601]
Treated × {Control period, t=2012/13}	0.0269	0.0218	0.0206	0.0208	0.0184	-0.0039	0.0277	0.0234
	(0.0185)	(0.0180)	(0.0180)	(0.0158)	(0.0159)	(0.0190)	(0.0269)	(0.0229)
	[0.1474]	[0.2275]	[0.2505]	[0.1890]	[0.2459]	[0.8367]	[0.3023]	[0.3064]
Treated × {Control period, t=2013/14}	0.0348	0.0307	0.0315	0.0290	0.0262	-0.0094	0.0380	0.0272
	(0.0150)	(0.0145)	(0.0143)	(0.0116)	(0.0114)	(0.0131)	(0.0214)	(0.0170)
	[0.0798]	[0.1088]	[0.0926]	[0.0822]	[0.1063]	[0.6107]	[0.1884]	[0.2755]
Treated {Control period, t=2014/15}	0.0451	0.0180	0.0172	0.0186	0.0188	0.0022	0.0041	0.0076
	(0.0199)	(0.0191)	(0.0187)	(0.0167)	(0.0162)	(0.0185)	(0.0289)	(0.0249)
	[0.0025]	[0.2131]	[0.2293]	[0.1082]	[0.0996]	[0.8680]	[0.8483]	[0.6564]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	0.0198	0.0270	0.0292	0.0296	0.0256	-0.0078	0.0271	0.0224
	(0.0172)	(0.0166)	(0.0163)	(0.0142)	(0.0141)	(0.0159)	(0.0242)	(0.0204)
	[0.2494]	[0.1040]	[0.0735]	[0.0366]	[0.0698]	[0.6248]	[0.2617]	[0.2725]
Treated × {Policy period, t=2016/17}	0.0055	0.0147	0.0174	0.0149	0.0140	-0.0063	0.0284	0.0208
	(0.0175)	(0.0168)	(0.0164)	(0.0145)	(0.0145)	(0.0157)	(0.0244)	(0.0214)
	[0.7532]	[0.3788]	[0.2875]	[0.3038]	[0.3351]	[0.6869]	[0.2459]	[0.3301]
Treated × {Policy period, t=2017/18}	-0.0194	-0.0079	-0.0063	-0.0121	-0.0141	-0.0017	-0.0084	-0.0179
	(0.0208)	(0.0198)	(0.0193)	(0.0191)	(0.0197)	(0.0159)	(0.0290)	(0.0282)
	[0.3512]	[0.6915]	[0.7445]	[0.5257]	[0.4729]	[0.9152]	[0.7734]	[0.5257]
Treated × {Policy period, t=2018/19}	0.0131	0.0263	0.0277	0.0265	0.0277	0.0080	0.0258	0.0193
	(0.0181)	(0.0170)	(0.0166)	(0.0144)	(0.0143)	(0.0180)	(0.0248)	(0.0216)
	[0.4683]	[0.1227]	[0.0957]	[0.0663]	[0.0536]	[0.6558]	[0.2989]	[0.3725]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.004	0.0351	0.0484	0.0533	0.0719	0.3111	0.0549	0.0585
N	179,607	179,607	179,607	179,453	178,533	107,482	107,446	107,202

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 (IV)-(VI) are smaller as singletons are dropped.
Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (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)	(VIII)
Treated × {Control period, t=2011/12}	-0.0005	0.0000	-0.0015	0.0016	0.0129	0.0145	0.0000	-0.0088
	(0.0155)	(0.0140)	(0.0138)	(0.0134)	(0.0133)	(0.0174)	(0.0203)	(0.0195)
	[0.9727]	[0.9995]	[0.9157]	[0.9038]	[0.3299]	[0.4059]	[0.9982]	[0.6507]
Treated × {Control period, t=2012/13}	-0.0029	-0.0049	-0.0038	-0.0005	0.0084	0.0082	-0.0064	-0.0071
	(0.0148)	(0.0134)	(0.0134)	(0.0131)	(0.0128)	(0.0178)	(0.0189)	(0.0190)
	[0.8441]	[0.7162]	[0.7755]	[0.9723]	[0.5118]	[0.6452]	[0.7332]	[0.7078]
Treated × {Control period, t=2013/14}	0.0135	0.0112	0.0134	0.0153	0.0219	0.0203	0.0268	0.0250
	(0.0110)	(0.0099)	(0.0098)	(0.0096)	(0.0095)	(0.0125)	(0.0143)	(0.0142)
	[0.3801]	[0.4212]	[0.3278]	[0.2468]	[0.0912]	[0.2676]	[0.1760]	[0.1937]
Treated {Control period, t=2014/15}	0.0539	0.0312	0.0325	0.0245	0.0167	-0.0024	0.0487	0.0399
	(0.0153)	(0.0139)	(0.0137)	(0.0132)	(0.0130)	(0.0184)	(0.0198)	(0.0192)
	[0.0000]	[0.0016]	[0.0009]	[0.0109]	[0.0793]	[0.8466]	[0.0006]	[0.0049]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0178	-0.0146	-0.0136	-0.0064	0.0008	0.0024	-0.0320	-0.0230
	(0.0128)	(0.0117)	(0.0116)	(0.0114)	(0.0114)	(0.0148)	(0.0162)	(0.0162)
	[0.1650]	[0.2108]	[0.2404]	[0.5745]	[0.9470]	[0.8694]	[0.0474]	[0.1573]
Treated × {Policy period, t=2016/17}	-0.0271	-0.0192	-0.0192	-0.0098	-0.0019	-0.0174	-0.0341	-0.0229
	(0.0134)	(0.0121)	(0.0120)	(0.0118)	(0.0117)	(0.0158)	(0.0168)	(0.0169)
	[0.0437]	[0.1113]	[0.1107]	[0.4068]	[0.8692]	[0.2700]	[0.0426]	[0.1768]
Treated × {Policy period, t=2017/18}	-0.0367	-0.0246	-0.0243	-0.0159	-0.0070	0.0036	-0.0351	-0.0173
	(0.0136)	(0.0120)	(0.0120)	(0.0117)	(0.0116)	(0.0157)	(0.0177)	(0.0172)
	[0.0070]	[0.0408]	[0.0419]	[0.1741]	[0.5482]	[0.8206]	[0.0470]	[0.3139]
Treated × {Policy period, t=2018/19}	-0.0286	-0.0170	-0.0164	-0.0089	-0.0025	-0.0073	-0.0323	-0.0247
	(0.0130)	(0.0115)	(0.0115)	(0.0113)	(0.0113)	(0.0146)	(0.0159)	(0.0161)
	[0.0278]	[0.1403]	[0.1528]	[0.4334]	[0.8272]	[0.6179]	[0.0426]	[0.1256]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0039	0.0527	0.0723	0.0805	0.1051	0.397	0.0785	0.0973
N	115,912	115,912	115,912	115,705	114,794	68,618	68,572	68,315

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI).

TABLE A6: 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)	(VIII)
Treated × {Control period, t=2011/12}	0.0096	0.0094	0.0103	0.0093	0.0089	0.0001	0.0134	0.0117
	(0.0052)	(0.0051)	(0.0052)	(0.0047)	(0.0047)	(0.0083)	(0.0068)	(0.0068)
	[0.0676]	[0.0672]	[0.0468]	[0.0472]	[0.0592]	[0.9917]	[0.0499]	[0.0848]
Treated × {Control period, t=2012/13}	-0.0009	-0.0009	-0.0001	-0.0017	-0.0020	-0.0016	0.0041	0.0045
	(0.0052)	(0.0051)	(0.0052)	(0.0048)	(0.0049)	(0.0068)	(0.0076)	(0.0075)
	[0.8690]	[0.8528]	[0.9880]	[0.7144]	[0.6807]	[0.8104]	[0.5851]	[0.5504]
Treated × {Control period, t=2013/14}	0.0027	0.0023	0.0038	0.0012	0.0011	0.0006	0.0087	0.0064
	(0.0036)	(0.0035)	(0.0035)	(0.0031)	(0.0031)	(0.0040)	(0.0046)	(0.0044)
	[0.6206]	[0.6659]	[0.4735]	[0.8114]	[0.8371]	[0.9324]	[0.2732]	[0.3870]
Treated {Control period, t=2014/15}	-0.0004	0.0000	-0.0008	0.0037	0.0045	0.0196	0.0027	0.0048
	(0.0054)	(0.0053)	(0.0053)	(0.0050)	(0.0051)	(0.0073)	(0.0079)	(0.0075)
	[0.9080]	[0.9926]	[0.8115]	[0.2282]	[0.1459]	[0.0000]	[0.5601]	[0.2768]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	0.0489	0.0496	0.0517	0.0504	0.0502	0.0322	0.0544	0.0532
	(0.0049)	(0.0049)	(0.0050)	(0.0047)	(0.0048)	(0.0061)	(0.0071)	(0.0065)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Treated × {Policy period, t=2016/17}	0.0064	0.0071	0.0080	0.0068	0.0059	0.0061	0.0097	0.0086
	(0.0049)	(0.0048)	(0.0049)	(0.0047)	(0.0048)	(0.0061)	(0.0071)	(0.0070)
	[0.1878]	[0.1421]	[0.0998]	[0.1460]	[0.2214]	[0.3185]	[0.1738]	[0.2148]
Treated × {Policy period, t=2017/18}	0.0091	0.0105	0.0113	0.0117	0.0099	-0.0027	0.0105	0.0149
	(0.0051)	(0.0050)	(0.0051)	(0.0047)	(0.0048)	(0.0089)	(0.0071)	(0.0068)
	[0.0732]	[0.0361]	[0.0259]	[0.0139]	[0.0391]	[0.7582]	[0.1383]	[0.0299]
Treated × {Policy period, t=2018/19}	0.0107	0.0122	0.0130	0.0127	0.0125	0.0080	0.0136	0.0126
	(0.0047)	(0.0046)	(0.0046)	(0.0043)	(0.0043)	(0.0059)	(0.0064)	(0.0058)
	[0.0228]	[0.0079]	[0.0047]	[0.0030]	[0.0035]	[0.1771]	[0.0345]	[0.0301]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0108	0.0155	0.0351	0.0669	0.0912	0.2916	0.0487	0.0671
N	100,587	100,587	100,575	100,352	99,434	59,243	59,186	58,938

