IMPACT OF NATIONAL LIVING WAGE ON BUSINESSES

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EXECUTIVE SUMMARY

There has been limited evidence so far of the minimum wage affecting businesses. But as the level of the minimum wage increases relative to median pay, so the bite increases, and effects may become more apparent. In particular, the introduction of the National Living Wage (NLW) in April 2016 represented an increase in hourly pay of over 10 per cent compared with April of the previous year.

From theory, there are a number of possible impacts on businesses, including increased prices, decreased profitability, substitution from labour to capital and firm exit. We explore these using a two-pronged approach, looking in parallel at impacts at the firm level (such as employment and revenue growth) and in terms of prices.

Firm-level analysis

Data

Given the relative coverage of the various datasets available, and in order to complement previous research (which focuses on larger firms in datasets such as the the ONS's Annual Business Survey (ABS) or the Bureau van Dijk's Financial Analysis Made Easy (FAME)), we focus on employment impacts using the Business Structure Database (BSD). This allows us to extend the analysis to cover smaller firms, and also explore impacts on business creation and destruction. The advantage of the BSD is that it is virtually exhaustive, covering all VAT- or PAYE-registered firms, giving much larger sample sizes. It also allows us to see what is happening in multi-unit enterprises ('chains'), which are an important component within low-pay sectors.

The BSD is used to conduct a 'difference-in-difference' analysis, where we explore the change over time in performance of minimum wage firms ('treatment group') and other businesses ('control group'), comparing a 2015 (pre-NLW) baseline year with outcomes 3 years later. We control for relevant drivers of performance, such as sectoral and regional-level trends, and interpret the remaining differences between groups to be an impact of the minimum wage increase.

Identifying minimum wage firms

We assign firms to the treatment and control groups using the Annual Survey of Hours and Earnings (ASHE), which is a 1% random sample across the population of UK employees. This can be linked directly to the employee's workplace in the BSD. We assign on the basis of the level of hourly pay observed. An employee paid below the incoming minimum wage is placed in the treatment group. An employee paid in excess of 120% of the incoming minimum wage is placed in the control group.[1]

As the pay of a randomly sampled individual may not be representative of minimum wage exposure at the firm level, this approach could potentially lead to measurement error. For example, a managerial or senior staff member might be

[1] Due to concerns around minimum wage increases affecting pay further up in the distribution ('spillover effects'), those paid between 100% and 120% of the incoming minimum wage are excluded.

drawn in a workplace where low pay is prevalent. Or a support staff member may be drawn from a professional services firm, where pay for the 'main' occupation is much higher. We develop a number of robustness checks to test this. We find strong correlation between the ASHE assignment variable and labour cost thresholds used elsewhere in the literature. We also use the Workplace Employment Relations Survey (which includes the pay distribution within a workplace), to simulate the ASHE assignment procedure. Comparing with the labour cost ratios used elsewhere in the literature, we find that our approach provides as accurate a signal of minimum wage exposure at the firm level as other approaches used by previous research.

Findings

We first undertook descriptive analysis to explore the characteristics of minimum wage firms, and how they differ from others. While minimum wage establishments are of a similar size to others, turnover per employee is markedly lower, around 24% lower than control firms in the same sector, which shows the expected correlation between low pay and lower productivity (insofar as turnover per employee can be used as a proxy for productivity). We then see that, following the introduction of the NLW, employment growth is weaker in the treatment group, around 2% lower than in the control group after 3 years.

The question is whether this 'raw difference' in performance can be explained by other differences between the groups, such as firm characteristics, regional performance, etc. So we control for these factors in a difference-in-difference regression, estimating the change in log employment (2018 vs 2015) as a function of firm-level characteristics (e.g. size, age, turnover per employee, sector, region), worker-level characteristics (occupation, age and gender), and the minimum wage variable derived from ASHE.

The model can be written as:

$$Log\left(employment_{i,g,s,2018}\right) - Log\left(employment_{i,g,s,2015}\right) = \beta_1 + \beta_2 X_i + \beta_3 W_a + \beta_4 Sector_S + \beta_5 Region_g + \beta_6 Minwage_a + u_i$$

for sampled worker a in firm i in region g and sector s

To describe the variables in more detail:

- X_i is a vector of firm-level characteristics (e.g. size, firm age, turnover per employee);
- W_a is a vector of worker-level characteristics (age, occupation and gender);
- Sector and region indicator variables are included; and
- '*Minwage*' is an indicator variable for a worker observed in ASHE to be paid below the incoming minimum wage, thus giving an estimate of the 'treatment effect'.

We find that above and beyond the various factors controlled for, employment growth was in the region of 2 to 3 percentage points weaker in high-bite firms, with the effect statistically significant at the 1% level. We find the employment effect to be most obvious in relation to smaller workplaces, in multi-unit enterprises (chains), and in the retail and food service sectors. It seems plausible that minimum wage firms substitute capital for labour, for example using self-scan checkouts and computerised ordering systems.

We also find that minimum wage firms have higher survival rates, by around 2 percentage points. This is counterintuitive, as we would expect minimum wage rises to negatively impact survival of exposed firms. However, if we look at survival rates alongside start-up rates,[2] there is some indication that start-up rates are lower in higher-bite sector-regions. Overall, this result is consistent with lower business churn than any 'net' effect on business numbers. We also find that turnover per employee fell in high-bite firms by around 3 percentage points relative to others. One explanation would be that adverse conditions that place downward pressure on wages (and hence assignment into the minimum wage group) are persistent and correlated with weaker performance in the following period.

Overall, the regression results are quite similar to the raw differences. Note that the model includes sector-level and region-level fixed effects, so the estimated minimum wage effect comes on top of any variation that exists at the sector-region level. Of course, there will be many other differences between minimum wage and comparator firms within the same sector and region that go beyond the modelled characteristics, such as product positioning and operating model, which cannot be addressed within an econometric analysis.

Robustness and interpretation

The empirical results hold over various changes to specification and how the variables are defined. This includes using a number of different pay thresholds for defining the treatment and control groups, and exploring different employment measures, due to concerns around the timelines of data in the BSD.

However, we also find similar results if repeating the same procedure over different years, going back to 2004, and using each year in turn as the baseline period. This finds similar sorts of impacts as in the core analysis focused on the NLW introduction, i.e. minimum wage firms have lower employment growth, lower growth in turnover per employee, and higher survival. Some effects are found in years where the upratings are not especially large. This would suggest that the empirical relationships we identify are not due specifically to the introduction of the NLW, but may reflect wider trends over the period. For example, we observe various forms of capital-labour substitution, such as self-scan checkout and computerised ordering systems in supermarkets. These trends may be more prevalent among minimum wage firms, but it is difficult to causally attribute this to the NLW or specific upratings. As such we cannot be confident that the lower employment effects we find in firms which are more reliant on minimum wage labour are in fact related to the introduction of the NLW.

[2] This is explored using an aggregated analysis, as we have no prior characteristics (ASHE assignment) on which to condition likelihood of start-up at the firm-level.

Price impacts

The price impacts are analysed by exploring whether inflation rises in response to minimum wage increases. This extends the approach of Wadsworth (2010) and includes more recent increases, in particular the introduction of the NLW.

We first construct a theoretical model of price setting by monopolistically competitive firms operating in perfectly competitive labour markets with identical, constant return to scale production functions. Under these assumptions, the elasticity of prices with respect to minimum wages should equal the share of minimum wage labour costs in total costs for the product market. This identity motivates our empirical strategy and provides a benchmark against which findings can be contextualised.

Data

The dependent variable is monthly and year-on-year price inflation. The unit of analysis is 'items'; which are the product categories used to construct the consumer price index. This is very granular, for example 'Minicab fare for 2 miles', 'Hair gel 150-200ml', 'Electrician daytime rate per hour'. We extract monthly item indexes from January 2005 to January 2020 inclusive for around 1,100 items, and chain link the observations to construct a consistent price index with a base of 100 in 2015.

Items are assigned to the treatment and control groups on the basis of the estimated share of minimum wage labour costs in total costs. This is approximated by using ASHE data to measure the share of workers earning less than or equal to the incoming minimum wage in each sector. This is combined with a measure of employment costs as a share of total turnover at the four-digit sector level derived from the Annual Business Survey. Finally, we take the product of these two values to construct a measure of how 'exposed' each sector is to an increase in the minimum wage, which is a proxy for the share of minimum wage labour costs in total costs. An ordinal ranking of these values is taken, and we find the most exposed sectors are those related to cleaning services, the provision of care and the preparation and service of food and drink. The sector-level 'bite' measures are then mapped manually to the item codes, so that we can distinguish high-bite and low-bite items. A number of sensitivity tests are used in terms of how these variables are derived and mapped.

We also use information from the LPC to construct a binary variable capturing whether the minimum wage increased in a given month, and a continuous variable capturing the percentage change in the minimum wage in a given month.

Analytical approach

For treated items only, we test whether monthly inflation is higher in months during which the minimum wage was uplifted. The core specification is as follows:

$$inflation_{m,i} = \alpha + \beta_1 \times uplift_m + \gamma_i + \delta_m + inflation_{m-1,i} + \epsilon_{m,i}$$

Where:

- $inflation_{m,i}$ is the month-on-month percentage change in price index for each item/month.
- $uplift_m$ is a binary variable equal to one if the minimum wage increased in that month and zero if it did not.
- γ_i is an item fixed effect (to capture between-item variation in inflation).
- δ_m is a month fixed effect (to capture seasonality).
- $inflation_{m-1,i}$ is a lagged dependent variable (to account for autocorrelation in the inflation time series).

The coefficient β_1 can be interpreted as the difference between monthly inflation in minimum wage-uplift months, and monthly inflation in months where there was no minimum wage uplift, i.e. the additional impact of minimum wage increases on prices for treated items.

Findings

Descriptively, we see that CPI inflation is indeed higher in minimum wage uplift months than in months in which there is no uplift: 0.02% higher. This finding is not sensitive to the choice of treatment definition. If we overlay CPI monthly inflation for treated items on the months in which the minimum wage was uplifted, we observe some correlation from 2014 onwards, but limited correlation in earlier years. While this may reflect the fact that the minimum wage increased substantially in 2016 and subsequent years, it may also reflect the fact that the month in which minimum wages are increased was changed from October to April from 2016.

Using the core panel specification outlined above, we find that inflation is no higher in months where the minimum wage was uplifted. This finding is robust to a number of sensitivities, including the use of lagged inflation terms, standard errors, fixed effects and month fixed effects, or treatment definition.

We then explore sensitivity to the time period of analysis, using a series of 5-year rolling windows. We see that the minimum wage effect increases over time, and is significant at the 1% level from 2014-2018 onwards. If we restrict the sample to the period starting from when the National Living Wage was introduced (2016), we find that inflation is 0.237 percentage points higher in months when the minimum wage was uplifted, significant at the 1% level. Again, this is robust to various sensitivities.

This result could reflect either the introduction of the NLW in 2016, or the fact that the month in which wages were increased was changed from October to April in that year. This is tested by regressing inflation on a binary variable equal to 1 if the month is April. Before 2016, the effect of April is positive but not statistically significant, suggesting it is unlikely that the strong post-2016 effect is a result of the change in uplift month. To put these coefficients in context, the mean minimum wage increase over the period was 5.22%. For the treatment items, the elasticity of prices with respect to the minimum wage is approximately 0.045, i.e. a 10% increase in the minimum wage could be expected to increase prices by 0.45%. This is lower than the increase predicted by the theoretical framework of 1.5% to 3%, but that framework ignores price-adjustment costs and makes some strict assumptions about product and labour market competition and firms' production functions.

A number of alternative specifications are explored:

- We include terms to account for two lagged months and two leading months, thus capturing persistence or anticipatory effects. This does not substantially alter the results.
- We include a continuous measure of minimum wage exposure, to account for some upratings being larger than others. This gives statistically significant but slightly smaller results.
- Finally, we discard the panel model and test a difference-in-differences specification to identify whether the substantial minimum wage increase in April 2016 had a different impact on treated and control items. Although some specifications find an impact, the results here are sensitive to the time window used and how the treatment and control groups are defined, and should be interpreted with caution.

1 INTRODUCTION

Much of the early research into the unintended economic consequences of minimum wages has focused on employment effects. A consensus has emerged that the effects are broadly neutral in the UK. The literature has therefore begun exploring other channels through which the effect might be observed, which include changes in hours worked, productivity, profitability and prices.

Effect on businesses

One strand of research has focused on the impact on businesses using firm-level data. Draca et al (2008) focus on the impact on profits, using the introduction of a national minimum wage to the UK in 1999 as a quasi-experiment. They use pre-policy information on wage distributions to construct treatment and control groups, corresponding to minimum wage-exposed and non-exposed firms. This is used to implement a difference-in-difference approach on two different panel datasets: firstly, the FAME dataset which captures accounting data for firms and allows an economy-wide analysis, and secondly a dataset focusing on residential care homes. The authors find that firm profitability was significantly reduced and wages significantly raised by the introduction of the minimum wage. The authors also find that net entry rates had fallen, although the changes in exit and entry rates are statistically insignificant.

Riley and Rosazza-Bondibene (2015) use as a quasi-experiment the introduction of the national minimum wage, as well as subsequent increases, exploring the effects on productivity. Labour cost per employee ratios are used to identify minimum wage firms and construct treatment and control groups for a difference-in-difference analysis. The authors use FAME and Annual Respondent Database, which both have economy-wide coverage. The authors find that the NMW increased average labour costs for companies that employ low-paid workers, both upon the introduction of the NMW and during the 2009 global financial crisis. This resulted in companies raising labour productivity in response to labour cost increases. This was not through a reduction in firms' workforce, but rather through increases in total factor productivity, which could come about through training or efficiency wage responses.

For example, both Draca et al. (2008)[3], and Riley and Rosazza-Bondibene (2015)[4] use labour cost per employee, which is reported in data such as company accounts and the Annual Business Survey. The authors validate these thresholds against the Workplace Employment Relations Survey (WERS), which provides a full overview of pay structure within the firm (and hence reliance on minimum wage labour) to establish that the thresholds are appropriate.

This paper builds on the preceding literature in several ways:

[3] Draca, Machin and Van Reenen, NBER 2008, available at: https://www.nber.org/system/files/working_papers/w13996/w13996.pdf

[4] NIESR discussion paper 449, available at: https://www.niesr.ac.uk/sites/default/files/publications/Minimum%20wages%20and%20firm%20productivity%20NIESR%20DP%20449.pdf

- We use the Business Structure Database (BSD), which includes small firms and detail at the individual workplace level. This information is not captured as well in the other datasets.
- Our sample covers the introduction of the National Living Wage, which is large in proportional terms; impacts may be larger / more discernible as the 'bite' increases.

We identify low-pay firms using worker-level microdata from the Annual Survey of Hours and Earnings (ASHE), linked directly to the BSD, which is a novel approach. This is used to implement a difference-in-difference analysis at the firm level.

Effects on prices

There is a small body of literature testing the relationship between wage floors and consumer prices in the UK. Wadsworth (2010) studies the introduction and uprating of the UK minimum wage on the price of goods and services, comparing sectors where minimum wage workers account for a substantial share of total costs to those where it does not. He finds limited evidence that prices were higher in the months corresponding to the minimum wage uplift, but stronger evidence of a long-term effect in the years following the introduction of the minimum wage.

Draca et al. (2005) examine the impact that the 1999 introduction of the national minimum wage had in three 'exposed' sectors (restaurants, canteens and takeaway food) but find no evidence of a price effect. Machin et al. (2003) looks at the effect in the residential care sector and also finds no effect, although they note that the sector was price regulated.

Elsewhere, there is stronger evidence of price effects; see Lemos (2008) or MaCurdy (2015) for a summary. Harasztosi & Lindner (2019) exploits a large increase in the minimum wage in Hungary and firm-level data, finding that the doubling of the minimum wage led to a 7% to 14% increase in prices over a four year period. The authors also find strong evidence that prices of non-tradable products are more likely to rise than those of products that are exposed to international competition. Aaronson, French and MacDonald (2005) use store-level data on restaurant prices in the USA to show an unambiguous price effect that is stronger where the store employs more minimum wage workers and when the minimum wage increase is larger.

Card and Krueger (1995) find that minimum wages led to a small price increase in their sample of affected New Jersey fast-food restaurants. Aaronson (2001), also examining fast food prices, finds that a 10% increase in the minimum wage raises prices by <1%, particularly when overall inflation is high. MacDonald and Aaronson (2006) find most fast food restaurants only raised prices on a subset of their product range in response to higher minimum wages, suggesting item-specific fixed costs.

This paper builds on this literature in two ways:

- We use monthly price data on a broad sample of around 1,100 'items'.
- We measure prices and minimum wage exposure at the region level, rather than using national averages.

We use data from the Annual Survey of Hours and Earnings (ASHE) and the Annual Business Survey (ABS) to identify sectors and regions that are more or less likely to be exposed to increases in the minimum wage, mapping this to monthly price data at the item at the region level. We then test whether these ‘exposed’ firms raised prices more in months where minimum wages increased than in other months.

Report structure

The remainder of this paper proceeds as follows. **Section 2** sets out the institutional context of minimum wage setting in the UK, and describes the theoretical framework underpinning our analysis. **Section 3** presents the datasets, analytical approach and findings regarding effects on businesses, while **Section 4** presents the same for effects on prices. **Section 5** concludes.

2 BACKGROUND AND CONTEXT

Institutional context

In April 1999, the UK Government introduced a universal statutory minimum wage on employers. The Low Pay Commission (LPC), an independent public body, makes recommendations to government on the size of any minimum wage uplift, based on monitoring and evaluation evidence. It was then uprated in October of each year until 2016.

Since then, the minimum wage has increased in April. The LPC typically makes its recommendation in October the previous year, with the Government announcing its response to the recommendation in November.

