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# The Extent of Downward Nominal Wage Rigidity: New Evidence from Payroll Data

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# The Extent of Downward Nominal Wage Rigidity: New Evidence from Payroll Data

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**Abstract**

We use over a decade of representative payroll data from Great Britain to study the nominal wage changes of employees who stayed in the same job for at least one year. We show that basic hourly pay drives the cyclicality of marginal labour costs, making this the most relevant measure of wages for macroeconomic models that incorporate wage rigidity. Basic hourly pay adjusts much less frequently than previously thought in Britain, particularly in small firms. We find that firms compress wage growth when inflation is low, which indicates that downward rigidity constrains firms' wage setting. We demonstrate that the empirical extent of downward nominal wage rigidity (DNWR) can theoretically cause considerable long-run output losses. Combined, our results all point to the importance of including DNWR in macroeconomic and monetary policy models.

*Keywords*: Downward nominal wage rigidity; Unemployment fluctuations; Macroeconomic policy; Marginal labour costs
*JEL codes*: E24, E32, J31, J33

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# 1. Introduction

Over eighty years have passed since Keynes (1936) proposed that workers simply refuse to accept nominal wage cuts, thus preventing the real price of labour from falling during recessions and leading to rising unemployment. An implication of Keynes' theory is that moderate inflation can *"grease the wheels of the labor market"* (Tobin, 1972) by bringing down real wages and boosting employment. With low average inflation being a persistent feature in many developed economies over recent decades, the question of whether labour markets display signs of downward nominal wage rigidity (DNWR) has attracted renewed attention. The answer has wide-ranging consequences for policy-makers; macroeconomic interventions can be justified if excess supply in the labour market does not self-correct through wage adjustments (Schmitt-Grohé and Uribe, 2016). Furthermore, the effects of monetary policy on the real economy depend crucially on the degree of nominal wage rigidity in the workhorse model of monetary policy analysis (Christiano, Eichenbaum, and Evans, 2005).

Despite being one of the central issues in macroeconomics, the empirical extent of downward nominal wage rigidity remains an open question. The key reason why economists disagree is that studies on DNWR have used datasets that were ill-suited to the task. The findings from household survey data have frequently been discounted on the grounds that self-reported wages contain substantial response errors, which can bias estimates.[1] Recent studies have turned to more accurate administrative or payroll data, which suggest that wages are far more flexible than previously thought. The wealth of evidence has indeed become so great that researchers now question the often invoked assumption of DNWR (e.g., Elsby and Solon, 2019). But the datasets that have been used in these recent studies typically give total earnings, which consist of basic wages and extra payments, such as overtime or commission. This makes it tricky to interpret any results and inform macroeconomic models; nominal wage rigidity is typically incorporated to dampen the cyclical response of marginal labour costs, hence only the pay components that drive the cyclical responsiveness of the marginal costs actually matter for these models.[2]

Our study overcomes these challenges and provides novel empirical evidence on the extent of DNWR, using a unique longitudinal dataset from Great Britain, the Annual Survey of Hours and Earnings (ASHE), which offers six main advantages. First, the dataset comprises a one percent random sample of income tax-paying workers, allowing us to derive results that are representative of the entire labour market. This matters because we will document

[1] Studies on nominal wage rigidity using US household surveys include: McLaughlin (1994); Akerlof, Dickens, and Perry (1996); Card and Hyslop (1996); Kahn (1997); Altonji and Devereux (2000); Lebow, Saks, and Wilson (2003), and more recently Barattieri, Basu, and Gottschalk (2014); Elsby, Shin, and Solon (2016). Smith (2000) and Fehr and Goette (2005) similarly analysed household survey data from Great Britain and Switzerland, respectively. Dickens et al. (2007) provide results for a sample of European countries and the US.

[2] Administrative data on total earnings show little evidence of DNWR, except in countries where nominal wage cuts are legally impossible, e.g., Portugal and Sweden (see the recent survey by Elsby and Solon, 2019).

substantial cross-sectional heterogeneity over workers and firms in the extent of nominal wage changes; for example, wage freezes are notably more frequent among smaller firms than larger firms. Second, this survey is administered to employers who are legally obliged to report information from their payrolls, making the data more accurate than those obtained from household surveys (Elsby, Shin, and Solon, 2016). Third, employers are explicitly asked whether an employee has been working in the same job and role for more than a year, meaning that we can study actual *job* stayers instead of firm stayers. This distinction matters because we are interested in a firm's tendency to reduce its employees' pay for the same work. Fourth, the data provide detailed records on basic wages, hours worked, and extra pay components. These allow us to study basic wages *per hour* separately from extra pay components, comparing like-for-like measures of hourly pay over time, as well as to analyse separately the cyclicality of each pay component. Fifth, we can distinguish between hourly-paid and salaried (non-hourly-paid) employees. Previous research has found conflicting evidence on the extent to which nominal wage adjustments appear to differ between salaried and hourly-paid workers, questioning whether studies based on only hourly-paid workers generalise to the aggregate labour market.[3] Sixth and finally, the data cover a period of substantial macroeconomic instability, with significant variation in unemployment and low inflation. This matters, since there are reasons to expect that high inflation would hide clear signs of DNWR over the business cycle (Card and Hyslop, 1996).

Our study makes three main contributions to the literature. First, we add a second set of data points on the adjustments and cyclicality of basic wages to those recently documented by Grigsby, Hurst, and Yildirmaz (2021) for the US. We confirm that basic wages in Great Britain are similarly procyclical. Furthermore, other extra pay components, for job stayers who receive them, do not substantively increase the estimated cyclicality of hourly earnings. We also show that changes to an employee's basic wages are longer-lasting than changes to extra pay, implying that the former tend to have the greater impact on labour costs. Combined, these findings suggest that the measure of basic wages is the most relevant empirical counterpart to the notion of a wage in macroeconomic models that incorporate nominal wage rigidities to generate cyclical fluctuations in unemployment. Furthermore, we show that the distributions of basic wage changes fall off sharply to the left of zero, which supports the recent US evidence and was previously undocumented for Great Britain.

Our second contribution goes further than Grigsby, Hurst, and Yildirmaz, by presenting some evidence that firms' wage setting is actually constrained by downward nominal rigidity. To do so, we leverage the theoretical insights of Elsby (2009). His model predicts that forward-looking firms, if constrained by DNWR, will refrain from raising nominal wages today, because this increases the likelihood of having to cut wages, at some cost, in the future.

[3] For example, Card and Hyslop (1996) analysed the Current Population Survey, concluding that DNWR is not lower for salaried than for hourly-paid workers in the US. Contrary to this, Kahn (1997) found that hourly wage rates exhibit substantially more signs of DNWR than salaries in the US Panel Study of Income Dynamics.

We use unconditional quantile regression to show that the distribution of job-stayer wage growth is indeed considerably compressed according to the prevailing rate of consumer price inflation and regional productivity growth.

Our third contribution documents substantial heterogeneity in basic wage adjustments within payroll data. We find that firm size is especially significant in accounting for the frequency of wage freezes and cuts. In small firms with up to 50 employees, the conditional likelihood of a year-to-year wage freeze for job stayers is 27 percent for salaried workers and 34 percent for hourly-paid workers. These values drop to 15 percent in firms with more than 5,000 employees. These differences between jobs, as well as others discussed later, such as across industry sectors or depending on whether wages are affected by collective bargaining, highlight the importance of using nationally representative data when assessing the prevalence and macroeconomic significance of DNWR.

Combined, our findings support the assumption of DNWR invoked in recent macroeconomic models of business cycle fluctuations (e.g., Daly and Hobijn, 2014; Dupraz, Nakamura, and Steinsson, 2019), as well as the degree of nominal rigidity typically assumed in New Keynesian models (e.g., Christiano, Eichenbaum, and Evans, 2005).Our findings also imply that DNWR might cause sizeable output losses. To demonstrate this, we calibrate the dynamic stochastic general equilibrium model of Benigno and Ricci (2011), which incorporates downward rigid wage setting by firms, to match our empirical estimates on the prevalence of wage freezes and cuts. Simulations of this model suggest that the *long-run* output loss caused by DNWR is around 0.7 to 1.3 percent of GDP in a low-inflation environment of two to one percent per annum.

There are three previous studies on nominal wage changes in Great Britain, which are highly relevant to our own. Smith (2000) found that cuts in weekly earnings were quite common among a subsample of respondents to the 1991-96 waves of the British Household Panel Survey who were prompted to check their payslips when asked about pay. Nickell and Quintini (2003) studied the precursor to the datasets that we are using, the New Earnings Survey for 1975-99, which neither identified hourly-paid workers, nor separated basic wages from extra pay components besides overtime. Nickell and Quintini confirmed Smith's findings: job-stayer earnings were frequently cut in Great Britain in the early 1990s. Recent work by Elsby, Shin, and Solon (2016) updated the study of Nickell and Quintini, finding that job-stayer earnings per hour, excluding overtime, are frequently cut in Great Britain and earnings freezes do not occur excessively often, concluding that DNWR may be less binding than is often supposed. We replicate those findings, but we also show that the ASHE survey design likely leads to previously undocumented recording errors in the data on hours worked, implying that earlier studies may have over-estimated the frequency of nominal wage changes.

Three research teams have recently investigated nominal wage adjustments in US administrative or payroll data for job stayers.[4] Jardim, Solon, and Vigdor (2019) and Kurmann and McEntarfer (2019) used administrative data from the State of Washington. Unusually for the US, Washington requires firms to report hours worked, and so these authors could derive measures of total earnings per hour. Both studies have documented a considerable proportion of cuts among job-stayer total earnings per hour, and Jardim, Solon, and Vigdor also found this in a subsample of employees where overtime was likely to have been rare. Their results provide important insights for total labour costs, but they cannot separate cyclical from non-cyclical pay components, nor do they identify hourly-paid workers.

Using a proprietary dataset from a US payroll processing firm, Grigsby, Hurst, and Yildirmaz (2021) found that changes to basic wages occur significantly less frequently than was previously thought. We confirm many of their results, suggesting that their findings are unlikely to be driven by idiosyncrasies of the US labour market, and thus providing a valuable second set of data points on nominal basic wage adjustments. We extend their insightful work in two important ways: first, we document that nominal wages are rigid downwards and assess what the consequences of DNWR for the aggregate economy might be, and second, we study basic wages for a sample of jobs that is representative of an entire national labour market. Importantly, as Grigsby, Hurst, and Yildirmaz acknowledged, the data they used under-represented very large firms and they excluded firms with 50 or fewer employees from their analysis.[5]

The rest of the paper is structured as follows: Section 2 describes the data and our sample selection; Section 3 argues that basic wages drive (marginal) labour costs; Section 4 analyses job-stayer basic wages, separately for hourly-paid and salaried workers, and documents wage growth compression; Section 5 uncovers heterogeneity across workers and firms in the conditional probability of basic wage freezes and cuts; Section 6 discusses the macroeconomic implications; and Section 7 concludes.

[4] Hazell and Taska (2020) find substantial evidence of DNWR for new hires within online vacancy data from the US. They argue that this evidence is consistent with DNWR at the job level being the key for unemployment fluctuations. They do not study wages of incumbent employees.

[5] Carneiro, Portugal, and Varejao (2014) studied Portuguese administrative microdata and found strong signs of DNWR, with around 45 percent of job stayers having exactly the same basic wage in 2012 as in 2011. However, as the authors explained, nominal wage cuts are explicitly prohibited by law in Portugal. Sigurdsson and Sigurdardottir (2016) analysed Icelandic payroll-based data, where they observed that wage cuts were rare and the share of year-to-year basic wage freezes for job stayers was around 16 percent. These studies did not analyse whether firms were constrained by DNWR and what the consequences for the aggregate economy might be.

## 2. Description of the Annual Survey of Hours and Earnings

Our analysis of nominal wage adjustments uses the Annual Survey of Hours and Earnings (ASHE) (Office for National Statistics, 2019). The ASHE is an ongoing longitudinal panel of employees, starting in 2004, based on a one percent random sample of workers in Great Britain who pay income tax or make National Insurance contributions. The last observations in our study are from 2018. Employers respond to the survey by providing information from the pay period that includes a specific date in April, either by returning a survey questionnaire or directly through their payroll by a special arrangement with the Office for National Statistics (ONS). This setup implies that we only have data each year for individuals in the panel who were employees on the survey reference dates.[6]

The ASHE offers a unique combination of features which make it ideal for this study. First, employers are legally obliged to report employee earnings with reference to payrolls, making the data more accurate than those obtained from household surveys (Nickell and Quintini, 2003; Elsby, Shin, and Solon, 2016). Second, employers are explicitly asked whether an employee has been working in the same job and role for more than a year, meaning that we can study actual *job* stayers instead of firm stayers. This distinction matters, because we are interested in a firm's tendency to reduce its employees' pay for the same work; a promotion or change in the job may cause some adjustment of an employee's wage, but there is little reason to expect much nominal rigidity for such job changers. Third, the ASHE has a large sample size, with up to 100,000 wage-change observations of job stayers per year. Finally, the ASHE is representative of jobs in Great Britain, being based on a one percent random sample of employees.[7]

Relative to its precursor, the well-studied New Earnings Survey Panel Dataset (NESPD), the ASHE contains two major improvements. First, it identifies workers who received an hourly pay rate, hereafter 'hourly-paid' workers. We refer to all other employees as 'salaried'. This allows us to assess how far results based on hourly-paid workers generalise to the aggregate labour market, as is often implicitly assumed to be the case (e.g., Barattieri, Basu, and Gottschalk, 2014). Second, the ASHE contains details on a worker's composition of pay, reporting separately basic pay and extra pay components. The latter include overtime pay, shift premium pay, incentive pay received for work carried out in the April pay period, and

[6] We only use data starting from 2006, because questionnaire changes in 2005 and 2006 introduced inconsistencies between these years. Specifically, new instructions were included on how firms should report employee hours worked. See Schaefer and Singleton (2019, 2020) for further descriptions of the dataset.

[7] The ASHE does not include the very low-paid employees; around 3-4 percent of employees in Great Britain (ONS estimates of this undercoverage). The design of the sampling frame implies that it contains no observations of employees with earnings below the National Insurance threshold or who work for businesses that have a turnover below the Value Added Tax (VAT) threshold, e.g., £5,564 and £73,000 per year in 2012-13, respectively.

other pay (e.g., meal or travel allowances).[8] The ASHE reports the amounts of basic pay, extra pay components, and hours worked for the pay period. For example, if an employee is paid weekly, the dataset contains the amounts received by the employee in the week that includes ASHE reference date. In practice, most employees in Great Britain are paid once per calendar month. We observe basic wages for hourly-paid workers directly from the hourly basic rates of pay recorded by their employers. For salaried workers, we compute their average hourly basic wages within their surveyed pay periods as the ratio of basic pay to basic hours worked. Other studies have analysed basic pay directly, either due to a lack of data on hours worked or due to a perceived lack of reliability when tracking salaried employees' hours. Here we prefer to analyse basic wages because Borowczyk-Martins and Lalé (2019) have documented that fluctuations in part-time employment play a major role in movements in hours per worker during cyclical swings in the labour market. That evolution of part-time employment is predominantly explained by transitions from full-time to part-time employment at the same employer. Additionally, Borowczyk-Martins and Lalé also found that hours worked within full-time and part-time jobs are mildly procyclical. Even if basic pay per hour was constant during recessions, such cyclical reductions in hours worked would tend to increase the amount of cuts in basic pay. Since our main objective is to understand how flexibly employers can adjust nominal wages downward for the same work, we want to exclude pay changes that are only due to hours changes. We provide more details on the pay components and their exact definitions in Appendix A.

We study employees aged 16-64, who did not incur any loss of pay in the April reference period (e.g., unpaid sick leave) and who were not paid at an apprenticeship or a trainee rate. We drop person-year observations if a worker held multiple jobs, was reported as having worked on average less than one or more than 100 hours per week in April, was reported as being paid less than 80 percent of the age-relevant statutory National Minimum Wage, or had missing or imputed values for any of the pay variables which we are interested in. Altogether, our sample selection criteria result in a working dataset of 1,843,172 employee-year observations over our 13-year sample period, 2006-2018. Appendix A describes and justifies our sample construction in more detail. We define a 'job stayer' as an employee whom we observe working in the same job as in the previous April, such that we can measure within-job year-to-year wage changes.

Table 1 shows descriptive statistics for the samples of job stayers and all employees in the working dataset, where the latter includes job switchers, (re-)entrants to employment and job stayers in 2006. On average, the ASHE suggests that around 92 percent of British employees who remain in employment from one year to the next are job stayers, as opposed to job

[8] Using the NESPD, the previous studies by Nickell and Quintini (2003) and Elsby, Shin, and Solon (2016) were restricted to analysing a worker's total earnings per hour, excluding overtime. Additionally, these authors could not distinguish hourly-paid from salaried workers.

switchers.[9] Hourly-paid job stayers are more likely to be working for a private sector company, are slightly younger, and are less likely to be employed on a full-time basis than salaried job stayers. Around half of all job stayers are covered by a collective agreement, defined as any arrangement affecting the pay of more than one employee. The median and average basic wages are higher for salaried job stayers. Firms in the industry sectors of wholesale & retail trade and hotels & restaurants represent 32 percent of hourly-paid job stayers, while only 13 percent of salaried job stayers work in those industries. Professional services and financial services employ around 42 percent of the salaried job stayers (see Appendix Table A2).

TABLE 1: Descriptive statistics of employees in Great Britain, Annual Survey of Hours and Earnings, 2006-18

| | Job stayers | | All employees |
|---|---|---|---|
| | Hourly-paid (I) | Salaried (II) | (III) |
| Private sector (%) | 76.3 | 60.5 | 66.5 |
| Female (%) | 51.8 | 49.8 | 51.1 |
| Age (years) | 42.6 | 41.7 | 40.7 |
| Full-time ($\geq$ 30 hours, %) | 65.4 | 79.7 | 73.0 |
| Collective agreement (%) | 49.3 | 49.5 | 46.6 |
| Firm size (no. of employees, median) | 2,570 | 2,640 | 2,200 |
| Weekly basic hours (median) | 36.8 | 37.0 | 37.0 |
| Basic wage (mean, £) | 10.65 | 15.42 | 13.80 |
| Basic wage (median, £) | 8.91 | 12.95 | 11.10 |
| *N* | 320,087 | 594,709 | 1,843,172 |

*Notes*: Basic wages are deflated to 2015 GB Pounds (GBP) using the UK Consumer Price Index. Firm size rounded to the nearest ten for statistical disclosure control.

Basic wages are the primary source of labour income for the vast majority of job stayers: in the April pay period, 94 percent of an employee's income is accounted for by basic earnings (basic wages times basic hours worked) on average, and over half of all job stayers have no other labour income besides basic wages. Figure 1A shows the share of basic earnings in total earnings (basic plus extra earnings for the month of April) along the basic wage distribution. To generate this figure, first we group job stayers into percentiles of the basic wage distribution, and then compute the average share of basic earnings in total earnings within each percentile. The share of basic earnings in total earnings is roughly increasing with the level of the basic wage, reaching over 95 percent in the top percentile for both hourly-paid and salaried job stayers.

Figure 1B shows that incentive pay, i.e., bonuses and commission for work carried out during the reference period in April, contributes less to total weekly earnings when moving

[9] We can observe a small minority of job stayers who switch between being hourly-paid and salaried, but all our analysis only considers job stayers who are paid in the same way in consecutive years. This does not affect any of our results.

