British Journal of Industrial Relations

WILEY

BJIR An International Journal of Employment Relations LSE

ORIGINAL ARTICLE OPEN ACCESS

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

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

[1]Faculty of Management, Bayes Business School, City St. Georges, University of London, London, UK | [2]Economic Statistics Centre of Excellence, London, UK | [3]Institute of Labor Economics (IZA), Bonn, Germany | [4]Stirling Management School, University of Stirling, Stirling, UK | [5]Social Research Institute, University College London, London, UK | [6]Department of Accounting, Economics and Finance, University of the West of England, Bristol, UK | [7]Competition and Markets Authority, London, UK

Correspondence: John Forth (john.forth@citystgeorges.ac.uk)

Funding: This study was supported by ADR UK (Administrative Data Research UK) and the Economic and Social Research Council (grant no. ES/T013877/1).

Keywords: low pay | National Living Wage | on-the-job search | UK labour market

ABSTRACT

In April 2016, the National Living Wage (NLW) raised the statutory wage floor for employees in the United Kingdom 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%. 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.

JEL Classification: J23, J38, J68, J88

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 and Shirley 2022). Such reviews typically conclude that minimum wage policies have improved wages with little or no impact on employment. In the United Kingdom 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, 2015; Dolton et al. 2012; Fidrmuc and Tena 2018; and for a meta-regression analysis, see de Linde Leonard et al. 2014). 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

This is an open access article under the terms of the Creative Commons Attribution License, which permits use, distribution and reproduction in any medium, provided the original work is properly cited.

© 2025 The Author(s). *British Journal of Industrial Relations* published by John Wiley & Sons Ltd.

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 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 United States 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. 2022), consistent with reduced search. But there is limited evidence in this regard for the United Kingdom. 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.

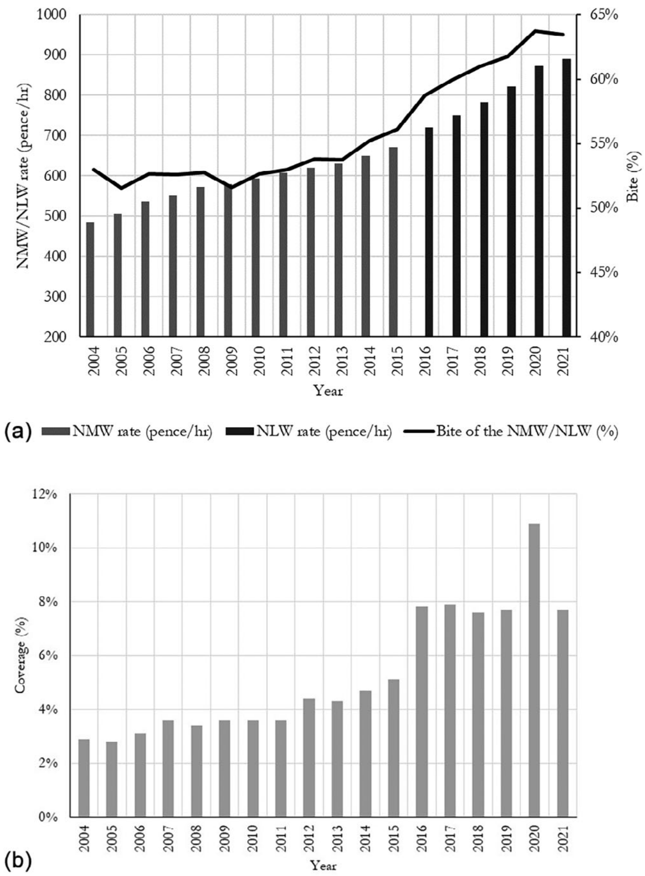

FIGURE 1 | Nominal rates, bite, and coverage of the UK National Minimum Wage (NMW) and National Living Wage (NLW among employees aged 25 or more, by year. (a) Nominal hourly rates and bite. (b) Coverage of employee jobs. *Source:* ASHE. *Note:* Bite and coverage of the NMW/NLW are estimated for all employees aged 25+, main job, with no loss of pay (except furlough), adult rates, using the revised ASHE weights developed by Forth et al. (2024). Ninety-five per cent confidence intervals around the estimates shown in Figure 1b are all within ± 0.2 percentage points.