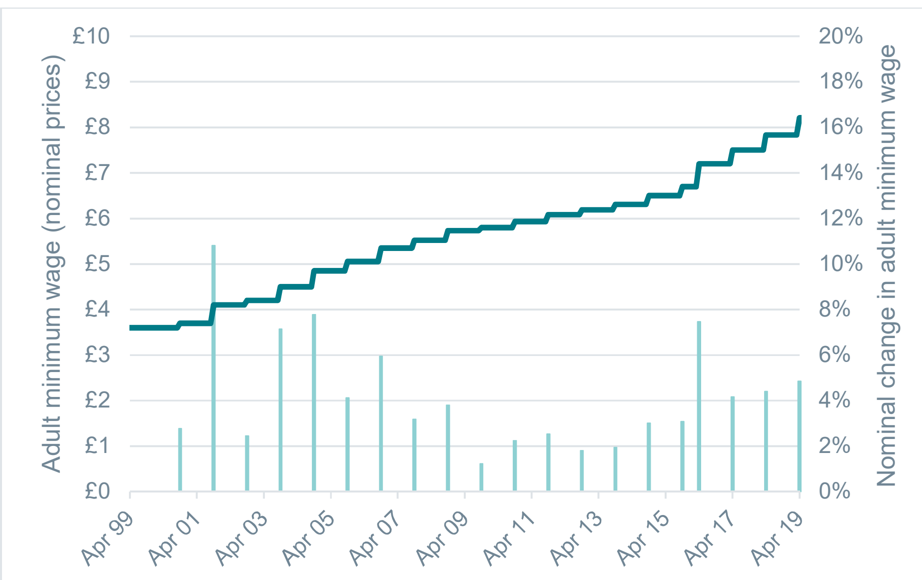
There are now five minimum wage rates:

- The National Living Wage (NLW);
- Age-specific minimum wages for those aged 21-24, 18-20 and 16-17; and
- an apprenticeship rate.

The rate applicable to those aged 25 and over has increased substantially since its introduction: from £3.60 in 1999 to £8.72 in 2020, equivalent to an average annual increase of 4.3% in nominal terms (see Figure 1).

The annual uplift was relatively high in the early part of the 21st century, but growth slowed to an average annual increase of 2.5% in the years during and following the financial crisis (2008 to 2015).

Figure 1 Minimum wage for those aged 25 and over



Source: Low Pay Commission (2019), '20 years of the National Minimum Wage: A history of the UK minimum wage and its effects'

Note: Nominal prices

The introduction of the National Living Wage in 2016 increased the applicable minimum wage by 10.8% above its level the previous April, and annual growth averaged 6.1% between 2015 and 2020. The minimum wage is now equivalent to around 60% of median earnings.

The Low Pay Commission (2019) estimated that around 2 million jobs, 7% of the UK total, are directly affected by minimum wages. However, this necessarily affects some sectors and regions of the economy more than others; variation we exploit in this study.

Theoretical framework – firm-level impacts

Microeconomic theory suggests that any binding increases in the minimum wage may encourage firms to substitute away from minimum wage/low-skilled labour to capital (and/or to more skilled labour), depending on the production process. It may also result in exit by firms that had marginal profitability prior to the uprating. This can be illustrated with the following stylised example:

Consider a firm with Cobb-Douglas production function:

$$Y = \theta K^{\alpha} L^{\beta} \quad (1)$$

Where ϴ is firm-level productivity, K is capital input and L is labour input, and α is measure of capital / labour productivity. The firm faces prices p, capital cost c and wages w, and so has profit function:

$$\Pi = pY - cK - wL \quad (2)$$

Substituting (1) into (2):

$$\Pi = p\Theta K^{\alpha} L^{\beta} - cK - wL \quad (3)$$

Optimising with respect to K and L and, re-arranging we have:

$$\left(\frac{K}{L}\right) = \left(\frac{w}{c}\right)\left(\frac{\alpha}{\beta}\right) \quad (4)$$

So that the capital-labour ratio is proportional to the ratio of labour to capital costs and the relative factor productivity. This means that, ceteris paribus, an increase in labour costs will result in a decrease in labour input.

Setting (3) to zero, firms will exit if the wage is higher than the marginal profitability of labour.

$$p\theta K^{\alpha} L^{\beta-1} - \frac{cK}{L} < w \quad (5)$$

This results in exit by firms that are either less productive (low ϴ) or less capital-intensive.

The example can be extended to differentiate between minimum wage and high-skill labour. This generates similar findings regarding input substitution and exit.

This stylised perfectly competitive market example, however, does have limitations. Firstly, firms may have market power either in setting prices or wages. This would alter some of these equilibrium conditions, for example, with possibility of passing through some cost increase. But most importantly, in the 'real world' production processes do not necessarily allow for substitution between capital and labour at the margin as envisaged above. In many cases, capital will have a fixed labour requirement, so that there is no scope for substitution. And if investment in plant is made at infrequent intervals, this would mean that staffing remains fixed until the next investment round is due, at which point new investment and staffing decisions are made. Exit decisions will also be affected by the timing of investment, and there may be firms that continue trading in the short run (using existing capital), but not invest in future rounds.

The range of technologies and production processes will vary by sector, with different scope for input substitution, and different technologies becoming available. The most obvious examples of this are self-scan checkout facilities in retail and electronic ordering systems in food service. Given the mix of different business types, we may expect a range of different possible responses.

Theoretical framework – price effects

Microeconomic theory suggests that a firm's ability to raise prices in response to an increase in input costs depends on a number of factors:

- the price elasticity of demand for the good;
- the degree of competition in the product market, and the extent to which competitors are subject to the price shock; and
- the firm's ability to substitute to alternative inputs, or increase factor productivity.

We formalise this framework using a stylised version of the Hicks-Marshall style model described in Harasztosi and Lindner (2019). We consider a market of monopolistically competitive firms in a partial equilibrium framework, assuming that firms have identical, constant return to scale production functions and operate in perfectly competitive labour markets.

Consumer problem. Harasztosi and Lindner (2019) show that if consumers have a nested CES-type utility function, the demand response to a price increase (e_i) depends on the fraction of firms that raise prices. If only one firm raises prices, then the demand response for that firm will be relatively high, given by:

$$e_i = \frac{\partial q_i}{\partial p_i}\frac{p_i}{q_i} = -\kappa \qquad (6)$$

where κ is the elasticity of substitution between different varieties within the product market

We can show that if all firms in the product market raise their prices, the demand response for a given firm will be smaller such that $e_i > -\kappa$, reflecting consumers' willingness to substitute for other goods outside the product market.

Firm problem. We know that if firms face a constant return to scale production function with three inputs (minimum-wage labour l_m, high-wage labour l_h, and capital k with costs w_m, w_h and r), marginal cost is given by:

$$MC_i = l_m w_m + l_h w_h + kr \qquad (7)$$

Also, in perfectly competitive markets, profit maximising firms set marginal revenue to equal marginal cost.

$$MR_i = \frac{\partial R_i}{\partial \mathrm{q_i}} = \frac{\partial (p_i q_i)}{\partial \mathrm{q_i}} = p_i + q_i\frac{\partial p_i}{\partial \mathrm{q_i}} = p_i\left(1 + \frac{\partial p_i}{\partial \mathrm{q_i}}\frac{q}{\mathrm{p}}\right) = p_i\left(1 + \frac{1}{e_i}\right) \qquad (8)$$

$$p_i\left(1+\frac{1}{e_i}\right)=MC_i$$

$$p_i=\frac{MC_i}{1+e_i^{-1}}=\frac{MC_i}{1-\kappa^{-1}} \quad (9)$$

So, as κ is a constant, the relationship between prices and minimum wages can be defined as:

$$\frac{\partial \log p_i}{\partial w_m}=\frac{\partial \log MC_i}{\partial w_m}=\frac{\partial M\_\ i}{\partial w_m}\frac{1}{MC_i}=\frac{l_m}{MC_i} \quad (10)$$

Multiplying through by the minimum wage in order to express the left-hand side as the percentage change in price resulting from a percentage change in minimum wages, we see that this is equal to the share of minimum wage labour costs in total costs s_i.

$$\frac{\partial p_i}{\partial w_m}\frac{w_m}{p_i}=\frac{l_m w_m}{MC_i}=s_i \quad (11)$$

From this identity, we can draw two conclusions:

- First, the effect of a minimum wage increase on prices will be proportional to the importance of minimum wage labour costs in the production functions of firms in the product market.
- Second, the effect of a minimum wage increase on prices will be proportional to the share of firms in the product market that are affected by the minimum wage increase.

These two observations inform the identification strategy. We exploit variation in the share of minimum wage labour in the production functions of firms; and variation in the degree to which products are tradable (which influences the share of firms affected by the minimum wage increase).

Price adjustment mechanism. In our stylised model, firms do not incur price-adjustment costs, and therefore respond instantaneously to a minimum wage shock without lags or anticipation. However, empirical evidence suggests that firms adjust prices only once or twice per year, see e.g. Taylor (1999), with larger firms and firms operating in competitive market likely to adjust prices more often. Alvarez et al (2006) finds that firms in the food service sector adjust their prices most often, with non-food service industry sectors adjusting prices least often. Similarly, Bunn and Ellis (2011) and Klenow and Malin (2010) find that goods prices are adjusted more frequently than services prices.

These frictions may make it more difficult to empirically observe the effect of minimum wage uplift in the month that it occurs.

3 FIRM-LEVEL EFFECTS

This section describes the firm-level analysis in detail. Section 3.1 sets out the approach, methodology and datasets. Section 3.2 sets out some descriptive statistics. Section 3.3 follows with the econometric results. Annexes A and B provide further results.

3.1 Approach

We describe the approach in terms of overall hypotheses tested, datasets used and econometric specification.

3.1.1 Overview

The aim of this research is to explore how the NLW in particular has affected businesses. Existing evidence has found limited impact so far, and focused on larger firms in datasets such as the Annual Business Survey or FAME. This research is intended to complement previous findings, by extending the analysis to cover smaller firms, and exploring impacts on business creation and destruction. There is also value in analysing the most recent and significant increases in minimum wages, as it is possible that effects are only now being realised.

Ideally we would explore the following hypotheses as to firms' response:

1. Affected firms have substituted away from minimum wage labour
2. Affected firms with marginal profitability have exited and / or entry by such firms has been reduced
3. Affected firms have increased prices
4. Affected firms have reduced profitability

In this research we explore hypotheses (1) and (2). Hypothesis (3) is explored separately in parallel Frontier research focusing in detail on prices. Due to the datasets used in this study, it is not possible to explore hypothesis (4).

In order to complement previous research, we opted for a dataset not previously used in minimum wage research, the Business Structure Database (BSD). This dataset is very comprehensive, covering all VAT- or PAYE-registered firms – only excluding sole traders. It offers insights not available in other datasets:

- Sample sizes are much larger than other datasets, allowing for more disaggregated analysis.
- Small firms are included.
- Coverage is complete over time, allowing for longitudinal analysis.
- Data is presented both at local workplace level, and the firm level, allowing us to see what is happening in multi-unit firms ('chains').

The BSD is used to conduct a 'difference-in-difference' analysis, where we compare the change over time in performance of minimum wage and other businesses following the introduction of the NLW, and control for relevant drivers of performance, interpreting any difference as an impact of the minimum wage increase.

A key question is how we define minimum wage firms. Firm-level minimum wage research has generally used average pay thresholds, with pay below the threshold signalling minimum wage exposure. For example, both Draca et al. (2008)[5], and Riley and Rosazza-Bondibene (2015)[6] use labour cost per employee, which is reported in data such as company accounts and the Annual Business Survey. The authors validate these thresholds against the Workplace Employment Relations Survey (WERS), which provides a full overview of pay structures within the firm (and hence a measure of reliance on minimum wage labour) to establish that the thresholds are appropriate.

Assignment using labour cost thresholds is not possible for the BSD, as labour cost data is not collated. Instead we use Annual Survey of Hours and Earnings (ASHE), which can be linked directly to the BSD using the workplace identifier. We observe the pay of individuals sampled in ASHE in 2015, prior to the introduction of the NLW; those paid below the incoming 2016 NLW are assigned to the treatment (affected) group, and those above to the control group. However, there is a possibility of measurement error, as the sampling of individuals in ASHE is random, and the sampled individual may not be representative of the wider (true) minimum wage exposure of the firm.

We therefore undertake various tests to explore the extent of the measurement error, including comparing the correlation of the ASHE assignment variable with labour cost thresholds in the Annual Business Survey and simulating random assignment in WERS (see Annex A).

Using the BSD, we can then compare outcomes for the firms observed paying the minimum wage, and those observed to pay above. The main outcome of interest here is employment growth, but it is also possible to explore turnover growth and survival.

Section 2.2 describes the data sources in further detail.

3.1.2 Data Sources

In this section we describe the data sources in further detail.

Annual Survey of Hours and Earnings (ASHE)

The Annual Survey of Hours and Earnings (ASHE) follows a sample of 1% of UK workers, randomly selected on the basis of their national insurance number (NINO). The same individuals are followed over time. The survey is completed by employers, so should align with payroll data. Hence, the pay data is likely more accurate than in respondent surveys (e.g. Labour force Survey), which may suffer various inaccuracies, such as imperfect recall or other biases.

[5] Draca, Machin and Van Reenen, NBER 2008, available at: https://www.nber.org/system/files/working_papers/w13996/w13996.pdf

[6] NIESR discussion paper 449, available at: https://www.niesr.ac.uk/sites/default/files/publications/Minimum%20wages%20and%20firm%20productivity%20NIESR%20DP%20449.pdf

ASHE covers in the range of 140,000 to 185,000 workers per year.

ASHE contains various measures of pay and hours worked. For the purposes of our work, the key variable is hourly pay excluding overtime ('hexo'), which is the measure that aligns best with minimum wage calculations. This is the variable we use to assess whether the worker is paid more or less than the incoming minimum wage.

ASHE lists the enterprise reference number and census output area of the workplace. This lets us link to local units in the BSD.[7]

ASHE also contains detail on the age, occupation, and gender of workers. This data is used in some specifications to control for the characteristics of the worker sampled in ASHE. It must be acknowledged that these controls are not nearly as rich as what is available in, say, the Labour Force Survey. In particular, data on educational qualifications is not reported.

The ASHE snapshot is taken in April of each year. While minimum wage increases have typically come into force in October of each year, the NLW introduction occurred in April 2016. In that year, the pay reference date was 13 April. There were instances of employers having not implemented the increase by then but the legislation allows for some delay depending on the length of the pay reference period.. As a result, some employers might have been reporting 'old' minimum wages in ASHE 2016. It is also the case that employers can offset hourly rates where accommodation is included, for example. Both these features mean that hourly pay calculated in ASHE may be below minimum wage levels without necessarily being non-compliant.

Business Structure Database

The BSD is an annual extract taken from the Inter-Departmental Business Register (IDBR), and supplemented with data from additional sources. It covers all VAT- or PAYE-registered firms, and around 98% of all UK economic activity. As it is essentially an annual census of all firms, it gives a longitudinal dataset amenable to various panel analysis methods.

The key variables included in the BSD are employment and turnover. Along with whether active, these give a range of outcomes including employment growth, turnover growth, and survival.[8]

Key control variables are sector (SIC code) and location (available at a range of different levels of granularity). Variables can also be derived to control for current size, age, and prior growth history.

A particular advantage of the BSD is that data are collected at the level of the individual workplace ('local unit'), as well as at the firm level ('enterprise unit'). As local conditions will affect the impact of minimum wages, this means that the

[7] ASHE does not contain a local unit reference, which is required for a direct link to local units BSD. However, the link is made possible by using the enterprise reference in combination with geographical identifier n both datasets. There will be a small number of cases where there are multiple local units of an enterprise within the same output area; as it is not possible to distinguish them, they are combined in the analytical dataset. This is legitimate, as it is unlikely that the two units would face different labour market conditions.

[8] In other contexts, start-up rates can be analysed. This is not feasible in our main approach, as we condition on observed pay prior to the NLW coming into effect.

individual workplaces from firms operating in many locations can be included separately. It is also possible to control for these characteristics in terms of enterprise structure, i.e. whether a workplace is the sole business premises of the firm, or is one part of a much larger company. Finally, there is a set of ‘who owns who’ codes, to identifier subsidiaries owned by other companies. This allows exploration of whether, for example, subsidiaries in a conglomerate face different conditions to other companies.

However, there are also issues regarding the timeliness of outcome variables in the BSD. The BSD snapshot relates to data as at the financial year ending in that year. For example, the BSD 2017 reflects a snapshot at April 2017. The employment and turnover data in the BSD is collated from a number of different sources, with different processes for updating. The most appropriate variable or source to use will vary with firm type. Turnover, in particular, is understood to face serious time lags, such that it is necessary to use the next year’s value in place of this year’s (so that we use BSD 2018 value to reflect 2017). In the case of enterprises with multi local units, employment data is primarily drawn from the Business Register Employment Survey (BRES), which seeks to apportion out employment to the various local units. The data can be extended forwards if the firm is not sampled, leading to lags. In other cases, the data are drawn from other surveys or imputed from turnover. In the case of enterprises comprising only one local unit, the employment data are sourced from PAYE.

Also note that turnover data are not reported at the local unit level. While we can apportion turnover to the different local units of an enterprise, by definition this will conceal any intra-enterprise variation. The timing issues and inability to analyse at a local level (where local conditions will determine the bite of the minimum wage) all make turnover analysis problematic in this context.

3.1.3 Analytical approach

Our approach is to assign firms into different groups on the basis of wages of individuals observed in ASHE. The groups are intended to distinguish firms that are affected, or unaffected, by the minimum wage. The effect of being in one group or the other is measured in a difference-in-difference framework.

Assignment into treatment and control groups

The overall approach to assignment is to assign workers who are paid below the incoming minimum wage to the treatment group, and those paid above it (or some amount above it) in the control group. The workplace they are in is assigned accordingly. However, there is scope for measurement error in using the pay of a randomly sampled individual as a measure of minimum wage exposure at the firm level. For example, a managerial or senior staff member might be drawn in a workplace where low pay is prevalent. Or a junior staff member in a support role might be drawn from a professional services firm, where pay for the ‘main’ occupation is much higher. All of this means that the pay information observed in ASHE is an imperfect signal of the firm’s wider pay level.

We therefore employ a number of robustness checks to explore the materiality of the measurement error. These are detailed in Annex A, where we directly explore the correlation of the ASHE assignment variable with measures of minimum wage exposure from other datasets. To summarise:

- We find strong correlation between the ASHE assignment variable and labour cost thresholds from the ABS.
- We simulate the ASHE assignment procedure within the WERS dataset and find it acts as a stronger signal of minimum wage exposure than the labour cost ratios normally used in the literature.