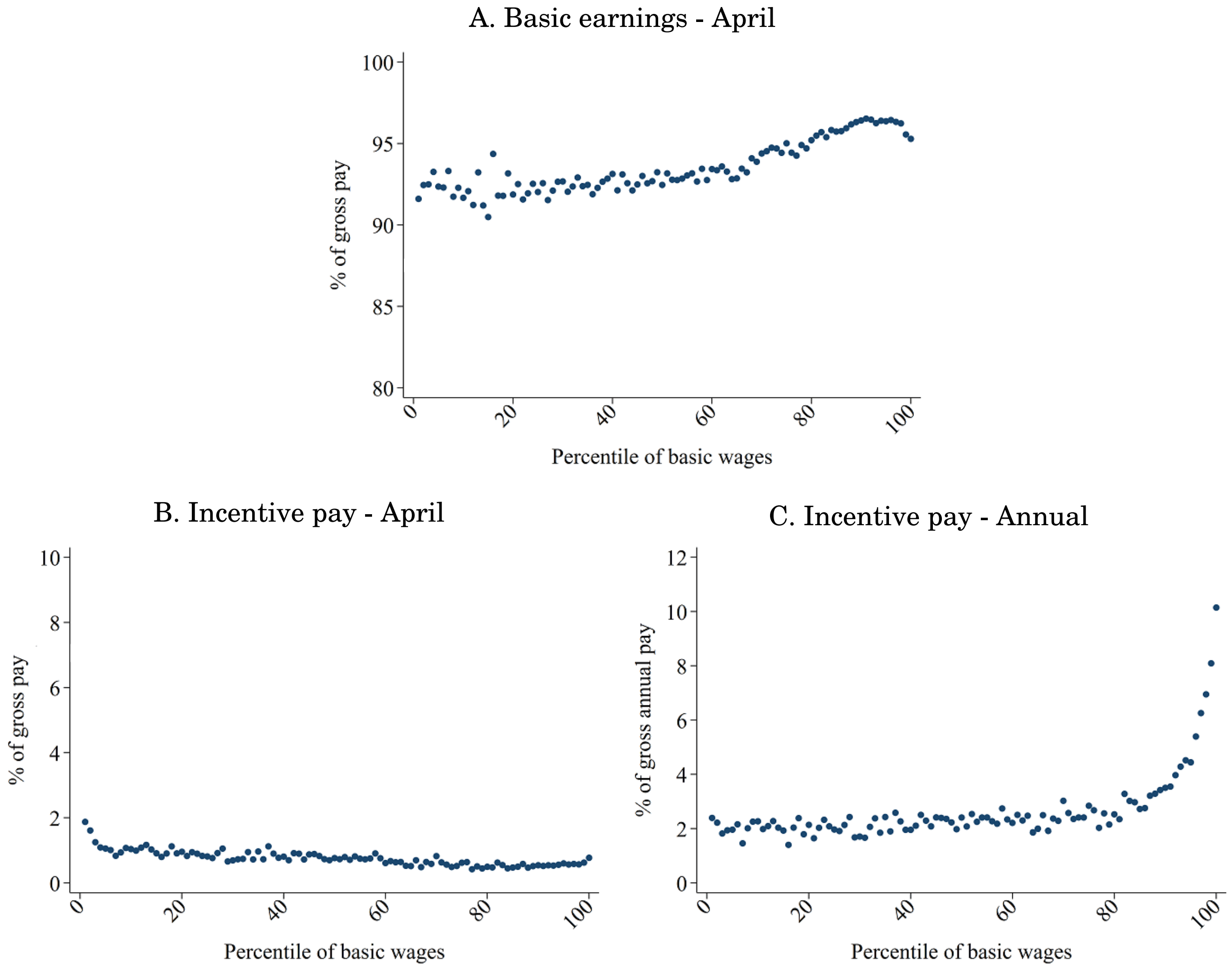


FIGURE 1: Importance of basic earnings along the basic wage distribution

*Notes*: Panel A: Average shares of basic earnings in total earnings within the corresponding percentile of the basic wage distribution. Data are pooled across the sample period, 2006-2018. Panel B: Incentive pay in total earnings within the corresponding percentile of the basic wage distribution in April. Panel C: Average shares of annual incentive pay in annual earnings within the corresponding percentile of the basic wage distribution. Data pooled across all years. See Appendix Figures B1 for equivalent statistics on the contributions of other components of pay to gross earnings.

up the basic wage distribution. When Nickell and Quintini (2003) studied incentive pay in Great Britain, they found that almost 22 percent of job stayers in the New Earnings Survey received such payments, compared with only 7 percent in our sample. This difference might be explained by a change in the incentive pay definition between their dataset and the ASHE. The new definition in the ASHE questionnaire, by focusing on incentive payments earned and paid in the April pay period, is more precise and gives more consistent estimates between years (Office for National Statistics, 2005). Hence, Figure 1A is likely to understate the relative importance of annual bonuses, because these are typically paid between January and March in the UK (Schaefer and Singleton, 2020).

To assess the importance of bonus payments outside the April reference period, we compute the share of *annual* incentive pay in *annual* earnings. Both values refer to the preceding tax year, and so complete information about those variables should have been available to employers when the questionnaire was completed. Figure 1C shows that the share of incentive pay in annual earnings increases along the basic wage distribution. This contrasts with the results for incentive pay earned and received in April. Within the highest decile, the importance of annual incentive payments is greater, especially among the top percentiles. This last finding is also consistent with recent US evidence from the National Compensation Survey, presented in Makridis and Gittleman (2021). The differences between Figure 1B and Figure 1C most likely originate in the composition of incentive pay. While the left panel probably reflects a larger share of commission-type payments earned throughout the year, annual incentive pay captures bonus payments, particularly for salaried high-earners. Appendix B provides further detail and statistics regarding the composition of employee earnings in Great Britain, including benefits-in-kind, such as employer provided health insurance, which have a much less important role compared with the US.

## 3. The Cyclicality of Basic Wages and Extra Pay Components

Nominal wage rigidity is typically incorporated into macroeconomic frameworks to dampen the cyclical movements of firms' marginal cost of labour (e.g., Christiano, Eichenbaum, and Evans, 2005; Smets and Wouters, 2007). Intuitively, if rigidities prevent the nominal marginal cost of labour from falling sufficiently in response to a negative shock, then firms will produce less output and demand less labour. Therefore, the relevant wage concept for macroeconomic models is best captured by the pay components with the largest impact on the cyclicality of marginal labour costs. In the previous section, we showed that basic wages are the only income source for the majority of workers and make up 94 percent of all labour income, on average. However, basic wages would only be the most relevant variable for the cyclicality of marginal labour costs if they were procyclical, while the extra pay components did not move systematically with the business cycle (Grigsby, Hurst, and Yildirmaz, 2021).

To understand which pay components drive the cyclicality of growth in real wages for job stayers, we estimate the response to regional unemployment rates, a proxy for the state of the business cycle, using least squares:

$$\Delta \log(w_{ijrt}) = \theta_{ij} + \beta^u U_{rt} + \mathbf{x}'_{it}\boldsymbol{\delta} + \varepsilon_{ijrt} \ , \tag{1}$$

where $w_{ijrt}$ are the various potential measures of real wages for individual $i$, who has worked in job $j$ for two consecutive years, $t$ and $t-1$, in region $r$. The unemployment rate (in percent) in region $r$ and year $t$ is given by $U_{rt}$ and $\theta_{ij}$ is a fixed effect for a continuous match between employer and employee. As such, since we focus on within-match wage growth, we only include job stayers who have at least two observed wage changes per match. The coefficient of interest is $\beta^u$, which measures the semi-elasticity of real wage growth to the regional unemployment rate within a match. This approximates how responsive real wage growth is to whether the labour market facing a job stayer and an employer is relatively slack or tight over the duration of their relationship. The vector $\mathbf{x}_{it}$ contains the April-to-April change in the log UK Consumer Price Index, time-varying controls for employee age and its square, and tenure squared.[10] The regions are the eleven EU-NUTS1 administrative regions of Great Britain (e.g., London, Wales, Scotland, North West).

TABLE 2: Estimated cyclicality of real wage growth for job stayers

| | Basic wages (I) | Basic wages plus shift, incentive, and other pay (II) | As (II) plus overtime (III) |
|---|---|---|---|
| **All job stayers** | | | |
| 1. Salaried | −0.711 | −0.711 | −0.742 |
| | (0.058) | (0.061) | (0.059) |
| 2. Hourly-paid | −0.613 | −0.569 | −0.612 |
| | (0.059) | (0.061) | (0.064) |

*Notes*: Least squares estimates of the semi-elasticity of real wages with respect to the regional unemployment rate (in %), $\beta^u$, in Equation (1), separately both for salaried and hourly-paid job stayers and for job stayers who had only basic wages or not. Controls are employee age, age squared, and tenure squared, in addition to worker-firm match fixed effects.
Standard errors in parentheses robust to two-way clustering over year-NUTS1-regions and worker-firm matches. Sample sizes of job stayers, after dropping singletons: All job stayer - salaried, 532,217; All job stayer - hourly-paid, 277,794;
*Sources*: NUTS1 regional unemployment and consumer price inflation rates are from the Office for National Statistics (ONS) for April of each year, corresponding to the reporting period of the ASHE.

Table 2 displays the estimates of $\beta^u$ from Equation (1) separately for salaried and hourly-paid employees, showing that real basic wages are considerably procyclical. When the regional unemployment rate increases by one percentage point, real basic wage growth is on average 0.71 percent lower among these salaried workers and 0.61 percent lower among these hourly-paid workers. These effects are quantitatively large, given that average annual real wage growth was only 1.4 percent and 1.1 percent for job stayers over the sample period.

Column (II) of Table 2 shows the estimates when including the extra pay components except overtime pay in the measure of real wage growth. Compared to Column (I) in absolute terms, the semi-elasticity estimates with respect to regional unemployment rates are unchanged for salaried job stayers and smaller for hourly-paid job stayers, though not substantially so. In the last column of Table 2, we include overtime pay, such that the coefficient estimates give the cyclical responses of real total earnings per hour. The cyclicality of total earnings per hour exceeds that of basic wages for salaried job stayers, but not for hourly-paid job stayers. In summary, this set of estimates shows that basic wages are substantially procyclical and extra pay components, especially overtime, do tend to increase that cyclicality, though not substantially. The results are also consistent with studies on

[10] A linear control for tenure is excluded because it would be perfectly collinear with an employee's age within an employer-employee match.

the cyclicality of US real wages. Devereux (2001) and Shin and Solon (2007) argue that adjustments in extra pay components tend to increase the procyclicality of real earnings. Taken together, these results suggest that the cyclicality of basic wages drives the majority of the cyclicality of firms' marginal labour costs of job stayers.

There are some theoretical reasons to question whether the cyclicality of current ('spot') wages has any consequences for employment and output. When workers and firms form long-term employment relationships, current wages are better understood within a stream of payments in which year-to-year fluctuations might not be allocative for employment (Becker, 1962). Any stream of *remitted* wages that adds up to the same present value should not affect a firm's employment decisions. Instead, what matters to a firm when deciding to continue an employment relationship is whether the expected present value of a worker's output exceeds the expected present value of her labour costs. To understand the typical effects of changes in current wages on expected labour costs, we follow Grigsby, Hurst, and Yildirmaz (2021) and estimate the persistence of each pay component for job stayers. Intuitively, the more persistent a pay component is, the larger will be its impact on the present value of labour costs. In Appendix C, we show that receiving a certain extra pay component in one year significantly increases the likelihood of receiving this same pay component again the next year, within the same job. However, relatively high amounts of extra pay earned in one year tend to be followed by relatively low amounts in the next year. In contrast, if the current basic wage was high relative to the stream of past and future basic wages within a job, then a worker expects next year's basic wage also to be relatively high. This implies that a rise in basic wages generally leads to an increase in labour costs exceeding the initial value of that rise, while the opposite is true for extra pay.

As long as the present value of the ex post rent of a worker-firm match is positive, dissolving such an employment relationship is inefficient; both worker and firm could agree on a way to share the rent so that neither prefers to end the match (Barro, 1977). It therefore seems reasonable that extra pay components might still be relevant occasionally. For example, if an employee is threatened by job loss unless their total pay is slightly decreased, but basic wages are rigid, then the employee and firm should agree to marginally decrease extra pay to prevent an inefficient separation. Such small adjustments in extra pay likely suffice to smooth over small fluctuations in the firm's share of the match rent. When the economy is hit by a large aggregate shock, adjustments in the main component of labour costs - basic wages - are more likely to be necessary to preserve the match. But the implicit contract between employee and firm might not be sufficiently thorough: workers may be unable to credibly commit to stick with the firm when the labour market tightens again, or firms may be unable to credibly commit to increase wages after the negative shock dissipates. Indeed, the empirical regularity that firms lay off workers into depressed labour markets during recessions suggests such commitment failures. The evidence presented in this and the preceding section supports the notion that basic wages are the most relevant measure of remuneration for macroeconomic models that rely on wage rigidities to generate muted responses of a firm's marginal labour costs to aggregate shocks.

## 4. The Extent of Downward Nominal Wage Rigidity

This section explores the data on job-stayer basic wages for evidence that nominal wages are rigid downwards. We begin by displaying commonly used statistics of nominal wage adjustments. Subsequently, we apply insights from the theoretical framework developed by Elsby (2009) to investigate empirically whether downward rigidity constrains firms' nominal wage setting.

### 4.1. Statistical Indicators

To give a first impression of the possible extent of downward nominal wage rigidity, we construct histograms of year-to-year nominal log basic wage changes. Figure 2 displays these distributions for hourly-paid and salaried job stayers, pooled across all years in the sample period. Four key characteristics are visible. First, large numbers of job stayers experience year-to-year basic wage freezes: 21 percent of hourly-paid workers and 17 percent of salaried job workers receive no basic wage change. The height of these spikes at zero is striking and shows that wage freezes occur substantially more frequently than previously thought in the UK. Nickell and Quintini (2003) and Elsby, Shin, and Solon (2016) documented comparable spikes never exceeding 10 percent in Britain, though they were unable to study basic wages and did not account for some probable, but previously undocumented, sources of measurement errors in how payroll records were transferred into the datasets. This may have caused true zero changes in nominal wages to be recorded as small non-zero changes. We discuss the likely impact of this source of measurement error further below, and justify our preferred approach of counting as freezes any absolute change in log basic wages of less than 0.005 (see also Appendix D). Second, the distribution of basic wage changes shown by Figure 2 is markedly asymmetric, dropping off sharply below zero. Although the sample period spans a severe recession between 2007 and 2009, on average only 11 percent of salaried and 4 percent of hourly-paid job stayers experienced a year-to-year decrease in nominal basic wages. Third, the histograms show a relative lack of small positive wage changes, directly to the right of zero. This would be consistent with the presence of adjustment costs, or 'menu costs', when firms adjust basic wages (e.g., Kahn, 1997). Fourth, basic wages appear to be more downward rigid for hourly-paid than salaried employees; the spike at zero in Figure 2 is 4 percentage points higher for hourly-paid than salaried employees, and the share of cuts is 7 percentage points lower.

Table 3 displays the relative frequencies of year-to-year basic wage freezes and cuts among job stayers for each year. Wage freezes were more frequent during the Great Recession and its

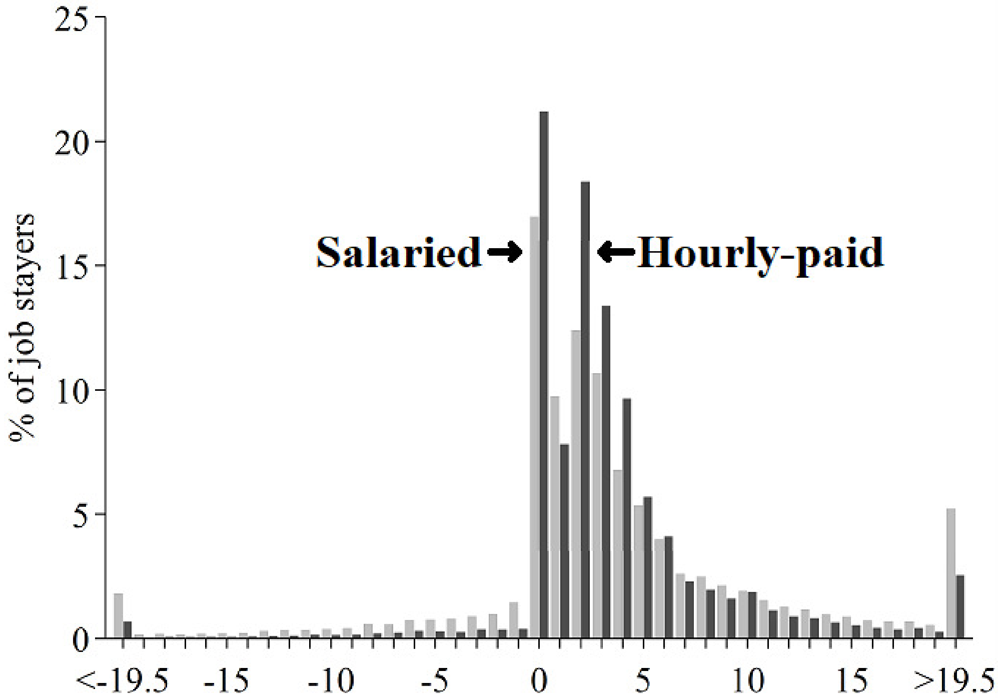


FIGURE 2: Frequency distribution of year-to-year changes in log basic wages of job stayers

*Notes*: The zero bin includes log changes in the interval $[-0.5, 0.5]$. The other bins are similarly one log point wide, with the positive changes excluding the lower limit and including the upper limit, and *vice versa* for negative changes. Data are pooled across all years in 2006-2018. **Light bars**: salaried. **Dark bars**: hourly-paid job.

aftermath. More than one-in-four job stayers experienced year-to-year basic wage freezes at the height of the recession, peaking at 28 percent and 27 percent of hourly-paid and salaried job stayers, respectively.[11] However, like most of the aforementioned literature, we only observe employee wages at an annual frequency. This might give rise to time aggregation issues. Suppose that wages are cut in response to negative economic conditions, but firms then reverse these cuts within the course of one year. In this case, we could underestimate the true extent of wage cuts among employees. Grigsby et al. (2021) found some evidence that the extreme labour market conditions of the COVID-19 pandemic led to relatively short-lived and reversed wage cuts, especially among high earners. Unfortunately, we cannot observe whether the Great Recession in Great Britain was associated with short-lived wage cuts becoming more common.

Our evidence contrasts with the results of two recent US studies on basic wages. First, Barattieri, Basu, and Gottschalk (2014) analysed the Survey of Income and Program

[11]Basic wages are the ratio of basic pay to basic hours. Basic pay excludes shift premium pay, while basic hours include shift premium hours. Although the ASHE questionnaire provides instructions to respondents to ensure that changes in shift hours do not affect basic wages, some respondents might incorrectly report hours worked. Following the excellent suggestion of an anonymous reviewer, we have checked how excluding job stayers with positive shift hours (6.7 percent of observations) affects our estimates of basic wage freezes and cuts. Pooled over 2006-07 to 2017-18, the relative frequency of basic wage cuts is virtually unchanged, but the relative frequency of basic wage freezes increases to 17.3 percent and 22.9 percent for salaried and hourly-paid job stayers, respectively. The increase in basic wage freezes follows no cyclical patterns.

TABLE 3: Year-to-year changes in nominal basic wages of job stayers

| Years | Salaried | | Hourly-paid | | Unemployment rate (%) | Inflation rate (%) |
|---|---|---|---|---|---|---|
| | Freezes (%) | Cuts (%) | Freezes (%) | Cuts (%) | | |
| | (I) | (II) | (III) | (IV) | (V) | (VI) |
| 2006-07 | 10.0 | 11.3 | 14.6 | 4.7 | 5.4 | 2.7 |
| 2007-08 | 7.5 | 10.0 | 11.9 | 3.4 | 5.4 | 2.9 |
| 2008-09 | 15.0 | 11.1 | 18.1 | 5.5 | 5.2 | 2.4 |
| 2009-10 | 21.1 | 14.5 | 26.5 | 4.6 | 7.6 | 3.7 |
| 2010-11 | 21.4 | 11.9 | 28.0 | 3.9 | 7.9 | 4.4 |
| 2011-12 | 27.2 | 12.2 | 27.4 | 3.4 | 7.8 | 3.0 |
| 2012-13 | 24.3 | 12.3 | 25.0 | 6.3 | 8.1 | 2.4 |
| 2013-14 | 13.9 | 9.9 | 22.7 | 3.2 | 7.8 | 1.8 |
| 2014-15 | 13.0 | 10.1 | 16.8 | 3.1 | 6.4 | −0.2 |
| 2015-16 | 17.6 | 11.1 | 19.5 | 5.7 | 5.6 | 0.3 |
| 2016-17 | 14.4 | 10.8 | 17.9 | 2.8 | 4.9 | 2.7 |
| 2017-18 | 15.1 | 11.2 | 19.8 | 2.7 | 4.4 | 2.4 |
| Average | 16.7 | 11.4 | 20.7 | 4.1 | 6.4 | 2.4 |

*Notes*: Freezes and cuts show the percentage of job stayers with year-to-year no change and a negative change in basic wages. See Appendix Table E1 columns (II) and (III) for annual sample sizes. See Appendix Figure E1 for time series plots of the percentages of wage freezes. Inflation is measured as the April-to-April log change in the UK Consumer Price Index (CPI). The unemployment rate refers to UK individuals aged 16 and over, seasonally adjusted and for the second April of each period, expressed as a percentage of the economically active population. Both series are from the Office for National Statistics.