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 programme and introduced the first statutory, national wage floor in the United Kingdom.[4] After substantial initial upratings, the decade from 2004 saw the adult rate rise at an average of 3% 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 2008–2009 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.

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

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

Treasury and Osborne 2015).[5] The NLW was set at £7.20 per hour in April 2016. This was 50 pence (7.5%) higher than the NMW rate of October 2015 (£6.70) and 70 pence (10.2%) 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% for all employees aged 25+, at a time when real median wage growth was 2.0%. The share of employee jobs among those aged 25+, paid at or below the minimum wage, rose from 5.1% in April 2015 to 7.8% in April 2016 (Figure 1b): 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 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' (Avram and Harkness 2025, 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% 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 2-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 United Kingdom (the UK Labour Force Survey, or the UK Household Longitudinal Study utilised by Avram and Harkness (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 and 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 to be 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, 6 months after the preceding ASHE and 6 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 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

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

to identify the impact of the particularly large NLW uprating in 2015/16.

We estimate the following using least squares:

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

where Y_i is the outcome of interest for worker observation i. Z_{ti} is a set of period dummy variables corresponding to when the worker is observed, where 2014/15 ($t = 4$) 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 (FEs) 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 two- or three-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, e.g., 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 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, δ_5, δ_6, δ_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 2-year period, t, that are at or above the wage floor applying in that year but below the floor that will apply in the second year of each t; these employees are directly affected by the policy change. The control group is all employees with earnings in the first year of each period, t, that are either at the incoming wage floor or up to 10% above it.[12] In the period before the introduction of the NLW, the wage floor is set at the prevailing rate of the NMW. Our estimation sample offers a minimum of 3000 observations in the treated group and 6000 observations in the control group, across a minimum of 800 firms, in each year (see Table S1). The two groups are similar in terms of personal, job and employer characteristics (see Table S2). 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, 25 and 50 pence above the level of the incoming wage floor, re-defining the control group accordingly as employees earning within 10% of that new threshold.[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 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% 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 around 2 in 2016/17.

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

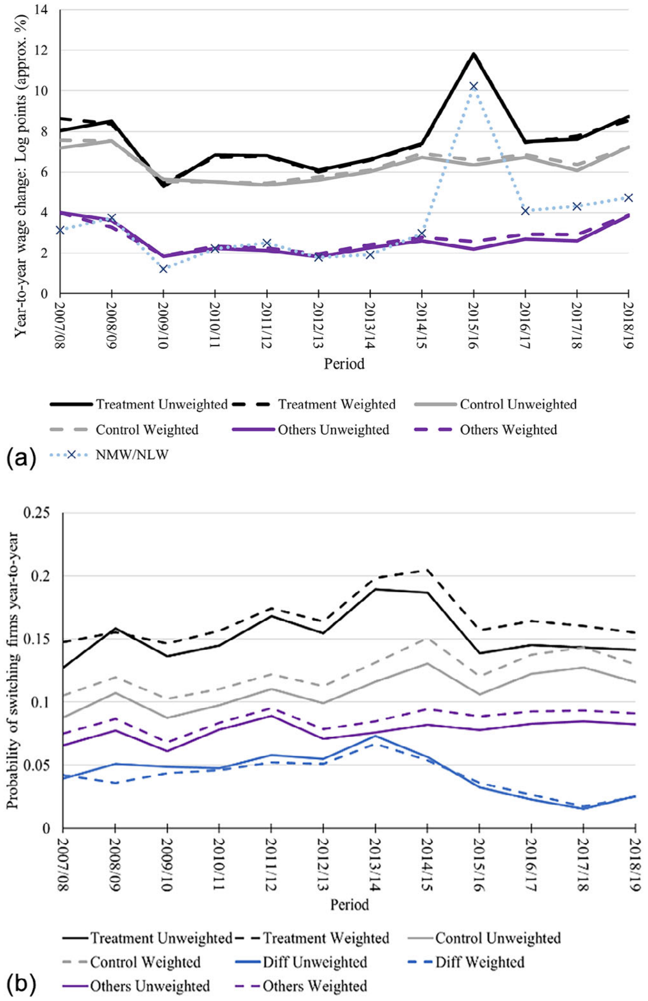

FIGURE 2 | Average annual wage growth for firm-stayers and rate of cross-firm mobility for treated and control groups (unweighted and weighted), and for all other employees in ASHE, and the annual growth in the NMW/NLW, by period. (a) Wage growth. (b) Cross-firm mobility. *Source:* ASHE. 'Diff' refers to the difference between the treated and control groups' average rates of switching. [Colour figure can be viewed at wileyonlinelibrary.com]