These both support the view that the ASHE assignment procedure acts as a reasonable proxy for minimum wage exposure. We also use other strategies to mitigate measurement error, which include adding worker-level controls, and restricting occupational and sectoral scope to reduce sampling error (see boxed text).

SECTOR AND OCCUPATION SCOPE

Firms employ a mix of staff in different occupations, and staff at different levels of seniority. The determination for which worker in a firm gets sampled in ASHE is essentially random. In some cases, the individual sampled might not be representative of the wider pay pattern within the workplace.

This problem is at least partly mitigated by including controls for worker characteristics in the regression. For example, we typically find that employment growth is positively correlated with the proportion of workers aged under 35. It is therefore legitimate to control for this when modelling firm performance. The worker-level controls comprise dummy variables for the different age bands (10-year increments), gender, and Standard Occupation Code.

In parallel, we use a more restrictive approach, which is to confine the analysis to firms in low-pay sectors, and only sample employees in low-pay occupations. This is intended to avoid dubious assignment, where the worker is not representative of the wider firm. **We use both the low-pay focused approach described here, as well as the unrestricted approach where all sectors and occupations are included.**

There two other points to mention regarding assignment in ASHE:

- **Youth rates.** The NLW applies to workers aged 25 and over. Below this age, youth rates apply. In some cases, we may observe a youth in ASHE paid well in excess of the youth rate, but below the full NLW. Overall, we consider that an employer paying below NLW rates to youth workers is likely to be affected by the minimum wage, as by definition some of their workers are paid below the prevailing NLW rate. It is also worth noting that use of youth rates is relatively rare. On this basis, we do not differentiate between youths and adults, and use the full adult NLW rate for the purposes of assignment.[9]

[9] While adopted primarily for simplicity, this approach to youth rates may be more problematic for some sectors than others. For example, hospitality generally has a younger workforce, compared with cleaning or social care.

- **Spillover effects and thresholds.** Minimum wages also affect pay further up the distribution.[10] This could be due to: focal points (employers settling on a round number above the minimum wage), desire to maintain an explicit pay differential (e.g. paying 50p more per hour), or more generally changes to the reservation wage induced by minimum wage increases. Given these considerations, we define the treatment group as those receiving less than 100% of the incoming minimum wage and the control group as those receiving more than 120% of it, thereby excluding the 100-120% 'buffer'. This reflects prior beliefs about the extent of spillover effects. We also undertake detailed sensitivity analysis using alternative thresholds which confirms this approach is reasonable (see Annex B).

Econometric approach

The workplace-level assignment variables from ASHE are linked to a dataset of workplaces from the BSD. A difference-in-difference approach is then used to estimate the impact of being affected by minimum wages on firm performance. In terms of outcome, we consider a variety of different employment growth measures, as well as survival and turnover growth. The main outcome measure we focus on is change in log employment from 2015 to 2018.

The outcome is regressed on current firm characteristics, the characteristics of the worker sampled in ASHE, and sector and region dummies. The 'treatment' variable measures the effect, above and beyond these features, of being high-bite, i.e. of the worker being observed to be paid below minimum wage in ASHE.

The model can be written as:

$$Log(employment_{i,g,s,2018}) - Log(employment_{i,g,s,2015}) = \beta_1 + \beta_2 X_i + \beta_3 W_a + \beta_4 Sector_S + \beta_5 Region_g + \beta_6 Minwage_a + u_i$$

for sampled worker a in firm i in region g and sector s

The coefficient β_1 is and intercept term. β_2 gives the effect of firm characteristics observed at the baseline year 2015. β_3 gives the effect of characteristics of the worker(s) sampled in ASHE. β_4 and β_5 give the effect of being in a particular region and sector. Finally, β_6 gives the effect, above and beyond the preceding variables, of being observed in ASHE to pay below the incoming minimum wage. This is interpreted as the 'treatment effect'

The control variables are described in further detail:

- Firm characteristics.
  - Age in years
  - Foreign-owned (dummy)
  - Rural (dummy)
  - Log employment

[10] For detailed discussion and recent empirical work see Avram and Harkness (2019)

  - Log turnover
  - Dummy for firm not existent in 2012[11]
- Worker characteristics
  - Age band dummies
  - Gender dummies
  - Occupation code dummies (defined at 2-digit SOC code level)
- Sector and region dummies. Sectors are generally defined at the 2-digit level, with several large sectors (e.g. hospitality) further disaggregated to 3-digit levels. Regions are defined at the GOR level.
- Treatment variable. This captures the incremental effect of being a minimum wage firm, relative to the base case of paying above minimum wage (or some amount above), controlling for the above characteristics.

The control variables were selected to capture different features that are considered relevant from a theoretical standpoint, and then tested empirically to identify the particular ones within each group with the best explanatory power. This involved testing multiple specifications of the variables, such as including non-parametric size and age variables, defining history to different horizon points, and testing alternative TTWA geographic variables, and sector-dummy interactions. This is intended to generate a rich yet parsimonious model.

3.2 Descriptive Statistics

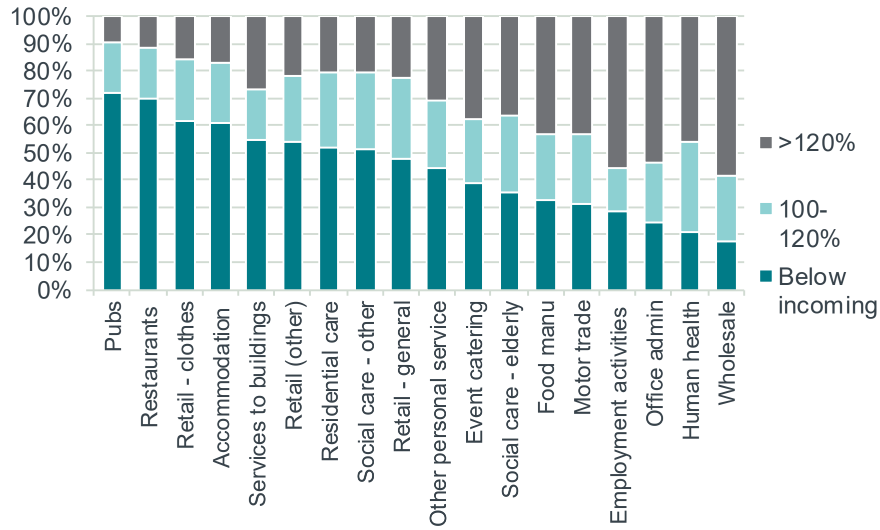
In this section we briefly summarise some descriptive statistics to show how the assignment procedure works in practice and draw insights on basic performance trends.

We explore differences in characteristics of treatment and control groups, as defined by the ASHE assignment procedure, breaking down by sector to understand which sectors are driving these effects. We focus on low-pay sectors, to gain the most clarity, as including other sectors for which low pay is very rare would blur the picture.

Figure 2 below shows a selection of minimum wage sectors ranked by the ASHE assignment category. We see that more than two-thirds of businesses are categorised in the treatment group for the pub and restaurant sector. In other sectors such as office administration, human health, and wholesale, the proportion is around 20%.

[11] In these cases, the missing values are replaced with zeros.

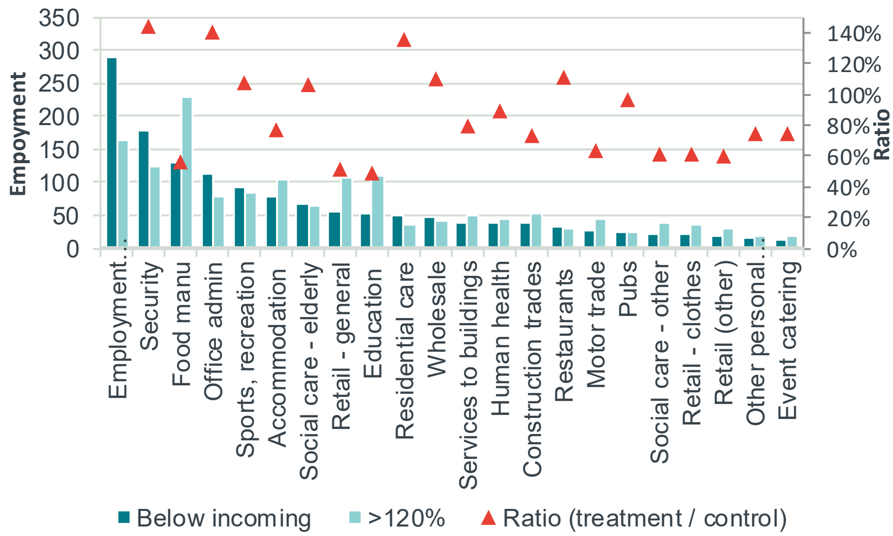
Figure 2 ASHE low-pay assignment groups by sector



Source: Frontier analysis of ASHE data (ONS)

In Figure 3, we show the average number of employees per workplace by sector and group. In general, minimum wage establishments are of a similar size to those observed to pay more. There are several exceptions to this, such as employment agencies (the minimum wage firms are considerably larger), and food manufacturing and non-specialised retail, where the opposite is the case.

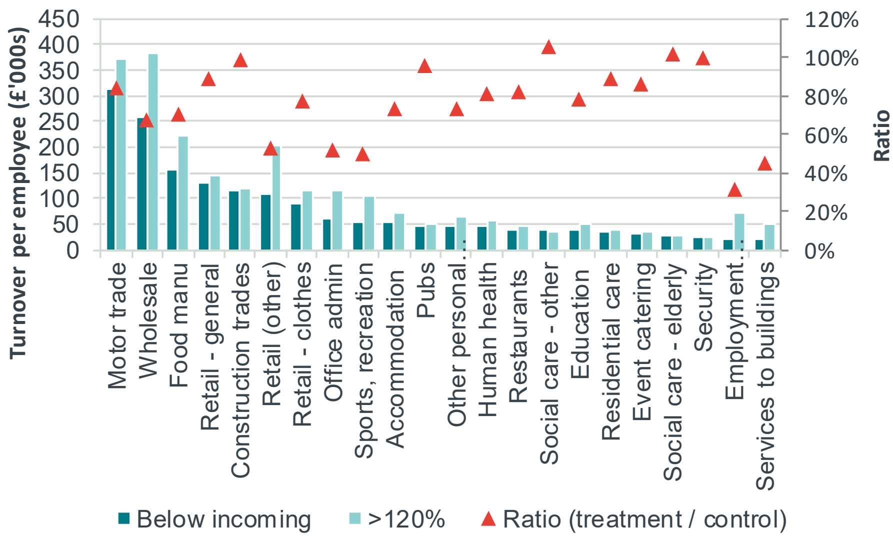
Figure 3 Average number of employees by sector



Source: Frontier analysis of BSD and ASHE data (ONS)

It is also apparent that the minimum wage group has lower turnover per employee than the control group. This is the case for most of the sectors analysed; on average minimum wage firms have 24% lower turnover per employee than control firms in the same sector. This is consistent with the hypothesis that minimum wage firms have lower productivity of labour, giving some assurance that the ASHE assignment approach is identifying the 'right' firms.[12]

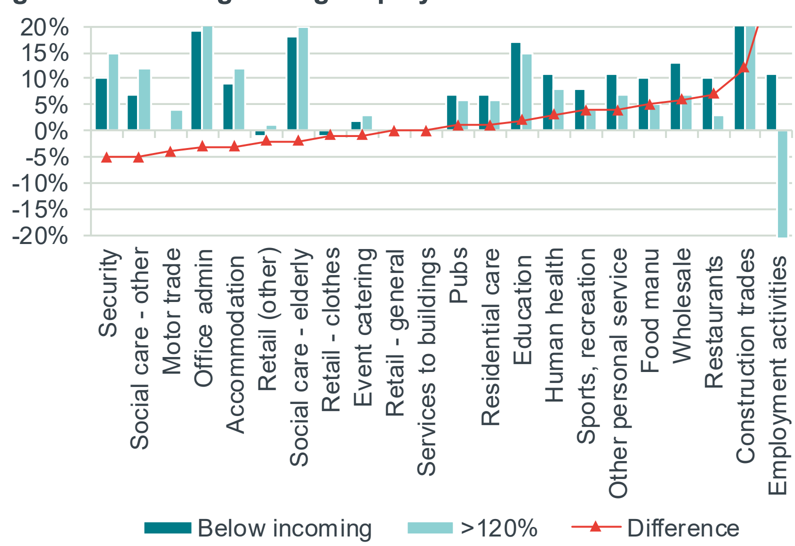
Figure 4 Turnover per employee (2015)



Source: Frontier analysis of BSD and ASHE data (ONS)

We now compare employment growth in the period prior to the baseline year. In general, firms in both treatment and control groups grew their staff during this period (the growth outcomes are for surviving firms). In some cases, the minimum wage firms grew faster than others; in others the opposite is the case. Overall, there is very little difference.

Figure 5 Change in log employment from 2012 to 2015

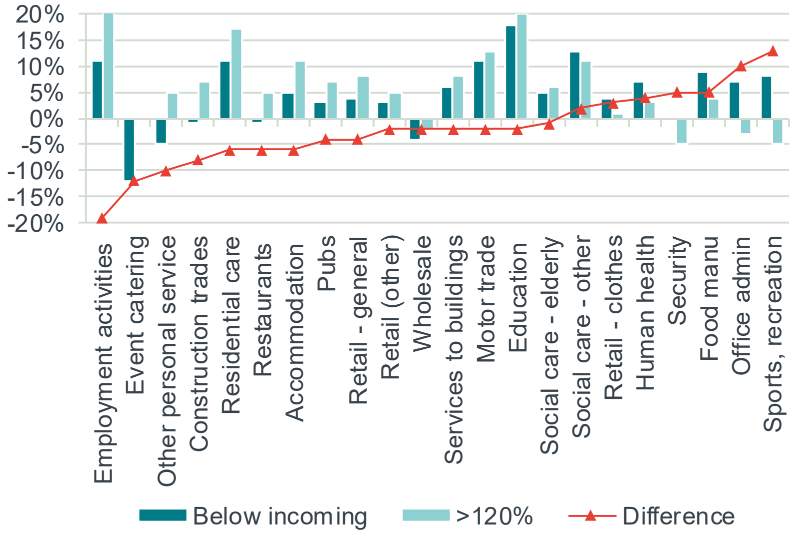


Source: Frontier analysis of BSD and ASHE data (ONS)

[12] Turnover per employee can be used as a crude measure of productivity, although there is scope for it to be affected by input costs and the degree of vertical integration.

In most sectors, turnover per employee growth was lower in the minimum wage firms than in those paying above. One interpretation would be that if prior turnover growth was lower, this would place downward pressure on wages and be correlated with assignment into the treatment group.[13]

Figure 6 Change on log turnover per employee 2012 to 2015

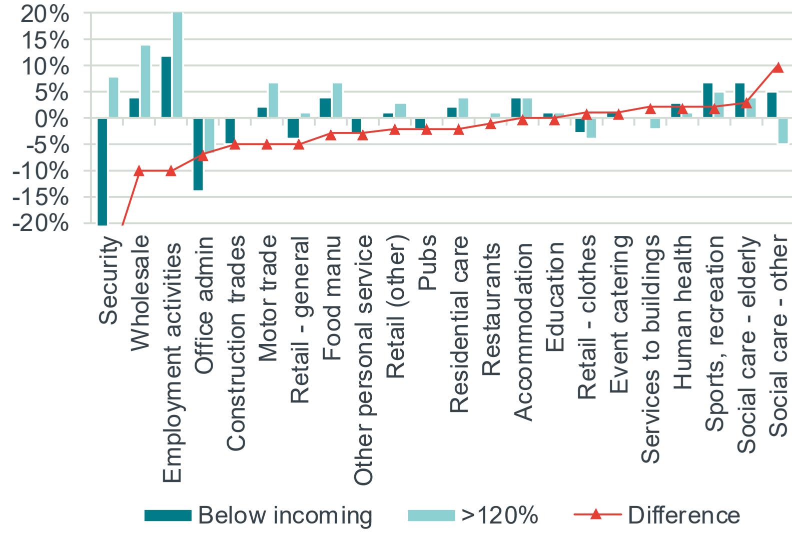


Source: Frontier analysis of BSD and ASHE data (ONS)

We now turn to employment growth in the period following the introduction of the NLW. This essentially shows the raw differences between treatment and control groups, before conditioning on any firm characteristics that describe performance. Here we see that in the majority of sectors, the firms paying above the incoming NLW in 2015 grew faster than those paying below it. These are 'raw differences', and we subsequently explore whether this gap can be explained by observable characteristics, or if it can be interpreted as a minimum wage impact. In median terms, the gap is around 2%.

[13] In a mean reversion sense, conditioning on being observed to be minimum wage in 2015, a firm is more likely to have undergone downward wage growth to get to that point.

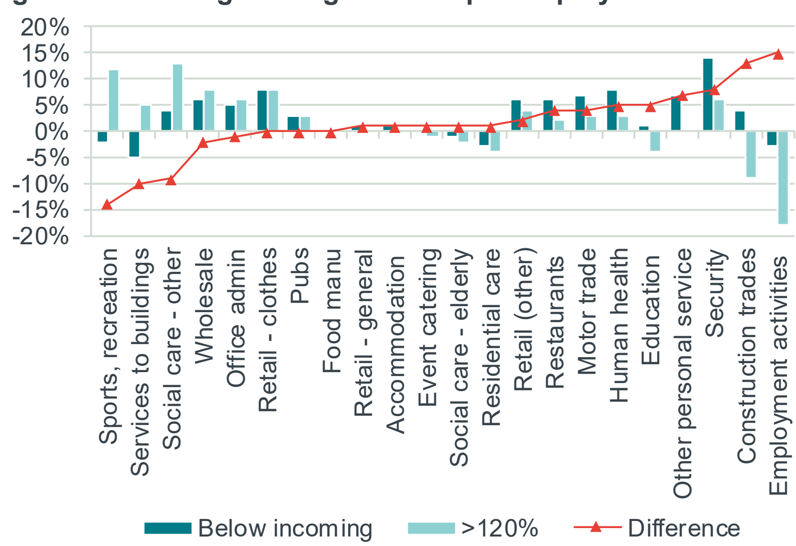
Figure 7 Change in log employment from 2015 to 2018



Source: Frontier analysis of BSD and ASHE data (ONS)

Across sectors, there is little systematic change in turnover per employee from 2015 to 2017, and no obvious difference between treatment and control groups. However, it is worth re-emphasising some caveats around turnover per employee data. While the employment and turnover data may each suffer time lags (such that we consider turnover likely to be one year out of date and use the forward year's value), the time periods for which each are measured do not necessarily coincide. Turnover is also more prone to effects of large outliers. Finally, it is not reported on a local unit basis. All of this reduces the weight that can be placed on it.