Participation (SIPP), reporting basic wage freezes of 40-50 percent in the US from year-to-year. However, wages can be reported with rounding errors in household surveys, tending to bias upward the observed prevalence of nominal wage freezes (Smith, 2000; Elsby and Solon, 2019). Second, Grigsby, Hurst, and Yildirmaz (2021) used data from a large US payroll service provider, finding that around a third of job stayers experience year-to-year basic wage freezes and only 2.4 percent of job stayers receive a basic wage cut.

Their results likely differ from ours, at least partially, because of differences in the analysed periods and differences between the US and UK labour markets. The sample of Grigsby, Hurst, and Yildirmaz covers the period from 2008 to 2016, while we additionally analyse also the pre-recession years 2006-08 and the post-recession years 2016-18. For the same shorter sample period as the US study, we find that the share of basic wage freezes among British job stayers increases by 3 percentage points compared with the longer 2006-2018 period.[12]

[12] Grigsby, Hurst, and Yildirmaz (2021) winsorised wage observations that fell below the US federal minimum wage. Since that minimum wage remained unchanged over their sample period from May 2008 to December 2016, apart from a single increase in July 2009, this potentially swept some true wage cuts into wage freezes, which would tend to overestimate freezes and underestimate wage cuts. However, the number of affected wage changes is likely not large enough to affect their estimates significantly: according to the Bureau of Labor Statistics, the share of employees paid at or below the federal minimum wage was 3 percent in

Furthermore, the annual US inflation rate was lower than in the UK over the respective study periods, on average, and the US experienced a significant period of deflation from March 2009 to October 2009, plummeting to an annualised inflation rate of -2.1 percent in July (see Appendix Figure E4). This deflationary episode approximately matches the period with the largest relative frequency of basic wage freezes among US job stayers, measured by Grigsby, Hurst, and Yildirmaz. In contrast, the UK inflation rate remained positive throughout the recession.

The US and UK labour markets also differ markedly in the proportions of employees covered by collective pay agreements (bargaining), which provides a proxy for the degree to which unions are capable of providing common standards of wages, working hours and working conditions. Holden and Wulfsberg (2014) have shown that the coverage of collective bargaining is negatively correlated with the incidence of wage cuts. According to OECD data, the proportion of US employees covered by a collective pay agreement was 12.8 percent on average throughout 2008-16, compared with 29.9 percent in the UK throughout 2006-18. All else equal, we would expect to observe fewer nominal wage cuts among job stayers in the UK than in the US, but the opposite is the case empirically. Finally, benefits-in-kind (e.g., private health insurance) provide an additional margin of adjustment for employers (Bewley, 1999; Lebow, Saks, and Wilson, 2003). The share of employees who receive fringe benefits is much smaller in the UK than in the US: over 75 percent of employees receive benefits-in-kind in the sample of Grigsby, Hurst, and Yildirmaz, while the share is only around 12 percent in the UK.[13] It is possible that firms prefer to cut benefits-in-kind rather than wages, because this labour cost adjustment might be less salient for employees. Finally, Grigsby, Hurst, and Yildirmaz exclude firms with 50 or fewer employees from their analysis. In Section 5, we will show that job stayers in such small firms are significantly more likely to receive basic wage freezes than in larger firms, which makes the large frequency of wage freezes documented by Grigsby, Hurst, and Yildirmaz in the US data even more striking. Taken together, it seems most likely that the greater prevalence of wage freezes found by Grigsby, Hurst, and Yildirmaz in the US than in our analysis of Great Britain is a result of the notably different inflation and recessionary environments across the two studies. The much greater tendency of employees to be compensated with benefits-in-kind in the US labour market than in the UK provides another plausible contributing factor.

Although the previous Section 3 argued that basic wages are the most relevant wage variable for macroeconomic models of wage stickiness, for comparison to the previous literature for Great Britain, specifically Nickell and Quintini (2003) and Elsby, Shin, and Solon (2016), we repeat the above analysis for exactly the same pay measure used in those studies: average earnings per hour, excluding overtime. We find evidence that this broader

2008 and reached 6 percent in 2010, before falling back to around 3 percent in 2016 (data available at https://www.bls.gov/cps/minwage2010.htm)

[13]We discuss the relevance of benefits-in-kind and evidence on their use in the UK in more detail in Appendix D.

pay measure also exhibits an asymmetric pay change distribution (Figure 3), albeit to a lesser degree than basic wages. Possible reasons why our conclusion differs from the previous UK

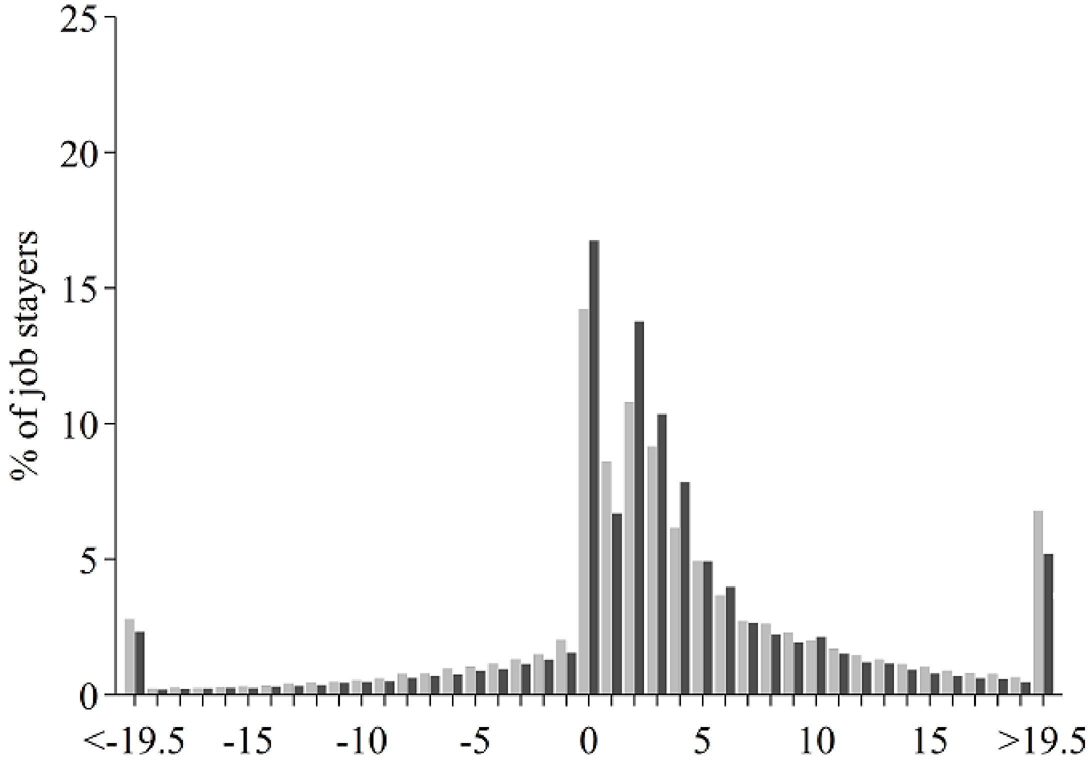


FIGURE 3: Frequency distribution of year-to-year changes in log earnings per hour, excluding overtime, of job stayers

*Notes*: See Figure 2. **Light bars**: salaried. **Dark bars**: hourly-paid.

literature, even when analysing the same pay measure, are that the rate of inflation was generally higher in earlier sample periods and that we define log changes of less than 0.005 as freezes to account for slight rounding errors in hours worked. Specifically, if an employee's pay period is a calendar month, but working hours are weekly, then the ASHE asks employers to multiply the weekly hours by 4.348 and to report the result as hours per calendar month. In our sample, almost 75 percent of job stayers are paid per calendar month. Because the ASHE questionnaire only allows employers to report hours and minutes worked, respondents have to round decimal values that result from the conversion of weekly hours. Unfortunately, no guidelines are provided to employers on how they should round in such cases. As Table 4 shows, using a range around zero of 0.005 to define zero nominal log wage changes increases the relative frequency of year-to-year nominal earnings per hour freezes from 6.8 percent to 14.6 percent for all job stayers in 2006-18, see columns (I) and (III), respectively. Similarly, when only log wage changes less than -0.005 are counted as cuts, the frequency of earnings per hour cuts in 2006-18 decreases from 21 percent to 16.3 percent. These results are consistent with findings in Elsby, Shin, and Solon (2016), who documented that for job stayers in 2008-09, for example, including small absolute changes in log earnings per hour of less than 0.01 within the definition of a freeze would increase the estimated frequency of wage freezes from 4.6 percent to 11.2 percent and reduce the estimated frequency of wage cuts from 19.4 percent to 16.1 percent.[14]

TABLE 4: Year-to-year changes in nominal earnings per hour, excluding overtime, of job stayers

| Years | *Exact* Freezes (%) (I) | *Exact* Cuts (%) (II) | Freezes (%) (III) | Cuts (%) (IV) |
|---|---|---|---|---|
| 2006-07 | 3.1 | 20.3 | 9.1 | 16.5 |
| 2007-08 | 3.2 | 17.9 | 7.4 | 14.4 |
| 2008-09 | 5.8 | 20.4 | 12.6 | 16.5 |
| 2009-10 | 8.7 | 24.3 | 17.7 | 18.8 |
| 2010-11 | 7.8 | 22.8 | 18.2 | 17.2 |
| 2011-12 | 9.7 | 23.4 | 20.6 | 17.0 |
| 2012-13 | 9.9 | 23.0 | 19.5 | 17.3 |
| 2013-14 | 6.5 | 20.4 | 13.8 | 16.1 |
| 2014-15 | 5.5 | 18.9 | 12.1 | 14.7 |
| 2015-16 | 6.6 | 20.7 | 14.5 | 16.1 |
| 2016-17 | 6.0 | 19.8 | 12.7 | 15.3 |
| 2017-18 | 6.4 | 18.8 | 13.8 | 14.4 |
| Average | 6.8 | 21.0 | 14.6 | 16.3 |

*Notes*: "*Exact* Freezes" show the percentage of job stayers with year-to-year changes in log earnings per hour, excluding overtime, of exactly zero. "*Exact* Cuts" show the percentage of job stayers with year-to-year changes in log earnings per hour, excluding overtime, of less than zero. "Freezes" and "Cuts" show the percentage of job stayers with year-to-year log changes in the interval [-0.5,0.5] and log changes less than -0.5, respectively, in earnings per hour, excluding overtime. See Appendix Table E1 column (I) for annual sample sizes.

### 4.2. Wage Growth Compression

Though striking, our discoveries, that wage freezes are far more likely than previously thought and that wage cuts are rare in Great Britain, do not provide clear evidence for the presence of DNWR on their own. For example, if wages were set according to implicit contracts between firms and workers, then this could account for the observed spike without wages necessarily being subject to DNWR (Barro, 1977). Therefore, to investigate whether firms are constrained by DNWR, we go beyond purely statistical indicators using the main insights and predictions derived from the theoretical framework of Elsby (2009).[15]

In Elsby's intertemporal framework, firms have a non-trivial wage-setting decision: they can cut nominal wages if they wish, but it will lead to a sharp reduction in worker productivity. This assumption is based on Bewley (1999), who was told by employers that the main reason they did not cut nominal wages was their belief that this would harm employee morale, a key

[14] We thank an anonymous reviewer for making us aware of the similarity between our results and those in Elsby, Shin, and Solon (2016).

[15] In a related study, Stüber and Beissinger (2012) also used the insights from Elsby (2009) to analyse real wage changes in West Germany.

determinant of worker productivity.[16] In this way, if a firm raises nominal wages to increase productivity but has to cut them by an equal amount in the future, then the result will be an overall loss in productivity. This means that nominal wage increases become partially irreversible. In an uncertain world, forward-looking firms would thus refrain from raising wages today, or at least be conservative when doing so, because otherwise there would be an increased likelihood of having to cut wages in the future. This wage-setting power of firms can be rationalised by, for example, search frictions in the labour market generating a positive *ex post* surplus once employer and employee are matched.

In Elsby's framework, when an idiosyncratic productivity shock hits an employer-employee match, the response of the nominal wage depends on the extent of DNWR. Without DNWR, the nominal wage change would equal the change in nominal productivity. With binding DNWR, the optimal wage change policy would instead take the form of a trigger strategy. In this case, large positive productivity shocks lead to wage increases, while large negative shocks lead to wage cuts. For intermediate values of the shock, there is a range of inaction where the nominal wage remains unchanged. When match-specific productivity grows at a common rate in the absence of shocks, and shocks are i.i.d. across employer-employee matches, this region of inaction shows up in the aggregated nominal wage change distribution as a spike at zero. Another consequence of DNWR implied by this theory is that if nominal wage changes occur, then they will be compressed relative to a world without DNWR.

According to Elsby (2009), DNWR can be detected by observing the differential effects of inflation and aggregate productivity growth on the percentiles of the nominal wage growth distribution. In the absence of DNWR, *real* wages should move one-to-one with aggregate productivity growth and inflation should have no effect. In contrast, if DNWR constrains firms' wage setting, then the theory predicts three effects: (1) A firm constrained by DNWR will moderate nominal wage raises, because raising today increases the likelihood of having to cut at a cost in the future. As inflation and productivity growth increase, the frictionless likelihood that the firm wishes to cut declines, and so the firm compresses nominal wage growth less. Therefore, on average, we should observe the upper percentiles of the real wage change distribution rising with inflation and more than one-for-one with productivity. (2) Optimal wage setting under DNWR implies a range of inaction where nominal wages are kept constant. Equivalently, real wage growth equals minus the inflation rate. This is where inflation can *"grease the wheels of the labor market"* (Tobin, 1972), by bringing down the real labour cost without cutting nominal wages. (3) In very low percentiles of the wage growth distribution, nominal cuts will be compressed because of the disproportionate fall in productivity that they would cause. Higher productivity growth and/or inflation will

[16]The explanation of Bewley (1999) for DNWR is not the only one possible. For example, MacLeod and Malcomson (1993) and Holden (1994) argue that past wages are the baseline while a worker and a firm are negotiating a new level of pay.

increase wage growth in this lowest range, because firms expect that they will likely be able to reverse nominal cuts in the future and so refrain from making them in the first place. These effects should lead to a more than one-for-one increase in wage growth with productivity growth in the lowest percentiles. Table 5 summarises these predicted effects of inflation and productivity growth on the real wage growth distribution.

TABLE 5: Predicted effects of the inflation rate and productivity growth on the percentiles of the real basic wage growth distribution, according to Elsby (2009)

| $n$-th percentile of the real wage growth distribution ($P_n$) | Coefficient on | |
|---|---|---|
| | Inflation rate | Productivity growth |
| $P_n >$ minus inflation rate | $> 0$ | $> 1$ |
| $P_n \approx$ minus inflation rate | $< 0$ | $< 1$ (attenuates toward zero) |
| $P_n <$ minus inflation rate | $> 0$ | $> 1$ |

To estimate the effects of inflation and productivity growth across the real wage growth distribution, we apply an unconditional quantile regression (UQR) approach (Firpo, Fortin, and Lemieux, 2009). As Stüber and Beissinger (2012) explain, this approach is preferable to the seemingly unrelated regressions (SUR) adopted by Elsby (2009) to test his theory, as UQR allows the whole distribution of the chosen set of explanatory variables to be taken into account when modelling real wage growth. To maintain consistency with the statistical evidence presented above, we estimate the UQR models separately for salaried and hourly-paid job stayers and for selected quantiles, $\tau$:

$$\widehat{Q}_\tau + \frac{\tau - \mathbf{1}\{y_{irt} \leq \widehat{Q}_\tau\}}{f_y(\widehat{Q}_\tau)} = \beta_{0,\tau} + \beta_{1,\tau}\mathrm{Inf}_t + \beta_{2,\tau}\mathrm{Prod}_{rt} + \beta_{3,\tau}U_{rt} + \beta_{4,\tau}U_{r,t-1} + \mathbf{x}'_{it}\boldsymbol{\delta}_\tau + \psi_{r,\tau} + \varepsilon_{irt,\tau}\ . \quad (2)$$

The dependent variables are the job-stayer re-centred influence functions (RIFs) for each selected quantile of log real basic wage changes, $y_{irt}$, where $\widehat{Q}_\tau$ gives the estimate of an unconditional quantile of $y_{irt}$ from the estimation sample and $f_y(\cdot)$ is the density of the marginal distribution of those log real basic wage changes in a given year, estimated using the standard combination of a Gaussian kernel and a Silverman plugin bandwidth. Our first explanatory variable is the national inflation rate, $\mathrm{Inf}_t$, measured as the change in the log of the UK Consumer Price Index (April-to-April). To proxy for the common rate of match-specific productivity growth in Elsby's framework, we follow Elsby (2009) and Stüber and Beissinger (2012) by using the average annual log real wage growth among all the job stayers in ASHE from the same EU-NUTS1 region $r$ as an individual $i$, given by $\mathrm{Prod}_{rt}$. Average real wage growth is a reasonable proxy, because Proposition 4 in Elsby (2009) states that, in steady state, DNWR has no effect on aggregate real wage growth. For robustness, we also carried out all the estimations using regional log gross value added per worker as an alternative proxy for productivity growth, finding that this did not alter the results notably. Having to rely on proxies for match-specific productivity growth likely introduces measurement error and attenuates the estimated effects of productivity growth towards zero. We prefer the regional log real wage growth among job stayers, as the proxy, because it takes into account the composition of our job stayer sample and indicates the alternatives for employees. We also include current and one-year lagged regional unemployment rates in the models as control variables, $U_{rt}$ and $U_{r,t-1}$, as well as region fixed effects, $\psi_{r,\tau}$. At the job-year level, we include further control variables in $\mathbf{x}_{it}$ for the gender, age, relative pay-level and collective agreement status of the employee, plus the private sector status and industry of the employer, as well as its size and employment growth. The latter set of control variables is also used in the heterogeneity analysis later (Section 5) and are further described in Appendices A & G. The coefficients in the UQR models can be interpreted as the effects on the $\tau$th quantile of real wage growth from a unit change in the associated explanatory variable for everybody in the estimation sample, i.e., all job stayers over all years.[17] For example, $\widehat{\beta}_{1,50}$ provides the estimated effect on median real wage growth from inflation being one percentage point higher in every year represented in the sample, holding all the other variables in the model constant.

The results from estimating these UQRs are reported in Table 6 for selected percentiles, displaying only the coefficients for the explanatory variables of interest, inflation and productivity growth, as well as the means (percentiles) of the dependent variable, where we are primarily interested in whether the signs of these estimates match the predictions in Table 5. The results provide clear evidence that the upper tail of the wage growth distribution in Great Britain is compressed by DNWR, for both hourly-paid and salaried job stayers. The estimated coefficients on inflation and productivity growth are consistent with the theoretical predictions in Elsby (2009). Specifically, at the 80-90th percentiles, the influence of inflation is significantly positive and the coefficients on productivity growth are substantially larger than one.