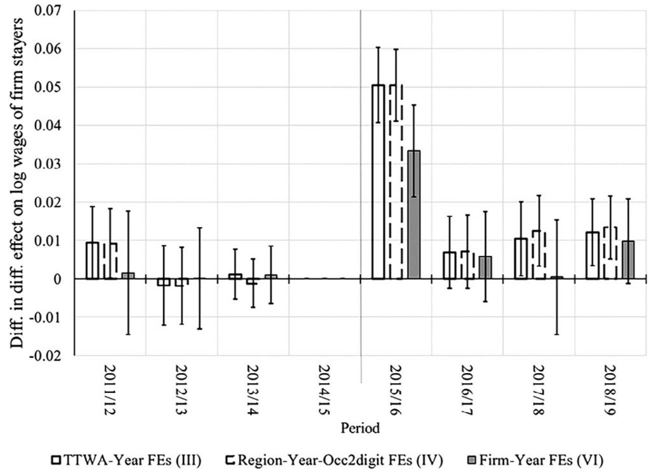

FIGURE 3 | Differences-in-differences estimates of the effects of the National Living Wage on year-to-year log wage changes for firm stayers (columns III, IV and VI, Table S3). *Source:* ASHE. *Notes:* The figure shows point estimates and 95% confidence intervals. All specifications control for gender × age (five categories) × hours worked (full-time/part-time); and tenure in the job (seven categories). (III) adds TTWA × Period FEs; (IV) adds Region × Period × Occ 2-digit FEs; (VI) adds Firm × Period FEs. For other notes, see Table S3.

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 Table S3 and Figure 3. We do not comment at length on the wage growth estimates in Figure 3 but, for specification II), 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), that is, when comparing among co-workers; 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.

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 ATE 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% level of statistical significance ($p = 0.098$). This difference is greater and statistically significant in 2016/17 ($\delta_5 = -0.023$; $p = 0.046$) and 2017/18 ($\delta_6 = -0.028$; $p = 0.019$). 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

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE 1 | Estimated effects of the NLW 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 two-digit FEs	No	No	No	Yes	No	No	No	Yes
Region × Period × Occ three-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

Note: γ_t omitted for brevity. Controls: gender × age (five categories) × hours worked (full-time/part-time); and tenure in the job (seven 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. The sample size in (VI) is smaller as singletons are dropped. Sample sizes in (VII) and (VIII) drop again because they estimate models (III) and (IV) starting with the sample in (VI). Numbers of observations by {Treated × Period} are shown in Table S1 for columns (III) and (VI). *Source:* ASHE.

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.

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

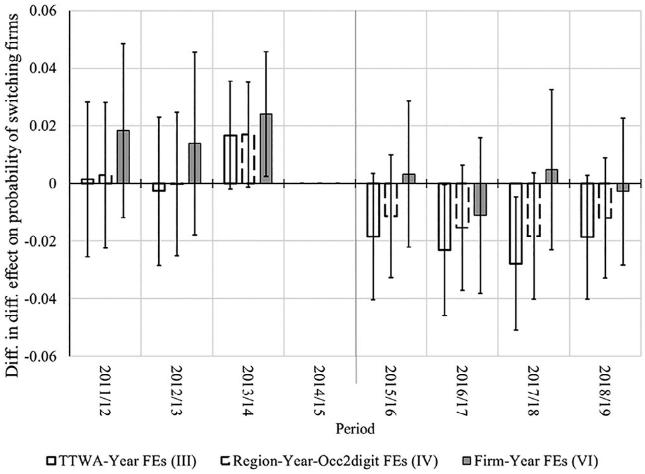

FIGURE 4 | Differences-in-differences estimates of the effects of the National Living Wage on the probability of year-to-year firm switching (columns III, IV and VI, Table 1). *Source:* ASHE. *Note:* The figure shows point estimates and 95% confidence intervals. All specifications control for gender × age (five categories) × hours worked (full-time/part-time); and tenure in the job (seven categories). (III) adds TTWA × Period FEs; (IV) adds Region × Period × Occ 2-digit FEs; (VI) adds Firm × Period FEs. For other notes, see Table 1.