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (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 – With IMPUTED firm-switching information

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Treated × {Control period, t=2011/12}	0.0018	0.0024	0.0012	0.0035	0.0158	0.0083	-0.0027	-0.0049
	(0.0157)	(0.0139)	(0.0136)	(0.0128)	(0.0121)	(0.0146)	(0.0200)	(0.0186)
	[0.9082]	[0.8638]	[0.9319]	[0.7818]	[0.1908]	[0.5684]	[0.8917]	[0.7911]
Treated × {Control period, t=2012/13}	-0.0011	-0.0037	-0.0026	-0.0013	0.0080	0.0024	-0.0104	-0.0113
	(0.0154)	(0.0135)	(0.0134)	(0.0128)	(0.0117)	(0.0156)	(0.0190)	(0.0185)
	[0.9448]	[0.7839]	[0.8474]	[0.9220]	[0.4936]	[0.8799]	[0.5855]	[0.5402]
Treated × {Control period, t=2013/14}	0.0159	0.0135	0.0164	0.0156	0.0214	0.0183	0.0269	0.0274
	(0.0115)	(0.0101)	(0.0100)	(0.0096)	(0.0088)	(0.0107)	(0.0145)	(0.0138)
	[0.3251]	[0.3408]	[0.2382]	[0.2274]	[0.0728]	[0.2559]	[0.1741]	[0.1392]
Treated {Control period, t=2014/15}	0.0589	0.0378	0.0383	0.0317	0.0219	0.0045	0.0554	0.0476
	(0.0162)	(0.0142)	(0.0139)	(0.0129)	(0.0119)	(0.0161)	(0.0198)	(0.0185)
	[0.0000]	[0.0002]	[0.0001]	[0.0009]	[0.0128]	[0.6720]	[0.0001]	[0.0006]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0270	-0.0215	-0.0202	-0.0130	-0.0044	-0.0037	-0.0379	-0.0301
	(0.0128)	(0.0114)	(0.0113)	(0.0109)	(0.0103)	(0.0123)	(0.0160)	(0.0153)
	[0.0354]	[0.0587]	[0.0731]	[0.2330]	[0.6676]	[0.7655]	[0.0181]	[0.0497]
Treated × {Policy period, t=2016/17}	-0.0377	-0.0285	-0.0273	-0.0195	-0.0101	-0.0216	-0.0442	-0.0349
	(0.0141)	(0.0121)	(0.0119)	(0.0112)	(0.0105)	(0.0131)	(0.0172)	(0.0162)
	[0.0074]	[0.0182]	[0.0216]	[0.0816]	[0.3376]	[0.0994]	[0.0101]	[0.0307]
Treated × {Policy period, t=2017/18}	-0.0439	-0.0312	-0.0304	-0.0213	-0.0111	-0.0046	-0.0413	-0.0268
	(0.0146)	(0.0123)	(0.0121)	(0.0113)	(0.0106)	(0.0137)	(0.0180)	(0.0168)
	[0.0027]	[0.0113]	[0.0122]	[0.0605]	[0.2949]	[0.7380]	[0.0221]	[0.1103]
Treated × {Policy period, t=2018/19}	-0.0339	-0.0223	-0.0207	-0.0149	-0.0058	-0.0076	-0.0368	-0.0307
	(0.0132)	(0.0113)	(0.0112)	(0.0108)	(0.0101)	(0.0125)	(0.0160)	(0.0154)
	[0.0103]	[0.0485]	[0.0644]	[0.1684]	[0.5674]	[0.5467]	[0.0219]	[0.0469]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0045	0.0507	0.0657	0.0729	0.0934	0.3776	0.0764	0.0852
N	127,999	127,999	127,999	127,803	126,915	76,866	76,824	76,570

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (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 10p

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Treated × {Control period, t=2011/12}	-0.0153	-0.0087	-0.0096	-0.0063	0.0035	0.0036	-0.0125	-0.0087
	(0.0148)	(0.0128)	(0.0123)	(0.0112)	(0.0107)	(0.0144)	(0.0178)	(0.0158)
	[0.3007]	[0.4972]	[0.4336]	[0.5721]	[0.7453]	[0.8009]	[0.4828]	[0.5820]
Treated × {Control period, t=2012/13}	-0.0231	-0.0212	-0.0204	-0.0211	-0.0139	-0.0039	-0.0291	-0.0327
	(0.0137)	(0.0117)	(0.0115)	(0.0110)	(0.0101)	(0.0150)	(0.0160)	(0.0154)
	[0.0927]	[0.0711]	[0.0761]	[0.0552]	[0.1706]	[0.7953]	[0.0696]	[0.0335]
Treated × {Control period, t=2013/14}	-0.0079	-0.0040	-0.0011	0.0003	0.0034	0.0104	-0.0028	0.0024
	(0.0099)	(0.0082)	(0.0079)	(0.0077)	(0.0071)	(0.0105)	(0.0113)	(0.0109)
	[0.5757]	[0.7380]	[0.9257]	[0.9817]	[0.7349]	[0.4740]	[0.8632]	[0.8731]
Treated {Control period, t=2014/15}	0.0709	0.0478	0.0489	0.0443	0.0362	0.0079	0.0606	0.0541
	(0.0141)	(0.0121)	(0.0117)	(0.0109)	(0.0100)	(0.0145)	(0.0162)	(0.0152)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.4557]	[0.0000]	[0.0000]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0455	-0.0366	-0.0360	-0.0304	-0.0223	-0.0084	-0.0467	-0.0391
	(0.0113)	(0.0098)	(0.0095)	(0.0094)	(0.0090)	(0.0125)	(0.0133)	(0.0129)
	[0.0001]	[0.0002]	[0.0002]	[0.0012]	[0.0129]	[0.5039]	[0.0004]	[0.0026]
Treated × {Policy period, t=2016/17}	-0.0454	-0.0339	-0.0334	-0.0277	-0.0182	-0.0193	-0.0425	-0.0329
	(0.0122)	(0.0103)	(0.0100)	(0.0096)	(0.0092)	(0.0132)	(0.0140)	(0.0136)
	[0.0002]	[0.0010]	[0.0008]	[0.0041]	[0.0471]	[0.1422]	[0.0025]	[0.0156]
Treated × {Policy period, t=2017/18}	-0.0514	-0.0352	-0.0350	-0.0293	-0.0208	-0.0043	-0.0405	-0.0302
	(0.0130)	(0.0108)	(0.0104)	(0.0100)	(0.0094)	(0.0134)	(0.0152)	(0.0144)
	[0.0001]	[0.0011]	[0.0008]	[0.0033]	[0.0274]	[0.7480]	[0.0078]	[0.0366]
Treated × {Policy period, t=2018/19}	-0.0476	-0.0331	-0.0331	-0.0288	-0.0223	-0.0112	-0.0436	-0.0378
	(0.0116)	(0.0097)	(0.0094)	(0.0092)	(0.0088)	(0.0125)	(0.0131)	(0.0129)
	[0.0000]	[0.0006]	[0.0004]	[0.0018]	[0.0111]	[0.3688]	[0.0009]	[0.0034]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R²	0.0044	0.0495	0.0651	0.0722	0.0925	0.3772	0.0758	0.0846
N	124,994	124,994	124,994	124,817	123,886	74,560	74,520	74,249

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (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, plus 25p