Table 6 also displays the estimates for the impact of productivity growth and inflation on the lower percentiles of the wage growth distribution. The theoretical predictions for these percentiles depend on the position of zero real wage growth in the overall distribution, which varies year-to-year and with the inflation rate. Over our whole sample period, annual average CPI inflation in the UK was 2.4 percent. As predicted by the theory, we find the most negative coefficient on inflation at the 20th percentile, and the coefficient on productivity growth is the most attenuated toward zero at this percentile. The results for the 10th percentile are also consistent with the theory, because relative to the 20th percentile the effect of inflation is diminished and the effect of productivity growth is larger. The evidence is somewhat mixed for intermediate percentiles of the wage growth distribution. Wage growth above the rate of inflation is predicted to increase with higher inflation rates, and the coefficient on productivity growth should theoretically be above one. However, annual inflation rates fluctuated from

[17] See Firpo, Fortin, and Lemieux, 2009; Rios-Avila, 2020; Rios-Avila and de New, 2022 for discussions on the interpretation of unconditional partial effects using RIF-regressions and UQR specifically.

-0.2 to 4.4 percent over 2006-2018. These values imply that the 10-50th percentiles of the real wage growth distribution were at some point equal to minus the rate of inflation, for which the theory predicts a negative coefficient on inflation and an attenuation toward zero for the coefficient on productivity growth. Taken together, our findings provide significant evidence for compression across the basic wage growth distribution, and hence for the presence of binding DNWR.

TABLE 6: The estimated effects of inflation and productivity growth on percentiles of real basic wage growth for job stayers

| | Salaried | | | Hourly-paid | | |
|---|---|---|---|---|---|---|
| Percentile $\tau$ | Wage growth ($\widehat{Q}_\tau$) (I) | Inflation ($\widehat{\beta}_{1,\tau}$) (II) | Productivity ($\widehat{\beta}_{2,\tau}$) (III) | Wage growth ($\widehat{Q}_\tau$) (IV) | Inflation ($\widehat{\beta}_{1,\tau}$) (V) | Productivity ($\widehat{\beta}_{2,\tau}$) (VI) |
| p10 | −0.047 | −0.247 | 0.354 | −0.029 | −0.281 | 0.299 |
| | | (0.028) | (0.033) | | (0.011) | (0.011) |
| p20 | −0.027 | −0.393 | 0.226 | −0.023 | −0.621 | 0.075 |
| | | (0.008) | (0.008) | | (0.010) | (0.009) |
| p30 | −0.017 | −0.311 | 0.765 | −0.014 | −0.399 | 1.053 |
| | | (0.014) | (0.015) | | (0.025) | (0.023) |
| p40 | −0.005 | −0.138 | 0.911 | −0.004 | −0.196 | 0.752 |
| | | (0.008) | (0.009) | | (0.015) | (0.015) |
| p50 | 0.003 | −0.156 | 0.757 | 0.002 | −0.351 | 0.520 |
| | | (0.007) | (0.006) | | (0.011) | (0.013) |
| p60 | 0.013 | −0.199 | 0.843 | 0.010 | −0.580 | 0.575 |
| | | (0.012) | (0.012) | | (0.019) | (0.021) |
| p70 | 0.027 | 0.062 | 0.952 | 0.019 | −0.348 | 0.820 |
| | | (0.020) | (0.018) | | (0.023) | (0.021) |
| p80 | 0.055 | 0.374 | 1.244 | 0.033 | 0.420 | 1.249 |
| | | (0.030) | (0.028) | | (0.036) | (0.044) |
| p90 | 0.115 | 0.789 | 1.651 | 0.073 | 1.576 | 3.045 |
| | | (0.052) | (0.048) | | (0.102) | (0.104) |

*Notes*: Results of unconditional quantile regressions, Equation (2): selected coefficients; bootstrapped standard errors in parentheses, 50 replications; Gaussian kernel and Silverman's plug-in optimal bandwidth.
First and fourth columns give the percentiles of real wage growth (i.e., the means of the respective dependent variables in Equation (2)).
Sample sizes: Salaried job stayers, 725,324; hourly-paid job stayers, 319,822.
Controls included: regional unemployment rate and its one-year lag, indicator variables for NUTS1 British regions, as well as the control variables included in the probit model estimates described in Section 5 & Appendix G, except the indicator for a rounded hourly rate of pay.
The explanatory variables are 'Inflation': measured as the change in the log of the UK CPI (April-to-April); and 'Productivity': the year-to-year change in average log real wages in the corresponding region for salaried or hourly-paid job stayers.

## 5. Heterogeneity in Wage Freezes and Cuts across Workers and Firms

The ASHE data contain detailed job characteristics reported by employers (e.g., collective pay agreements) or retrieved from administrative data sources (e.g., number of employees in the firm). To isolate the effects of various worker, job, and firm characteristics on the probability that a year-to-year basic wage cut or freeze is observed, we estimate probit models separately for salaried and hourly-paid job stayers. We include year-fixed effects to account for the state of the aggregate business cycle. Appendix G contains full details and results, and here we provide a brief summary, focusing on the most salient patterns.

We find statistically significant gender differences in conditional year-to-year basic wage changes. Cuts are more common for job stayers who are male rather than female, and basic wages freezes are more commonly found among hourly-paid men (Tables G1 and G2, first and second rows). However, the magnitudes of these differences do not seem economically significant for the average job stayers.[18] Private sector job stayers are less likely to experience wage freezes than non-private sector workers. Job stayers in a firm where wages are set according to a collective agreement are on average significantly less likely to see their wages cut or frozen. We find stark age-specific differences in the conditional probabilities of having basic wages cut or frozen: Job stayers aged 15-29 years are significantly less likely to receive a wage freeze than 45-64 year-olds (salaried, 9.6 percentage points less; hourly-paid, 7.4 percentage points less), and their probabilities of basic wage cuts are also lower. Note that these results control for firm growth and the place of workers in the earnings distribution, which control to some extent for worker life-cycle effects. Job stayers with earnings below 2/3 of the median are both significantly less likely to receive the same wage next year and significantly more likely to receive a basic wage cut, compared to median or high earners. The model estimates also show that the probability of a wage cut is significantly higher outside the "Wholesale & Retail Trade, Hotels & Restaurants" sectors, and basic wage freezes for hourly-paid job stayers occur significantly more frequently (5.5 percentage points) outside this sector.

Focusing on one of the starker sets of differences between jobs, Figure 4 displays the distribution of job-stayer log changes in basic wages separately for small firms with less than 50 employees and those with at least 50. As rows 13-16 in Table G1 show, wage freezes in very large British firms with more than 5,000 employees are strikingly less common than in smaller firms, conditional on several other observable characteristics of job stayers. The conditional probability of basic wage freezes for salaried employees in small firms is 12.0 percentage points greater than in very large firms, and 18.2 percentage points greater for hourly-paid job stayers. This is mainly accounted for by less common moderate basic wage growth rather than fewer wage cuts in smaller firms, as Figure 4 shows. These results support

[18] At the sample average and conditional on other characteristics, salaried (hourly-paid) male job stayers are 0.008 (0.013) more likely to have their wages cut than salaried (hourly-paid) female job stayers.

the US-based findings of Kurmann and McEntarfer (2019), who documented similar features for the distribution of changes in total earnings per hour (basic wages plus extra pay) by firm size. The US payroll dataset studied by Grigsby, Hurst, and Yildirmaz (2021) substantially under-represents very large firms with more than 5,000 employees, which can account for a sizable portion of employment - the median firm size in our British payroll data for job stayers is over 2,500 employees (see Table 1). Grigsby, Hurst, and Yildirmaz acknowledged this as a limitation in their study.

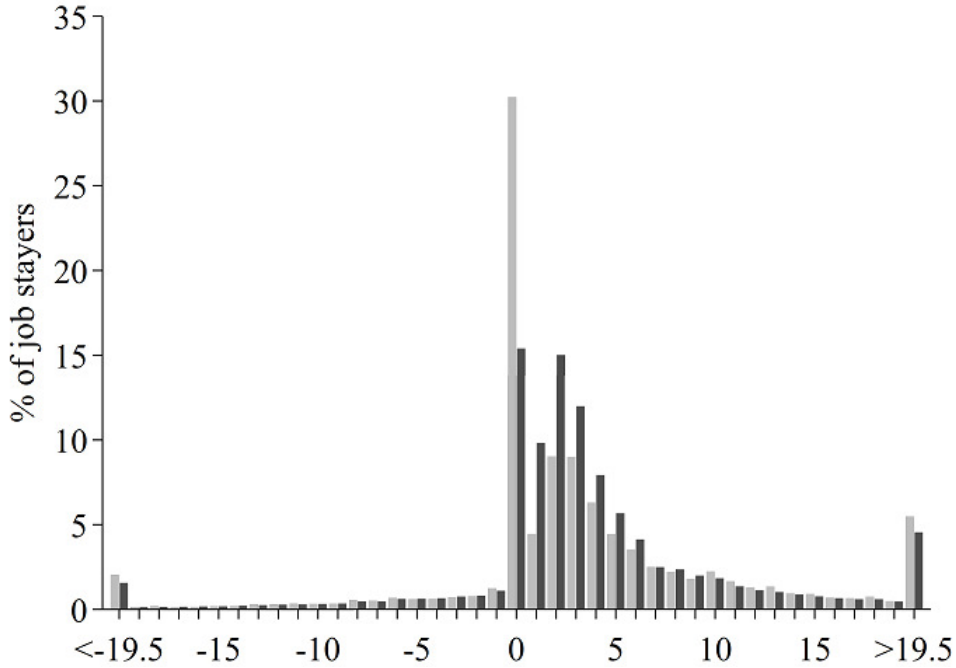


FIGURE 4: Frequency distribution of year-to-year changes in log basic wages of salaried and hourly-paid job stayers by firm size

*Notes*: See Figure 2. Pooled data for 2006-18. **Light grey bars**: small firms (< 50 employees). **Dark grey bars**: medium and large firms (≥ 50 employees). The size of a job stayer's firm is defined in the first of two consecutive years. See Appendix Figure E2 for a breakdown of the distribution for firms ≥ 50 employees into medium (50-249) and large (250+).

After controlling for other observable differences between job stayers and the state of the aggregate business cycle, wage freezes are significantly more likely in shrinking than expanding firms, approximately by 4-5 percentage points (rows 19-21, Tables G1 & G2). These results support the same inverse relationship between the frequency of total earnings per hour freezes and firm growth found by Kurmann and McEntarfer (2019) in the US. However, our data do not allow us to assess empirically how many workers in shrinking firms lost their jobs because of the nominal rigidity in basic wages, since we cannot exclude that negative idiosyncratic shocks to firm-level productivity caused both more layoffs and more freezes when comparing shrinking and expanding firms.[19]

[19]We also find that the results are not notably different when we focus on firms with more substantial employment changes of at least 10% or 1000 employees. Further, see Appendix Figure E3 for the distribution of year-to-year log basic wage changes, dependent on whether a firm's total number of employees was shrinking or expanding between the same years. A relatively large zero spike for shrinking firms is clearly visible, and

Lastly, we focus on hourly-paid workers, for whom employers directly report basic wage rates in the ASHE. We find that year-to-year freezes are significantly more common among hourly-paid job stayers who had basic wages in the previous year that were multiples of ten pence (rows 22-23, Table G1).[20] There are two possible explanations. First, employers are incorrectly reporting rounded basic wages. Second, round basic wages are accurately reported and just happen to be more rigid, for example, if round basic wages are somehow preferred. This idea can be found in the price-point literature, where a set of pre-specified prices, or wages in our case, can simplify the decision problem of boundedly rational workers and employers. For example, Hahn and Marenčák (2020) have shown that incorporating output-price points can improve how New Keynesian models match key business cycle statistics, such as the dynamics of the inflation rate. The extent to which 'wage-points' can improve the performance of monetary models might be a promising area for future research.

## 6. Macroeconomic Consequences of Downward Nominal Wage Rigidity

What are the macroeconomic implications of our main findings? In the New Keynesian model, the workhorse model of monetary policy analysis, wage rigidity is the crucial nominal friction required to match the persistence of inflation and output, and for monetary policy to have real effects (Christiano, Eichenbaum, and Evans, 2005). This class of models normally assumes that nominal wages can be adjusted every quarter with a constant probability, so-called 'Calvo wage setting' (Calvo, 1983). Our findings that basic wage freezes affect on average 17 percent of salaried job stayers and 21 percent of hourly-paid job stayers imply average quarterly wage-change probabilities of 0.36 and 0.32, respectively.[21] These values support the degree of nominal wage rigidity that is required as an assumption in New Keynesian models to fit the observed persistence of output, inflation, and unemployment in the US (Christiano, Eichenbaum, and Evans, 2005) and the UK (Faccini, Millard, and Zanetti, 2013).

Our estimates for the extent of DNWR in Great Britain have potentially sizeable output consequences. We demonstrate this using the theoretical framework of the Phillips curve from the dynamic stochastic general equilibrium model of Benigno and Ricci (2011) (hereafter BR).[22] In their model, the long-run inflation-output trade off caused by DNWR is the result

positive wage growth is more likely in expanding firms. Note, job stayers in 2007-08 and 2017-18 are excluded from this analysis because it appears as though the ONS imputed the number of employees in firms using the previous years' values.

[20] Kahn (1997), using data on household heads from the Panel Study of Income Dynamics in 1970-1988, documented that hourly pay rates ending in exact dollar or half-dollar amounts made up 50 percent of the job stayers with hourly rate freezes.

[21] The values are converted as follows: $p_q = 1-(1-p_y)^{1/4}$, with $p_q$ denoting the quarterly wage-change probability, and $p_y$ the yearly wage-change probability.

[22] We use and adapt the model's replication code available on the website of the *American Economic Review*. Daly and Hobijn (2014) develop a similar framework of the Phillips curve, assuming a different wage setting process than Benigno and Ricci (2011), however, this does not affect the model's long-run outcomes.

of nominal rigidities that distort wage adjustments. Aggregate and idiosyncratic shocks lead to intratemporal and intertemporal shifts of real wages and employment across sectors. If a negative demand shock hits a firm, then it would like to reduce its real wages. When inflation is low, firms are more likely to need to decrease the nominal wage to bring about the desired real wage cut. With binding DNWR, nominal wages cannot be cut and, as a consequence, firms will instead reduce their employment and output. By allowing real wages to fall sufficiently, moderate inflation in the model can grease the wheels of the labour market. We provide more details of this framework and further results in Appendix H.

We first calibrate the model to match the shares of basic wage freezes and cuts that we observe in British payroll data (Section 4), and then we simulate the long-run Phillips curve. The solid line in Figure 5 shows the long-run relationship between the annual rate of inflation (vertical axis) and the equilibrium output gap (in percent), implied by our calibration.[23] For comparison, we also add a long-run Phillips curve for the extreme scenario in which nominal wages can never be cut (dashed line). The long-run output loss is around 0.2 percent of GDP, relative to an economy with flexible wages, when the annual rate of inflation is 4 percent. When the annual rate of inflation decreases to 2 (1) percent, the output gap becomes more substantial: for our calibrated parameter values, the solid line shows that the equilibrium output loss amounts to around 0.7 (1.3) percent of GDP. When the annual inflation rate is low, the constraint on downward nominal wage changes is more likely to bind and be more costly in terms of higher unemployment. The substantial economic costs point to an optimal rate of inflation that may be significantly positive rather than zero or negative (the Friedman rule).

## 7. Conclusion

Recent periods of stubbornly low inflation in many developed countries has pushed the topic of DNWR back to the centre stage of macroeconomics. In this paper, we use unique longitudinal employer-employee data from Great Britain, the Annual Survey of Hours and Earnings (ASHE), to analyse the extent of DNWR. These payroll-based data allow us to accurately distinguish between basic wages and extra pay components, such as overtime and incentive pay. Since a wealth of recent findings from administrative data has led researchers to question whether DNWR is actually a pervasive and consequential feature of labour markets, the need for additional evidence on this subject is great.

We provide several new findings. First, basic wages, which we argue are the relevant wage measure from a macroeconomic perspective, are downward rigid. Among employees who are constantly employed in the same job from year-to-year, every fifth job stayer, on average, receives a constant basic wage and this figure increased to around 28 percent during the

[23] We impose the constraint that the long-run equilibrium employment level cannot exceed the population size.

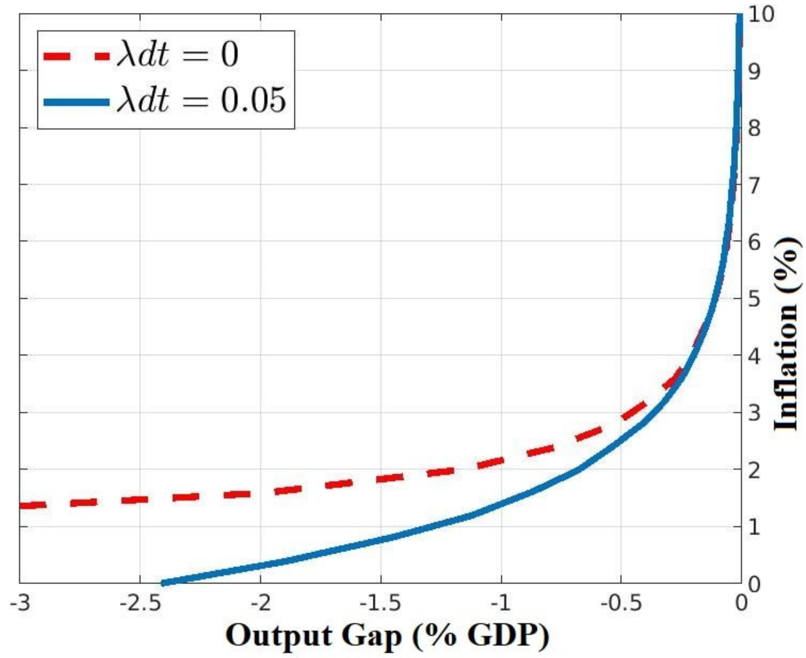


FIGURE 5: Long-run Phillips curve for different degrees of DNWR

*Notes*: Equivalent to Benigno and Ricci (2011): Figure 6, p. 1459. Simulation uses the same parameter values as BR, except calibrated $\lambda dt = 0.05$. The dashed line shows the long-run output gap (in percent of GDP) when nominal wages can never be cut. The solid line shows the gap when nominal wages become downward flexible once every five years, on average.

Great Recession. Basic wage cuts occur far less frequently than previously thought, with 11 percent of salaried job stayers and only 4 percent of hourly-paid job stayers receiving them on average each year. We show that the distribution of wage changes is markedly asymmetric, and provide strong evidence for the compression of wage growth in continuing employment relationships, suggesting that firms are constrained by binding DNWR.

Our findings support the anecdotal evidence presented by Bewley (1999), who gathered information on nominal pay setting by interviewing US employers. Bewley found that employers were reluctant to cut nominal wages because they feared it would damage worker morale, and in this way it would negatively impact productivity, labour turnover, and the recruitment of new employees. However, Bewley also found that most managers believed that cutting incumbent workers' wages would not prevent layoffs; labour is only a small share of variable costs and the short-run price-elasticity of product demand is low, such that pay cuts would create little extra work.[24] Although it is not clear how this anecdotal evidence generalises to the aggregate economy, it highlights the need to explore channels other than layoffs through which DNWR can have real effects.

[24] Employers that responded to financial distress during a recession with pay cuts reported to Bewley only minor problems with worker morale and productivity. However, it is possible that those employers that faced the least adverse consequences were exactly the ones enacting pay cuts.

Our second contribution demonstrates substantial heterogeneity in the apparent extent of DNWR, conditional on observable firm and worker-characteristics. Most notably, we find a strong negative correlation between firm size and the share of employees that experience basic wage freezes. Job stayers in firms with less than 50 employees are around twice as likely to receive the same wage year-to-year as job stayers in very large firms with more than 5,000 employees.

Our findings have important implications for macroeconomic models and monetary policy. We find empirical support in representative payroll microdata for the degree of nominal rigidity typically required in New Keynesian models to match the persistence of output and inflation (Christiano, Eichenbaum, and Evans, 2005). Additionally, our results justify the assumption of downward nominal rigidity invoked in recent macroeconomic models of business cycle fluctuations (e.g., Benigno and Ricci, 2011; Daly and Hobijn, 2014; Dupraz, Nakamura, and Steinsson, 2019). Researchers who are investigating macroeconomic variables in a low-inflation environment should consider incorporating DNWR into their models. Our result that basic wage freezes are far more common when wage rates are in multiples of ten pence suggests a potentially fruitful area for future research. Akin to the literature on price-points, where a set of pre-specified prices can simplify the decision problem of boundedly rational agents and improve the match of New Keynesian models to key business cycle statistics (Hahn and Marenčák, 2020), wage-points could improve the performance of monetary models further. Related, there are other patterns in these data which merit further thought and study, such as the fact that basic wage freezes are far more common in smaller firms.