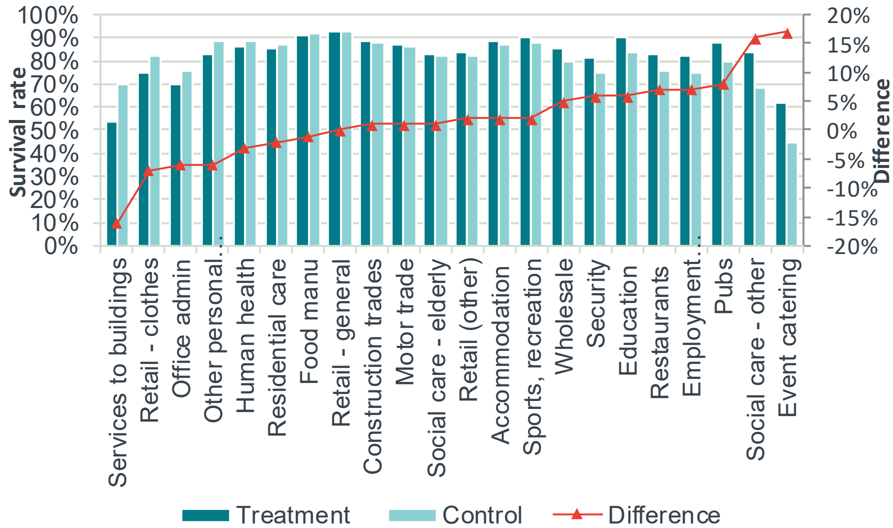
Figure 8 Change on log turnover per employee 2015 to 2017



Source: Frontier analysis of BSD and ASHE data (ONS)

Finally, we consider survival rates. However, this could reflect differences in churn rates, as well as differences in net terms (births minus deaths).

Figure 9 Survival rates from 2015 to 2018



Source: Frontier analysis of BSD and ASHE data (ONS)

3.3 Findings

3.3.1 Core results

As stated above our main focus is on analysing the change in employment from 2015 to 2018. This is as a function of firm characteristics (e.g. size and age at baseline year), sector and region dummies, prior growth, and characteristics of the worker sampled in ASHE. A dummy variable is included for high-bite firms – the so-called 'treatment group'.

In order to maximise timeliness of the employment outcome variable, employment is sourced from PAYE in the case of single-unit enterprises, or where sourced from BRES in the BSD. We therefore seek to avoid relying on imputed or less timely data.

Results are shown for both a full sample in which all sectors are used (column (1)), as well as an approach whereby only low-pay sectors and occupations are included in the analysis (column (2)). The key coefficient of interest is the treatment dummy. In column (1) this implies that above and beyond the various factors controlled for, employment growth was around 3% weaker in high-bite firms. This effect is statistically significant at the 1% level. Specification (2) shows the effect estimated for low-pay occupations / sectors specifically. The effect of –2.3%, which is also statistically significant.

Other results in the model are discussed in turn:

- The sectoral dummies are generally insignificant, as is the dummy for foreign-owned firms. In other words, we can see no systematic variation in employment growth along these dimensions between 2015 and 2018. However, the rural dummy is negative and significant.

- The firm size coefficients are consistent with intuition. The negative effect of employment size at 2015 indicates that firms that are already large grew more slowly. But the positive coefficient on turnover per employee at 2015, suggests that firms that were trading more per employee will grow faster as employment is adjusted to accommodate the greater workload. This also points to substitution of employment from less productive to more productive workplaces.
- The worker characteristics are generally less significant in column (2) which only uses low-pay occupations and sectors in the assignment stage. Our hypothesis is that the low-pay-focused version removes variation in the range of worker types observed, so less power is then attached to these variables in driving firm performance.

Figure 10 Base regression results for change in log employment, 2018 against 2015

	(1) All firms		(2) Low-pay	
	beta	p-value	Beta	p-value
Foreign-owned	0.002	0.59	0.014	0.01
Firm age	-0.002	0.00	-0.001	0.04
Rural	-0.012	0.04	-0.029	0.00
Treatment	-0.030	0.00	-0.023	0.00
Log employment 2015	-0.059	0.00	-0.066	0.00
Log turnover per employee 2015	0.043	0.00	0.053	0.00
Sector dummies	Included		Included	
Region dummies	Included		Included	
New firm	0.024	0.01	0.015	0.03
Worker age 25-34	0.052	0.01	0.016	0.33
Worker age 35-44	0.040	0.01	0.013	0.44
Worker age 45-54	0.024	0.01	0.001	0.96
Worker age 55-64	0.018	0.01	0.008	0.65
Worker age 65+	base	.	base	.
Worker age <25	0.059	0.01	0.033	0.04
Worker Male	-0.019	0.00	-0.018	0.00
Worker occupation dummies	Included		Included	
Constant	0.223	0.23	0.132	0.00
N	93193		45469	
R-squared	0.047		0.06	

Source: Frontier analysis of ONS data (BSD and ASHE)

Sector-level and region-level fixed effects are also included. These capture the average performance of being in each sector or region, so the effect of being assigned to the treatment group comes on top of any variation that exists at the sector-region level. This means that the identification of the treatment effect rests on differences *within* groups, e.g. by comparing a high-bite food manufacturing in the North West with a low-bite food manufacturing in the North West, high-bite retail in the South East with low-bite, etc. The sector and region effects therefore absorb a lot of variation in overall minimum wage exposure, yet we continue to see effects even once this is stripped out.[14] Of course, there will be many other differences between minimum wage and comparator firms within the same sector and region that go beyond the modelled characteristics, such as product positioning and operating model, and these will also affect performance. In other words, firms are not high-bite by chance.

[14] We also explored using dummies for Travel To Work Area alongside sector dummies, as well as sector-region interaction dummies. These gave very similar results but were cumbersome in terms of computer run-time, so do not focus on those approaches.

3.3.2 Further results

Results for other outcome variables

Using the same set of control variables, we now look at a number of alternative outcome variables.

Results for survival are shown in Figure 11. This shows that, survival is higher on average among larger, more productive firms, and if they are older. Interestingly, minimum wage firms have higher survival, by around 2 percentage points. In theory, we would expect minimum wage rises to negatively impact survival of exposed firms. In a separate analysis, disaggregating results by sector, it is noted that the survival result was largely driven by the food and drink sector. However, it is not obvious that these are necessarily 'positive' impacts, as survival rates should interpreted alongside start-up rates.[15] Aside from net changes, the survival could also affect churn rates. This is explored separately in analysis using aggregated data.

Figure 11 Base regression results for survival, 2018 against 2015

	(1) All firms		(2) Low-pay	
	Beta	**p-value**	**Beta**	**p-value**
Foreign-owned	-0.084	0.00	-0.099	0.00
Firm age	0.001	0.00	0.000	0.93
Rural	-0.007	0.04	0.013	0.02
Treatment	0.021	0.00	0.018	0.00
Log employment 2015	0.039	0.00	0.042	0.00
Log turnover per employee 2015	0.018	0.00	0.042	0.00
Sector dummies	Included		Included	
Region dummies	Included		Included	
New firm	-0.049	0.00	-0.036	0.00
Worker age 25-34	-0.013	0.08	0.008	0.48
Worker age 35-44	-0.020	0.01	-0.002	0.86
Worker age 45-54	-0.016	0.04	0.005	0.64
Worker age 55-64	-0.006	0.46	0.014	0.24
Worker age 65+	Base		Base	
Worker age <25	-0.036	0.00	-0.025	0.03
Worker Male	-0.005	0.07	-0.006	0.14
Worker occupation dummies	Included		Included	
Constant	0.695	0.00	0.625	0.00
N	115820		58200	
R-squared	0.10		0.12	

Source: Frontier analysis of ONS data (BSD and ASHE)

[15] In this context it not straightforward to derive a 'comparator' for start-up rates, as we have no prior characteristics (ASHE assignment) on which to condition.

Next we look at change in turnover per employee as an outcome.

This shows that turnover per employee fell in high-bite firms by around 3 percentage points relative to others. This is contrary to the hypothesis that minimum wage increases cause substitution towards capital and skilled labour, which result in positive impacts on turnover per employee. One possible explanation would be adverse shocks disproportionately affecting high-bite firms during the period of interest. For example, it is reasonable to suppose that adverse conditions would place downward pressure on wages, as well as weaker performance in the following period.

In any case, it is worth noting some important limitations with the turnover data. Most importantly, the data is collated at the enterprise rather than local unit level; this means we cannot see what is happening within multi-unit enterprises, which account for a large portion of the sample.[16] There are also concerns around the timeliness of the data. Finally, turnover exhibits wider fluctuation than employment, which gives greater scope for outlying observations to affect the results.

[16] The approach we have taken is to allocate turnover to local units on the basis of headcount, i.e. assume turnover per employee is constant across the different locations.

Figure 12 Base regression results for turnover per employee, 2017 against 2015

	(1) All firms		(2) Low-pay	
	Beta	p-value	Beta	p-value
Foreign-owned	0.057	0.00	0.045	0.00
Firm age	0.001	0.00	-0.001	0.00
Rural	0.010	0.05	0.003	0.56
Treatment	-0.028	0.00	-0.035	0.00
Log employment 2015	0.007	0.00	0.019	0.00
Log turnover per employee 2015	-0.163	0.00	-0.199	0.00
Sector dummies	Included		Included	
Region dummies	Included		Included	
New firm	0.017	0.00	0.003	0.52
Worker age 25-34	0.051	0.00	0.029	0.00
Worker age 35-44	0.035	0.00	0.018	0.08
Worker age 45-54	0.033	0.00	0.013	0.23
Worker age 55-64	0.004	0.65	0.000	0.97
Worker age 65+	Base		Base	
Worker age <25	0.011	0.21	-0.013	0.20
Worker Male	0.017	0.00	0.037	0.00
Worker occupation dummies	Included		Included	
Constant	0.737	0.00	0.714	0.00
N	101900		50029	
R-squared	0.07		0.10	

Source: Frontier analysis of ONS data (BSD and ASHE)

Results for different types of business

Returning to the core ‘change in employment’ outcome, we now explore how the outcome varies along several dimensions of business type.

- **Corporate structure**. This is a classification of whether the enterprise is a subsidiary of a larger organisation, and how many enterprises are owned under that umbrella.[17] Looking at the ‘all sectors’ results, effects are generally larger in groups with more enterprises in them. The hypothesis would be that larger conglomerates place subsidiaries under more stringent requirements, with tighter staffing responses.
- **Local unit structure**. This counts the number of local units within the enterprise. Across both runs (1) and (2), it appears that there is more response in large chains. Again, this could be related to intra-firm performance management. It is also worth noting that many retail and food service units, which we shall see are the sectors driving much of the result, are in large chains.

[17] This is analysed using the ‘who owns who’ code in the BSD.

- **Workplace size.** Workplaces are banded according to the number of employees. In both runs (1) and (2) we see that the employment effect appears to be driven by the workplaces with between 1 and 9 employees, with little effect from those with 50+ employees. On the one hand this is counterintuitive, in the sense that one might expect the smaller workplaces to have less leeway to adjust staffing levels downwards. However, any downward adjustment by this group will result in a much larger reduction in the proportional sense.

Figure 13 Treatment for effects for different sample cuts

Sample cuts	(1) All sectors Beta	(1) All sectors p-value	(2) Low-pay Beta	(2) Low-pay p-val
Corporate structure				
Single enterprise	-0.008	0.24	0.003	0.77
Conglomerate 2-9	-0.016	0.03	-0.021	0.01
Conglomerate 10+	-0.048	0.00	-0.037	0.00
Local unit structure				
Single local unit	-0.027	0.00	-0.019	0.11
2-9 units in enterprise	0.027	0.07	0.030	0.08
10-99 units in enterprise	0.004	0.73	-0.003	0.85
100+ units in enterprise	-0.061	0.00	-0.046	0.00
Workplace size				
1-9 employees	-0.052	0.00	-0.038	0.00
10-49 employees	-0.010	0.05	-0.010	0.10
50+ employees	-0.015	0.14	0.003	0.80

Source: Frontier analysis of ONS data (BSD and ASHE)

Results for different sectors

We now break down the general regression by sector to see which are driving the results. This is done by repeating the regression, and focusing on each 2-digit SIC code in turn.

Retail and food service, the two largest low-paying sectors, both show statistically significant negative effects on employment. The third largest sector (education) also shows a negative impact that is just above the 10% significance level. In other cases, we see impacts that are statistically significant or positive and in some cases significant.

It should be noted that sample sizes get smaller, particularly for some sectors. Given concerns around measurement error in the ASHE assignment variable, this level of disaggregation may be excessive and produce spurious results if the sample sizes get too small. It may also reflect sectors having different mechanisms for adjustment. For example, retail has scope to use self-scan scan tills, or food service to use more technologically intensive ordering systems. Not all sectors will have similar ways to cope with rising labour costs.

In terms of survival, most of the impacts are positive and are significant in the case of food service, cleaning, and social care. A mix of results are observed for turnover, with some sectors showing positive impacts and other showing negative.

Figure 14 Treatment effects for main sectors

Sector	Employment change		Survival		Turnover per employee		Sample size
	Beta	p-value	Beta	p-value	Beta	p-value	
Retail	-0.043	0.00	-0.004	0.49	-0.014	0.00	23137
Accommodation	0.046	0.14	0.031	0.21	0.013	0.62	1305
Food service	-0.031	0.01	0.038	0.00	0.001	0.82	11519
Employment agency	0.127	0.06	0.024	0.34	-0.091	0.03	1407
Cleaning	0.016	0.71	0.179	0.00	-0.313	0.00	2219
Education	-0.018	0.12	-0.010	0.29	0.126	0.00	16145
Health	0.056	0.08	0.027	0.19	-0.085	0.00	7528
Residential care	0.032	0.04	-0.004	0.71	0.130	0.00	4743
Social / child care	0.004	0.91	0.051	0.01	0.075	0.09	3215

Source: Frontier analysis of ONS data (BSD and ASHE)

Detailed employment impacts

As noted earlier, the employment effects are strongest in relation to smaller workplaces, with less discernible impact along the larger size bands. One reason is that a downward adjustment in employment in a small workplace will be large in proportionate terms compared with if there are 50+ employees. The concern is that the logarithmic specification we use masks adjustments in larger workplaces that are small in proportionate terms.

We explore this further by using alternative outcome variables to understand the nature of size effects:

- *Linear change in employment*. For surviving firms, this is the difference between 2018 and 2015 employment. The effects are insignificant across specifications. The reason for using the logarithmic specification is that it gives the control variables a proportionate effect, e.g. for employment in retail to grow by 1% more than other sectors. But in the case of the linear dependent variable, this imposes the same increment in employment, regardless of starting size, whether the workplace be a large supermarket or a small store. This is particularly problematic in the context of using many control variables and such a diverse range of businesses in the dataset.
- *Binary variable for whether employment fell by at least 1 from 2018 against 2015*. The advantage of this variable is that it captures downward adjustment across the range of firm sizes, but does not fall into the same problems as seen with the linear dependent variable. We see that the effect is similar across size bands. Controlling for other firm characteristics, being high-bite adds around 3.7 percentage points to the likelihood of downward adjustment. But obviously a 1-employee reduction is much larger in proportionate terms when it occurs in a small workplace of 10, compared with occurring in a workplace of 50.
- *Binary variables for whether employment fell or increased more than 10% of 2015 levels*. These variables aim to pick up material changes in employment

regardless of size band and shows statistically significant effects in each of them. We see that the overall effect comes from both increased probability of contraction and decreased probability of growth.[18] The fact that large places show significant effects with this variable but not with change in log employment is perhaps puzzling. Obviously, because the binary variable is so attenuated, it removes the influence of outlying observations, which may affect the results.

Figure 15 Alternative employment change outcome variables by size band

	All sizes	1-9 employees	10-49 employees	50+ employees
All sectors				
Log employment change	-0.029***	-0.052***	-0.010**	-0.015
Linear employment change	0.006	-0.49	0.722	-3.24
Lost 1 or more employees	0.037***	0.039***	0.037***	0.034***
Lost 10% employees	0.032***	0.033***	0.028***	0.036***
Added 10% employees	-0.039***	-0.06***	-0.034***	-0.015*
Low-pay focus				
Log employment change	-0.021***	-0.038***	-0.010*	0.003
Linear employment change	0.597	-0.626	1.075	-2.017
Lost 1 or more employees	0.027***	0.018*	0.029***	0.029**
Lost 10% employees	0.018***	0.013	0.015*	0.028**
Added 10% employees	-0.027***	-0.032***	-0.034***	-0.007

Source: Frontier analysis of ONS data (ASHE and BSD)

3.3.3 Robustness tests

We now undertake several tests to assess the robustness of our results.