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Treated × {Control period, t=2011/12}	-0.0157	-0.0086	-0.0104	-0.0072	0.0005	0.0029	-0.0188	-0.0149
	(0.0141)	(0.0118)	(0.0115)	(0.0103)	(0.0096)	(0.0122)	(0.0162)	(0.0142)
	[0.2658]	[0.4671]	[0.3655]	[0.4823]	[0.9573]	[0.8125]	[0.2443]	[0.2942]
Treated × {Control period, t=2012/13}	-0.0279	-0.0212	-0.0213	-0.0196	-0.0132	-0.0103	-0.0349	-0.0343
	(0.0131)	(0.0106)	(0.0104)	(0.0103)	(0.0090)	(0.0124)	(0.0142)	(0.0142)
	[0.0333]	[0.0448]	[0.0409]	[0.0559]	[0.1436]	[0.4070]	[0.0140]	[0.0159]
Treated × {Control period, t=2013/14}	0.0017	0.0044	0.0066	0.0072	0.0087	0.0100	0.0017	0.0026
	(0.0099)	(0.0077)	(0.0074)	(0.0073)	(0.0065)	(0.0092)	(0.0103)	(0.0102)
	[0.9072]	[0.7092]	[0.5679]	[0.5150]	[0.3788]	[0.4341]	[0.9162]	[0.8680]
Treated {Control period, t=2014/15}	0.0649	0.0422	0.0437	0.0393	0.0318	0.0071	0.0555	0.0494
	(0.0144)	(0.0118)	(0.0115)	(0.0110)	(0.0099)	(0.0128)	(0.0164)	(0.0158)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.4362]	[0.0000]	[0.0000]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0466	-0.0350	-0.0356	-0.0298	-0.0215	-0.0103	-0.0481	-0.0396
	(0.0115)	(0.0094)	(0.0092)	(0.0090)	(0.0084)	(0.0114)	(0.0125)	(0.0125)
	[0.0000]	[0.0002]	[0.0001]	[0.0010]	[0.0104]	[0.3657]	[0.0001]	[0.0015]
Treated × {Policy period, t=2016/17}	-0.0463	-0.0338	-0.0337	-0.0271	-0.0184	-0.0190	-0.0428	-0.0322
	(0.0119)	(0.0097)	(0.0094)	(0.0092)	(0.0086)	(0.0117)	(0.0132)	(0.0128)
	[0.0001]	[0.0005]	[0.0004]	[0.0032]	[0.0332]	[0.1049]	[0.0012]	[0.0120]
Treated × {Policy period, t=2017/18}	-0.0426	-0.0284	-0.0287	-0.0235	-0.0160	-0.0036	-0.0342	-0.0257
	(0.0129)	(0.0101)	(0.0098)	(0.0095)	(0.0087)	(0.0116)	(0.0143)	(0.0136)
	[0.0009]	[0.0050]	[0.0035]	[0.0134]	[0.0674]	[0.7552]	[0.0165]	[0.0593]
Treated × {Policy period, t=2018/19}	-0.0382	-0.0274	-0.0273	-0.0239	-0.0173	-0.0075	-0.0396	-0.0365
	(0.0115)	(0.0091)	(0.0089)	(0.0087)	(0.0081)	(0.0113)	(0.0121)	(0.0120)
	[0.0009]	[0.0026]	[0.0020]	[0.0061]	[0.0315]	[0.5055]	[0.0010]	[0.0023]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0041	0.0493	0.0631	0.0706	0.0904	0.3763	0.073	0.0812
N	138,532	138,532	138,532	138,380	137,432	83,712	83,673	83,419

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (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 - Sensitivity Check, plus 50p

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Treated × {Control period, t=2011/12}	-0.0038	0.0006	0.0012	0.0024	0.0029	0.0155	0.0103	0.0127
	(0.0086)	(0.0082)	(0.0083)	(0.0078)	(0.0080)	(0.0099)	(0.0113)	(0.0107)
	[0.6577]	[0.9446]	[0.8853]	[0.7631]	[0.7169]	[0.1174]	[0.3634]	[0.2353]
Treated × {Control period, t=2012/13}	-0.0076	-0.0035	-0.0025	-0.0042	-0.0037	0.0136	-0.0046	-0.0073
	(0.0086)	(0.0081)	(0.0080)	(0.0077)	(0.0080)	(0.0104)	(0.0107)	(0.0103)
	[0.3795]	[0.6634]	[0.7520]	[0.5872]	[0.6447]	[0.1917]	[0.6658]	[0.4780]
Treated × {Control period, t=2013/14}	0.0008	0.0028	0.0047	0.0070	0.0032	0.0042	0.0069	0.0081
	(0.0060)	(0.0057)	(0.0057)	(0.0054)	(0.0055)	(0.0078)	(0.0079)	(0.0072)
	[0.9380]	[0.7642]	[0.6163]	[0.4360]	[0.7205]	[0.6767]	[0.6040]	[0.5401]
Treated {Control period, t=2014/15}	0.0402	0.0243	0.0257	0.0235	0.0231	-0.0031	0.0271	0.0250
	(0.0098)	(0.0092)	(0.0094)	(0.0090)	(0.0090)	(0.0101)	(0.0133)	(0.0131)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.6892]	[0.0006]	[0.0006]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0199	-0.0175	-0.0186	-0.0147	-0.0120	-0.0019	-0.0207	-0.0154
	(0.0082)	(0.0077)	(0.0077)	(0.0074)	(0.0074)	(0.0100)	(0.0104)	(0.0101)
	[0.0155]	[0.0235]	[0.0163]	[0.0476]	[0.1082]	[0.8526]	[0.0478]	[0.1245]
Treated × {Policy period, t=2016/17}	-0.0182	-0.0136	-0.0136	-0.0116	-0.0092	0.0054	-0.0082	-0.0047
	(0.0091)	(0.0083)	(0.0083)	(0.0078)	(0.0077)	(0.0103)	(0.0116)	(0.0108)
	[0.0440]	[0.1004]	[0.1005]	[0.1389]	[0.2323]	[0.6033]	[0.4790]	[0.6618]
Treated × {Policy period, t=2017/18}	-0.0205	-0.0137	-0.0137	-0.0114	-0.0108	0.0091	-0.0094	-0.0062
	(0.0096)	(0.0085)	(0.0084)	(0.0079)	(0.0078)	(0.0100)	(0.0120)	(0.0112)
	[0.0330]	[0.1069]	[0.1010]	[0.1499]	[0.1681]	[0.3641]	[0.4371]	[0.5817]
Treated × {Policy period, t=2018/19}	-0.0172	-0.0139	-0.0143	-0.0135	-0.0136	0.0045	-0.0168	-0.0154
	(0.0085)	(0.0076)	(0.0076)	(0.0072)	(0.0072)	(0.0099)	(0.0103)	(0.0095)
	[0.0438]	[0.0672]	[0.0610]	[0.0600]	[0.0572]	[0.6495]	[0.1028]	[0.1053]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0025	0.0482	0.0607	0.0678	0.0868	0.3745	0.0691	0.0778
N	160,272	160,272	160,272	160,147	159,177	97,853	97,816	97,596

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (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 switching - Sensitivity Check, plus 10p - WEIGHTED