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 NLW 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).

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 Table S4 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 Table S5 and Figure S1). In contrast, the treatment effects on wages are virtually unchanged when these weights are applied (Table S6).

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% larger estimation sample, the treatment effects are generally larger and more negative (Table S7). 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% 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 and 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 Tables S8–S10. 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% level in column (III) with the 25 pence extension; it is also significant at 10% in columns (III) and (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 and 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 S11–S13 and Figure S2).

Our fourth sensitivity check defines the control group as comprising all those paid above the NLW (see Table S14). 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

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

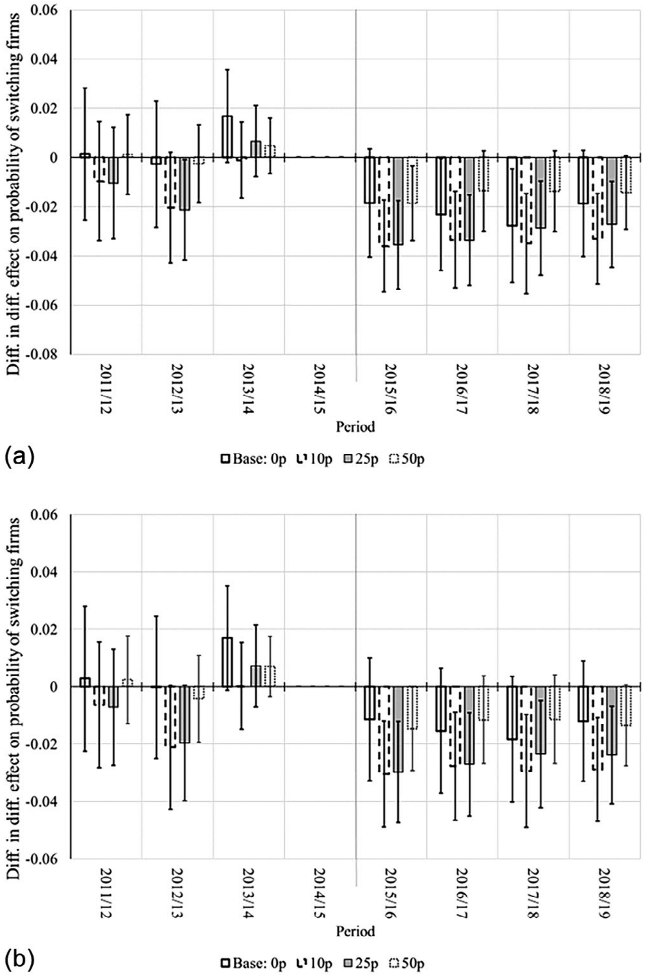

FIGURE 5 | Differences-in-differences estimates of the effects of the National Living Wage on the probability of year-to-year firm switching: allowing for wage-rate rounding effects and spillovers (Tables 1 and S8–S10). (a) Within TTWA, column (III). (b) Within Region × Occ, column (IV). *Source:* ASHE. *Note:* The figure shows point estimates and 95% confidence intervals. Both specifications control for: gender × age (five categories) × hours worked (full-time/part-time); and tenure in the job (seven categories). (III) adds TTWA × Period FEs; (IV) adds Region × Period × Occ two-digit FEs. For other notes, see Tables 1 and Tables S8–S10.