Repeating analysis over multiple years

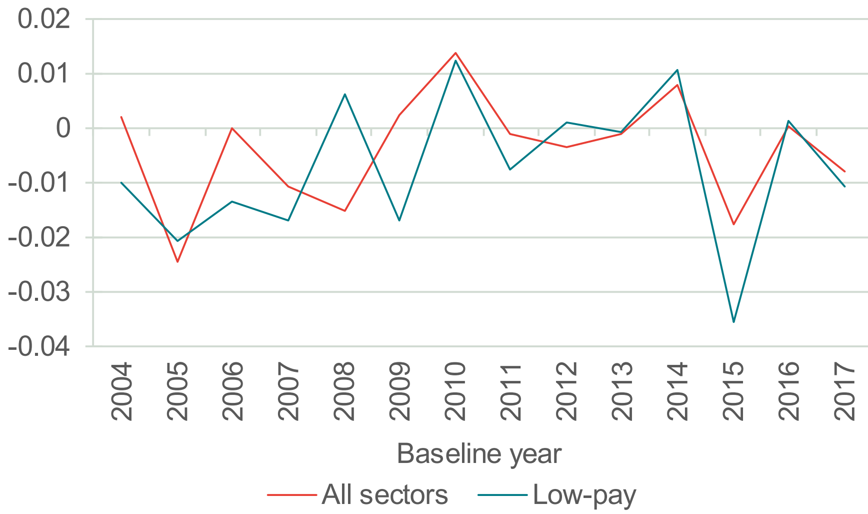
We explore whether similar results are obtained if using a different time period to analyse the outcomes. In proportional terms, the introduction of the NLW was larger than other increases, so would be expected to have a bigger impact than for years where the increase is smaller. But if we find big impacts in years where the increases are small, then this would suggest the model is prone to finding 'false positives', which may reflect the large sample sizes. This is a form of falsification

[18] We also explored binary variables for changes in excess of 50%, but these showed very little effect.

test. We explore this by looking at results for different baseline years, starting at 2004.[19] We first look at year-on-year changes in log employment.[20]

To set the scene, we begin by looking at raw differences, i.e. average employment growth in the treatment growth minus average employment growth in the control group. The chart below shows each baseline year along the horizontal axis, and the vertical axis shows the difference in year-on-year employment growth between treatment and control groups. For example, along the axis at 2015, we are looking at change in employment at 2016 relative to 2015. We see that the treatment group grew less than the control group, hence the negative raw difference (Here we should focus on the 'low-pay' series, as 'all sectors' will include many parts of the economy facing different economic pressures to low-pay sectors).[21] Aside from the 2015 dip, some other years also show negative impacts, in particular 2005.

Figure 16 Raw differences between treatment and control groups, change in log employment 1 year after baseline



Source: Frontier analysis of ONS data (BSD and ASHE)

Moving on from raw differences, we add in all the controls used in the regression and look at how the treatment effect varies over time. The red line shows the coefficient, with the other lines showing the 10% confidence intervals. We see that in most years the treatment effect is negative, but not always statistically significant.

The largest impacts are for the 2005, 2009 and 2015 baseline years. In terms of interpretation, the 2015 result is consistent with the hypothesis that the NLW affected low-pay firms relative to others. The result for 2009 is presumably driven by the global financial crisis, and implies that minimum wage firms were more

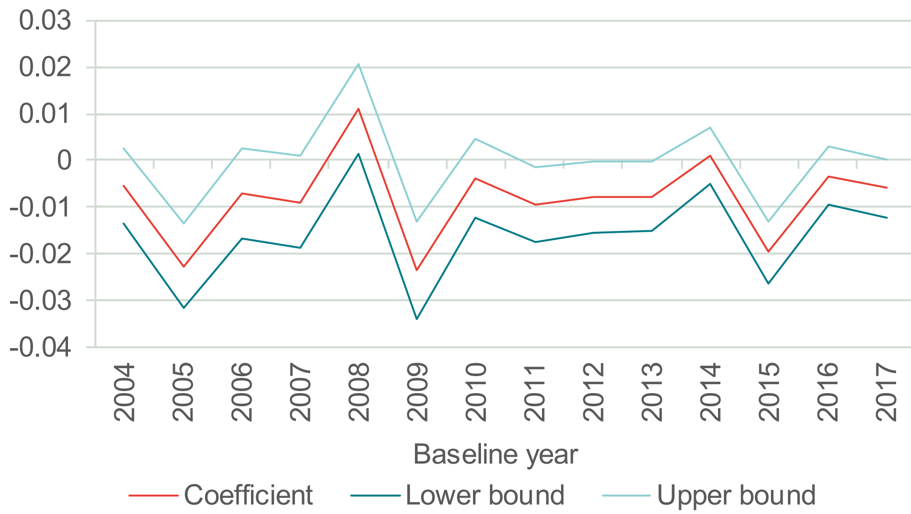
[19] While ideally we would go back to 1998, the choice of time period reflects the considerable task of combining BSD and ASHE data over many multiple years. The changes in various lookups and hierarchies used in the analysis (such as output area, SIC and SOC codes) and differences in how they are coded and defined over time, raises problems for constructing a master dataset. This means data quality and may vary over time.

[20] Note that PAYE employment is not available pre-2013. We are therefore unable to construct our preferred employment measure over the time period, so for consistency use the 'headline' employment measure. It is therefore worth bearing in mind the caveats around timeliness of BSD data.

[21] While much of this is controlled for in a regression context (e.g. with sector dummies), this is not the case for raw differences.

affected than others (mean reversion may also drive the positive impacts for 2008 and 2010). The reason for the 2005 result is less clear, as the minimum wage increase in that period was relatively moderate (£4.85 to £5.05, i.e. 4.1%).[22]

Figure 17 Treatment effects on change in log employment for multiple years, 1 year after



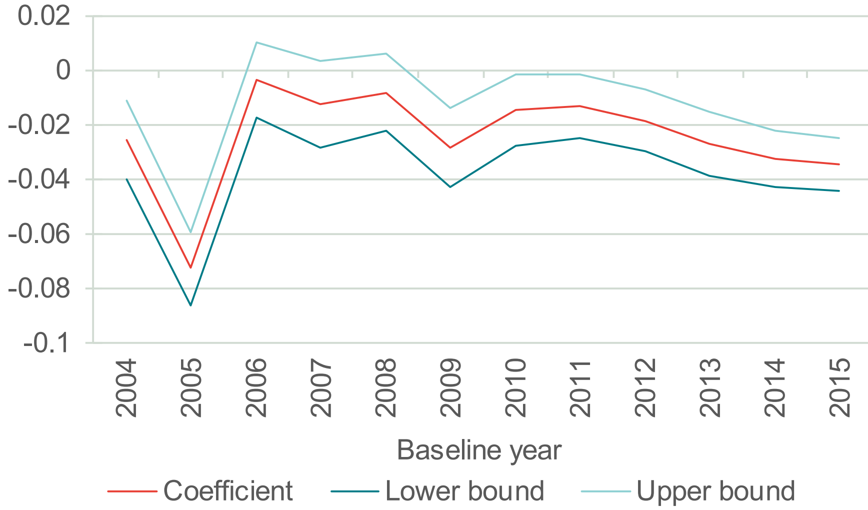
Source: Frontier analysis of ONS data (BSD and ASHE)

Regression covering all sectors. Low-pay specific version gives similar results

For consistency with our main results, we do a similar exercise using a 3-year outcome horizon. For example, with a 2015 baseline, we compare 2018 and 2015 employment. Here we see even larger treatment effects in 2005, and 2009 continues to show as a negative kink. From baseline years of 2012 onwards we see negative treatment effects. Note that as we extend the horizon, the smoothing means we capture impact in later years. So the 2013 3-year impacts will include 2013/14, 2014/15, and 2015/16. As a result the baseline years that catch 2015/16 incorporate the 2015/16 negative impact.

[22] Although at that time there was a small downturn particularly affecting retail, it is unclear why this would produce such bigger coefficients for the 2005 baseline year.

Figure 18 Treatment effects on change in log employment for multiple years, 3 years after



Source: Frontier analysis of ONS data

Regression covering all sectors. Low-pay specific version gives similar results

In terms of explaining these results, it should be acknowledged that the difference-in-difference procedure only controls for observable characteristics. Treatment and control groups will clearly differ in other respects not captured in these variables, and may face different commercial, technological and cost pressures, such that they may be affected differently. While it is apparent that employment growth has been weaker in the treatment group, the fact this is observed in multiple years could point to wider changes affecting these firms that are not controlled for in the regression. There are a number of mechanisms by which this could happen.

There are technologies that substitute capital for labour, for example self-service checkouts in retail and automated food ordering. It is plausible that may be more acceptable and hence rolled out more extensively in low-pay settings. The availability and diffusion of these technologies will vary by sector, with opportunities to use them present in some but not others. Note that the sectors for which the econometric results are strongest are retail and food service, and are sectors for which the introduction of these technologies if most apparent.

Another hypothesis is that the decision to pay low wages is correlated with adverse economic conditions, so that the firms already under financial pressure cannot afford to pay more. This may induce them to downscale their activities. It is also worth recalling that the treatment group is generally observed to have weaker turnover growth prior to the baseline year, which is also consistent with this hypothesis.

Moving on from estimating the same regression year by year, an alternative is to pool the data over years, so that each observation is a firm in a year and we look at changes in employment x years hence. In the first instance, we estimate a partially interacted model, with common parameters on the firm and worker-level controls, year dummies, and we estimate separate treatment effects for each baseline year. This is like the year-on-year analysis, but constrains the other coefficients (e.g. sector dummy, region, firm size) not to vary over time, with the hope that this avoids overfitting. In fact, this makes very little difference to the overall results, which are very similar to when estimated in separate yearly regressions.

A further refinement to this approach is, instead of estimating a separate treatment effect for each year, to include a linear term measuring the size of the minimum wage increase. This is to address the question of whether large minimum wage increases have bigger impacts than smaller increases. The *percent_increase* variable is zero for non-minimum wage firms and set equal to the size of increase in statutory rates if the firm pays below the incoming minimum wage.

This can be written as follows:

$$\begin{aligned} Log\left(employment_{i,g,s,t+z}\right) - Log\left(employment_{i,g,s,t}\right) \\ = \beta_1 + \beta_2 X_{it} + \beta_3 W_{at} + \beta_4 Sector_S + \beta_5 Region_g + \beta_6 Minwage_a \\ + \beta_7 Year_t + \beta_8 Percent\_increase_t + u_i \end{aligned}$$

for sampled worker a in firm i in region g and sector s in year t, with outcome z years later

It is like the main specification, but instead of a treatment effect we have the following parameters:

- β_6 measures the average difference between low-pay and other firms in any year, regardless of the size of any minimum wage uprating. This absorbs the general ‘background noise’ pressures that these minimum wage firms face over time relative to others.
- β_7 captures contemporaneous shocks that are common to all firms, so for example capturing the effects of the 2009 global financial crisis.
- β_8 measures the effect of the size of uprating, conditional on a firm being a minimum wage one. This is measuring whether minimum wage firms suffer more impact in years when the uprating is larger.

The results are shown in Figure 19 below. The control variables have broadly similar values as in the base specification. In particular, new firms are more likely to grow, larger firms grow less (because they are already large), and younger workers and higher turnover per employee predict future growth. The coefficient of most interest is the percentage increase in the minimum wage. The results imply that a 10% minimum wage increase is associated with a roughly 1% decrease in employment for minimum wage firms in the all-sector specification, and 2% in the low-pay specification. However, this effect is statistically insignificant.[23]

[23] We cluster standard errors by year, which is appropriate given the contemporaneous shocks, e.g. financial crisis affecting all sectors. The clustering accounts for the fact that although we have a very large sample size in terms of firms, the size of increase term is being estimated by comparing the magnitude of effect across different years, so in this sense the sample size is smaller.

Figure 19 Multi-year regression results, change in employment 1 year after baseline

	(1) All firms		(2) Low-pay	
	Beta	p-value	Beta	p-value
Foreign-owned	0.000	0.908	-0.005	0.083
Firm age	-0.002	0.000	-0.001	0.000
Treatment dummy	-0.003	0.513	0.008	0.226
% minimum wage increase in minimum wage firms	-0.110	0.251	-0.217	0.148
Log employment 2015	-0.045	0.000	-0.043	0.000
Log turnover per employee 2015	0.052	0.000	0.057	0.000
Year dummies	Included		Included	
Sector dummies	Included		Included	
Region dummies	Included		Included	
New firm	0.061	0.000	0.064	0.000
Worker age 25-34	0.030	0.000	0.016	0.000
Worker age 35-44	0.016	0.000	0.012	0.002
Worker age 45-54	0.008	0.001	0.007	0.073
Worker age 55-64	0.004	0.106	0.001	0.770
Worker age 65+	Base		Base	
Worker age <25	0.042	0.000	0.027	0.000
Worker Male	-0.001	0.249	-0.001	0.595
Worker occupation dummies	Included		Included	
Constant	-0.079	0.996	-0.286	0.000
N	704248		280753	
R-squared	0.052		0.051	

Source: Frontier analysis of ONS data (BSD and ASHE)

The same analysis has been repeated for the other outcomes and included in Annex B. To summarise briefly:

- *Survival.* Survival rates are higher overall amongst minimum wage firms. This is seen in most years, and consistent with a hypothesis of lower churn. We also see that larger minimum wage increases reduce survival, but again the effect is statistically insignificant due to clustering.
- *Turnover per employee.* Turnover growth is weaker in minimum wage firms, and this is observed in all years, perhaps indicating that continuing adverse economic conditions are correlated with lower pay at baseline and weaker growth after.

Aggregation to sector-location level

As noted earlier, the ASHE firm-level assignment variable will suffer measurement error, as the individual sampled in ASHE may not be representative of wider pay in the firm or its dependence on minimum wage labour. Although this is mitigated by adding controls for worker-level characteristics and restricting sample to low-pay occupations and sectors, as a cross-check we develop an alternative approach aggregating data over location and sector. The aim is that sampling error is ‘evened out’ by having a greater number of ASHE observations in each unit (sector-location).

The aggregated approach also lets us explore the effect on business start-ups alongside exits, which is important for placing the survival analysis in context.

The model is written as follows:

$$Log(employment_{g,s,2018}) - Log(employment_{g,s,2015}) = \beta_1 + \beta_{2s}Sector_s + \beta_{3g}Region_g + \beta_4 Percent\_minwage_{g,s} + u_{g,s}$$

for region g and sector s

Note that the identification strategy used here is quite different from the firm-level analysis. In the firm-level analysis we used sector and region fixed effects, which absorb all variation at that level, so any effect of the treatment variable comes from differences in outcomes between high and low bite firms that are in the same sector and region, i.e. a high-bite food manufacturing business in the midlands with a low-bite one.

By contrast, the aggregated approach identifies the effect by comparing higher-bite and lower-bite sector-regions, estimating the effect of minimum wage bite above and beyond sector-level and region-level fixed effects. For example, this means comparing North East restaurants with South East retail, controlling for the fact that North East grew slower than South East and restaurants grew quicker than retail (stylised example).

The key question with implementing this approach regards the level of aggregation to use. If the level of aggregation is too fine, the dataset will contain some very idiosyncratic units based on small numbers of observations in ASHE, which give scope for outliers to distort analysis. And whereas the firm-level analysis has the richness of using worker-level and firm-level controls, this is lost in the aggregated version. But at too broad a level, there will not be enough data to pick apart minimum wage effects from sector and location effects, which are highly correlated with each other. The choice of aggregation is as follows:

- **Sector**. We consider 2-digit SIC to be reasonable, as it is at this level at which the largest low-pay sectors are generally defined. Although there is scope to manually split out the larger sectors further, e.g. sub-sectors of retail and food service, we consider that this is not warranted, on the basis that they use similar labour.
- **Location.** We considered using either region or Travel To Work Area (TTWA of which there are more than 400). There is clearly much attraction in using TTWA, as they correspond most closely to local labour markets. However, aggregation to TTWA results in many small observational units, with a median ASHE count[24] of 3 or 4. This is unlikely to sufficiently mitigate sampling error to

[24] By ‘ASHE count’ we mean the number of workplaces in ASHE that contribute towards the sector-location measure of minimum wage exposure.

give a valid cross-check. On this basis, we focus on region, which gives median ASHE counts of 30 (across all sectors) or 120 (for minimum wage sectors).

A further question to consider is around weighting. Even with aggregation to sector-region level, there will be still be some very small units that are prone to unduly affect the analysis and add noise, as small cells are more prone to measurement error. This is particularly the case in the 'all-sector' approach. The rationale for weighting is that it will reduce the effect of these units. Classical measurement error assumes that the size of the error is uncorrelated with the level of the X variable (in this case minimum wage exposure). However, we find that cell size is negatively correlated with the minimum wage, which means that this is no longer classical measurement error and the bias is not necessarily attenuated to zero.

The results are shown in Figure 20 below. To interpret the coefficients, the first row shows that a 10% increase in the proportion of firms classified as high-bite within the sector-region is associated with an employment change of 0.1 x -0.08 = -0.008 = -0.8%. The birth rate and death rate results show the percentage point change in these rates associated with a change in the proportion of high-bite firms on the sector-region.

Figure 20 Coefficient on minimum wage bite variable in sector-region aggregated analysis

Outcome variable	Specification	Coefficient	P-value
Change in log employment	MW sectors, unweighted	-0.08	0.14
	MW sectors, weighted	-0.04	0.46
	All sectors, weighted	-0.08	0.21
Birth rate (start-ups from 2018 vs 2015 as a percentage of firms present in 2015)	MW sectors, unweighted	-0.19	0.07
	MW sectors, weighted	-0.08	0.20
	All sectors, weighted	-0.06	0.30
Death rate (exits from 2018 vs 2015 as a percentage of firms present in 2015)	MW sectors, unweighted	-0.07	0.09
	MW sectors, weighted	-0.07	0.20
	All sectors, weighted	-0.06	0.16
Change in log turnover per employee	MW sectors, unweighted	-0.06	0.49
	MW sectors, weighted	-0.04	0.70
	All sectors, weighted	-0.01	0.88

Source: Frontier analysis of ONS data (BSD and ASHE)

The coefficients show higher bite sector-regions have lower employment growth, birth rate and death rate (so consistent with the positive survival impact observed in the firm-level analysis). However, most of these effects are statistically insignificant, which reflects a high degree of correlation between the sector and region controls and the minimum wage variable.[25] In other words, when aggregated to this level, there is not enough variation in the data to reliably pick

[25] This results in a high 'Variance Inflation Factor' (VIF). The VIF measures the ratio of variance in the model to the variance when just that one variable is included. A VIF in excess of 2 or 3 may be considered problematic. The minimum wage VIFs encountered here range from 8 to 14, suggesting it is a serious problem.

apart the effect of minimum wage effects from sector and region effect. Nevertheless, the results are broadly consistent with the firm-level analysis

4 PRICE IMPACTS

The section of the report examines the relationship between minimum wages and consumer prices.

Section 4.1 sets out the data sources we use to construct dependent and independent variables for empirical analysis.

Section 4.2 describes the two analytical approaches we employ to identify the empirical relationship, as well as the sensitivity and robustness checks to be conducted.

Sections 4.3 and 4.4 present the findings of the time series and difference-in-differences approaches respectively, including descriptive statistics.