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Treated × {Control period, t=2011/12}	-0.0180	-0.0115	-0.0137	-0.0100	-0.0010	-0.0007	-0.0156	-0.0140
	(0.0144)	(0.0127)	(0.0124)	(0.0117)	(0.0115)	(0.0159)	(0.0184)	(0.0165)
	[0.2116]	[0.3659]	[0.2684]	[0.3945]	[0.9337]	[0.9640]	[0.3990]	[0.3948]
Treated × {Control period, t=2012/13}	-0.0242	-0.0220	-0.0223	-0.0221	-0.0146	-0.0109	-0.0304	-0.0353
	(0.0137)	(0.0120)	(0.0119)	(0.0116)	(0.0112)	(0.0160)	(0.0169)	(0.0162)
	[0.0771]	[0.0666]	[0.0622]	[0.0566]	[0.1924]	[0.4951]	[0.0727]	[0.0292]
Treated × {Control period, t=2013/14}	-0.0131	-0.0081	-0.0060	-0.0042	-0.0001	0.0032	-0.0073	-0.0020
	(0.0099)	(0.0085)	(0.0084)	(0.0082)	(0.0080)	(0.0118)	(0.0122)	(0.0116)
	[0.3500]	[0.5121]	[0.6163]	[0.7176]	[0.9942]	[0.8440]	[0.6733]	[0.8983]
Treated {Control period, t=2014/15}	0.0689	0.0443	0.0462	0.0417	0.0346	0.0097	0.0610	0.0557
	(0.0140)	(0.0123)	(0.0120)	(0.0115)	(0.0112)	(0.0163)	(0.0173)	(0.0159)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.4101]	[0.0000]	[0.0000]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0403	-0.0330	-0.0335	-0.0287	-0.0213	-0.0087	-0.0460	-0.0386
	(0.0117)	(0.0104)	(0.0103)	(0.0102)	(0.0101)	(0.0142)	(0.0144)	(0.0139)
	[0.0006]	[0.0015]	[0.0011]	[0.0048]	[0.0346]	[0.5412]	[0.0014]	[0.0055]
Treated × {Policy period, t=2016/17}	-0.0413	-0.0322	-0.0327	-0.0277	-0.0188	-0.0256	-0.0416	-0.0334
	(0.0123)	(0.0108)	(0.0107)	(0.0104)	(0.0104)	(0.0149)	(0.0153)	(0.0145)
	[0.0008]	[0.0030]	[0.0022]	[0.0079]	[0.0695]	[0.0859]	[0.0065]	[0.0215]
Treated × {Policy period, t=2017/18}	-0.0472	-0.0320	-0.0325	-0.0286	-0.0206	-0.0054	-0.0369	-0.0285
	(0.0126)	(0.0109)	(0.0107)	(0.0105)	(0.0103)	(0.0148)	(0.0157)	(0.0150)
	[0.0002]	[0.0034]	[0.0023]	[0.0063]	[0.0458]	[0.7164]	[0.0192]	[0.0577]
Treated × {Policy period, t=2018/19}	-0.0466	-0.0322	-0.0327	-0.0283	-0.0232	-0.0186	-0.0445	-0.0403
	(0.0118)	(0.0103)	(0.0101)	(0.0100)	(0.0100)	(0.0139)	(0.0144)	(0.0138)
	[0.0001]	[0.0018]	[0.0013]	[0.0048]	[0.0199]	[0.1811]	[0.0020]	[0.0035]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0039	0.0523	0.0707	0.0795	0.1033	0.3953	0.0832	0.0964
N	124,955	124,955	124,955	124,778	123,849	74,528	74,488	74,217

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (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 - Sensitivity Check, plus 25p - WEIGHTED

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Treated × {Control period, t=2011/12}	-0.0176	-0.0099	-0.0125	-0.0101	-0.0033	-0.0011	-0.0217	-0.0192
	(0.0138)	(0.0118)	(0.0116)	(0.0108)	(0.0105)	(0.0135)	(0.0169)	(0.0148)
	[0.2021]	[0.3995]	[0.2810]	[0.3492]	[0.7549]	[0.9357]	[0.1985]	[0.1963]
Treated × {Control period, t=2012/13}	-0.0277	-0.0203	-0.0206	-0.0197	-0.0130	-0.0182	-0.0385	-0.0389
	(0.0129)	(0.0109)	(0.0108)	(0.0107)	(0.0101)	(0.0136)	(0.0150)	(0.0148)
	[0.0320]	[0.0612]	[0.0555]	[0.0648]	[0.1974]	[0.1808]	[0.0103]	[0.0087]
Treated × {Control period, t=2013/14}	0.0001	0.0028	0.0044	0.0055	0.0074	0.0116	0.0010	0.0009
	(0.0097)	(0.0079)	(0.0078)	(0.0076)	(0.0073)	(0.0103)	(0.0110)	(0.0107)
	[0.9918]	[0.8119]	[0.7022]	[0.6191]	[0.4861]	[0.4169]	[0.9535]	[0.9541]
Treated {Control period, t=2014/15}	0.0637	0.0393	0.0405	0.0371	0.0311	0.0083	0.0558	0.0512
	(0.0139)	(0.0117)	(0.0115)	(0.0112)	(0.0106)	(0.0143)	(0.0169)	(0.0161)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.4171]	[0.0000]	[0.0000]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0439	-0.0338	-0.0348	-0.0314	-0.0246	-0.0147	-0.0513	-0.0432
	(0.0116)	(0.0099)	(0.0098)	(0.0097)	(0.0095)	(0.0128)	(0.0135)	(0.0133)
	[0.0002]	[0.0006]	[0.0004]	[0.0013]	[0.0094]	[0.2532]	[0.0001]	[0.0011]
Treated × {Policy period, t=2016/17}	-0.0455	-0.0346	-0.0345	-0.0299	-0.0226	-0.0253	-0.0436	-0.0357
	(0.0119)	(0.0102)	(0.0100)	(0.0099)	(0.0097)	(0.0132)	(0.0142)	(0.0136)
	[0.0001]	[0.0007]	[0.0006]	[0.0024]	[0.0194]	[0.0545]	[0.0021]	[0.0089]
Treated × {Policy period, t=2017/18}	-0.0375	-0.0250	-0.0249	-0.0221	-0.0161	-0.0043	-0.0298	-0.0231
	(0.0124)	(0.0103)	(0.0101)	(0.0100)	(0.0097)	(0.0129)	(0.0149)	(0.0143)
	[0.0025]	[0.0151]	[0.0140]	[0.0268]	[0.0961]	[0.7367]	[0.0445]	[0.1051]
Treated × {Policy period, t=2018/19}	-0.0384	-0.0275	-0.0272	-0.0244	-0.0195	-0.0142	-0.0421	-0.0409
	(0.0117)	(0.0097)	(0.0096)	(0.0094)	(0.0092)	(0.0126)	(0.0132)	(0.0128)
	[0.0010]	[0.0045]	[0.0044]	[0.0096]	[0.0344]	[0.2611]	[0.0015]	[0.0014]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0037	0.0522	0.0686	0.0778	0.1013	0.3954	0.0805	0.0928
N	138,491	138,491	138,491	138,339	137,393	83,678	83,639	83,385

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI).

TABLE A13: 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 - WEIGHTED

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Treated × {Control period, t=2011/12}	-0.0094	-0.0047	-0.0040	-0.0037	-0.0034	0.0151	0.0105	0.0129
	(0.0093)	(0.0089)	(0.0090)	(0.0087)	(0.0089)	(0.0110)	(0.0120)	(0.0115)
	[0.3125]	[0.5981]	[0.6563]	[0.6685]	[0.7002]	[0.1694]	[0.3853]	[0.2633]
Treated × {Control period, t=2012/13}	-0.0086	-0.0038	-0.0018	-0.0044	-0.0035	0.0130	-0.0029	-0.0064
	(0.0093)	(0.0088)	(0.0088)	(0.0086)	(0.0088)	(0.0116)	(0.0116)	(0.0113)
	[0.3566]	[0.6697]	[0.8417]	[0.6121]	[0.6963]	[0.2632]	[0.8035]	[0.5723]
Treated × {Control period, t=2013/14}	-0.0048	-0.0028	-0.0005	0.0021	-0.0014	0.0021	0.0063	0.0073
	(0.0066)	(0.0063)	(0.0063)	(0.0061)	(0.0062)	(0.0088)	(0.0084)	(0.0079)
	[0.6374]	[0.7681]	[0.9593]	[0.8284]	[0.8802]	[0.8514]	[0.6503]	[0.5948]
Treated {Control period, t=2014/15}	0.0445	0.0258	0.0261	0.0241	0.0238	-0.0026	0.0270	0.0251
	(0.0102)	(0.0096)	(0.0097)	(0.0095)	(0.0095)	(0.0112)	(0.0140)	(0.0137)
	[0.0000]	[0.0000]	[0.0000]	[0.0001]	[0.0001]	[0.7698]	[0.0014]	[0.0015]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0237	-0.0229	-0.0231	-0.0208	-0.0188	-0.0068	-0.0240	-0.0184
	(0.0089)	(0.0085)	(0.0086)	(0.0084)	(0.0085)	(0.0115)	(0.0113)	(0.0110)
	[0.0079]	[0.0074]	[0.0070]	[0.0139]	[0.0276]	[0.5523]	[0.0345]	[0.0962]
Treated × {Policy period, t=2016/17}	-0.0216	-0.0183	-0.0169	-0.0162	-0.0134	0.0014	-0.0073	-0.0052
	(0.0094)	(0.0088)	(0.0089)	(0.0086)	(0.0087)	(0.0116)	(0.0123)	(0.0116)
	[0.0218]	[0.0380]	[0.0575]	[0.0608]	[0.1224]	[0.9040]	[0.5511]	[0.6533]
Treated × {Policy period, t=2017/18}	-0.0247	-0.0178	-0.0169	-0.0154	-0.0143	0.0059	-0.0087	-0.0062
	(0.0098)	(0.0089)	(0.0089)	(0.0087)	(0.0087)	(0.0113)	(0.0126)	(0.0120)
	[0.0114]	[0.0458]	[0.0589]	[0.0752]	[0.0989]	[0.6002]	[0.4898]	[0.6015]
Treated × {Policy period, t=2018/19}	-0.0242	-0.0198	-0.0191	-0.0186	-0.0186	-0.0021	-0.0206	-0.0189
	(0.0093)	(0.0084)	(0.0085)	(0.0082)	(0.0083)	(0.0112)	(0.0114)	(0.0106)
	[0.0092]	[0.0189]	[0.0254]	[0.0243]	[0.0247]	[0.8536]	[0.0702]	[0.0746]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R²	0.0025	0.052	0.0664	0.0754	0.0982	0.3945	0.0764	0.0892
N	160,222	160,222	160,222	160,097	159,127	97,814	97,777	97,557

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 (IV)-(VI) are smaller as singletons are dropped.
Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI).