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# The Extent of Downward Nominal Wage Rigidity: New Evidence from Payroll Data
# Online Appendix

Daniel Schaefer Carl Singleton[†]

## Appendix A. Further Description of the Data

In what follows, we provide further details on the datasets used. All the relevant documentation and variable descriptions are publicly available from the UK Data Service. The ONS has also published various documents concerning the quality and consistency of the ASHE.

For each year in the period 2004-2018, the ASHE should in principle be a random sample of all UK employees, irrespective of occupation, size of employer, etc. Given the legal obligation of employers to respond using their payrolls, it has a high response rate and is generally considered to be very accurate. Conditional on a hundred percent response and success in locating employees and employers, the ASHE should be a true one percent random sample of UK employees in each year: all those with an administrative life-long National Insurance number which has a numerical part ending in two specific digits are included in the sampling frame every year, making it a worker-level panel dataset. However, there are two major sources of systematic under-sampling of employees, both occurring if individuals do not have a current tax record when they are looked up in January each year. This could happen for some individuals who have recently moved jobs, or for those who earn too little (mostly part-time) to pay income tax or National Insurance. From 2004, the ASHE, after it replaced its predecessor the New Earnings Survey, aimed to sample some of those employees which were likely under-represented before. It added supplementary responses for those without income-tax payments, and also attempted to track employees whose jobs changed between the determination of the sampling frame in January and the reference period in April. There is no cumulative attrition from the panel, as any individual not included in the ASHE in any year, for whatever reason, remains in the sampling frame the following year. One exception to this occurs where individuals have been assigned temporary National Insurance numbers, typically in the case of non-UK nationals. Following ONS recommendations, we drop all person observations where there are inconsistencies in age or sex within the National Insurance number-based ASHE longitudinal person identifier variable, *piden*.

The ASHE data contain information on the legal status of a firm, obtained from the administrative Inter-Departmental Business Register (IDBR). We classify private companies, sole proprietors, and partnerships as belonging to the private sector, while state-owned enterprises, nationalised industries, central government, local authorities, and non-profit organisations form the non-private sector in our analysis. The measure of firm size is also taken from the IDBR and refers to the total number of employees in the enterprise. An employee is working full-time hours when she works at least thirty hours per week. Some businesses have an arrangement with the ONS to provide their data electronically, which seems to be the case for very large enterprises in particular. Employees of such enterprises tend to have lower basic wages. Whenever it is reported that an employee's basic wage is

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set with reference to an agreement affecting more than one worker, e.g., pay agreed by a trade union or a workers' committee, we classify this employee's wage as affected by a collective agreement.

From 2005, a new questionnaire was introduced to the ASHE, which was intended to reduce the latitude for respondents' own interpretations of what was being asked of them. Prior to 2005, job stayers were identified by asking whether the job title and the description were the same as in the last reference period. From 2005 onward, the question became more specific, asking whether the employee had worked in the same job and role for more than a year. This relates to the ASHE identifier variable for job stayers, *sjd*, which we use to define job stayers for our analysis. For a very small number of job stayers, apparent inconsistencies can arise in the dataset: a worker can be marked as being in the same job, but the enterprise reference number can change. This is probably because of a change in a firm's ownership structure (e.g., a merger or a succession), which would result in a new administrative enterprise reference number being assigned, while the ASHE questionnaire explicitly tells the respondent to ignore such changes when answering whether an employee is working in the same job. We also ensure that each job stayer in our analysis features consecutive year-to-year job and wage records.

To avoid any potential errors, we also drop any worker observations for years with multiple job holdings, because in these cases it is not clear to which job the same-job marker refers to in the following year. Although the ASHE dataset is available from 2004 onward, we use only data starting from 2006 because some questionnaire changes in 2005 and 2006 introduced inconsistencies between these earlier years. Specifically, new instructions were included on how firms should report employees' hours worked. Before applying any further sample selection, the 2006-2018 ASHE panel dataset contains 2,096,927 worker-year observations. From this, we keep only observations where an employee is aged 16-64, and which have not been marked as having incurred a loss of pay in a reference period through absence, employment starting in the period, or short-time working, and which are marked as being on an adult rate of pay, dropping trainees and apprenticeships. This is practically the same filter applied by the ONS in their published results on UK "Patterns of Pay" using the ASHE. We drop observations with missing basic hours or earnings variables. We drop observations with over a hundred or less than one basic hour worked, as these could reflect measurement error or the inclusion of overtime. Applying these steps leaves 1,924,873 worker-year observations. To further address some potential for measurement error, we drop a further 3,571 observations whose derived hourly rates of pay, excluding overtime, are less than 80 percent of the applicable National Minimum Wage (NMW) each April, with allowance for the different age-dependent rates of the NMW over time. We set the threshold lower to avoid dropping observations where employers have rounded figures about the NMW, where the degree of rounding could vary with the actual value of the NMW, a behaviour which has been hypothesized by the ONS. Note, any such rounding for pay measures, conditional on accurate hours records, would tend to bias upwards the incidence of nominal wage rigidity. The ASHE has introduced some earnings imputations, using similar matched 'donor' observations where responses were, for example, missing an entry of basic hours but had recorded basic pay. Hourly pay rates were never imputed. We exclude from our main results the approximate one percent of the remaining observations where imputations were made to hours or basic pay, in the period they can be identified of 2013-2018. By focusing on this period, we were able to confirm that not dropping these observations did not meaningfully alter any of our results. Finally, before focusing on job stayers, we trim the dataset by sequentially dropping the top and bottom 1% of basic wage observations. We trim twice as there are a tiny number of observations where there are clear coding errors for basic wage changes, such as £9.34 per hour in one year but £934 in the next year for a job stayer.

We use the following pay variables from the ASHE, summarised in Table A1. Basic pay is a worker's regular pay before adding anything extra. The dataset contains two measures of labour input: basic hours and overtime, whereby the former also includes hours worked at a shift premium. If an employer calculates an employee's basic pay by multiplying their basic hours worked by a pay rate, then the employer is also required to report this rate. The employer also reports the pay period, which is most often either weekly, fortnightly, four-weekly or a calendar month. We do not observe the reported totals for these periods, but instead the ONS derives weekly averages of variables.

TABLE A1: Overview of pay variables

| | | Description |
|---|---|---|
| | **ASHE variables** | |
| 1. | Basic pay | All basic pay, excluding any extra payment |
| 2. | Basic hours | Hours relating to basic pay (incl. hours paid at shift premium) |
| 3. | Hourly pay rate | Reported hourly rate of pay where applicable (see text) |
| 4. | Overtime pay | Total overtime pay in reference period |
| 5. | Shift premium pay | Premium payments for shift work, night or weekend work |
| 6. | Incentive pay | Incentive pay received for work carried out in the pay period |
| 7. | Other pay | Pay received for other reasons, e.g., travel allowances |
| 8. | Gross pay | Total pay received |
| 9. | Annual gross pay | Annual gross earnings paid for the preceding tax year |
| 10. | Annual incentive pay | Component of annual gross pay from incentive payments |
| | **Derived variables** | |
| 11. | Basic wage | Basic pay divided by basic hours |
| 12. | Earnings per hour, excl. overtime | Gross pay, excluding overtime pay, divided by basic hours |
| 13. | Gross earnings per hour | Gross pay divided by the sum of basic and overtime hours |

The key earnings variables that we analyse are the answers to the following questions in the ASHE questionnaire, whereby monetary values are measured in Pound sterling (GBP), including pence, and time variables are reported in hours and minutes:

BPAY: "How much basic pay, before deductions, did the employee receive in the pay period?
Include: all basic pay, relating to the pay period, before deductions for PAYE, National Insurance, pension schemes, student loan repayments and voluntary deductions. Include paid leave (holiday pay), maternity/paternity pay, sick pay and area allowances (e.g., London).
Exclude: pay for a different pay period, shift premium pay, bonus or incentive pay, overtime pay, expenses and the value of salary sacrifice schemes and benefits in kind."

BHR: "How many basic hours does [basic pay] relate to?
If your pay period is calendar month and hours are weekly, multiply the weekly hours by 4.348 to get calendar month hours. If the employee uses a decimal clock, please convert to hours and minutes. For example, 4.3 hours should be 4 hours and (0.3 multiplied by 60) minutes = 4 hours 18 minutes.

Include: any hours paid at shift premium and paid hours even if not worked.
Exclude: any hours paid as overtime."

HPAY: "[W]as the employee's basic pay in the pay period calculated by multiplying the number of hours they worked by an hourly rate of pay? [If yes,] what was the employee's hourly rate of pay in the pay period?"

OVPAY: "How much overtime pay did the employee receive for work carried out in the pay period?
Exclude: any basic, shift premium and bonus or incentive pay in this period, as well as overtime pay from the previous pay period."

SPPAY: "How much shift premium pay did the employee receive in the pay period?
Include: the element of shift premium pay. For example, for a 35 hour pay period, if the basic rate is £10 per hour and the premium rate is £12 per hour, multiply the difference of £2 by the hours worked (i.e. 35 multiplied by 2). The shift premium pay reported would therefore be £70.
Exclude: any basic, overtime and bonus or incentive pay."

IPAYIN: "How much [bonus or incentive payments did the employee receive,] related to work carried out in the pay period?
For example, if [an annual bonus was paid], the value should be divided by 12 if the employee was paid on a calendar month basis.
Include: profit sharing, productivity, performance and other bonus or incentive pay, piecework and commission.
Exclude: basic, overtime and shift premium pay."

OTHPAY: "How much pay did the employee receive for other reasons in the pay period?
Include: for example, car allowances paid through the payroll, on call and standby allowances, clothing, first aider or fire fighter allowances.
Exclude: paid leave (holiday pay), basic, overtime, shift premium, maternity/paternity, sick, bonus or incentive pay, redundancy, arrears of pay, tax credits, profit share and expenses."

GPAY: "How much gross pay, before deductions, did the employee receive for work carried out in the pay period?
Include: pay before deductions for PAYE, National Insurance, pension schemes, student loan repayments and voluntary deductions. Include basic, overtime, shift premium, bonus or incentive pay and any other pay.
Exclude: expenses and the value of salary sacrifice schemes."

AGP: "How much annual gross pay did the employee receive in their current job?
Include: pay before deductions for PAYE, National Insurance, pension schemes and voluntary deductions. Include basic, overtime, shift premium, profit sharing, productivity performance and bonus or incentive pay.
Exclude: any payments for expenses or previous employment."

ANIPAY: "How much of [AGP] is related to bonus or incentive payments for their current job?
Include: profit sharing, productivity performance and other bonus or incentive pay, piecework

and commission.

Exclude: basic, overtime and shift premium pay."

One take-away from these questions is that basic pay excludes shift premium pay, while basic hours include shift premium hours. Therefore, if the shift pattern of a worker changes between years, basic hours might change while basic pay remains unchanged, all else equal.

For reference, Table A2 display the distribution of observation over industries, separately for salaried and hourly-paid job stayers.

TABLE A2: Distribution of job stayers over industry sectors

| Industry (SIC2003) | Hourly-paid (I) | Salaried (II) |
|---|---|---|
| A-F: Agric., Mining, Manuf., Energy, Constr. etc. | 0.20 | 0.15 |
| G-H: Wholesale & Retail Trade, Hotels & Restaurants | 0.32 | 0.13 |
| M-N: Education & Health | 0.27 | 0.30 |
| I-L, O: Other Services | 0.22 | 0.42 |

*Notes*: Statistics use the later period for each job-stayer observation. Classification according to the ONS Standard Industrial Classification 2003. We convert ONS Standard Industrial Classification (SIC) 2007 to 2003, using files made available by the UK Data Service. This conversion uses the 2008 Annual Respondents Dataset, where both classifications were applied, and where any 2007 code mapping to multiple 2003 codes is decided using whichever of the two bore a greater share of economic output.

## Appendix B. Further Details on the Composition of Pay

This section provides more details on the extra pay components besides basic wages, combined for hourly-paid and salaried job stayers.

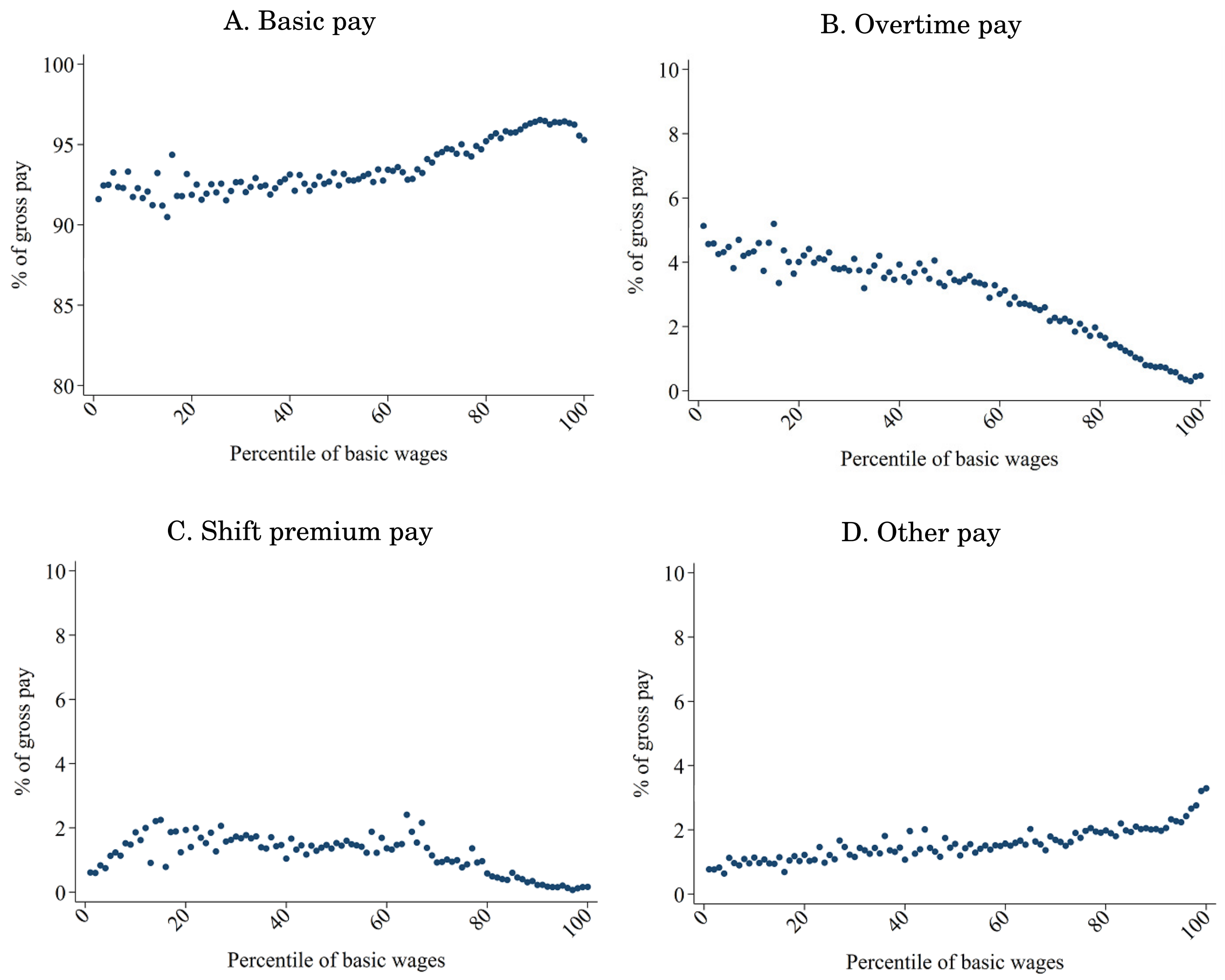


FIGURE B1: Non-incentive pay along the basic wage distribution

*Notes*: Average shares of pay component within the corresponding percentile of the basic wage distribution. Data pooled across all years.

The importance of paid overtime for job stayers declines with the basic wage (Figure B1B). While overtime accounts for almost 5 percent of total earnings in the bottom percentile and around 3 percent at the median, its share in total earnings is less than 1 percent in the highest decile. Shift premiums typically contribute less than 2 percent of total earnings, and are negligible in the top percentiles of basic wages (Figure B1C). A high basic wage is associated with a high share of total earnings from other pay, such as travel allowances (Figure B1D). Generally, overtime, shift premiums, and incentive pay all contribute relatively little to the level of total earnings in the top percentiles.

Benefits-in-kind, such as employer-sponsored health insurance, are not captured by the ASHE. According to Gu, Prasad, and Moehrle (2020), the value of not-legally required benefits in the US is substantial, accounting for about 21 percent of the total compensation of US employees on average over 1982-2018. The largest benefit is employer-provided health insurance, which accounts for over 8 percent of total compensation in the US. Benefits are also pervasive in the US labour market: Grigsby, Hurst, and Yildirmaz (2021) find that over 75 percent of job stayers in their payroll dataset receive benefits, again largely accounted for by employer-provided health insurance. To better understand the importance of benefits-in-kind in the UK, we use public aggregate data from the Benefits in Kind Statistics Table 4.2 and Table 4.5 from HM Revenue & Customs.[3] The two main categories of benefits-in-kind are private health and dental insurance, and company-provided car, which together account for almost all benefits-in-kind (other benefits include beneficial loans and provided accommodation). For each year, we separately compute the shares of employees who receive a positive amount of any form of benefit, private health insurance, and company-provided car. Figure B2 shows that the share of employees who received any type of benefit declined from over 13 percent in 2006 to 11 percent in 2018. The share of employees who received private health insurance was roughly constant at almost 8 percent. The share of employees who had a company car declined from 4 percent in 2006 to only 2 percent in 2018. Figure B3 shows the value of benefits-in-kind relative to annual gross pay for the tax year April 2016 to April 2017, as reported by firms to HM Revenue & Customs. The results for other years are very similar. The data allow us to analyse bands of annual gross pay, as indicated on the horizontal axis. The largest share of benefits is around 5 percent of annual gross pay, which is received by employees who earn between 40,000 to 50,000 GBP per year (the lower bound corresponds to the 79th percentile of the UK annual gross pay distribution, and the upper bound to the 88th percentile). Benefits-in-kind account for less than 3 percent of pay among employees with annual gross pay above 100,000 (97th percentile of the annual gross pay distribution).

[3]Data are from Table 4.2 - Total expenses and benefits for directors and employees by range of total income, and Table 4.5 - Time series of recipients and amounts of taxable benefits, available at https://www.gov.uk/government/statistics/total-expenses-and-benefits-for-directors-and-employees-by-range-of-total-income and https://www.gov.uk/government/statistics/number-of-recipients-and-amounts-of-taxable-benefits-by-type-of-benefit, respectively.