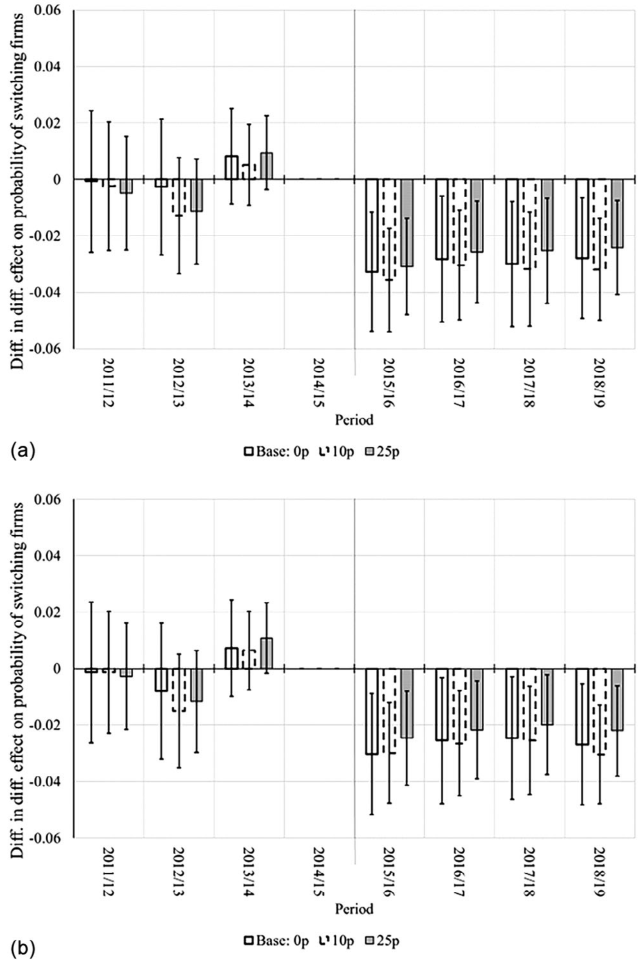

FIGURE 6 | Differences-in-differences estimates of the effects of the National Living age on the probability of year-to-year firm switching: allowing for wage-rate rounding effects and spillovers – with a consistent control group (Tables S15–S17). (a) Within TTWA, column (III). (b) Within Region × Occ, column (IV). *Source:* ASHE. *Note:* The figure shows point estimates and 95% confidence intervals. Both specifications control for gender × age (five categories) × hours worked (full-time/part-time) and tenure in the job (seven categories). (III) adds TTWA × period FEs; (IV) adds Region × Period × Occ two-digit FEs. For other notes, see Tables S15–S17.

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 10 pence, 25 pence and 50 pence 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 72 pence 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 50 pence. 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 and Figures 4 and 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 +10 pence and +25 pence groups. In sum, we find evidence of spillover effects on job-to-job mobility up to 25 pence above the NLW, aligning somewhat with Giupponi et al.'s (2024) evidence on wages, discussed earlier.

Table S18 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% 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.

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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 Table S19 and Figure S3. 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 two-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 in 2016/17 and 5.1 percentage points in 2018/19, both of which are significant at the 1% level. Hence, adding to the ATE 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.

Finally, we consider an alternative identification strategy, using the age-based nature of the NLW, which only applied to workers aged 25 or older when it was introduced. 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 three-digit occupational controls and firm-year fixed effects are omitted, due to much smaller age-based sample sizes. The results in Tables S20 and S21 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 and 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 and 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 United Kingdom, 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. 2022).

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 and Lentz 2019; Haltiwanger et al. 2018; Lise et al. 2016; Moscarini and 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).

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE 2 | Estimated effects of the NLW 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 = 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

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

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 the 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 United Kingdom.

Acknowledgements

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

Lucy Stokes completed this research whilst employed at the National Institute of Economic and Social Research, UK. This work represents the views of the individual authors, and not the views of the Competition and Markets Authority.

Ethics Statement

The project involves the use of anonymised personal data and received ethical approval from the University of the West of England.

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

Conflicts of Interest

The authors declare no conflicts of interest.

Data Availability Statement

The data that support the findings of this study are openly available through the ONS (Office for National Statistics) Secure Research Service at http://doi.org/10.57906/x25d-4j96.

Endnotes

[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).

[2] See Caldwell et al. (2025) 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.

[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 has calculated the bite of the NMW in April 2015 at 52.5% of median hourly wages (Low Pay Commission 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 Forth 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.