TABLE A14: 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)	(VIII)
Treated × {Control period, t=2011/12}	-0.0237	-0.0202	-0.0205	-0.0100	-0.0036	0.0029	-0.0217	-0.0102
	(0.0122)	(0.0116)	(0.0116)	(0.0120)	(0.0106)	(0.0111)	(0.0168)	(0.0166)
	[0.0528]	[0.0824]	[0.0763]	[0.4043]	[0.7380]	[0.7979]	[0.1964]	[0.5397]
Treated × {Control period, t=2012/13}	-0.0200	-0.0197	-0.0206	-0.0137	-0.0091	0.0020	-0.0300	-0.0214
	(0.0108)	(0.0103)	(0.0103)	(0.0107)	(0.0099)	(0.0121)	(0.0145)	(0.0145)
	[0.0640]	[0.0553]	[0.0443]	[0.2016]	[0.3569]	[0.8701]	[0.0380]	[0.1408]
Treated × {Control period, t=2013/14}	0.0099	0.0094	0.0096	0.0120	0.0123	0.0149	0.0152	0.0178
	(0.0075)	(0.0070)	(0.0070)	(0.0075)	(0.0067)	(0.0078)	(0.0099)	(0.0100)
	[0.4169]	[0.4188]	[0.4097]	[0.3149]	[0.2491]	[0.2204]	[0.3757]	[0.2963]
Treated {Control period, t=2014/15}	0.1028	0.0578	0.0595	0.0560	0.0466	0.0136	0.0722	0.0694
	(0.0122)	(0.0116)	(0.0116)	(0.0119)	(0.0107)	(0.0122)	(0.0172)	(0.0171)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0823]	[0.0000]	[0.0000]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0423	-0.0334	-0.0335	-0.0305	-0.0232	-0.0140	-0.0500	-0.0470
	(0.0097)	(0.0089)	(0.0089)	(0.0094)	(0.0082)	(0.0096)	(0.0123)	(0.0124)
	[0.0000]	[0.0002]	[0.0002]	[0.0011]	[0.0049]	[0.1440]	[0.0000]	[0.0002]
Treated × {Policy period, t=2016/17}	-0.0412	-0.0285	-0.0289	-0.0278	-0.0205	-0.0145	-0.0376	-0.0343
	(0.0113)	(0.0097)	(0.0097)	(0.0099)	(0.0086)	(0.0100)	(0.0138)	(0.0135)
	[0.0003]	[0.0034]	[0.0028]	[0.0049]	[0.0180]	[0.1462]	[0.0066]	[0.0109]
Treated × {Policy period, t=2017/18}	-0.0463	-0.0302	-0.0307	-0.0286	-0.0217	-0.0041	-0.0407	-0.0376
	(0.0119)	(0.0101)	(0.0100)	(0.0101)	(0.0089)	(0.0120)	(0.0145)	(0.0142)
	[0.0001]	[0.0027]	[0.0022]	[0.0046]	[0.0151]	[0.7308]	[0.0051]	[0.0080]
Treated × {Policy period, t=2018/19}	-0.0441	-0.0279	-0.0284	-0.0274	-0.0225	-0.0097	-0.0412	-0.0387
	(0.0103)	(0.0089)	(0.0089)	(0.0093)	(0.0082)	(0.0096)	(0.0124)	(0.0125)
	[0.0000]	[0.0018]	[0.0013]	[0.0033]	[0.0058]	[0.3077]	[0.0009]	[0.0019]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R²	0.0043	0.0418	0.0452	0.0504	0.0599	0.3501	0.0438	0.0503
N	825,854	825,854	825,854	825,778	825,620	630,529	630,529	630,473

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 (IV)-(VI) are smaller as singletons are dropped.
Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI).

TABLE A15: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching – Consistent control group

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Treated × {Control period, t=2011/12}	-0.0058	-0.0022	-0.0007	-0.0014	0.0009	0.0074	0.0133	0.0095
	(0.0140)	(0.0129)	(0.0128)	(0.0127)	(0.0128)	(0.0197)	(0.0190)	(0.0185)
	[0.6786]	[0.8638]	[0.9559]	[0.9131]	[0.9416]	[0.7069]	[0.4827]	[0.6078]
Treated × {Control period, t=2012/13}	-0.0095	-0.0069	-0.0027	-0.0080	-0.0018	0.0078	-0.0144	-0.0166
	(0.0133)	(0.0122)	(0.0123)	(0.0123)	(0.0125)	(0.0210)	(0.0176)	(0.0180)
	[0.4775]	[0.5735]	[0.8256]	[0.5193]	[0.8820]	[0.7098]	[0.4121]	[0.3585]
Treated × {Control period, t=2013/14}	0.0045	0.0044	0.0082	0.0073	0.0088	0.0125	0.0255	0.0223
	(0.0095)	(0.0086)	(0.0086)	(0.0087)	(0.0087)	(0.0150)	(0.0122)	(0.0125)
	[0.7507]	[0.7320]	[0.5282]	[0.5651]	[0.4928]	[0.5468]	[0.2062]	[0.2646]
Treated {Control period, t=2014/15}	0.0716	0.0413	0.0418	0.0386	0.0341	0.0075	0.0583	0.0503
	(0.0142)	(0.0129)	(0.0130)	(0.0128)	(0.0128)	(0.0208)	(0.0202)	(0.0200)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0001]	[0.6178]	[0.0000]	[0.0001]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0399	-0.0328	-0.0327	-0.0303	-0.0234	-0.0181	-0.0471	-0.0390
	(0.0117)	(0.0107)	(0.0108)	(0.0109)	(0.0109)	(0.0174)	(0.0147)	(0.0155)
	[0.0006]	[0.0023]	[0.0025]	[0.0055]	[0.0323]	[0.2973]	[0.0014]	[0.0121]
Treated × {Policy period, t=2016/17}	-0.0391	-0.0298	-0.0283	-0.0255	-0.0198	-0.0245	-0.0389	-0.0287
	(0.0127)	(0.0114)	(0.0114)	(0.0114)	(0.0113)	(0.0183)	(0.0161)	(0.0164)
	[0.0021]	[0.0088]	[0.0130]	[0.0248]	[0.0810]	[0.1810]	[0.0154]	[0.0798]
Treated × {Policy period, t=2017/18}	-0.0463	-0.0314	-0.0300	-0.0247	-0.0192	0.0057	-0.0368	-0.0200
	(0.0129)	(0.0113)	(0.0113)	(0.0111)	(0.0111)	(0.0176)	(0.0168)	(0.0164)
	[0.0003]	[0.0055]	[0.0077]	[0.0263]	[0.0854]	[0.7461]	[0.0289]	[0.2231]
Treated × {Policy period, t=2018/19}	-0.0417	-0.0295	-0.0280	-0.0269	-0.0234	-0.0160	-0.0426	-0.0380
	(0.0124)	(0.0108)	(0.0109)	(0.0109)	(0.0109)	(0.0175)	(0.0148)	(0.0153)
	[0.0008]	[0.0062]	[0.0099]	[0.0133]	[0.0313]	[0.3604]	[0.0041]	[0.0132]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0052	0.0543	0.0733	0.0832	0.1081	0.4038	0.0811	0.1029
N	113,517	113,517	113,517	113,347	112,300	65,409	65,342	65,125

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 (IV)-(VI) are smaller as singletons are dropped.
Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI).