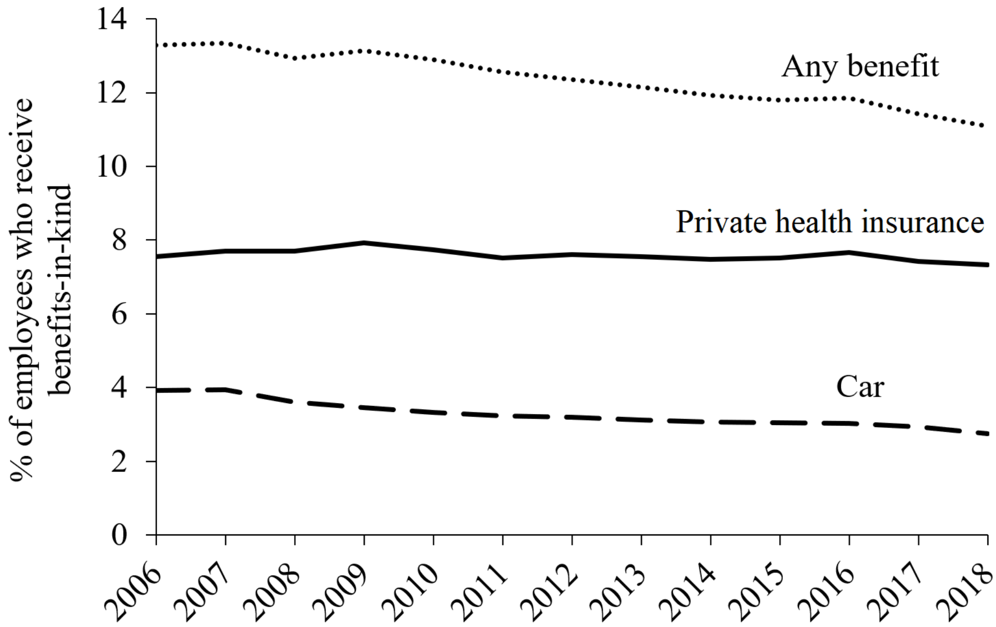


FIGURE B2: Share of all employees who receive benefits-in-kind, 2006-18

*Notes*: 'Private health insurance' shows the share of employees who receive at least some amount of taxable benefits-in-kind in the form of a private health insurance. 'Car' shows the share of employees who have a company car. 'Any benefit' is the sum of the two above categories plus a residual term that contains, e.g., beneficial loans or provided accommodation.
*Sources*: Number of employees from UK National Accounts, the Blue Book time series, ID: MGRZ; receipt of benefits-in-kind: Taxable benefits in kind and expenses payments statistics, HM Revenue & Customs.

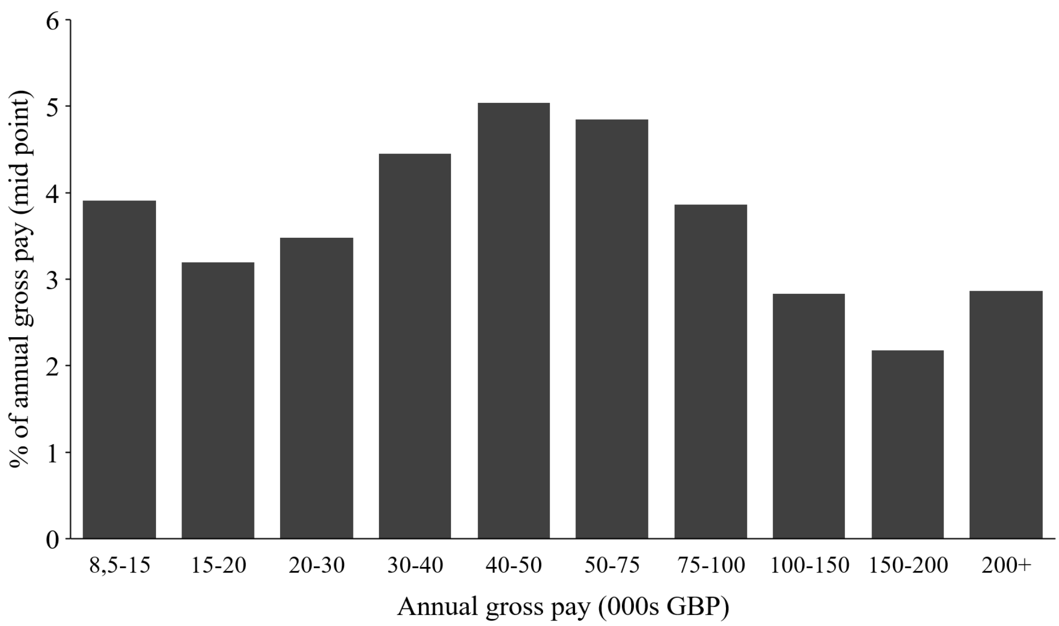


FIGURE B3: Taxable value of benefits-in-kind as share of annual gross pay in the tax year 2016-17, all employees

*Notes*: The share in annual gross pay is computed using the mid point of the displayed range of each category as the denominator, except for the last category which uses 200,000 as the denominator.
*Sources*: Taxable benefits in kind and expenses payments statistics, HM Revenue & Customs.

# Appendix C. The Persistence of Pay Components

To assess the persistence of the different components of pay, we ask two questions about the extensive and intensive margin of extra pay. First, how likely is it that a job stayer receives some form of extra pay, conditional on her having received the same component last year? Second, conditional on receiving extra pay in consecutive years, how is the pay amount changing? To answer the first question, we estimate a linear probability model for the propensity to receive extra pay components:

$$D_{ijt} = \gamma D_{ijt-1} + u_{ijt} \tag{3}$$

where $D_{ijt}$ equals one if worker $i$ in job $j$ in year $t$ receives a particular extra pay component. We restrict the estimation sample to year-to-year job stayers. The results are reported in Table C1. Columns (I)-(IV) show that the propensity to receive non-basic pay in year $t$, conditional on having received the same extra pay in year $t-1$, is positive and significant. For example, salaried job stayers who received a positive amount of overtime pay last year, have a 52.3 percent likelihood of being paid for overtime in the current year, on average.

TABLE C1: Estimated year-to-year persistence of receiving extra pay components for job stayers

| | Overtime<br>(I) | Incentive pay<br>(II) | Shift premium pay<br>(III) | Other pay<br>(IV) |
|---|---|---|---|---|
| Salaried | $0.523^{***}$ | $0.481^{***}$ | $0.523^{***}$ | $0.614^{***}$ |
| | (0.002) | (0.003) | (0.002) | (0.002) |
| Hourly-paid | $0.546^{***}$ | $0.539^{***}$ | $0.546^{***}$ | $0.522^{***}$ |
| | (0.002) | (0.004) | (0.002) | (0.002) |

*Notes*: Least squares coefficient estimates of $\gamma$ in Equation (3).
$^{***}$ indicates significance from zero at the 0.1% level, two-sided tests, and standard errors in parentheses are robust to clustering within match.
Sample sizes: 691,521 salaried job-stayers and 416,497 hourly-paid job-stayers

For a better understanding of the persistence of individual pay components and, therefore, their likely impacts on the present value of labour costs, we follow Grigsby, Hurst, and Yildirmaz (2021) and estimate first-order autocorrelation coefficients at the intensive margin of each component, conditional on the receipt of the extra pay component in consecutive periods. For our sample of job stayers, we estimate the following regression, using least squares:

$$\log(\overline{w}_{ijt}) = \theta_{ij} + \rho \log(\overline{w}_{ijt-1}) + \overline{\mathbf{x}}'_{it}\overline{\boldsymbol{\delta}} + v_{ijt} \tag{4}$$

where $\overline{w}_{ijt}$ is the wage of worker $i$, in job $j$, and in year $t$; $\theta_{ij}$ is a fixed effect for an employer-employee match; $\rho$ measures the first-order autocorrelation; and $v_{ijt}$ is a random error term. The vector $\overline{\mathbf{x}}_{it}$ includes time-varying controls for firm size, age, age squared, and tenure squared.

Table C2 shows the autocorrelation estimates, computed separately for salaried and hourly-paid job stayers. The first column shows that basic wages are significantly positively autocorrelated. If basic wages are high in the current year, relative to the within-match average, then firms and workers can expect that basic wages will also be relatively high in the next year. This implies a strong impact of basic wage changes on the present value of the labour cost: for example, not only are the current basic wages high, they are also expected to exceed their average value in the match in subsequent years, conditional on the age of the employee and length of the match. The extra pay components affect the present value of labour costs less. For example, conditional on receiving overtime pay in two consecutive years, column (II) of Table C2 shows that the amount of overtime pay is significantly negatively autocorrelated across years within a match. This means that higher overtime pay than normal in one year is followed by lower overtime pay than normal in the next year, on average. This would serve as a strong offsetting force on the effect of overtime on the present value of labour costs. Similar results hold for incentive pay and shift premium pay (columns (III)-(IV)). However, for salaried job stayers, the autocorrelation of shift premium pay and other pay is not significantly different from zero at the 5 percent level, suggesting that, within a match, the amounts of these components are i.i.d., conditional on receipt. For hourly-paid job stayers, shift premium pay and other pay are significantly negatively autocorrelated. Taken together, these results suggest that basic wages are considerably more important for the present value of labour costs than extra pay components; the estimates here point to the allocative wage for job stayers in the ASHE being most closely approximated by basic wages.

TABLE C2: Estimated persistence of basic wages and non-basic pay components, job stayers

| | Basic wages (I) | Overtime (II) | Incentive pay (III) | Shift premium pay (IV) | Other pay (V) |
|---|---|---|---|---|---|
| Salaried | $0.220^{***}$ | $-0.116^{***}$ | $-0.068^{***}$ | $-0.021$ | 0.020 |
| | (0.005) | (0.007) | (0.016) | (0.017) | (0.011) |
| Hourly-paid | $0.199^{***}$ | $-0.125^{***}$ | $-0.129^{***}$ | $-0.063^{***}$ | $-0.064^{***}$ |
| | (0.008) | (0.007) | (0.021) | (0.012) | (0.012) |
| $N$ (in 000s) | | | | | |
| Salaried | 592 | 47 | 16 | 31 | 78 |
| Hourly-Paid | 325 | 54 | 11 | 35 | 29 |

*Notes*: Least squares coefficient estimates of $\rho$ in Equation (4).
$^{***}$, $^{*}$, $^{*}$ indicate significance from zero at the 0.1%, 1%, 5% levels, respectively. Two-sided tests, and standard errors in parentheses are robust to clustering within match.
Sample for each pay component restricted to job stayers who received the pay component in consecutive years.

## Appendix D. Measurement Error in Job-Stayer Wage Changes

This appendix discusses a previously undocumented source of errors in the ASHE dataset, concerning the accuracy of the reported hours of work. As mentioned earlier, if an employee's pay period is a calendar month, but working hours are weekly, then the ASHE asks employers to multiply the weekly hours by 4.348 and to report the result as hours per calendar month. In our sample, almost 75 percent of job stayers are paid per calendar month. Because the questionnaire only allows employers to report hours and minutes worked, respondents have to round decimal values that result from the conversion of weekly hours. Unfortunately, no guidelines are provided to employers on how they should round in such cases.

For example, if an employee works 40 basic hours per week, then the hours worked in the calendar month are 173 hours, 55 minutes, 12 seconds ($40 \times 4.348 = 173.92$). It is in the discretion of the person who is answering the questionnaire whether she rounds up this number to 173 hours and 56 minutes, or rounds it down to 173 hours and 55 minutes. Another potential source of error is the conversion from decimal to minutes: the employer might incorrectly supply decimal numbers instead of hours and minutes. For example, a 38-hour week results in a monthly value of 165.224 ($38 \times 4.348 = 165.224$), so 165 hours and 13 minutes (omitting seconds). It is conceivable that some employers might incorrectly report 165 hours and 22 minutes.[4] The ONS converts these reported values back to average weekly hours, dividing monthly hours by 4.348. This can potentially explain the relatively high frequency of weekly hours worked in the ASHE dataset with values of 39.999 and 40.003.

To gain a better understanding of the significance of this novel source of measurement error, we compute the share of job stayers with non-zero changes in working hours of less than one minute from year-to-year. This is the case for around 21.5 percent of salaried job stayers who are paid per calendar month in consecutive years. To check whether these small hours changes are more likely to be erroneous or actual changes, we turn to these affected job stayers' *weekly* pay. These data should be reported directly from payroll, without requiring a conversion for the pay period, and so we expect them to be relatively more accurate. Only 6 percent of the affected job stayers show adjustments in weekly basic pay. This strongly suggests that the recorded marginal changes in weekly hours are more likely to be measurement error than actual changes.

We perform another check by looking at changes in weekly basic pay which are unaffected by any measurement error in basic hours worked in the data. We consider *exact* zero changes, instead of a range around zero, to define a year-to-year freeze, because payroll data on weekly basic pay are more reliable. The results are displayed in Table D1 columns (I) & (II). The shares of exact freezes and cuts in weekly basic pay are not notably different from the shares found using basic (hourly) wages in a 0.005 range around zero (columns (III) & (IV), Table D1). If anything, basic wages appear less rigid than basic weekly pay, suggesting our approach of using a range around zero to define a freeze is a conservative choice. Since weekly basic pay changes can also occur when basic hours worked change, we repeat the same exercise for a sub-sample of job stayers who recorded an exactly zero year-to-year change in hours worked (around 20 percent of salaried job stayers). The results are displayed in Table D2 columns (I) & (II). The average spike at zero is 18 percent and the frequency of cuts is only

[4]This is no hypothetical issue. The authors know from personal employment experience that some employers in the Scottish hospitality sector frequently make mistakes when converting decimal values into hours and minutes worked.

7 percent, suggesting that our adjustment in the main results of using a 0.005 range around zero provides a lower bound on the true proportion of basic wage freezes and an upper bound on the share of cuts.

TABLE D1: Nominal changes in weekly basic pay and basic wages, salaried job stayers.

| | Weekly basic pay | | Basic wages | |
|---|---|---|---|---|
| Years | *Exact* Freezes (%) (I) | *Exact* Cuts (%) (II) | Freezes (%) (III) | Cuts (%) (IV) |
| 2006-07 | 10.3 | 9.0 | 10.0 | 11.3 |
| 2007-08 | 7.8 | 7.7 | 7.5 | 10.0 |
| 2008-09 | 15.6 | 10.2 | 15.0 | 11.1 |
| 2009-10 | 21.8 | 12.5 | 21.1 | 14.5 |
| 2010-11 | 21.3 | 10.8 | 21.4 | 11.9 |
| 2011-12 | 27.3 | 10.3 | 27.2 | 12.2 |
| 2012-13 | 25.1 | 10.1 | 24.3 | 12.3 |
| 2013-14 | 13.8 | 8.6 | 13.9 | 9.9 |
| 2014-15 | 12.5 | 9.1 | 13.0 | 10.1 |
| 2015-16 | 17.7 | 9.8 | 17.6 | 11.1 |
| 2016-17 | 13.9 | 9.3 | 14.4 | 10.8 |
| 2017-18 | 15.3 | 9.2 | 15.1 | 11.2 |
| Average | 16.9 | 9.7 | 16.7 | 11.4 |

*Notes*: Freezes and cuts show the percentage of job stayers with no change and negative change in the pay measure indicated. See Appendix Table E1 column (III) for annual sample sizes.

TABLE D2: Nominal changes in weekly basic pay with unchanged hours worked, salaried job stayers.

| | Weekly basic pay, constant hours subsample | | Basic wages | |
|---|---|---|---|---|
| Years | *Exact* Freezes (%) (I) | *Exact* Cuts (%) (II) | Freezes (%) (III) | Cuts (%) (IV) |
| 2006-07 | 14.1 | 5.0 | 10.0 | 11.3 |
| 2007-08 | 8.3 | 5.0 | 7.5 | 10.0 |
| 2008-09 | 15.1 | 8.3 | 15.0 | 11.1 |
| 2009-10 | 22.9 | 11.3 | 21.1 | 14.5 |
| 2010-11 | 21.8 | 7.5 | 21.4 | 11.9 |
| 2011-12 | 29.6 | 8.0 | 27.2 | 12.2 |
| 2012-13 | 27.0 | 8.2 | 24.3 | 12.3 |
| 2013-14 | 14.1 | 6.4 | 13.9 | 9.9 |
| 2014-15 | 12.5 | 6.8 | 13.0 | 10.1 |
| 2015-16 | 18.2 | 7.6 | 17.6 | 11.1 |
| 2016-17 | 14.8 | 7.1 | 14.4 | 10.8 |
| 2017-18 | 16.5 | 7.0 | 15.1 | 11.2 |
| Average | 17.9 | 7.4 | 16.7 | 11.4 |

*Notes*: Freezes and cuts show the percentage of job stayers with no change and negative change in the pay measure indicated.

## Appendix E. Additional Tables and Figures

Here we provide further statistical evidence on nominal wage changes to allow a comparison to previous studies We consider a statistical indicator proposed by Lebow, Saks, and Wilson (2003), hereafter the 'LSW statistic', to assess the asymmetry of the wage change distribution. Underlying the LSW statistic is the assumption that the notional wage change distribution is symmetric around its median, and that its upper half is largely unaffected by any nominal rigidities.[5] The LSW statistic is then calculated as the difference between the notional and observed mass of negative wage changes, and is interpreted as the missing mass of wage cuts.[6]

Figure E1 displays the LSW statistic for job stayers and each year in 2007-2018, as well as the UK unemployment rate and the inflation rate. For reference, we also show the share of year-to-year freezes among job-stayer wage changes over time.

See Appendix Tables E1 for sample sizes by year. Inflation is measured as the April-to-April log change in the UK Consumer Price Index (CPI). The unemployment rate refers to UK individuals aged 16 and over, seasonally adjusted and for the second April of each period, expressed as a percentage of the economically active population. Output per hour worked is calculated as gross value added (GVA) divided by the number of hours worked. All series are from the Office for National Statistics.

The LSW statistic suggests that the missing mass of wage cuts increased during the recession to 15.3 percent for salaried job stayers and to 19.3 percent for hourly-paid job stayers, indicating that the wage change distribution became more asymmetric. The substantial spike in 2016 is due to the introduction of the National Living Wage to the UK.[7] The visible large spikes at zero and the lack of wage cuts in the histograms, combined with the cyclical increases in both year-to-year wage freezes and the missing mass of wage cuts, provide statistical evidence that basic wages exhibit significant downward nominal rigidity in Great Britain.

[5] The assumption that the notional wage change distribution is symmetric is commonly invoked, see, for example, Card and Hyslop (1996) or Dickens et al. (2007).

[6] The LSW statistic is computed as $[1-F(2\cdot P_{50}+0.005)]-F(-0.005)$, where $F(\cdot)$ is the cumulative distribution function of wage growth and $P_{50}$ is its median. See Lebow, Saks, and Wilson (2003) for further discussion.

[7] In 2015, the UK government announced the introduction of a new National Living Wage that would apply to all workers aged 25 and over from April 2016. The rate was set at £7.20, which represented a substantial increase of 7.5 percent over the previous National Minimum Wage.

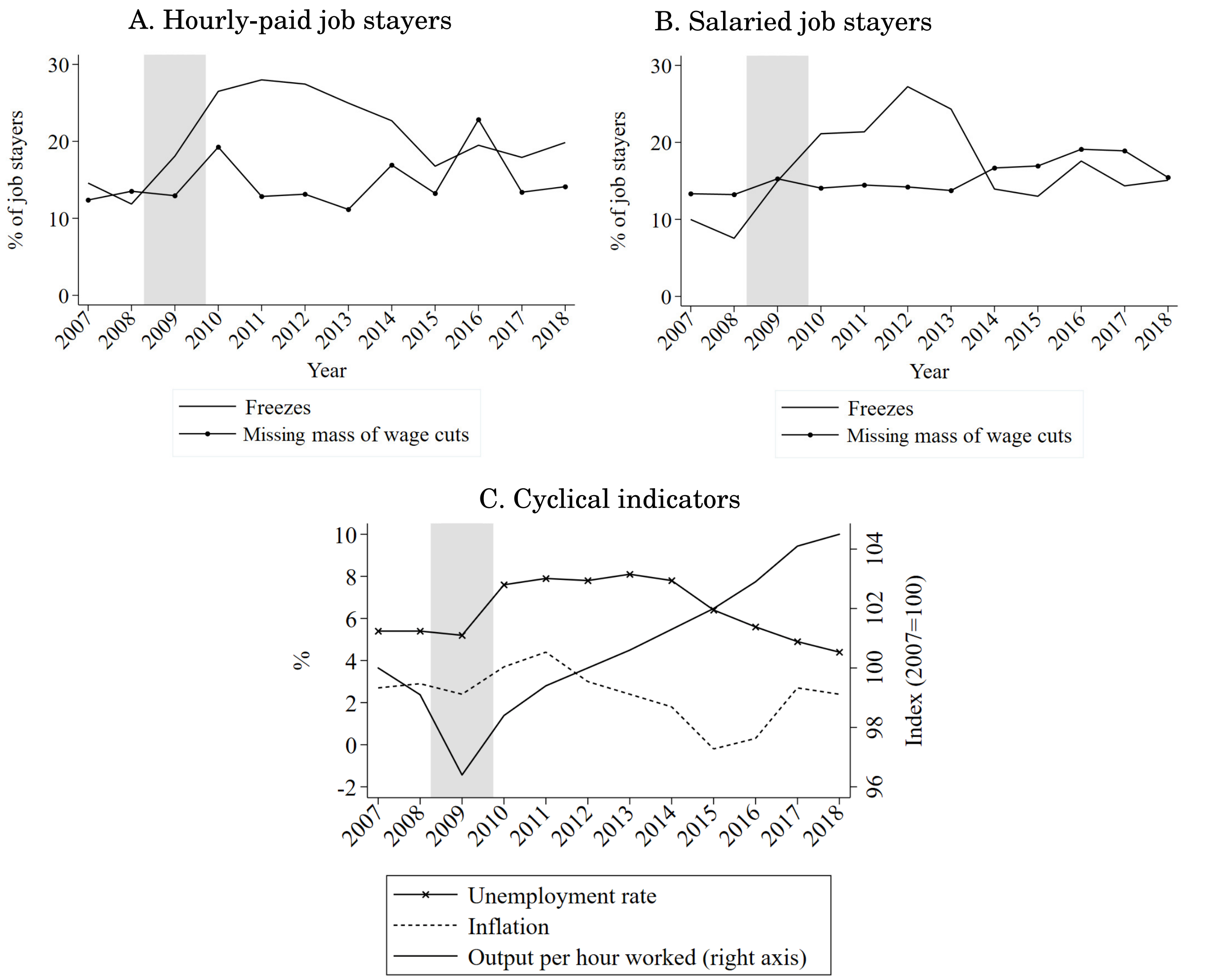


FIGURE E1: Statistical indicators of DNWR in the year-to-year basic wages of job stayers, and cyclical indicators

*Notes*: 'Freezes' show year-to-year changes in log basic wages for job stayers in the interval $[-0.005, 0.005]$. 'Missing mass of wage cuts' show the LSW statistic. Inflation is measured as the April-to-April log change in the UK Consumer Price Index (CPI). The unemployment rate refers to UK individuals aged 16 and over, seasonally adjusted and for the second April of each period, expressed as a percentage of the economically active population. Output per hour worked is calculated as gross value added (GVA) divided by the number of hours worked. All series in panel C. are from the Office for National Statistics. See Appendix Tables E1 for the sample sizes by year.