[7] The survey is mandatory, but the Office for National Statistics (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 Forth et al. 2024, for further details.)

[9] Employees whose pay was affected by absence during the reference period are excluded.

[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.)

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

[12] Employees earning more than 10% 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.

[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.

[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.

[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.

References

Aitken, A., P. Dolton, and R. Riley. 2019. "The Impact of the Introduction of the National Living Wage on Employment, Hours and Wages." NIESR Discussion Paper No. 501. National Institute of Economic and Social Research.

Akerlof, G. A. 1982. "Labor Contracts as Partial Gift Exchange." *The Quarterly Journal of Economics* 97, no. 4: 543–569.

Avram, S., and S. Harkness. 2025. "Do High Minimum Wages Harm the Progression of Minimum Wage Workers? Evidence From the United Kingdom." *Industrial Relations: A Journal of Economy and Society*. Ahead of print. 28 January. https://doi.org/10.1111/irel.12389.

Bagger, J., and R. Lentz. 2019. "An Empirical Model of Wage Dispersion With Sorting." *The Review of Economic Studies* 86, no. 1: 153–190.

Caldwell, S., I. Haegele , and J. Heining. 2025. Firm pay and worker search. NBER Working Paper No. 33445.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer. 2019. "The Effect of Minimum Wages on Low-Wage Jobs." *The Quarterly Journal of Economics* 134, no. 3: 1405–1454.

de Linde Leonard, M., T. D. Stanley, and H. Doucouliagos. 2014. "Does the UK Minimum Wage Reduce Employment? A Meta-Regression Analysis." *British Journal of Industrial Relations* 52, no. 3: 499–520.

Dickens, R., R. Riley, and D. Wilkinson. 2009. "The Employment and Hours of Work Effects of the Changing National Minimum Wage." Report to the Low Pay Commission, mimeo.

Dickens, R., R. Riley, and D. Wilkinson. 2014. "The UK Minimum Wage at 22 Years of Age: A Regression Discontinuity Approach." *Journal of the Royal Statistical Society Series A: Statistics in Society* 177, no. 1: 95–114.

Dickens, R., R. Riley, and D. Wilkinson. 2015. "A Re-Examination of the Impact of the UK National Minimum Wage on Employment." *Economica* 82, no. 328: 841–864.

Dolton, P., C. R. Bondibene, and J. Wadsworth. 2012. "Employment, Inequality and the UK National Minimum Wage Over the Medium-Term." *Oxford Bulletin of Economics and Statistics* 74, no. 1: 78–106.

Dube, A. 2019. "Impacts of Minimum Wages: Review of the International Evidence." Independent Report. UK Government Publication, 268–304.

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

Dube, A., T. W. Lester, and M. Reich. 2016. "Minimum Wage Shocks, Employment Flows, and Labor Market Frictions." *Journal of Labor Economics* 34, no. 3: 663–704.

Dube, A., S. Naidu, and M. Reich. 2007. "The Economic Effects of a Citywide Minimum Wage." *Industrial and Labor Relations Review* 60, no. 4: 522–543.

Dustmann, C., A. Lindner, U. Schönberg, M. Umkehrer, and P. Vom Berge. 2022. "Reallocation Effects of the Minimum Wage." *The Quarterly Journal of Economics* 137, no. 1: 267–328.

Fidrmuc, J., and J. D. D. Tena. 2018. "UK National Minimum Wage and Labor Market Outcomes of Young Workers." *Economics* 12, no. 1: 20180005.

Forth, J., A. Bryson, V. Phan, et al. 2024. "Revisiting Sample Bias in the UK's Annual Survey of Hours and Earnings, with Implications for Estimates of Low Pay and The Bite of the National Living Wage." IZA Discussion Paper No. 17291. IZA.

Foster, L., J. Haltiwanger, and C. Syverson. 2008. "Reallocation, Firm Turnover, and Efficiency: Selection on Productivity or Profitability?" *American Economic Review* 98, no. 1: 394–425.

Frederiksen, A., T. Halliday, and A. Koch. 2016. "Within- and Cross-Firm Mobility and Earnings Growth." *Industrial and Labor Relations Review* 69, no. 2: 320–353.