TABLE A16: 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 - Consistent control group

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Treated × {Control period, t=2011/12}	-0.0095	-0.0031	-0.0024	-0.0013	0.0016	0.0109	0.0041	0.0064
	(0.0131)	(0.0117)	(0.0116)	(0.0110)	(0.0109)	(0.0166)	(0.0173)	(0.0159)
	[0.4682]	[0.7938]	[0.8395]	[0.9051]	[0.8827]	[0.5125]	[0.8104]	[0.6871]
Treated × {Control period, t=2012/13}	-0.0181	-0.0143	-0.0129	-0.0151	-0.0122	0.0109	-0.0235	-0.0254
	(0.0119)	(0.0104)	(0.0105)	(0.0103)	(0.0100)	(0.0170)	(0.0154)	(0.0149)
	[0.1298]	[0.1699]	[0.2175]	[0.1412]	[0.2236]	[0.5208]	[0.1260]	[0.0869]
Treated × {Control period, t=2013/14}	-0.0008	0.0019	0.0052	0.0064	0.0057	0.0149	0.0072	0.0075
	(0.0085)	(0.0073)	(0.0073)	(0.0071)	(0.0069)	(0.0125)	(0.0109)	(0.0102)
	[0.9537]	[0.8677]	[0.6593]	[0.5667]	[0.5944]	[0.3585]	[0.6837]	[0.6557]
Treated {Control period, t=2014/15}	0.0714	0.0456	0.0469	0.0435	0.0395	0.0037	0.0572	0.0531
	(0.0130)	(0.0115)	(0.0117)	(0.0112)	(0.0108)	(0.0162)	(0.0177)	(0.0169)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.7691]	[0.0000]	[0.0000]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0438	-0.0351	-0.0357	-0.0299	-0.0235	-0.0108	-0.0471	-0.0393
	(0.0104)	(0.0093)	(0.0093)	(0.0091)	(0.0089)	(0.0144)	(0.0134)	(0.0130)
	[0.0000]	[0.0002]	[0.0001]	[0.0011]	[0.0082]	[0.4530]	[0.0004]	[0.0025]
Treated × {Policy period, t=2016/17}	-0.0424	-0.0311	-0.0304	-0.0265	-0.0201	-0.0148	-0.0352	-0.0284
	(0.0117)	(0.0100)	(0.0099)	(0.0095)	(0.0092)	(0.0153)	(0.0146)	(0.0138)
	[0.0003]	[0.0018]	[0.0021]	[0.0054]	[0.0292]	[0.3318]	[0.0160]	[0.0397]
Treated × {Policy period, t=2017/18}	-0.0466	-0.0318	-0.0318	-0.0254	-0.0213	0.0104	-0.0338	-0.0249
	(0.0126)	(0.0105)	(0.0103)	(0.0098)	(0.0094)	(0.0147)	(0.0155)	(0.0146)
	[0.0002]	[0.0024]	[0.0020]	[0.0094]	[0.0243]	[0.4804]	[0.0290]	[0.0886]
Treated × {Policy period, t=2018/19}	-0.0426	-0.0319	-0.0319	-0.0304	-0.0278	-0.0051	-0.0425	-0.0409
	(0.0109)	(0.0092)	(0.0092)	(0.0089)	(0.0087)	(0.0146)	(0.0133)	(0.0126)
	[0.0001]	[0.0005]	[0.0005]	[0.0007]	[0.0014]	[0.7256]	[0.0014]	[0.0012]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0054	0.0508	0.0664	0.073	0.0937	0.3842	0.079	0.0879
N	123,340	123,340	123,340	123,182	122,163	71,974	71,913	71,690

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI).

TABLE A17: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching - Sensitivity Check, plus 25p – Consistent control group

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Treated × {Control period, t=2011/12}	-0.0118	-0.0050	-0.0049	-0.0028	-0.0005	0.0071	-0.0040	-0.0011
	(0.0116)	(0.0103)	(0.0103)	(0.0096)	(0.0096)	(0.0133)	(0.0148)	(0.0136)
	[0.3097]	[0.6277]	[0.6331]	[0.7738]	[0.9584]	[0.5943]	[0.7868]	[0.9333]
Treated × {Control period, t=2012/13}	-0.0181	-0.0123	-0.0114	-0.0117	-0.0094	0.0081	-0.0241	-0.0245
	(0.0110)	(0.0095)	(0.0095)	(0.0092)	(0.0091)	(0.0137)	(0.0132)	(0.0128)
	[0.0983]	[0.1951]	[0.2287]	[0.2059]	[0.3012]	[0.5541]	[0.0678]	[0.0559]
Treated × {Control period, t=2013/14}	0.0038	0.0065	0.0094	0.0108	0.0084	0.0065	0.0083	0.0088
	(0.0078)	(0.0067)	(0.0067)	(0.0064)	(0.0063)	(0.0104)	(0.0096)	(0.0090)
	[0.7620]	[0.5556]	[0.4027]	[0.3179]	[0.4175]	[0.6237]	[0.6197]	[0.5905]
Treated {Control period, t=2014/15}	0.0601	0.0374	0.0390	0.0351	0.0326	0.0055	0.0482	0.0440
	(0.0125)	(0.0111)	(0.0112)	(0.0108)	(0.0104)	(0.0132)	(0.0167)	(0.0164)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.5953]	[0.0000]	[0.0000]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0375	-0.0297	-0.0310	-0.0247	-0.0195	-0.0120	-0.0417	-0.0342
	(0.0099)	(0.0087)	(0.0087)	(0.0085)	(0.0083)	(0.0124)	(0.0121)	(0.0118)
	[0.0002]	[0.0006]	[0.0004]	[0.0037]	[0.0195]	[0.3349]	[0.0006]	[0.0038]
Treated × {Policy period, t=2016/17}	-0.0357	-0.0259	-0.0258	-0.0218	-0.0167	-0.0124	-0.0282	-0.0218
	(0.0107)	(0.0092)	(0.0092)	(0.0088)	(0.0086)	(0.0131)	(0.0133)	(0.0125)
	[0.0008]	[0.0050]	[0.0049]	[0.0129]	[0.0526]	[0.3458]	[0.0335]	[0.0815]
Treated × {Policy period, t=2017/18}	-0.0371	-0.0248	-0.0253	-0.0199	-0.0175	0.0035	-0.0267	-0.0190
	(0.0116)	(0.0096)	(0.0095)	(0.0090)	(0.0087)	(0.0125)	(0.0141)	(0.0132)
	[0.0014]	[0.0099]	[0.0074]	[0.0266]	[0.0444]	[0.7799]	[0.0586]	[0.1490]
Treated × {Policy period, t=2018/19}	-0.0328	-0.0244	-0.0243	-0.0221	-0.0207	-0.0053	-0.0344	-0.0318
	(0.0102)	(0.0085)	(0.0085)	(0.0082)	(0.0081)	(0.0124)	(0.0120)	(0.0113)
	[0.0013]	[0.0041]	[0.0042]	[0.0069]	[0.0101]	[0.6685]	[0.0040]	[0.0049]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0043	0.0492	0.0634	0.0702	0.0902	0.38	0.0742	0.0829
N	136,744	136,744	136,744	136,598	135,591	81,390	81,342	81,114

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 (IV)-(VI) are smaller as singletons are dropped.
Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI).

TABLE A18: 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)	(VIII)
Treated × {Control period, t=2011/12}	-0.0029	-0.0091	-0.0100	-0.0058	-0.0091	-0.0104	-0.0017	-0.0054
	(0.0130)	(0.0120)	(0.0121)	(0.0121)	(0.0127)	(0.0153)	(0.0154)	(0.0159)
	[0.8253]	[0.4475]	[0.4067]	[0.6345]	[0.4745]	[0.4946]	[0.9106]	[0.7360]
Treated × {Control period, t=2012/13}	-0.0066	-0.0067	-0.0076	-0.0046	-0.0066	-0.0219	-0.0113	-0.0092
	(0.0157)	(0.0139)	(0.0136)	(0.0127)	(0.0135)	(0.0142)	(0.0143)	(0.0137)
	[0.6759]	[0.6281]	[0.5752]	[0.7145]	[0.6249]	[0.1237]	[0.4288]	[0.4997]
Treated × {Control period, t=2013/14}	-0.0107	-0.0107	-0.0128	-0.0115	-0.0155	0.0089	-0.0015	-0.0026
	(0.0098)	(0.0085)	(0.0084)	(0.0084)	(0.0090)	(0.0106)	(0.0098)	(0.0099)
	[0.4450]	[0.3922]	[0.3156]	[0.3572]	[0.2397]	[0.5763]	[0.9257]	[0.8616]
Treated {Control period, t=2014/15}	0.0132	0.0135	0.0143	0.0135	0.0158	0.0093	0.0062	0.0087
	(0.0140)	(0.0126)	(0.0128)	(0.0125)	(0.0131)	(0.0159)	(0.0160)	(0.0151)
	[0.1770]	[0.1113]	[0.0890]	[0.1078]	[0.0777]	[0.3791]	[0.5296]	[0.3785]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0084	-0.0110	-0.0105	-0.0069	-0.0088	-0.0087	-0.0054	-0.0027
	(0.0110)	(0.0098)	(0.0098)	(0.0097)	(0.0103)	(0.0119)	(0.0113)	(0.0115)
	[0.4440]	[0.2601]	[0.2843]	[0.4757]	[0.3930]	[0.4640]	[0.6318]	[0.8125]
Treated × {Policy period, t=2016/17}	-0.0148	-0.0187	-0.0189	-0.0201	-0.0251	-0.0132	-0.0058	-0.0115
	(0.0120)	(0.0108)	(0.0108)	(0.0108)	(0.0114)	(0.0132)	(0.0125)	(0.0127)
	[0.2160]	[0.0833]	[0.0804]	[0.0626]	[0.0278]	[0.3156]	[0.6429]	[0.3645]
Treated × {Policy period, t=2017/18}	-0.0121	-0.0091	-0.0102	-0.0087	-0.0104	-0.0097	-0.0016	-0.0021
	(0.0120)	(0.0109)	(0.0109)	(0.0109)	(0.0115)	(0.0144)	(0.0131)	(0.0132)
	[0.3111]	[0.4043]	[0.3482]	[0.4224]	[0.3677]	[0.4999]	[0.9010]	[0.8710]
Treated × {Policy period, t=2018/19}	-0.0163	-0.0181	-0.0183	-0.0202	-0.0248	-0.0174	-0.0142	-0.0153
	(0.0121)	(0.0106)	(0.0106)	(0.0106)	(0.0111)	(0.0136)	(0.0125)	(0.0124)
	[0.1768]	[0.0878]	[0.0836]	[0.0561]	[0.0261]	[0.2025]	[0.2545]	[0.2186]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0004	0.0348	0.0574	0.0716	0.1272	0.4063	0.0727	0.0972
N	73,645	73,645	73,645	73,565	72,516	41,834	41,725	41,677

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 (IV)-(VI) are smaller as singletons are dropped.
Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI).