TABLE E1: Sample sizes of job-stayer wage change observations

| Years | Total (I) | Hourly-paid (II) | Salaried (III) |
|---|---|---|---|
| 2006-07 | 64,618 | 20,962 | 43,656 |
| 2007-08 | 64,558 | 21,494 | 43,064 |
| 2008-09 | 65,010 | 21,854 | 43,156 |
| 2009-10 | 82,258 | 29,140 | 53,118 |
| 2010-11 | 81,920 | 30,240 | 51,680 |
| 2011-12 | 81,973 | 30,603 | 51,370 |
| 2012-13 | 80,707 | 28,911 | 51,796 |
| 2013-14 | 83,362 | 28,672 | 54,690 |
| 2014-15 | 82,159 | 28,921 | 53,238 |
| 2015-16 | 78,952 | 27,578 | 51,374 |
| 2016-17 | 75,193 | 25,847 | 49,346 |
| 2017-18 | 74,086 | 25,865 | 48,221 |

*Notes*: Displays annual sample sizes of job stayers for the different sub-samples used.

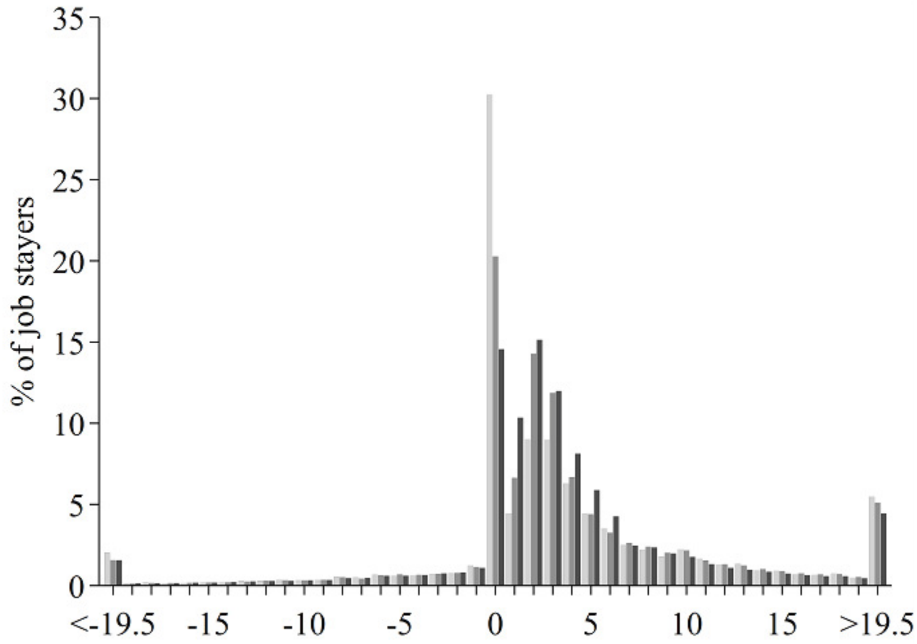


FIGURE E2: Frequency distribution of year-to-year changes in log basic wages of salaried and hourly-paid job stayers by firm size

*Notes*: See Figure 2. Pooled data for 2006-18. **Light grey bars**: small firms (< 50 employees). **Medium grey bars**: medium-sized firms (50 – 249 employees). **Dark grey bars**: large firms (≥ 250 employees). The size of a job stayer's firm is defined in the first of two consecutive years.

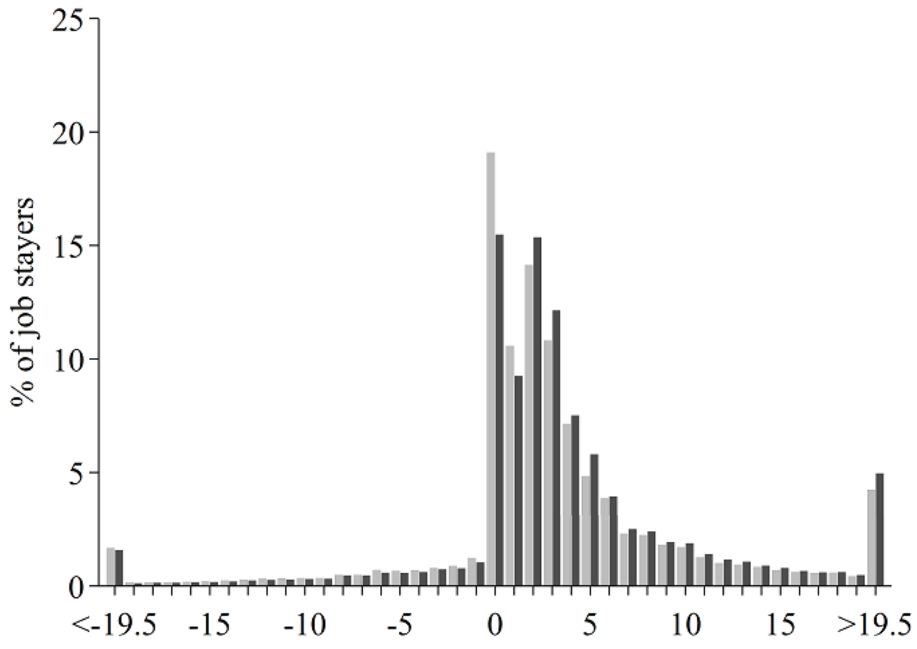


FIGURE E3: Frequency distribution of year-to-year changes in log basic wages, shrinking vs expanding firms, job stayers

*Notes*: See Figure 2. Pooled data for 2006-2018. **Light bars:** 277,001 job stayers whose firms experienced *negative* year-to-year employment growth over the same period. **Dark bars**: 259,236 job stayers whose firms experienced *positive* year-to-year employment growth over the same period.

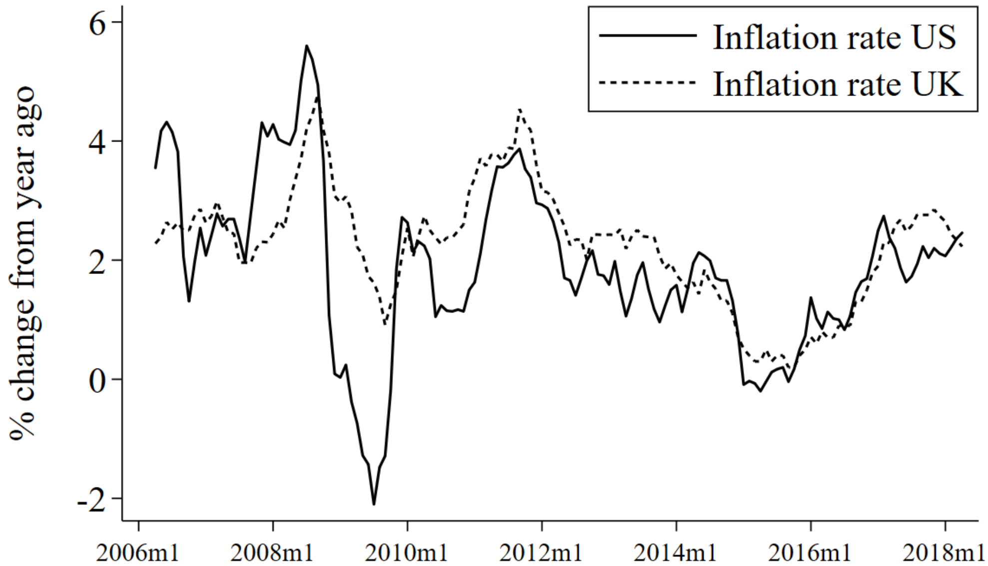


FIGURE E4: Change in Consumer Price Index from Year ago (in %), US and UK

*Notes*: Average UK inflation rate April 2006 - April 2018: 2.4 percent; Average UK inflation rate May 2008 - Dec 2016: 2.2 percent; Average US inflation rate May 2008 - Dec 2016: 1.6 percent.
*Sources*: US CPI: U.S. Bureau of Labor Statistics, Consumer Price Index for All Urban Consumers: All Items in U.S. City Average; UK CPI: Organization for Economic Co-operation and Development, Consumer Price Index of All Items in the United Kingdom.

## Appendix F. Year-To-Year Adjustments in Earnings Per Hour

In the main text, we presented evidence on DNWR using basic wages. We have argued that basic wages are the relevant wage notion for macroeconomic models that include stickiness in nominal wages to generate fluctuations in unemployment. Basic wages exclude various kinds of extra pay: shift premium pay, overtime pay, commissions and incentive pay, and other pay (e.g., meal and travel allowances). In this Appendix, we analyse changes in average earnings per hour, excluding overtime, for further comparison with the existing literature where data availability did not permit an analysis of basic wages. This is the same wage measure as previously analysed by Nickell and Quintini (2003) and Elsby, Shin, and Solon (2016). For a discussion on the likely effects of measurement error in hours worked on the results presented in the previous studies, and how our proposed corrections lead to different estimates of wage freezes and cuts, see Appendix D.

How should changes in earnings per hour, excluding overtime, be interpreted? Changes in labour income caused by changes in working hours do not affect a firm's marginal labour input cost per hour (except for the very first overtime hour). Similarly, changes in commissions and incentive pay or allowances are likely linked to a worker's output (e.g., sales) and labour input (e.g., business travel). Again, the marginal production costs a firm faces are likely unaffected by these extra pay components. However, from a worker's perspective, changes in earnings per hour might be perceived as income risk, depending on whether workers welcome reductions in long working times or less business travel (Devereux, 2001; Jardim, Solon, and Vigdor, 2019).

We begin by describing the year-to-year changes in job-stayer earnings per hour, excluding overtime. Figure F1, reproduced here for convenience, displays the results for hourly-paid (dark bars) and salaried (light bars) job stayers. The patterns in Figure F1 are similar to the ones seen in Figure 2 for basic wages. Nominal cuts are somewhat more common; the average share of hourly-paid job stayers receiving cuts is 11 percent, and for salaried job stayers this share is 17 percent. However, the distributions show strong signs of a censoring of earnings cuts. The spike at zero is substantial, though slightly lower than for basic wages. On average, 17 percent of hourly-paid and 14 percent of salaried job stayers have their earnings per hour, excluding overtime, frozen from year-to-year. The numbers imply constant quarterly wage-change probabilities of 0.36 and 0.39, respectively.[8] However, we caution against taking these probabilities at face-value as evidence against nominal rigidity, given the above described ambiguity in interpreting nominal changes in earnings per hour, excluding overtime.

Figure F2 displays time series evidence for the earnings per hour, excluding overtime, adjustments of job stayers comparable to Figure E1 for basic wages. The share of year-to-year freezes is strongly rising throughout the recession period for hourly-paid and salaried job stayers, peaking at 21.2 percent and 21.9 percent, respectively, in 2011-12. The Lebow, Saks, and Wilson (2003) statistic, indicating the missing mass of cuts, does not provide clear evidence for binding DNWR in earnings per hour, excluding overtime, for salaried workers. For hourly-paid workers, the missing mass falls from 12.2 percent to 9.1 percent from 2008 to 2009, but substantially increases to 14.1 percent for job stayers from 2009 to 2010.

[8]As before, we convert values as follows: $p_q = 1-(1-p_y)^{1/4}$, with $p_q$ denoting the quarterly wage-change probability, and $p_y$ the yearly wage-change probability.

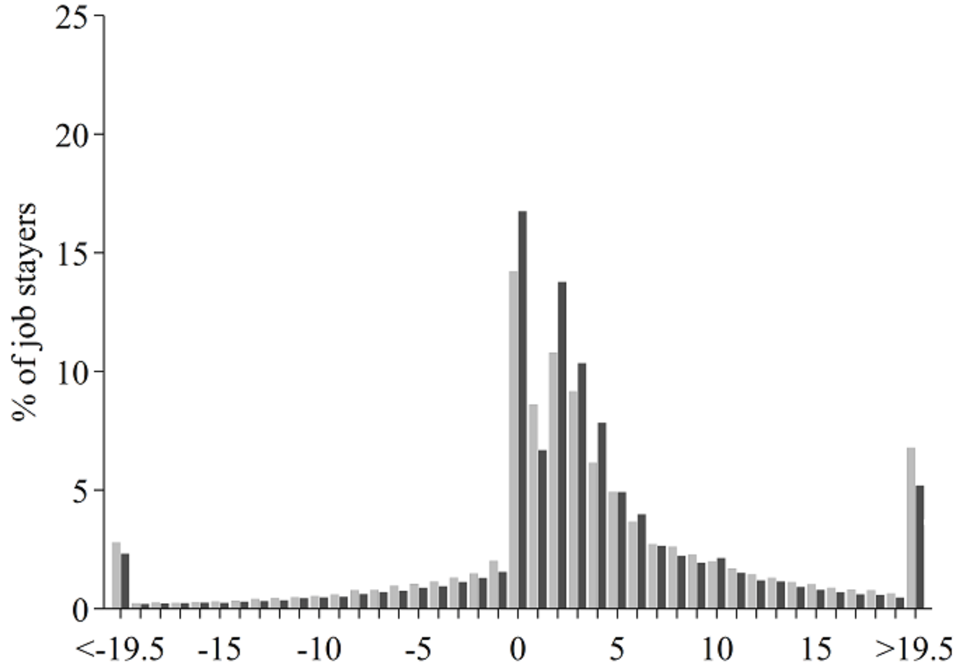


FIGURE F1: Frequency distribution of year-to-year changes in log earnings per hour, excluding overtime, job stayers

*Notes*: See Figure 2. **Light bars**: salaried. **Dark bars**: hourly-paid.

Our results for job-stayer adjustments in earnings per hour, excluding overtime, support the findings in the literature from studies of employer payroll records or payslips, recently summarised by Elsby and Solon (2019): cuts in average earnings per hour occur commonly, typically affecting more than 15 percent of job stayers. We view this as important evidence that workers face considerable earnings risk. Our results are also in line with findings for the US from Fallick, Villar, and Wascher (2020). They used data from the Employment Cost Index of the US Bureau of Labor Statistics, where the unit of analysis is not the worker, but the job. For each job, they observed the average nominal compensation per employee, including benefits such as pension payments. They reported an average proportion of year-to-year compensation freezes of 16 percent and nominal cuts of 15 percent. Because they cannot control for any changes to worker characteristics in the jobs, average nominal compensation changes can occur when the unobserved underlying skills of the employees in a job change. Consistent with that, they find much more variation in nominal compensation in jobs than we find here.

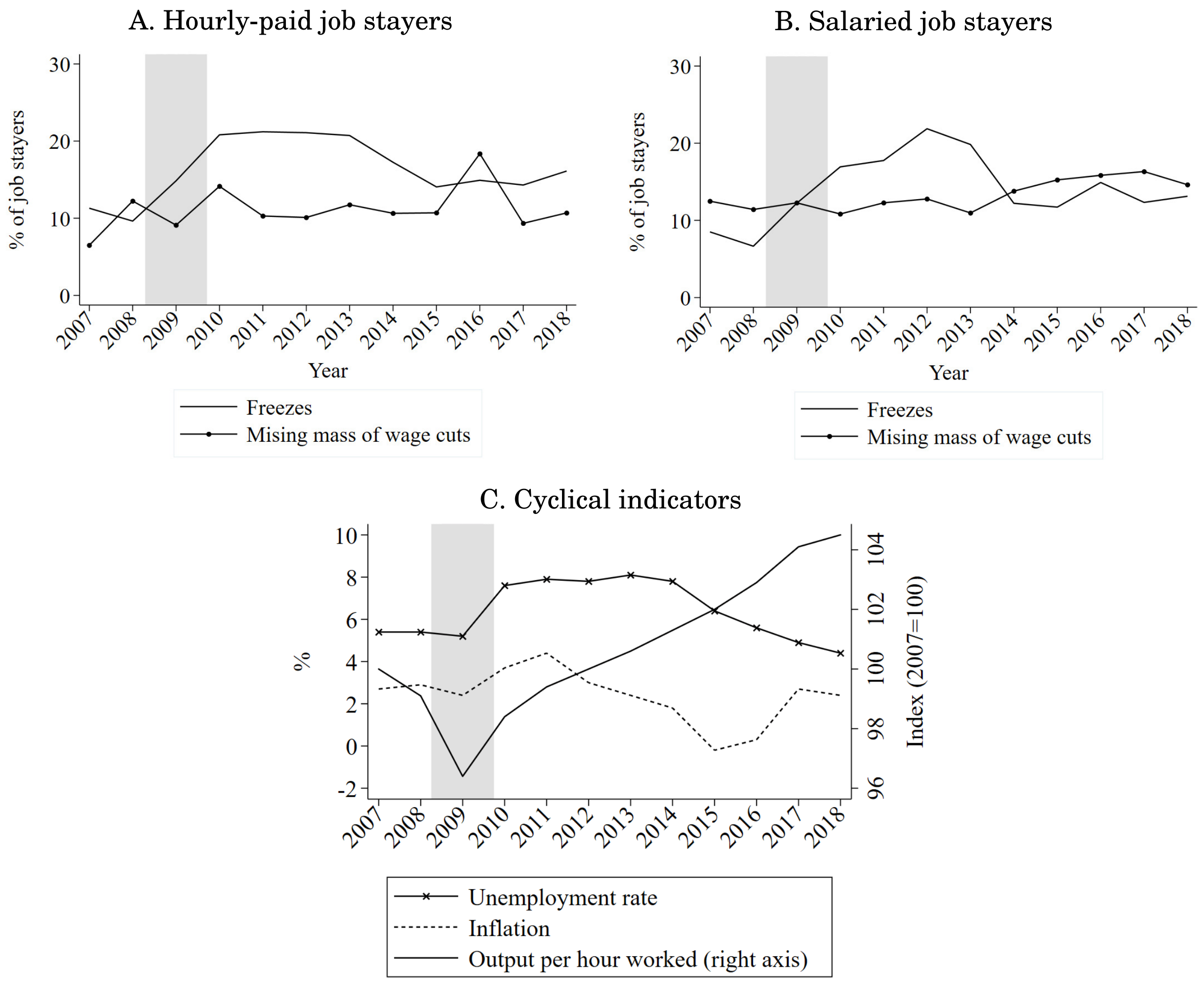


FIGURE F2: Quantitative indicators of DNWR, earnings per hour, excluding overtime

*Notes*: 'Freezes' show year-to-year changes in log earnings per hour, excluding overtime, for job stayers in the interval $[-0.005, 0.005]$. 'Missing mass of cuts' show the LSW statistic. See Appendix Tables E1 for sample sizes by year. Inflation is measured as the April-to-April log change in the UK Consumer Price Index (CPI). The unemployment rate refers to UK individuals aged 16 and over, seasonally adjusted and for the second April of each period, expressed as a percentage of the economically active population. Output per hour worked is calculated as gross value added (GVA) divided by the number of hours worked. All series are from the Office for National Statistics.