Fujita, S., G. Moscarini, and F. Postel-Vinay. 2024. "Measuring Employer-to-Employer Reallocation." *American Economic Journal: Macroeconomics* 16, no. 3: 1–51.

Giupponi, G., R. Joyce, A. Lindner, T. Waters, T. Wernham, and X. Xu. 2024. "The Employment and Distributional Impacts of Nationwide Minimum Wage Changes." *Journal of Labor Economics* 42, no. S1: S293–S333.

Giupponi, G., A. Lindner, S. Machin, and A. Manning. 2016. "The Impact of the National Living Wage on English Care Homes." Research Report for the Low Pay Commission. Centre for Economic Performance, London School of Economics.

Giupponi, G., and S. Machin. 2022. "Company Wage Policy in a Low-Wage Labor Market." CEP Discussion Paper (1689). London School of Economics and Political Science. Centre for Economic Performance.

Haltiwanger, J., H. Hyatt, and E. McEntarfer. 2018. "Who Moves Up the Job Ladder?" *Journal of Labor Economics* 36, no. S1: S301–S336.

HM Treasury & The Rt Hon George Osborne. 2015. "Chancellor George Osborne's Summer Budget 2015 Speech." July 8, 2015. House of Commons, London.

Jardim, E., M. C. Long, R. Plotnick, E. Van Inwegen, J. Vigdor, and H. Wething. 2022. "Minimum-Wage Increases and Low-Wage Employment: Evidence From Seattle." *American Economic Journal: Economic Policy* 14, no. 2: 263–314.

Jovanovic, B. 1979. "Job Matching and the Theory of Turnover." *Journal of Political Economy* 87, no. 5: 972–990.

Lam, K., C. Ormerod, Author, and P. Vaze. 2006. "Do Company Pay Policies Persist in the Face of Minimum Wages?." *Labour Market Trends* 114, no. 3: 69–81.

Lise, J., C. Meghir, and J. M. Robin. 2016. "Matching, Sorting and Wages." *Review of Economic Dynamics* 19: 63–87.

Low Pay Commission. 2022. *The National Living Wage Review (2015-2020)*, Low Pay Commission.

Machin, S., A. Manning, and L. Rahman. 2003. "Where the Minimum Wage Bites Hard: Introduction of Minimum Wages to a Low Wage Sector." *Journal of the European Economic Association* 1, no. 1: 154–180.

Melo, V., C. Kaiser, D. Neumark, L. Palagashvili, and M. Farren. 2025. "Minimum Wage Laws and Job Search." NBER Working Paper No. 33433. NBER.

Moscarini, G., and F. Postel-Vinay. 2018. "The Cyclical Job Ladder." *Annual Review of Economics* 10: 165–188.

Neumark, D. 2018. "Employment Effects of Minimum Wages." IZA World of Labor.

Neumark, D., and P. Shirley. 2022. "Myth or Measurement: What Does the New Minimum Wage Research Say About Minimum Wages and Job Loss in the United States?" *Industrial Relations: A Journal of Economy and Society* 61, no. 4: 384–417.

Office for National Statistics. 2022. "Annual Survey of Hours and Earnings." dataset, ONS SRS Metadata Catalogue, 10.57906/x25d-4j96.

Ritchie, F., A. Bryson, J. Forth, et al. 2023. *Enriched ASHE Quick Start Guide: Version 2*, Wage and Employment Dynamics Project.

Simmons, M. 2024. "Job-to-Job Transitions, Job Finding and the Ins of Unemployment." *Labour Economics* 80: 102304.

Snell, A., and J. P. Thomas. 2010. "Labor Contracts, Equal Treatment, and Wage-Unemployment Dynamics." *American Economic Journal: Macroeconomics* 2, no. 3: 98–127.

Van Huizen, T., and R. Alessie. 2019. "Risk Aversion and Job Mobility." *Journal of Economic Behavior and Organization* 164: 91–106.

Supporting Information

Additional supporting information can be found online in the Supporting Information section.

Supporting File 1: bjir70008-sup-0001-OnlineAppendix.docx.

14678543, 2025, 4, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/bjir.70008 by University College London UCL Library Services, Wiley Online Library on [08/12/2025]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License