TABLE A19: 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)	(VIII)
Treated × {Control period, t=2011/12}	0.0112	0.0120	0.0113	0.0129	0.0156	0.0068	0.0007	0.0013
	(0.0104)	(0.0093)	(0.0091)	(0.0088)	(0.0089)	(0.0142)	(0.0120)	(0.0117)
	[0.2777]	[0.1965]	[0.2122]	[0.1441]	[0.0808]	[0.6333]	[0.9508]	[0.9083]
Treated × {Control period, t=2012/13}	0.0076	0.0059	0.0075	0.0100	0.0117	0.0095	-0.0005	0.0026
	(0.0103)	(0.0093)	(0.0093)	(0.0091)	(0.0090)	(0.0141)	(0.0125)	(0.0120)
	[0.4637]	[0.5254]	[0.4212]	[0.2710]	[0.1954]	[0.5030]	[0.9695]	[0.8309]
Treated × {Control period, t=2013/14}	0.0175	0.0158	0.0168	0.0181	0.0191	0.0246	0.0204	0.0236
	(0.0075)	(0.0067)	(0.0067)	(0.0064)	(0.0064)	(0.0099)	(0.0090)	(0.0085)
	[0.1019]	[0.1000]	[0.0790]	[0.0482]	[0.0367]	[0.0941]	[0.1175]	[0.0577]
Treated {Control period, t=2014/15}	0.0249	0.0096	0.0106	0.0069	0.0054	-0.0073	0.0164	0.0122
	(0.0107)	(0.0096)	(0.0095)	(0.0092)	(0.0092)	(0.0147)	(0.0130)	(0.0124)
	[0.0009]	[0.1502]	[0.1131]	[0.2771]	[0.3956]	[0.4637]	[0.0677]	[0.1496]
NLW period - DiD effects:								
Treated × {Policy period, t=2015/16}	-0.0009	0.0024	0.0034	0.0079	0.0080	0.0062	-0.0084	-0.0038
	(0.0088)	(0.0080)	(0.0080)	(0.0078)	(0.0079)	(0.0116)	(0.0106)	(0.0102)
	[0.9178]	[0.7601]	[0.6682]	[0.3096]	[0.3068]	[0.5932]	[0.4313]	[0.7139]
Treated × {Policy period, t=2016/17}	-0.0105	-0.0033	-0.0024	0.0024	0.0040	-0.0015	-0.0058	-0.0001
	(0.0092)	(0.0081)	(0.0081)	(0.0078)	(0.0079)	(0.0121)	(0.0109)	(0.0104)
	[0.2527]	[0.6843]	[0.7647]	[0.7604]	[0.6087]	[0.9024]	[0.5933]	[0.9901]
Treated × {Policy period, t=2017/18}	-0.0150	-0.0055	-0.0061	0.0006	0.0032	0.0065	-0.0121	-0.0035
	(0.0096)	(0.0083)	(0.0083)	(0.0079)	(0.0080)	(0.0123)	(0.0114)	(0.0107)
	[0.1182]	[0.5081]	[0.4610]	[0.9420]	[0.6876]	[0.5952]	[0.2897]	[0.7421]
Treated × {Policy period, t=2018/19}	-0.0112	-0.0024	-0.0026	0.0023	0.0045	0.0012	-0.0073	-0.0022
	(0.0089)	(0.0078)	(0.0079)	(0.0076)	(0.0077)	(0.0115)	(0.0105)	(0.0101)
	[0.2080]	[0.7618]	[0.7434]	[0.7624]	[0.5564]	[0.9140]	[0.4834]	[0.8291]
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	No	No	No	No	No	No
TTWA × Period FEs	No	No	Yes	No	No	No	Yes	No
Region × Period × Occ 2-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ 3-digit FEs	No	No	No	No	Yes	No	No	No
Firm × Period FEs	No	No	No	No	No	Yes	No	No
R^2	0.0023	0.0422	0.0588	0.062	0.0842	0.2698	0.0681	0.0715
N	115,963	115,963	115,963	115,756	114,844	68,649	68,603	68,346

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 (IV)-(VI) are smaller as singletons are dropped.

Sample sizes in (VII) & (VIII) drop again because they estimate models (III) & (IV) starting with the sample in (VI).

TABLE A20: Estimated effects of the National Living Wage relative to other NMW rises on the probability of year-to-year firm switching: AGE-Based identification strategy

	(I)	(II)	(III)	(IV)
Treated × {Control period, t=2011/12}	0.0308	0.0298	0.0231	0.0332
	(0.0238)	(0.0231)	(0.0232)	(0.0232)
	[0.4014]	[0.4127]	[0.5260]	[0.3810]
Treated × {Control period, t=2012/13}	0.0097	0.0109	0.0136	0.0326
	(0.0363)	(0.0351)	(0.0352)	(0.0368)
	[0.7904]	[0.7573]	[0.6988]	[0.3757]
Treated × {Control period, t=2013/14}	0.0405	0.0360	0.0361	0.0380
	(0.0238)	(0.0231)	(0.0232)	(0.0232)
	[0.2535]	[0.2969]	[0.2945]	[0.2777]
Treated {Control period, t=2014/15}	-0.0708	-0.0616	-0.0644	-0.0777
	(0.0354)	(0.0345)	(0.0344)	(0.0350)
	[0.0030]	[0.0077]	[0.0055]	[0.0008]
NLW period - DiD effects:				
Treated × {Policy period, t=2015/16}	-0.0121	0.0149	0.0165	0.0340
	(0.0320)	(0.0309)	(0.0311)	(0.0320)
	[0.7052]	[0.6285]	[0.5961]	[0.2878]
Treated × {Policy period, t=2016/17}	-0.0251	-0.0112	-0.0060	0.0042
	(0.0350)	(0.0337)	(0.0336)	(0.0342)
	[0.4729]	[0.7398]	[0.8579]	[0.9012]
Treated × {Policy period, t=2017/18}	0.0113	0.0257	0.0311	0.0452
	(0.0364)	(0.0352)	(0.0352)	(0.0365)
	[0.7563]	[0.4645]	[0.3759]	[0.2152]
Treated × {Policy period, t=2018/19}	-0.0070	0.0093	0.0077	0.0156
	(0.0349)	(0.0338)	(0.0338)	(0.0347)
	[0.8418]	[0.7830]	[0.8206]	[0.6521]
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
R^2	0.0082	0.0493	0.0566	0.1148
N	16,704	16,704	16,704	16,704

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 A21: 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, t=2011/12}	0.0013	0.0014	0.0004	-0.0007
	(0.0091)	(0.0090)	(0.0092)	(0.0094)
	[0.9301]	[0.9231]	[0.9784]	[0.9631]
Treated × {Control period, t=2012/13}	-0.0059	-0.0060	-0.0057	-0.0066
	(0.0124)	(0.0125)	(0.0127)	(0.0134)
	[0.6366]	[0.6295]	[0.6539]	[0.6241]
Treated × {Control period, t=2013/14}	-0.0066	-0.0083	-0.0087	-0.0114
	(0.0091)	(0.0090)	(0.0092)	(0.0094)
	[0.6232]	[0.5381]	[0.5195]	[0.4127]
Treated {Control period, t=2014/15}	-0.0025	-0.0029	-0.0028	-0.0006
	(0.0135)	(0.0135)	(0.0135)	(0.0139)
	[0.7791]	[0.7482]	[0.7617]	[0.9506]
NLW period - DiD effects:				
Treated × {Policy period, t=2015/16}	-0.0056	-0.0051	-0.0043	-0.0039
	(0.0160)	(0.0160)	(0.0161)	(0.0166)
	[0.7265]	[0.7503]	[0.7889]	[0.8144]
Treated × {Policy period, t=2016/17}	-0.0172	-0.0163	-0.0169	-0.0201
	(0.0163)	(0.0161)	(0.0163)	(0.0168)
	[0.2902]	[0.3103]	[0.2985]	[0.2326]
Treated × {Policy period, t=2017/18}	0.0060	0.0073	0.0086	0.0025
	(0.0157)	(0.0158)	(0.0159)	(0.0174)
	[0.7037]	[0.6421]	[0.5902]	[0.8864]
Treated × {Policy period, t=2018/19}	-0.0134	-0.0128	-0.0133	-0.0103
	(0.0154)	(0.0153)	(0.0152)	(0.0153)
	[0.3864]	[0.4032]	[0.3839]	[0.4998]
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
R^2	0.0148	0.019	0.0249	0.0932
N	12,194	12,194	12,194	12,194