# Appendix G. Further Details on the Heterogeneity in Wage Freezes and Cuts across Workers and Firms

We study the conditional likelihood of year-to-year nominal wage cuts or freezes of job stayers, conditional on the state of the economy. The following process describes whether a wage cut or freeze between periods $t-1$ and $t$ is observed for job-stayer $i$:

$$y_{it} = \begin{cases} 1 & \text{if} \quad y^*_{it} > 0, \\ 0 & \text{if} \quad y^*_{it} \leq 0 \end{cases} \tag{5}$$

whereby the latent variable $y^*_{it}$ for job stayer $i$ is

$$y^*_{it} = \gamma_t + \mathbf{x}'_{it}\boldsymbol{\beta} + \varepsilon_{it}, \qquad \varepsilon_{it} \sim N(0,1) \tag{6}$$

Here, $y_{it}$ either represents basic wage freezes or cuts. We include year-fixed effects, $\gamma_t$, to account for the state of the business cycle. $\mathbf{x}_{it}$ includes dummy variables indicating an employee's gender, whether their employer is in the private or non-private sector, whether their wage is set according to any form of collective agreement, whether the job is in the 'Wholesale & Retail Trade, Hotels & Restaurants sectors' industry (SIC G-H), and the worker's age band. We also include dummies in $\mathbf{x}_{it}$ for employer sizes: 'small' ($<50$ employees), 'medium' (50-249 employees), 'large' (500-4,999 employees), and 'very large' ($\geq 5{,}000$ employees). Additionally, we include dummy variables for the employee's position in the basic weekly pay distribution among all ASHE employees in the respective year, whereby 'low earnings' or 'high earnings' indicate that the employee earned below 2/3 or above 4/3 of median basic weekly wages that year, respectively, and 'medium earnings' refers to all remaining job stayers. All these dummy variables refer to period $t-1$ values. We also include indicator variables for the year-to-year growth in the number of employees at a job-stayer's firm, over the corresponding period (shrinking, expanding or constant), which proxy for the state of the firm.[9] For hourly-paid job stayers, we further include a dummy variable indicating whether the reported hourly basic wage is a multiple of ten pence ('round'). The results of estimating the probit models are displayed in Tables G1 and G2, with both slope coefficients and margins (probabilities) displayed, with the latter evaluated at the sample averages using the delta method.

[9] The employee counts of firms are provided in the ASHE dataset from the Inter-Departmental Business Register (IDBR), which is the official list of UK enterprises. We use the more common term 'firm' interchangeably with 'enterprise', which refers to a UK-specific administrative definition of an employer that could contain several local units or plants.

TABLE G1: Probit estimates: Incidence of year-to-year basic wage freezes for job stayers

| | Salaried | | Hourly-paid | |
|---|---|---|---|---|
| | Coefficient (I) | Margin (Pr.) (II) | Coefficient (III) | Margin (Pr.) (IV) |
| 1. Female | | 0.157 | | 0.187 |
| 2. Male | 0.079*** | 0.177 | 0.112*** | 0.219 |
| 3. Non-private sector | | 0.169 | | 0.226 |
| 4. Private sector | −0.012* | 0.166 | −0.108*** | 0.195 |
| 5. No coll. agree. | | 0.177 | | 0.222 |
| 6. Coll. agreement | −0.072*** | 0.159 | −0.139*** | 0.183 |
| 7. Age 15-29 | | 0.103 | | 0.152 |
| 8. Age 30-44 | 0.261*** | 0.158 | 0.184*** | 0.200 |
| 9. Age 45-64 | 0.417*** | 0.199 | 0.275*** | 0.226 |
| 10. Low earnings | | 0.180 | | 0.178 |
| 11. Medium earnings | 0.011 | 0.183 | 0.140*** | 0.217 |
| 12. High earnings | −0.103*** | 0.154 | 0.137*** | 0.216 |
| 13. Small firms | | 0.265 | | 0.333 |
| 14. Medium firms | −0.266*** | 0.186 | −0.288*** | 0.236 |
| 15. Large firms | −0.395*** | 0.153 | −0.484*** | 0.180 |
| 16. V. large firms | −0.432*** | 0.145 | −0.603*** | 0.151 |
| 17. SIC G-H | | 0.171 | | 0.165 |
| 18. Not SIC G-H | −0.017* | 0.167 | 0.202*** | 0.221 |
| 19. Firm expanding | | 0.145 | | 0.179 |
| 20. Firm const. size | 0.156*** | 0.183 | 0.113*** | 0.210 |
| 21. Firm shrinking | 0.165*** | 0.186 | 0.171*** | 0.227 |
| 22. 'Non-round' | | | | 0.188 |
| 23. 'Round' rate | | | 0.211*** | 0.250 |
| Year-fixed effects | ✓ | | ✓ | |
| $N$ of job stayers | 503,330 | | 272,648 | |
| Log-likelihood | -225,418 | | -132,916 | |

*Notes*: The dependent variable is an indicator that takes the value one if a job stayer experienced a basic wage freeze from year-to-year, and zero otherwise. Marginal effects are evaluated at the sample average using the delta method. Excludes all job stayers for 2007-2008 and 2017-2018 because firm size was incorrectly recorded by the ONS in the second years of those periods.

***, **, * indicate significance from zero of the model coefficients at the 0.1%, 1% and 5% levels, respectively, two-sided tests and robust standard errors.

TABLE G2: Probit estimates: Incidence of year-to-year basic wage cuts for job stayers

| | Salaried | | Hourly-paid | |
|---|---|---|---|---|
| | Coefficient (I) | Margin (Pr.) (II) | Coefficient (III) | Margin (Pr.) (IV) |
| 1. Female | | 0.109 | | 0.033 |
| 2. Male | $0.040^{***}$ | 0.117 | $0.155^{***}$ | 0.046 |
| 3. Non-private sector | | 0.108 | | 0.042 |
| 4. Private sector | $0.048^{***}$ | 0.117 | $-0.047^{***}$ | 0.038 |
| 5. No coll. agree. | | 0.116 | | 0.040 |
| 6. Coll. agreement | $-0.033^{***}$ | 0.110 | $-0.031^{**}$ | 0.038 |
| 7. Age 15-29 | | 0.089 | | 0.035 |
| 8. Age 30-44 | $0.156^{***}$ | 0.117 | $0.075^{***}$ | 0.041 |
| 9. Age 45-64 | $0.164^{***}$ | 0.118 | $0.055^{***}$ | 0.039 |
| 10. Low earnings | | 0.164 | | 0.045 |
| 11. Medium earnings | $-0.204^{***}$ | 0.119 | $-0.058^{***}$ | 0.040 |
| 12. High earnings | $-0.302^{***}$ | 0.100 | $-0.243^{***}$ | 0.026 |
| 13. Small firms | | 0.133 | | 0.034 |
| 14. Medium firms | $-0.101^{***}$ | 0.112 | $0.035^{*}$ | 0.037 |
| 15. Large firms | $-0.122^{***}$ | 0.108 | 0.007 | 0.035 |
| 16. V. large firms | $-0.110^{***}$ | 0.111 | $0.125^{***}$ | 0.045 |
| 17. SIC G-H | | 0.109 | | 0.022 |
| 18. Not SIC G-H | $0.027^{***}$ | 0.114 | $0.353^{***}$ | 0.049 |
| 19. Firm expanding | | 0.113 | | 0.034 |
| 20. Firm const. size | 0.011 | 0.115 | 0.021 | 0.036 |
| 21. Firm shrinking | 0.004 | 0.113 | $0.148^{***}$ | 0.047 |
| 22. 'Non-round' | | | | 0.040 |
| 23. 'Round' rate | | | $-0.065^{***}$ | 0.035 |
| Year-fixed effects | ✓ | | ✓ | |
| $N$ of job stayers | 503,330 | | 272,648 | |
| Log-likelihood | -178,276 | | -46,819 | |

*Notes*: The dependent variable is an indicator that takes the value one if a job stayer experienced a basic wage cut from year-to-year, and zero otherwise. Marginal effects are evaluated at the sample average using the delta method. Excludes all job stayers for 2007-2008 and 2017-2018 because firm size was incorrectly recorded by the ONS in the second years of those periods.
$^{***}$, $^{**}$, $^{*}$ indicate significance from zero of the model coefficients at the 0.1%, 1% and 5% levels, respectively, two-sided tests and robust standard errors.

## Appendix H. Further Details on Benigno & Ricci (2011)

### H.1. Description of the Theoretical Framework

In BR's model, households enjoy the consumption of differentiated goods, which are supplied by firms operating in competitive output markets. The prices of consumption goods are fully flexible. The consumption output of each firm is produced using a constant elasticity of substitution production function, which takes as inputs differentiated types of labour inputs. This production function exhibits the 'love-for-variety' property; the differentiated types of labour inputs are imperfect substitutes. Therefore, taking real wages as given, each firm demands a combination of all types of labour inputs. Labour markets are characterised by monopolistic competition, in which each household supplies a variety of types of labour to the output producing firms. Firms' profit-maximisation means that each household faces a downward-sloping labour demand curve for each of its types of labour.

Each household's lifetime utility increases in consumption and falls in labour supply. To maximise lifetime utility, each household chooses nominal wages for each type of labour, taking as given the labour demand curves for each type of labour and the rate of inflation. With inflation given, any change in nominal wages translates into an equally sized change in real wages. Importantly, idiosyncratic preference shocks affect every period the disutility from the differentiated types of labour, generating the need to adjust labour supply across firms by changing real wages. Consider the case where a shock increases the disutility derived from supplying a certain type of labour while the rate of inflation is zero. Each household wants to increase the nominal wage for the type of labour affected, which implies that firms would demand less of that type of labour input. Similarly, if a shock decreases the disutility derived from supplying a certain kind of labour, each household wants to decrease the nominal wage for that type of labour.

To build the intuition, consider first the case where nominal wages cannot be cut under any circumstances and the rate of inflation is zero. Forward-looking firms and households internalise the effect that their current wage setting will have on the likelihood of being constrained by downward rigidity in the future, consistent with Elsby (2009); households that raise the nominal wage of a type of labour today are more likely to be bound by downward nominal rigidity in the future. Therefore, households, when they are hit by a preference shock that increases the disutility derived from supplying a certain type of labour, will raise nominal wages for that type of labour less than they would have done in the absence of nominal rigidities. DNWR thus leads to wage growth moderation. Despite this forward-looking wage setting, households are sometimes hit by sufficiently large preference shocks, given the current level of their real wages, that they would like to cut nominal wages to increase labour supply. This is how DNWR leads to an output loss: constrained households end up charging a higher real wage than they want to after being hit by a preference shock. Stuck with the high nominal wage, they end up supplying less labour inputs than they want to supply. This negative effect on employment is partially offset by wage growth moderation that makes downward nominal rigidity less likely to bind. However, as BR show analytically, the net effect is a drop in employment relative to the case of flexible wages. By allowing real wages to fall sufficiently to increase labour demand, inflation in the model can grease the wheels of the labour market.

So far, we assumed that nominal wages could not be cut. However, this is not supported by our data, since we have documented that some nominal wage cuts do occur. Here, we calibrate a version of BR's model, as presented in Section V.C of their article, wherein nominal wages cannot be cut except when a 'very large' preference shock hits the households. This captures the finding by Bewley (1999) that, under rare circumstances such as when the survival of the entire firm is at stake, workers are more likely to agree to wage cuts. When such a very large shock occurs, nominal wages can freely be adjusted upwards or downwards. This version of the model still results in an output loss relative to the flexible wage case, because nominal wages are typically stuck at too high levels despite wage growth moderation. But the output loss in this version of the model is smaller than in the version with completely rigid wages, since nominal wages occasionally become flexible downwards when a very large shock occurs.

### H.2. Description of the Calibration and Simulation

The model is calibrated at a quarterly frequency. We keep all parameters at the values calibrated by BR based on the US, except the annual inflation rate and the frequency at which the previously described very large shocks hit households. For the former we choose 2.4 percent annually, consistent with the average value observed in the UK during the sample period of our main analysis (see Table 3 in the main text). We choose the frequency of shocks to match the shares of basic wage freezes and cuts that we observe in British payroll data (Section 4). Intuitively, the more frequently these large shocks occur, the more nominal wage cuts will be observed, while the spike of wage freezes will decline. At the extreme case in which such shocks occur every period, we would observe no spike at zero and the distribution of wage changes would be symmetric around the rate of inflation, since the idiosyncratic 'normal' preference shocks are independent across households.

We initialise 100 workers and simulate their optimal wage setting based on the optimality conditions in BR, specifically equations (27) and (28) on page 1457. Each quarter, households are either subject to DNWR or they are hit by 'very large' preference shocks and nominal wages become flexible. We simulate 12,500 quarters to ensure that the economy is in steady state and that initial conditions do not affect outcomes. We discard the first 12,492 quarters, and compute year-to-year nominal wage changes in the remaining eight quarters for each worker, giving us 400 year-to-year wage change observations. We repeat this simulation 100 times. Our simulated dataset contains 40,000 wage change observations, from which we compute a relative frequency distribution of wage changes.

To match the share of wage cuts and freezes in our data, we set the model parameter governing the frequency of large shocks to $\lambda dt = 0.05$, implying that nominal wages are expected to become downward flexible one quarter in every five years. Figure H1 displays the frequency distribution of year-to-year log wage changes simulated by the model. It generates frequencies of wage freezes (18.7 percent) and wage cuts (9.1 percent), which correspond closely to our main empirical findings. Additionally, the model replicates the empirical feature of a markedly asymmetric wage-change distribution, with positive changes occurring far more frequently than negative changes.

Using the simulated wage paths, we compute the resulting labour demand from firms' first order conditions. Once the quantity of labour is determined, we can compute the level of output produced in

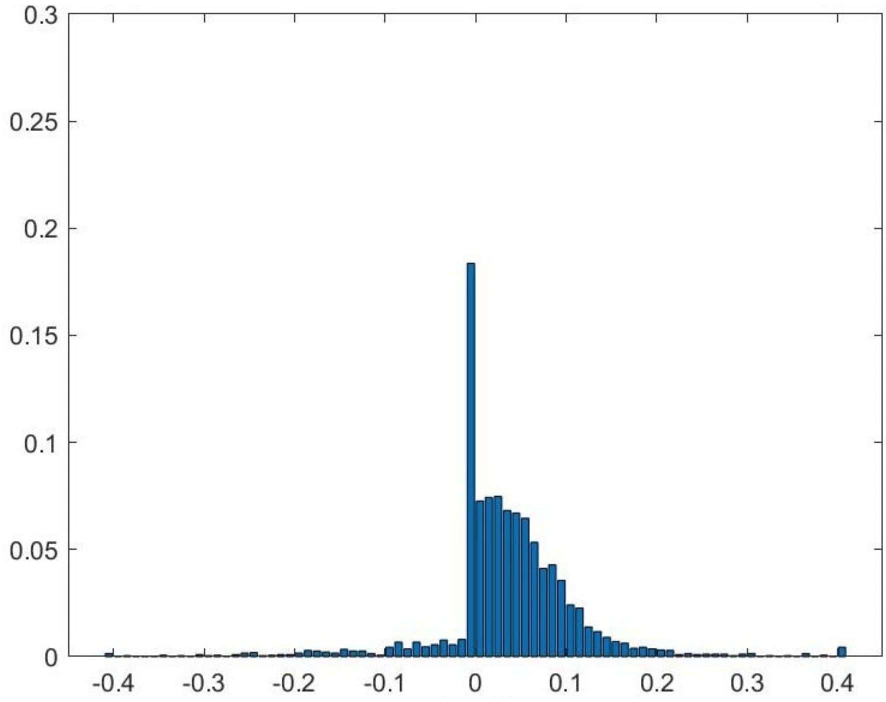


FIGURE H1: Simulation results for year-to-year changes in log wages

*Notes*: Equivalent to Benigno and Ricci (2011): Figure 5, p. 1458. Simulation uses exactly the same parameters as BR, except $\lambda dt = 0.05$, meaning nominal wages become flexible one quarter in every five years, on average.

each period. Comparing this level of output under DNWR with the level under full employment that results from flexible wages, we obtain the output gap. Repeating this computation for varying values for the annual inflation rate produces the results displayed in Figure 5 in the main text.

As a robustness check, we conduct a second simulation exercise, in addition to the results displayed in the main text. We consider an economy where only some workers are subject to DNWR, while the wages of the other workers can be adjusted flexibly at all times. We use the model presented in Section V.B of BR, no longer allowing for the previous very large idiosyncratic shocks. We parameterise the share of workers with rigid wages, $\alpha$, to the observed share of workers in our data for which basic wages make up their entire labour income: 53 percent. Figure H2 displays the long-run Phillips curve of this economy. Even if DNWR only affects around half of all workers, the resulting negative output gap is sizeable, with the model implying that around 0.9 percent of long-run equilibrium output is lost when mean annual inflation is 2 percent.

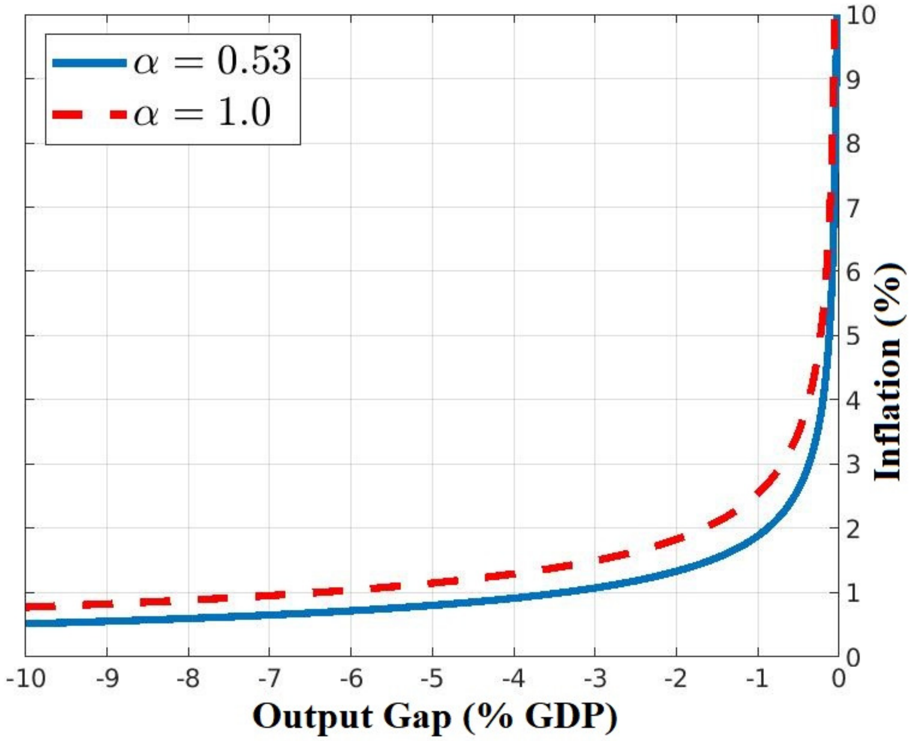


FIGURE H2: Long-run Phillips curve with binding DNWR for an $\alpha$-share of workers

*Notes*: Equivalent to Benigno and Ricci (2011): Figure 4, p. 1455. Simulation uses the same parameter values as BR, except $\alpha$. The dashed line shows the long-run output gap (in percent of GDP) when nominal wages can never be cut. The solid line shows the gap when nominal wages can never be cut for an $\alpha$-share or employees, while the remaining $(1-\alpha)$-share of employees has completely flexible nominal wages.