4.1 Data

This study draws on evidence from four datasets:

- ONS Consumer Price Inflation indexes by 'item' and month, published on the ONS website.[26]
- Annual Survey of Hours and Earnings (ASHE) microdata on a sample of hourly wages by sector, accessed from the ONS Secure Research Service.
- Annual Business Survey (ABS) aggregated data on turnover, gross value added and employment costs by sector, published on the ONS website.[27]
- Low Pay Commission information on the adult minimum wage over time, published in the 2019 report '20 years of the National Minimum Wage'.[28]

This section describes how these sources were used to construct the dependent and independent variables for the empirical analysis.

Dependent Variables

First, we construct a measure of **monthly and year-on-year price inflation**. The unit of analysis is 'items'; the product categories used by the ONS to construct the consumer price index. Items are one level of granularity below sub-classes in the United Nations' Classification of Individual Consumption by Purpose (COICOP). For example, 'Minicab fare for 2 miles', 'Hair gel 150-200ml', 'Electrician daytime rate per hour'. Items can be aggregated into sub-classes, classes, groups and divisions.

We extract monthly item indexes from January 2005 to January 2020 inclusive for around 1,100 items, and chain link the observations to construct a consistent price index with a base of 100 in 2015.

[26] ONS, 'Consumer price inflation item indices and price quotes', accessed 1 March 2020, https://www.ons.gov.uk/economy/inflationandpriceindices/datasets/consumerpriceindicescpiandretailpricesindexrpiitemindicesandpricequotes

[27] ONS (2019), 'Annual Business Survey: Non-financial business economy UK, 2018 provisional results', https://www.ons.gov.uk/releases/nonfinancialbusinesseconomyukannualbusinesssurvey2018provisionalresults

[28] Low Pay Commission (2019), '20 years of the National Minimum Wage: A history of the UK minimum wage and its effects'

Independent Variables

Next, we use data from **ASHE**, **ABS** and the **LPC** to identify sectors that are more or less likely to be exposed to an increase in the minimum wage, informed by our theoretical model which suggests that the price response to a minimum wage increase should be proportional to the share of minimum wage labour costs in total costs (see equation 11, Section 2).

First, sectors in which a large number of employees are paid at or just above the prevailing minimum wage are more likely to face an increase in input costs if the minimum wage is increased, and more likely to increase prices.

To account for this, we extract employee-level data from ASHE and match this with Low Pay Commission's timeseries data on the minimum wages applicable to workers aged 25 and over to construct a measure of **the share of workers earnings less than or equal to the incoming minimum wage** in each sector. For example, an employee earning £7.00 per hour in January 2016 would be considered earning below the incoming minimum wage of £7.20. We pool over all observations between January 2015 and December 2018 to ensure a sufficient sample and exclude sectors for which there were fewer than 10 observations.[29] Of those remaining, the median sector has 215 wage observations, with only a handful of sectors having fewer than 30 observations, suggesting that the pooled sample size is sufficient to draw inferences about the share of minimum wage workers at the sector level. Note that this approach implicitly assumes that the exposure of a sector to minimum wages is constant over time.

Second, sectors for which employment costs constitute a high share of total costs are also more likely to be affected by an increase in the minimum wage.

To account for this, we construct a measure of **employment costs as a share of total turnover** at the four-digit sector level using published ABS aggregates.[30] For example, in 2016, SIC 45.20 (Maintenance and repair of motor vehicles) employment costs and turnover were £4.2 billion and £24.1 billion respectively, implying an employment cost share of 17.6%. Again, we take the average cost share for 2015 through 2018.

Finally, we take the product of these two values to construct a measure of how 'exposed' each sector is to an increase in the minimum wage. This measure is a proxy for the share of minimum wage labour costs in total costs; the s_i term in equation 11, Section 2.

$$\frac{L_m}{L_m+L_h} \times \frac{L_m w_m + L_h w_h}{Revenue} \approx \frac{l_m w_m}{MC_i} = s_i$$

where L_m and L_h is the total quantity of minimum-wage and high-wage labour employed at wages w_m and w_h respectively; and l_m is the quantity of minimum-wage labour required to product one product.

It is an imperfect proxy for three reasons:

[29] 42 of the 700 sectors were suppressed because of small sample size.

[30] Where aggregates are not available at the four-digit level, we revert to three-digit or two-digit level aggregates accordingly.

- The first term is based on the share of workers earning less than or equal to the incoming minimum wage in a sector, rather than the share of the sector/region's wage bill attributable to workers earning less than or equal to the incoming minimum wage. We expect that the ratio between the share of workers and the share of wages to be relatively consistent between sectors and leave this refinement for future work.
- The second term is calculated as a share of turnover, rather than as a share of total costs, because of limitations in published ABS data. Given mark-up is constant assumes constant return to scale production functions, revenue is proportional to marginal cost. The rationale for choosing revenue is discussed further in Box 1.
- The measure does not account for the share of the product market that is not required to pay the minimum wages: notably overseas firms. This limitation is addressed by excluding tradable items from the sample, discussed in the following section.

By constructing an ordinal ranking of this measure, we are able to identify sectors that are more likely to be affected by an increase in the minimum wage, and those that are less likely.

BOX 1: TURNOVER OR GVA?

The theoretical model in Section 2 suggests that firms' response to a minimum wage increase is to raise prices by the product of the percentage increase in minimum wages and the share of minimum wage labour costs in total costs for that product. The ideal denominator at the firm level is therefore total costs. ABS gives us a choice of two proxies: turnover or GVA.

If a firm's GVA accounts for most of its turnover, for example cleaning, care work and professional services, both $\frac{L_m w_m}{GVA}$ and $\frac{L_m w_m}{Revenue}$ are good predictors of a firm's price response to a minimum wage increase $\frac{\partial p_i}{\partial w_m}\frac{w_m}{p_i}$.

But if a firm's GVA is small relative to turnover, for example in wholesaling, both measures are flawed:

- Using $\frac{L_m w_m}{GVA}$ assumes that the share of minimum wage labour costs on GVA is identical for the producers of non-labour inputs.
- Using $\frac{L_m w_m}{Revenue}$ assume that minimum wage labour is not required to produce any of the non-labour inputs to the product.

Without correspondence tables identifying the combination of sectors that are collectively responsible for producing, distributing and/or retailing a given item, we are unable to test this assumption. However, for the set of sector/regions that are most exposed and have substantial non-employment costs (mostly food services), it is reasonable to assume that most of these inputs are tradable and therefore not-exposed to minimum wage increases. For this reason, we calculate exposure using minimum wage labour costs as a share of turnover in our core specification, but test minimum wage labour costs as a share of GVA as a sensitivity.

Figure 21 presents the twenty sectors with the highest exposure to a minimum wage rise, along with share of employees that are low-paid and employment cost share. Note that we remove all 'Libraries, archives, museums and other cultural activities' (SIC 91) for which turnover is not a reliable proxy for total costs.

The most exposed sectors are those related to cleaning services, the provision of care and the preparation and service of food and drink. This is broadly consistent with the sectors that Wadsworth (2010) found to be most exposed in 1998/99 and 2004/05, with one notable exception: we do not find that 'Taxi operation' rank in the 20 most exposed sectors while Wadsworth (2010) ranks it in the top ten. The differences reflects different approaches to constructing the labour-cost share variable: we use employment costs as a share of turnover while Wadsworth (2010) uses employment costs as a share of value added (profit before interest plus wage bill).

For robustness, we construct two alternative measures of minimum wage exposure:

- First, using an alternative measure of labour-cost share; dividing employment costs by approximate gross value added (GVA), rather than turnover.
- Second, limiting the ASHE sample to employees aged 25 and over. Younger employees face lower minimum wages (although age thresholds vary over time). Excluding young employees reduces the risk that the share of low-paid employees is overstated for certain sectors.

The ranking of the most exposed 20 sectors is not particularly sensitive to the choice of measure.

- Using GVA as the denominator for the labour cost share measures results in a number of retail and food services sectors moving into the top 100, at the expense of social care and cleaning sectors. This reflects the fact that GVA constitutes a smaller share turnover in retail and food services sectors than in social care and cleaning, employment costs constitute a relatively greater share of GVA.
- Using only data for employees aged 25 and over results in some sectors dropping out because there are too few observations in ASHE.

Figure 21 List of sectors highly exposed to increases in the minimum wage

#	SIC	Sector description	Low-paid share	Labour-cost share	Exposure measure
1	81.21	General cleaning of buildings	0.56	0.61	0.34
2	88.91	Child day-care activities	0.45	0.61	0.27
3	85.10	Pre-primary education	0.37	0.62	0.23
4	87.10	Residential nursing care facilities	0.40	0.55	0.22
5	56.10/3	Take-away food shops and mobile food stands	0.72	0.30	0.22
6	96.01	Washing and (dry-)cleaning of textile and fur products	0.54	0.39	0.21
7	56.29	Other food services	0.42	0.45	0.19
8	96.02	Hairdressing and other beauty treatment	0.49	0.38	0.19
9	56.10	Restaurants and mobile food service	0.60	0.30	0.18
10	88.10	Social work activities without accommodation for the elderly and disabled	0.24	0.73	0.18
11	81.29/9	Other cleaning services	0.48	0.36	0.18
12	47.24	Retail sale of bread, cakes, flour confectionery and sugar confectionery in specialised stores	0.55	0.32	0.18
13	81.22/1	Window cleaning services	0.47	0.36	0.17
14	81.22/9	Other building and industrial cleaning activities	0.47	0.36	0.17
15	93.21	Activities of amusement parks and theme parks	0.55	0.30	0.17
16	87.90	Other residential care activities n.e.c.	0.25	0.61	0.15
17	87.30	Residential care activities for the elderly and disabled	0.29	0.52	0.15
18	56.30	Beverage serving activities	0.61	0.24	0.15
19	81.22/2	Specialised cleaning services	0.39	0.36	0.14
20	86.10/2	Medical nursing home activities	0.32	0.44	0.14

Source: *Frontier Economics*

Notes: *Note that SIC 56.10 (Restaurants and mobile food service activities) is included alongside more granular sector 56.10/3 (Take-away food shops and mobile food stands). This is to facilitate mapping from 'items' that may be provided at licenced restaurants, unlicensed restaurants or take-away food shops.*

We also use LPC information on the effective nominal minimum wage faced by workers aged 25 years or over who are not apprentices to construct two additional independent variables:

- a binary variable capturing **whether the minimum wage increased** in a given month;

- a continuous variable capturing the **percentage change in the minimum wage** in a given month; and

Figure 1 shows the minimum wage faced by 25 year olds from 1999 to 2020, along with the percentage change in the minimum wage in months where it was adjusted. Note that from 2016 (the introduction of the National Living Wage), minimum wage increases took place in April, rather than October as they had in previous years.

Data linking

Finally, we link the dependent variables (measured by CPI item) to the independent variable (measured by sector) in order to construct a consistent dataset for empirical analysis.

The objective of this linking is to assign each item/region a measure of exposure to changes in the minimum wage. Ideally, this would be done using published UK or international correspondence tables identifying the combination of sectors that are collectively responsible for producing, distributing and/or retailing a given item. However, in the absence of such tables, this process is completed manually with each item being linked to the sector judged to account for the highest share of gross value added at the point of consumption. For example:

- ‘Dry cleaning-man’s suit’ is mapped to SIC 96.01 (Washing and (dry-)cleaning of textile and fur products).
- ‘Washing machine repair’ is mapped to SIC 95.22 (Repair of household appliances and home and garden equipment).
- ‘Fish & chips takeaway’ is mapped to SIC 56.10/3 (Take away food shops and mobile food stands).

Of the 1,100 items in the dataset, approximately 900 are considered tradable, in that a substantial share of the inputs to the finished product are subject to international competition. For example, ‘Canned tomatoes 390-400g’, ‘Liquid soap 200-300ml’ and ‘Home office desk’. While the retail component of these items is largely non-tradable, we would expect that firms producing the items to face competition from international firms, making it difficult for domestic firms to pass on the cost of a minimum wage increase to consumers (see Section 2). Harasztosi and Lindner (2019) demonstrate this empirically using evidence from a minimum wage shock in Hungary.

Moreover, the prices of tradable items is likely to be affected by a range of macroeconomic factors such as exchange rates and oil prices that render them an inappropriate control group for non-tradable items. For this reason they are not mapped to a particular sector/region and are excluded from the core analysis. A full correspondence table from non-tradable items to sectors is presented in Annex C.

Figure 22 describes the structure of the dataset before and after the data linking.

Figure 22: Dataset structure before and after linking

Variable type	Variable	Aggregation	Aggregation after linking
Dependent	Monthly inflation	Item/Month	Item/Month
	Year-on-year inflation	Item/Month	Item/Month
Independent – treatment	Minimum wage exposed (binary)	Sector →	Item
Independent – time	Minimum wage uplift (binary)	Month	Month
	Minimum wage uplift (continuous)	Month	Month
	After April 2016	Month	Month

Source: Frontier Economics

Importantly, constructing the dataset at the item level has implications for the interpretation of the findings. An item level dataset may overweight some items with respect to their share of economic activity (the ONS over-samples some classes where the between-item price variability is high).[31] Conversely, a sector level dataset might overweight sectors that account for a relatively smaller share of economic activity. This could be addressed in further research by applying weighting to each item or sector. In this paper, we test the sensitivity of the findings to excluding some sectors (such as food services) for which a disproportionately large number of items are surveyed.

In total:

- There are 58 non-tradable items which map to the ten most exposed sectors.
- There are a further 22 non-tradable items which map to the next ten most exposed sectors.
- There are a further 113 non-tradable items which do not map to the twenty most exposed sectors.

In our core treatment definition, treated items are those which map to the 20 most exposed sector/regions (sample=80) and control items are the set of non-tradable items which do not map to the 20 most exposed sectors (sample=113).

For robustness, we test two alternative treatment definitions.

- First, we consider only those tradeable items which map to the 10 most exposed sector as treated (sample=58). The control group is unchanged (sample=113).
- Second, we use a continuous measure of treatment, normalised such that items mapping to the most exposed sector (General cleaning of buildings) are coded as 1 and items mapping to the least exposed sector (Urban and suburban passenger railway transport) are coded as 0. Tradeable items are excluded (total sample=193).

We add these definitions to the definitions of minimum wage exposure discussed above to construct five treatment definitions.

[31] ONS (2019), 'Consumer Price Index Technical Manual'

BOX 3: TREATMENT SPECIFICATIONS

We test the sensitivity of the analysis to the definition of treated items.

1. Labour cost share is measured as employment costs over **turnover**; share of low-paid employees is measured for **all employees**; item are listed as treated if they map to one of the **20 most exposed** sectors.
2. Labour cost share is measured as employment costs over **turnover**; share of low-paid employees is measured for **all employees**; items are listed as treated if they map to one of the **10 most exposed** sectors.
3. Labour cost share is measured as employment costs over **gross value added**; share of low-paid employees is measured for **all employees**; items are listed as treated if they map to one of the **20 most exposed** sectors.
4. Labour cost share is measured as employment costs over **turnover**; share of low-paid employees is measured for **employees aged 25 and over**; items are listed as treated if they map to one of the **20 most exposed** sectors.
5. Labour cost share is measured as employment costs over **turnover**; share of low-paid employees is measured for **all employees**; items are assigned a continuous measure of treatment ∊ $(0,1)$.

4.2 Analytical approach

To estimate the impact of minimum wages on prices, we employ a panel regression approach.

Limiting the sample to treated items only, we test whether monthly inflation is higher in months during which the minimum wage was uplifted. The core specification is as follows:

$$inflation_{m,i} = \alpha + \beta_1 \times uplift_m + \gamma_i + \delta_m + inflation_{m-1,i} + \epsilon_{m,i}$$

Where:

- $inflation_{m,i}$ is the month-on-month percentage change in price index for each item/month.
- $uplift_m$ is a binary variable equal to one if the minimum wage increased in that month and zero if it did not.
- γ_i is an item fixed effect (to capture between-item variation in inflation).
- δ_m is a month fixed effect (to capture seasonality).
- $inflation_{m-1,i}$ is a lagged dependent variable (to account for autocorrelation in the inflation time series).
- Clustered standard errors (sector) are specified to account for the ONS sampling approach.

The coefficient β_1 can be interpreted as the difference between monthly inflation in minimum wage-uplift months, and monthly inflation in months where there was no minimum wage uplift. In other words: the additional impact of minimum wage increases on prices for treated items.

In addition to testing the sensitivity of our findings to using treatment definitions (see Box 3) and different control variables and error terms, we also test four alternative empirical specifications.

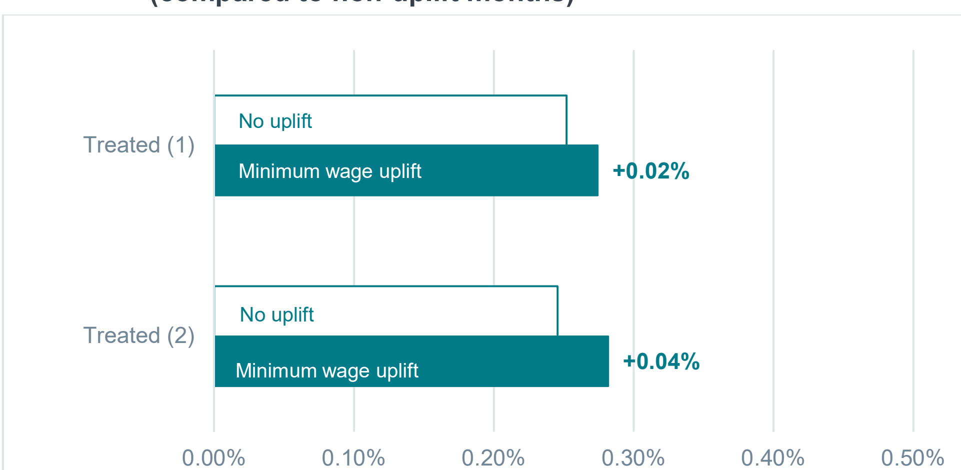
- a specification that controls for lags and leads before and after the uplift month, to account for potential anticipated or lagged responses to an increase in minimum wages;[32]
- a specification where we include both treatment and control item and test the interaction of treatment and minimum wage uplift (rather than testing the uplift effect on treated item),
- a specification where uplift is a continuous variable measuring the percentage change in minimum wage in each month (the coefficient on percentage change can be interpreted as the price elasticity of minimum wages); and
- a difference-in-differences specification to identify whether the substantial minimum wage increase in April 2016 had a different impact on treated item/region and a synthetic control group.