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 III, IV & VI, Table A5)
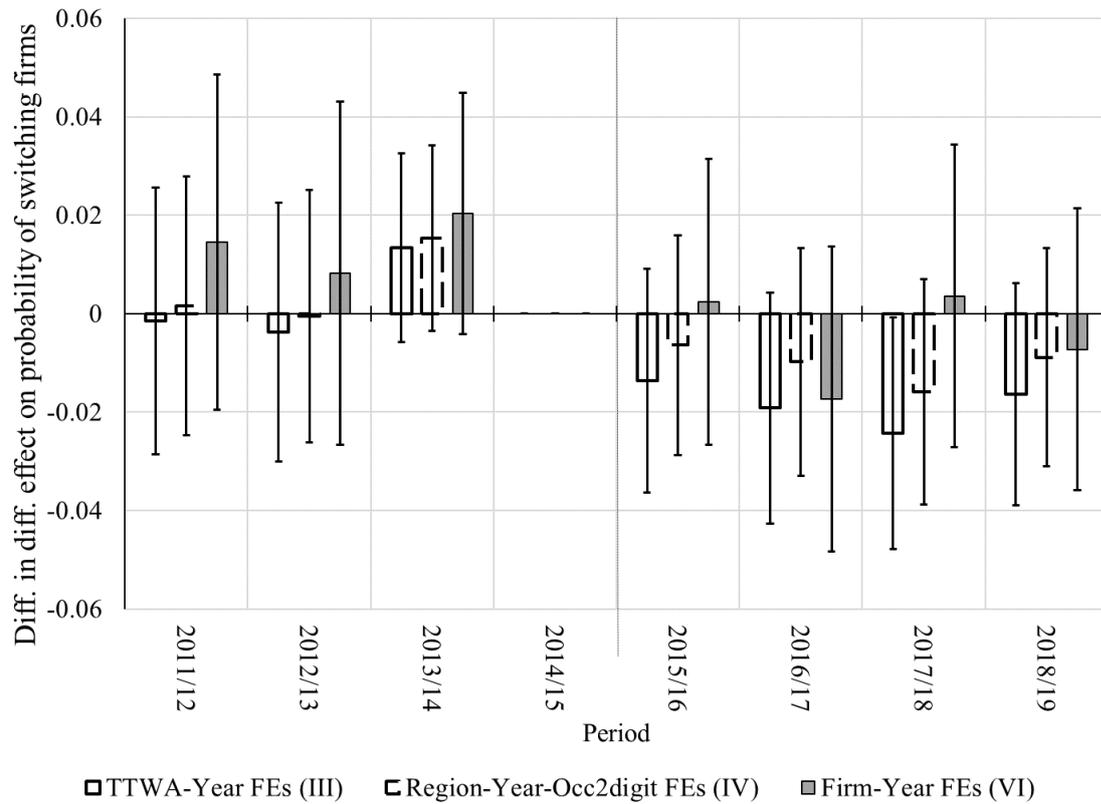
☐TTWA-Year FEs (III)   ⊏Region-Year-Occ2digit FEs (IV)   ▨Firm-Year FEs (VI)

Source: ASHE
Notes: The figure shows point estimates and 95% confidence intervals. All specifications control for: gender × age (5 categories) × hours worked (full-time/part-time); and tenure in the job (7 categories). (III) adds TTWA x Year FEs; (IV) adds Region × Year × Occ 2-digit FEs; (VI) adds Firm × Year FEs. 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 switching: allowing for wage-rate rounding effects and spillovers – WEIGHTED (Tables A5 & A11-A13)
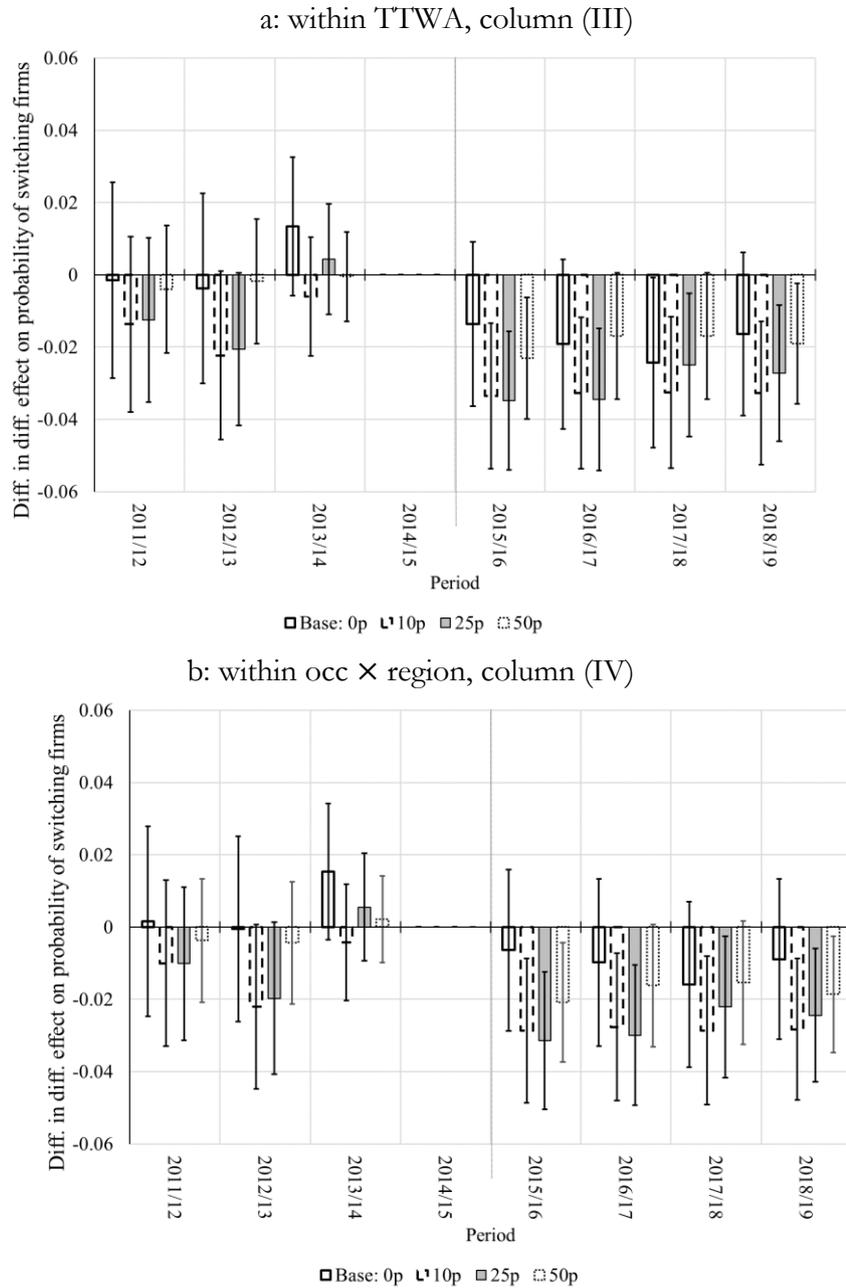
a: within TTWA, column (III)

b: within occ × region, column (IV)

Source: ASHE

Notes: The figure shows point estimates and 95% confidence intervals. Both specifications control for: gender × age (5 categories) × hours worked (full-time/part-time); and tenure in the job (7 categories). (III) adds TTWA × Year FEs; (IV) adds Region × Year × Occ 2-digit FEs. For other notes, see Tables A5 & A11-A13.

FIGURE A3: Differences-in-differences estimates of the effects of the National Living Wage on the probability of year-to-year firm & occupation switching (columns III, IV & VI, Table A16)

Source: ASHE

Notes: The figure shows point estimates and 95% confidence intervals. All specifications control for: gender × age (5 categories) × hours worked (full-time/part-time); and tenure in the job (7 categories). (III) adds TTWA × Year FEs; (IV) adds Region × Year × Occ 2-digit FEs; (VI) adds Firm x Year FEs. For other notes, see Table A16.