4.3 Findings

Descriptive statistics

Descriptively, we see that CPI inflation is indeed higher in minimum wage uplift months than in months in which there is no uplift: 0.02% higher for the core treatment definition (1). This finding is not sensitive to the choice of treatment definition: the difference between uplift and non-uplift months is higher for a more narrowly defined treatment group (2) (see Figure 23).

Figure 23 Monthly inflation in the month minimum wage uplift months (compared to non-uplift months)



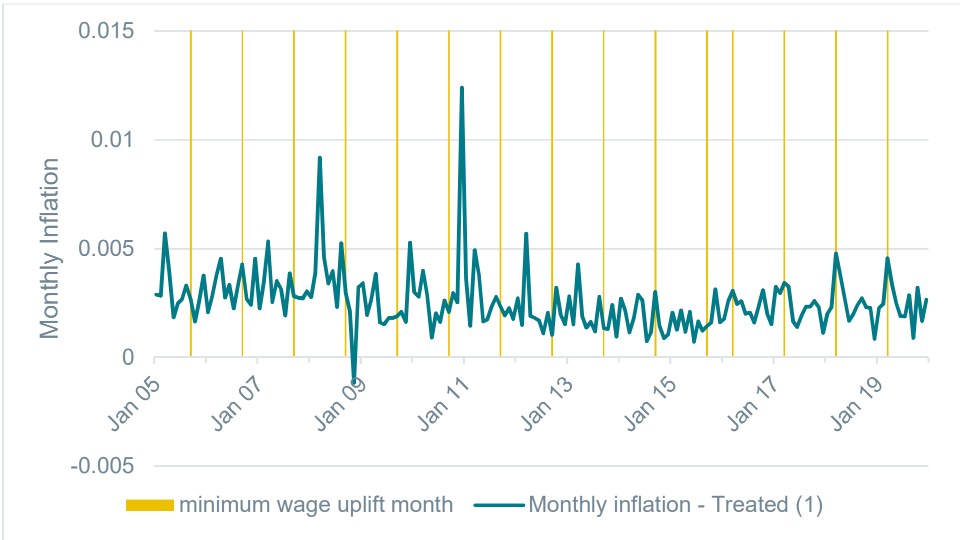
Source: Frontier Economics

[32] We also considered anticipatory effects in the month that the minimum wage uplift was announced, but found no statistically significant effect.

Note: Treated (1) refers to the core treatment definition. Treated (2) refers to the only those items that map to the 10 most exposed sectors.

If we overlay monthly CPI inflation for treated items on the months in which the minimum wage was uplifted, we observe some correlation from 2014 onwards, but limited correlation in earlier years (see Figure 24).

Figure 24 Monthly inflation over time – treated items

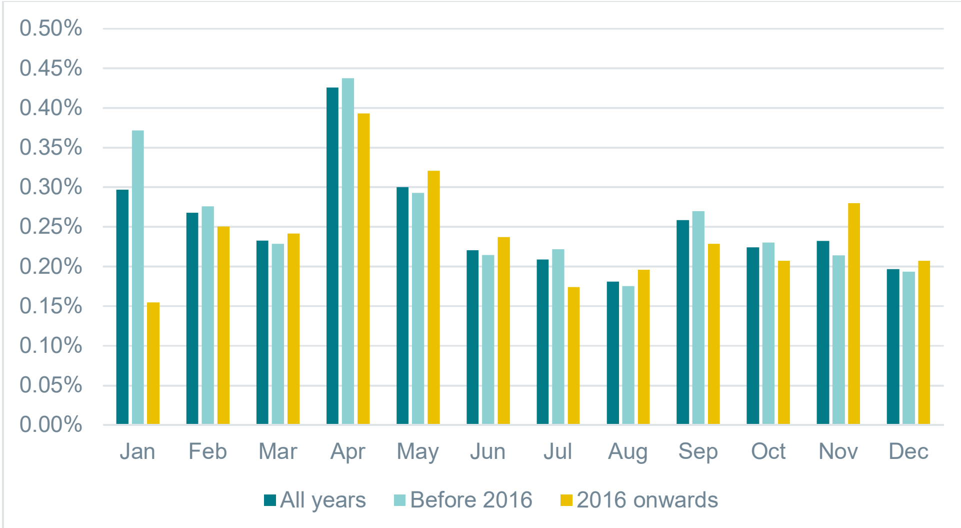


Source: Frontier Economics

While this may reflect the fact that the minimum wage increased substantially in 2016 and subsequent years, it may also reflect the fact that the month in which minimum wages are increased was changed from October to April in 2016. If we consider average monthly inflation for treated items, we see that the price of these items tended to increase in the month of April, even before 2016 when minimum wage increases took place in October (see Figure 25).

This confounding factor is explored econometrically in the following section.

Figure 25 Monthly inflation for treated items, by month



Source: Frontier Economics

Core specification

Using the core panel specification outlined in section 4.2, we find that inflation is no higher in months where the minimum wage was uplifted (see Figure 26 column 1).

- The finding is not sensitive to the inclusion of lagged inflation in the specification.
- The finding is not sensitive to the specification of standard errors: the coefficient remains small and insignificant with regular standard errors, robust standard errors, or if errors are presumed to be clustered by item.[33]
- The finding is not sensitive to the inclusion of item fixed effects or month fixed effect.
- The finding is not sensitive to the choice of treatment definition (see Figure 26). While the coefficient is positive for those items which map to the 10 most exposed sectors, it remains not significant at the 10% level. See Box 3 for a definition of the four treatment definitions used in the panel analysis.

Figure 26 Effect of minimum wage uplift on inflation

	Dependent Variable: Percentage change in the item price index			
	(1)	**(2)**	**(3)**	**(4)**
MW uplift month	**-0.010**	**0.034**	**-0.019**	**-0.008**
	(0.044)	(0.023)	(0.049)	(0.041)
Sample	9,339	7,037	8,543	9,832
Adj. R^2	0.044	0.036	0.033	0.042

Source: Frontier Economics

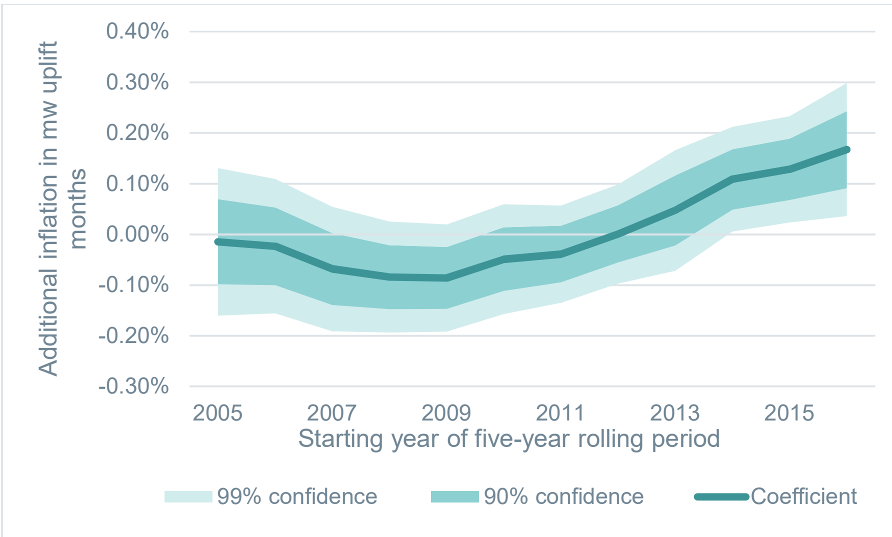
Note: Clustered standard errors (by sector) in parentheses; Significant at the 1% (***), 5% (**) and 10% (*) levels.

[33] The coefficient is only significant at the 5% level if standard errors are assumed to be clustered by item.

Figure 27 shows the finding is somewhat sensitive to the time period of analysis. The effect of minimum wage uplift on inflation for a series of 5-year rolling windows, starting with 2006-2009 and ending with 2016-2020, with 90% and 99% percentile confidence intervals shaded to show significance. The minimum wage effect increases over time, and is significant at the 1% level from 2014-2018 onwards.

If we restrict the sample to the period starting from when the National Living Wage was introduced (2016), we find that inflation is **0.237 percentage points** higher in months where the minimum wage was uplifted, significant at the 1% level (see Figure 28, column 1).

Figure 27 Effect of minimum wage uplift on inflation over time



Source: Frontier Economics

Note: Each point represents a 5-year rolling windows, starting from the year listed on the x axis. Shading shows the 90% and 99% percentile confidence interval. Month fixed effects are removed in this specification.

Figure 28 Effect of minimum wage uplift on inflation, 2016 onwards

	Dependent Variable: Percentage change in the item price index			
	(1)	**(2)**	**(3)**	**(4)**
MW uplift month	**0.237**	**0.182**	**0.370**	**0.242**
	(0.06)***	(0.047)***	(0.138)**	(0.057)***
Sample	2,661	2,023	2,433	2,764
Adj. R^2	0.072	0.058	0.079	0.074

Source: Frontier Economics

Note: Clustered standard errors (by sector) in parentheses; Significant at the 1% (***), 5% (**) and 10% (*) levels.

As before, this finding is not sensitive to the specification of the error term, the inclusion of lagged dependent variables, or the inclusion of fixed effects, although the coefficient is smaller if month fixed effects are not included in the specification. Figure 28 shows that the finding is somewhat sensitive to treatment definition. Notably, using gross value added as the denominator for labour cost share (3) increases the size of the effect.

As previously discussed, this may reflect two changes:

- That the National Living Wage was introduced in April 2016.
- That the month in which wages were increased was changed from October to April in 2016.

To test this, we regress inflation on a binary variable equal to 1 if the month is April. We find that the effect of April before 2016 is positive but not statistically significant, regardless of whether month fixed effect, item fixed effects or lagged dependent variables are included in the specification. This suggest it is unlikely that the strong post-2016 effect is a result of the change in uplift month.

To put these coefficients in context, the mean minimum wage increase over the period was 5.22%. For the set of items in the core treatment definition, the elasticity of prices with respect to the minimum wage (the term in equation 11, Section 2) is approximately 0.045. In other words, a 10% increase in the minimum wage could be expected to increase prices by 0.45%.

This elasticity is significantly lower than the share of minimum wage labour costs in total costs for these firms which is in the order of 0.15 to 0.3, implying a 10% increase in the minimum wage should theoretically translate to a 1.5% to 3% increase in prices under the assumptions discussed in Section 2.

Robustness checks

We consider robustness of these findings in three ways:

1. We test that the inflation panel is **stationary** using the Im-Pesaran-Shin and Fisher tests for unit roots in panel data, and reject the unit root null hypothesis at the 1% level. For completeness, we conduct Dickey-Fuller tests on each item in the panel, rejecting the null hypothesis of unit roots for each item/region time series at the 1% level.
2. We do not explicitly test for **autocorrelation** in the inflation error terms; the standard tests are frustrated by the dynamic panel structure of the data. While the inclusion of lagged dependent variables can lead to the underestimation of remaining coefficients, we do not expect this effect to be large enough to impact our hypothesis tests (see e.g. Kelly and Keele, 2004).
3. We consider that the regression results might be **skewed because the ONS over-samples some product classes** (those for which the between-item price variability is high). This is the case for SIC 56 (Food and beverage service activities) which account for 4 of the 20 most exposed sector, but 62 of the 80 most exposed item/regions. We find that inflation for non-food and drink items is 0.103 percentage points higher in minimum wage uplift months, significant at the 5% level, around half of the overall effect. This suggests that if the basket of items were reweighted to reflect their share of consumer spending, the average minimum wage effect would likely be smaller than the 0.237 percentage points estimated by the core specification.

4.4 Alternative specifications

Panel with lags and leads

The effect of a minimum wage increase may be anticipated by some firms, while others may delay price increases until the following months. To account for this, we use an alternative specification which accounts for two lagged months and two leading months.

$$inflation_{m,i} = \alpha + \beta_1 \times uplift_m + uplift_{m+l} \times +\gamma_i + \delta_m + inflation_{m-1,i} + \epsilon_{m,i}$$

where l is a vector of lags and leads: $-2, -1, 1, 2$.

There are large and significant lagged effects, with inflation increasing 0.172 percentage points the month after the minimum wage uplift (significant at the 1% level), and 0.085 percentage points two months after uplift (significant at the 10% level) (see Figure 30).

Figure 29 and Figure 30 show that inflation in minimum wage uplift months is higher than in other months, even when lags and leads are controlled for (statistically significant at the 1% level). This is very similar to the effect identified in the core specification, because the lagging/leading months are to a large extent substituting for the month fixed effects.

There are large and significant lagged effects, with inflation increasing 0.172 percentage points the month after the minimum wage uplift (significant at the 1% level), and 0.085 percentage points two months after uplift (significant at the 10% level) (see Figure 30).

Figure 29 Effect of minimum wage uplift on inflation with lags and leads, 2016 onwards

	Dependent Variable: Percentage change in the item price index			
	(1)	**(2)**	**(3)**	**(4)**
2 months before	0.071 (0.047)	0.034 (0.044)	0.167 (0.095)	0.073 (0.045)
1 month before	0.101 (0.085)	0.010 (0.042)	0.235 (0.165)	0.099 (0.082)
MW uplift month	**0.238** (0.062)***	**0.179** (0.049)***	**0.376** (0.143)**	**0.242** (0.060)***
1 month after	0.172 (0.045)***	0.138 (0.047)**	0.305 (0.143)*	0.171 (0.043)***
2 months after	0.085 (0.044)*	0.052 (0.044)	0.208 (0.163)	0.086 (0.042)*
Sample	2,550	1,939	2,331	2,648
Adj. R^2	0.049	0.033	0.052	0.052

Source: *Frontier Economics*

Note: *Robust standard errors in parentheses; Significant at the 1% (***), 5% (**) and 10% (*) levels. Item fixed effects and lagged inflation not reported; these are generally not significant at the 5% level.*

Figure 30 Effect of minimum wage uplift on inflation with lags and leads, 2016 onwards (core treatment definition)

Source: Frontier Economics

Note: Error bars represent the 95% confidence interval of the coefficient.

If we attribute these three effects (the uplift month and two lags) to the minimum wage, this suggests an overall effect of 0.5 percentage points from 2016 onwards. As the mean minimum wage increase from April 2016 onwards was 5.22%, this suggests an elasticity of prices with respect to the minimum wage of 0.95. In other words, a 10% increase in the minimum wage could be expected to increase prices by 0.95%. This elasticity is still lower than the theoretical prediction, but is similar to the long-run elasticity identified in Harasztosi & Lindner (2019).

Panel with continuous measure of minimum wage uplift

The preceding specifications treat all increases in the minimum wage the same. However, Figure 1 shows that the magnitude of the minimum wage increase varies over the period, from a minimum of 1.8% in October 2010 to a maximum of 7.5% with the introduction of the National Living Wage in April 2016. For this reason, we replace the binary uplift variable with a continuous uplift variable equal to the percentage change in minimum wages (set to zero in months where the minimum wage was not uplifted).

$$inflation_{m,i} = \alpha + \beta_1 \times \%\Delta mw + \gamma_i + \delta_m + inflation_{m-1,i} + \epsilon_{m,i}$$

An additional advantage of this approach is that the coefficient can be interpreted directly as the elasticity of price with respect to minimum wages, the term defined in equation 11 of Section 2.

Figure 31 shows that the effect of minimum wage uplift remains statistically significant at the 1% level (at the 5% level for specifications 2 and 3). The coefficient implies that a 10% increase in the minimum wage increases prices in the uplift month by 0.28%. This is slightly smaller than the interpretation of the findings from core specification.[34]

Figure 31 Effect of minimum wage uplift on inflation, 2016 onwards

	Dependent Variable: Percentage change in the item price index			
	(1)	**(2)**	**(3)**	**(4)**
% change in minimum wage	**0.028** (0.007)***	**0.022** (0.006)***	**0.029** (0.01)**	**0.028** (0.007)***
Observations	2,661	2,023	2,433	2,764
Adj. R^2	0.033	0.021	0.043	0.035

Source: Frontier Economics

Note: Clustered standard errors (by sector) in parentheses; Significant at the 1% (***), 5% (**) and 10% (*) levels.

Difference in difference

Finally, we discard the panel model and test a difference-in-differences specification to identify whether the substantial minimum wage increase in April 2016 had a different impact of treated and control items.

$$inflation_{m,i,r} = \alpha + \beta_1 \times treated_{i,r} + \beta_2 \times after_m + \beta_3 \times treated\_after_{m,i,r} + \gamma\ time_m + \epsilon_{m,i,r}$$

Where:

- $inflation_{m,i}$ is the annual percentage change in the price index for an item/month.
- $treated_i$ is a binary variable equal to one if the item is in the treated group and zero if it is in the control group.
- $after_m$ is a binary variable equal to one if the month is April 2016 or later and zero otherwise.
- $treated\_after_{m,i}$ is a binary variable equal to one if the item is in the treated group and the month is April 2016 or later.
- $time_m$ is a continuous time variable (to capture long-run inflation trends).

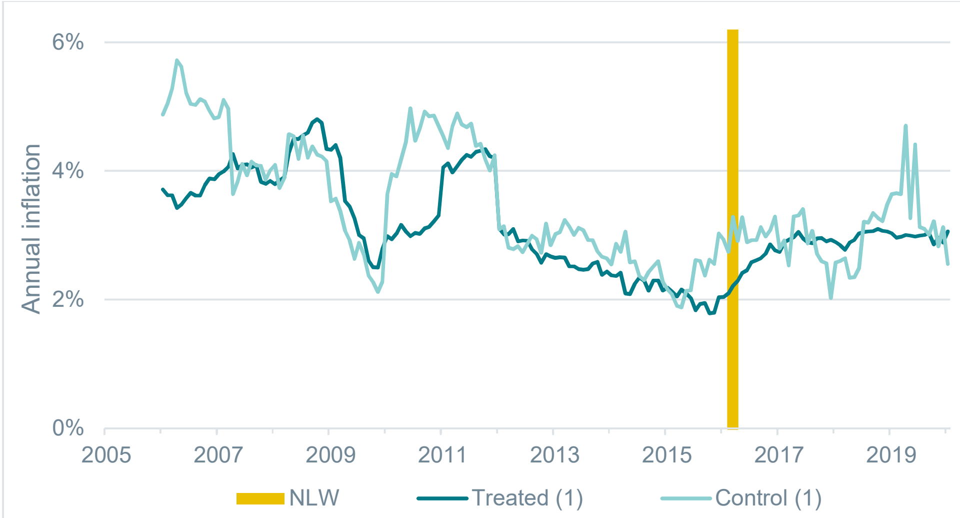
The coefficient β_3 can be interpreted as the additional impact of the April 2016 minimum wage increase on prices for the treatment group relative to the control group (assuming that the control group would otherwise have been expected to follow the same inflation trend as the treatment group).

Figure 32 shows that the unweighted average inflation for treated items trends up around the introduction of the National Living Wage in April 2016, stabilising from 2018.

The unweighted average annual inflation for control group items remains relatively stable after 2016, apart from a brief spike in late 2018. These observations are not sensitive to alternative definitions of the treatment group, however as expected, including tradable items in the control group significantly changes the inflation trend.

[34] Note that month fixed effects are not used in this specification to ensure the effect of minimum wages on inflation is captured by the coefficient on the minimum wage change variable.

Figure 32 Annual inflation over time, treated and control items

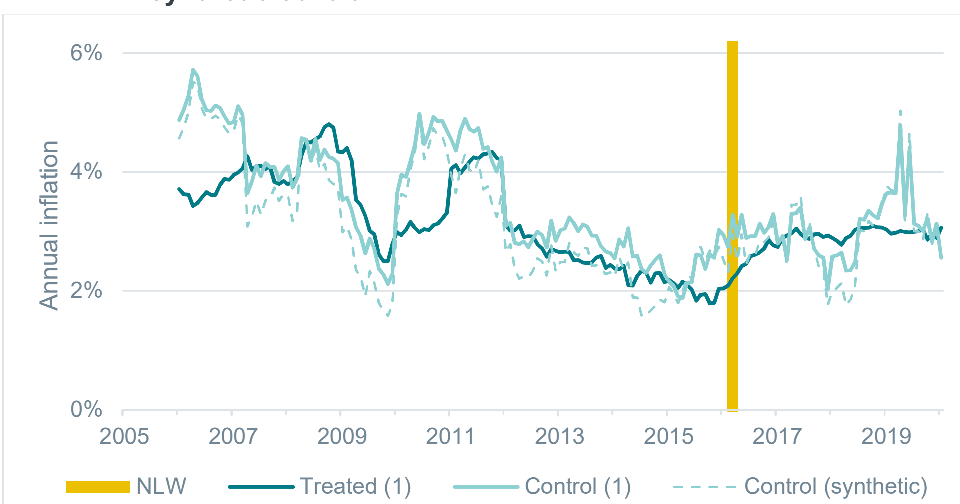


Source: Frontier Economics

Figure 32 shows that inflation for treated and control items follow a relatively similar trend from 2008 until the introduction of the National Living Wage. However, there is some divergence between 2013 and 2016, implying that the common trends assumption may not be appropriate.

For this reason, we construct a reweighted synthetic control group (see e.g. Abadie et al. 2010 for the detailed approach). However, the synthetic control group tracks the treated group only marginally better than the unweighted control group (see Figure 33). This was true regardless of the time period used to calibrate the reweighting formula.

Figure 33 Annual inflation over time, treated and control items with synthetic control



Source: Frontier Economics

Figure 34 shows that the coefficient on the difference-in-differences term is generally not significant regardless of the approach to constructing the control group (standard or synthetic) or the number of months of inflation observations included (24, 48 or 96). The exception is the specification with the synthetic control group and a 24 month time window, which suggests that the inflation effect of the National Living Wage on treated items was **0.426 percentage points** higher than the effect on control items, significant at the 1% level. While this is consistent with the findings in the core specification, the sensitivity of the finding to the time window and uncertainty regarding the validity of the common trends assumption imply that this finding should be interpreted with caution.

The finding is not sensitive to the inclusion of the time control or to the period used to calibrate the synthetic control weightings.[35]

Figure 34 Effect of treatment and NLW introduction on annual inflation, treatment definition 1

	Dependent Variable: Annual percentage change in the item price index					
Control group	**Standard control group**			**Synthetic control group**		
Window (months)	**24**	**48**	**96**	**24**	**48**	**96**
After NLW & treatment	**0.270** (0.308)	**0.298** (0.226)	**0.187** (0.158)	**0.426** (0.122)***	**0.132** (0.143)	**-0.106** (0.123)
After NLW	0.393 (0.193)**	0.396 (0.141)***	0.356 (0.097)***	-0.255 (0.127)*	0.308 (0.158)*	0.549 (0.136)***
Treatment	-0.581 (0.217)***	-0.398 (0.159)**	-0.347 (0.111)***	-0.624 (0.096)***	-0.127 (0.108)	0.006 (0.089)
Observations	3,355	6,732	13,333	-	-	-
Adj. R^2	0.005	0.004	0.003	0.730	0.460	0.292

Source: Frontier Economics

Note: Standard errors in parentheses; Significant at the 1% (***), 5% (**) and 10% (*) levels. The time window refers to the total number of years included in each specification; 24 implies that the period starts on April 2015 and finishes in Mar 2017.

[35] The coefficients presented in Figure 34 use weighting calibrated over the period January 2012 to Mar 2016. The coefficient is 0.398 if the weightings are calibrated between January 2006 and March 2016 and 0.539 if the weightings are calibrated between January 2014 and March 2016.

5 CONCLUSIONS

Firm-level impacts

Our core specification finds that employment growth from 2015 to 2018 was in the region of 2 or 3 percentage points lower in firms affected by the introduction of the National Living Wage. The effect is most obvious in relation to small firms, and in the retail and food service sectors. However, by looking at alternative outcome variables, we can see also see impacts among larger firms. We also see that survival is higher amongst affected firms, which may reflect differences in business churn, while growth in turnover per employee is weaker. The latter result is counterintuitive, but may be due to prior adverse conditions affecting both wages (and hence assignment into the treatment group) and subsequent performance.

These empirical results hold over various changes to specification and how the variables are defined. We have also undertaken detailed validation checks against other datasets to establish that the measurement error inherent from using ASHE to define minimum wage exposure is not overly problematic.

However, it is difficult to draw casual inferences from our analysis. In particular, we draw on a falsification strategy by repeating the same experiment over different years, going back to 2004. This finds similar sorts of impacts as in the core analysis focused on the NLW introduction, i.e. minimum wage firms have lower employment growth, lower growth in turnover per employee, and higher survival. This would suggest that the effects we find are not specific to the NLW introduction, but are observed over multiple years, including some where the upratings are not especially large. For example, we observe various forms of capital-labour substitution over the period, such as self-scan checkout and computerised ordering systems, that may be rolled out more in a minimum wage setting.

Price impacts

We find evidence that, for the sectors most exposed to changes in the cost of minimum wage labour, inflation is higher in months when the minimum wage is increased than at other times of the year, but only after the introduction of the National Living Wage in 2016.

The effect is small relative to the size of the minimum wage increase. Since 2016, inflation for treated items was **0.237 percentage points** higher in minimum wage uplift months, compared to an average increase in minimum wages of 5.22%. This is equivalent to an elasticity of prices with respect to minimum wage of **0.045**. If we attribute the elevated inflation in the two months following uplift to the minimum wage, the elasticity could be as high as **0.095**.

In other words, a 10% increase in the minimum wage would be expected to increase prices by 0.45% to 0.95%. This is lower than the increase predicted by the theoretical framework of 1.5% to 3%, but the framework ignores price-adjustment costs and makes a number of relatively strict assumptions about the level of competition in product and labour markets and the shape of firms' production functions.

These findings are similar to those studies elsewhere in the literature that identify a significant effect:[36]

- Wadsworth (2010) finds a long-term effect in the order of **0.2 to 0.9 percentage points** per year for the most exposed sectors using a difference-in-differences approach on the 1999 introduction of the UK National Minimum Wage. He finds no significant effect using the uplift month approach used in the core specification of this study.
- Harasztosi & Lindner (2019) find that the doubling of the minimum wage led to a 7% to 14% increase in prices over a four year period, equivalent to an elasticity of **0.07 to 0.14**.
- Aaronson (2001) finds an elasticity of prices with respect to minimum wages of **0.074** for restaurants in both Canada and the United States. Aaronson et al. (2005) find an elasticity of prices with respect to minimum wages of **0.07** for restaurants, increasing to **0.15** for those restaurants more exposed to minimum wages. Both sets of elasticities include leading and lagging periods in the core specification means they are comparable to the 0.095 elasticity in our results.

Future research could further refine the treatment assignment rule, notably by measuring the share of labour costs attributable to minimum wage workers (rather than the share of minimum wage workers) and by accounting for the share of minimum wage labour costs in respective supply chains (rather than assuming that all inputs are tradable). Future research might also investigate differences in price adjustment frictions in different sectors to explore the time profile of price responses to minimum wages.

[36] Neither Draca et al. (2005) nor Machin et al. (2003) find evidence that the introduction of the UK minimum wage had an effect on inflation in exposed sectors.

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ANNEX A FIRM-LEVEL IMPACTS: VALIDATING ASHE ASSIGNMENT USING ABS AND WERS

As noted previously, the assignment into treatment and control groups using ASHE is an imperfect signal of a firm's minimum wage exposure, giving rise to measurement error. We explore the materiality of this in two ways:

- Correlation between ASHE assignment variable and ABS labour cost thresholds
- Correlation between randomly sampled individual's pay and minimum wage exposure at the workplace level using WERS

Annual Business Survey labour cost thresholds

Firstly we explore the relationship between pay observed in ASHE and labour cost per employee ratios reported in Annual Business Survey. To do this we link employees in ASHE to their workplace in the ABS. Note that in many cases there will be no such link, as only a subset of businesses will be sampled in the ABS. We then retrieve two pieces of information:

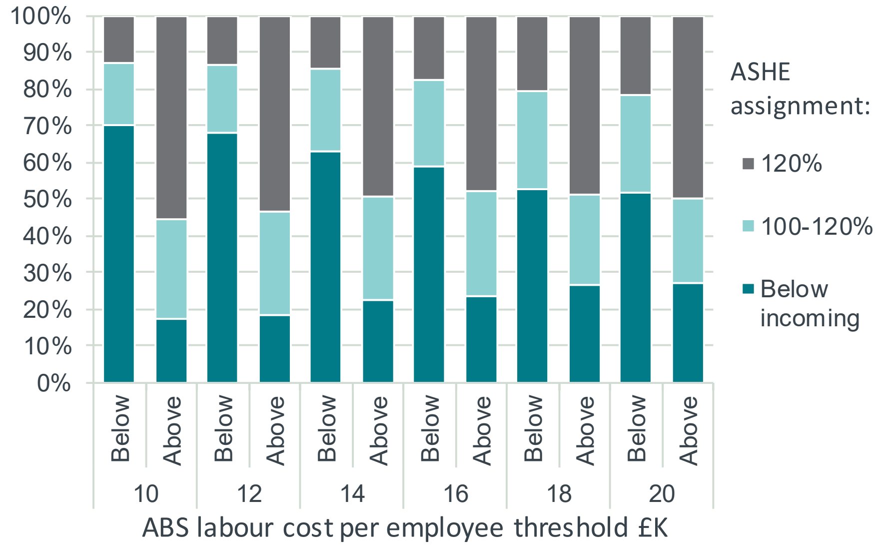
- ASHE pay assignment. Whether the pay hourly pay observed in ASHE is below the incoming minimum wage, between 100 and 120% of it, or greater than 120%.
- ABS labour cost per employee ratio. This is given by dividing labour cost by number of employees. We then use a number of thresholds in turn, and classify whether labour cost is above or below that level. The thresholds are intended to follow those used in Riley and Bondibene-Rosazza.[37]

The relationship between the two pay measures is shown in Figure 35. The horizontal axis shows groups of firms based on whether they are above or below a specific threshold value. For example, the first column represent firms for which labour cost per employee is below £10k and the second column firms for which labour cost per employee is above this. The next columns do this using a £12k threshold, £14k, £16k , etc. The columns are shaded to show the proportion of firms assigned to the different groups in ASHE.

For example, of firms that were observed to have labour cost per employee below £12k, 68% of the firms sampled in ASHE are in the below incoming minimum wage, compared to 17% if we look at firms with labour cost ratio above this level.

[37] We use the approximate historical range presented in Riley and Bondibene-Rosazza and adjust for inflation. However, it is not feasible to validate these against any recent WERS data, due to the survey being discontinued.

Figure 35 ASHE pay group assignment proportions by ABS labour cost thresholds



Source: Frontier analysis of ONS data (ASHE and ABS)

Clearly, none of these are perfect measures. In ASHE, there is scope for sampling a worker unrepresentative of the wider pay pattern prevalent in the workplace. ABS is also imperfect. For example, skewness in the pay distribution within a firm may conceal low pay, if a small number of high-paid individuals bring up the average. Alternatively part-time working will reduce the labour cost per employee for a given wage rate. While we cannot directly observe the full pay distribution within a workplace in either of these datasets, this analysis demonstrates that the measures from ABS and ASHE provide signals of low pay.

Random sampling using Workplace Employment Relations Survey

The Workplace Employment Relations Survey (WERS) samples up to 25 employees in a workplace. This provides a detailed view of the pay distribution within a firm, including how many individuals are paid below the incoming wage. Using this information, we can explore how well the random sampling in ASHE acts as a signal of firm-level minimum wage exposure. We can repeat the exercise for labour cost thresholds.

The WERS data does not report exact hourly pay rates, but only provides weekly pay in bands. We derive hourly pay by taking the mid-point of each band and dividing by weekly hours. This obviously reduces precision of how hourly pay can be measured in WERS.

WERS was last published in 2011, with sampling between 2009 and 2011. The incoming minimum wage was £5.93 in 2011 (note there is some staggering in terms of exactly when the workplaces are sampled. The hourly pay estimates suggest that 11% of employees were paid below this amount, which is a higher proportion than it was in reality (i.e. on average the true pay is less than the midpoints implied by the bands). We therefore explore several lower thresholds in terms of defining minimum wage employees.

We then randomly sample one individual in each workplace and classify them as minimum wage if they are paid below the threshold amount. We calculate the proportion of employees paid below threshold at the firm-level, which is the measure of firm-level minimum wage exposure. We also calculate average labour cost per employee, which is the assignment variable used most commonly in the preceding firm-level minimum wage analysis.

The correlation between these different variables is shown in Figure 36 below. The rows represent alternative thresholds to define low pay. The columns show correlation coefficient of firm-level minimum wage exposure (proportion of employees paid below threshold) with the randomly sampled individual and with average pay at the firm level. For example, using a £5.93 threshold, the correlation coefficient between intra-firm minimum wage exposure and the randomly sampled individual is 0.69. Between intra-firm minimum wage exposure and average pay, the correlation is -0.58. Overall, therefore, we see that the randomly sampled individual provides a slightly stronger signal of minimum wage exposure as compared with average pay.[38]

Figure 36 Correlation between firm-level minimum wage exposure, randomly sampled individual and average annual pay

Hourly pay threshold	*Correlation of firm-level exposure with:*	
	Random individual	Average pay
£5.00	0.62	-0.47
£5.50	0.68	-0.55
£5.90	0.69	-0.58
£5.93	0.69	-0.58
£6.08	0.71	-0.60
£6.20	0.70	-0.62

Source: Frontier analysis of WERS 2011 data (ONS)

We can also test this by testing the random individual and average pay variables in a regression to explain firm-level minimum wage exposure. This is first done in a multivariate regression including both variables as controls (1), followed by univariate regressions (2) and (3). We report both the t-statistics and R-squared, and repeat for different thresholds (separate rows). In all cases we see that the random individual variable acts as a stronger signal, having a larger t-statistic and (in the univariate regressions) a large R-squared.

[38] The correlation we estimate between firm-level exposure and average pay is very similar to that reported by Draca et al (2008) who report a value of -0.61. They used WERS 1998.

Figure 37 Power of random individual and average pay variables in explaining firm-level minimum wage exposure

Threshold		(1) Random individual	Average pay	(2) Random individual	(3) Average pay
£5.00	T-statistic	29.83	-17.32	33.62	-22.07
	R-squared	0.48		0.39	0.22
£5.50	T-statistic	32.33	-20.09	38.84	-27.57
	R-squared	0.56		0.46	0.30
£5.90	T-statistic	32.21	-21.68	39.70	-29.99
	R-squared	0.58		0.47	0.34
£5.93	T-statistic	32.43	-21.73	40.00	-30.13
	R-squared	0.59		0.48	0.34
£6.08	T-statistic	33.25	-22.42	41.81	-31.77
	R-squared	0.61		0.50	0.36
£6.20	T-statistic	32.39	-23.20	41.32	-32.81
	R-squared	0.61		0.49	0.38

Source: Frontier analysis of WERS 2011 data (ONS)

Intra-firm pay distributions

As a cross-check, we also explored intra-firm pay distributions within WERS. The purpose here is to understand by how much pay varies within a workplace, to see how dispersed it is, and the sources of skewness. This is done by measuring pay at differentials within a workplace, and then looking at the distribution of these values over different workplaces. This gives insight into how ‘wrong’ we might be when sampling an individual randomly, with wide dispersion giving greater scope for measurement error. It is also informative in understanding the relationship between average labour cost and minimum wage exposure at the firm level.

First we analyse intra-firm pay in relative terms, calculating pay of an individual as a percentage of the median employee within the workplace. Consider, for example, the pay of an individual at the 10th percentile of a workplace compared to the median employee at that workplace. On average (at the median across firms) they are paid around 2/3 of the workplace median. The employee at the 90th percentile in the workplace is paid 160% of the median. Some firms have larger or smaller pay differentials than this (as shown by the different rows).

Figure 38 Relative intra-firm pay differentials

	10pc/median employee	25pc/median employee	75pc/median employee	90pc/median employee
10th percentile firm	0.47	0.65	1.08	1.22
25th percentile firm	0.56	0.73	1.15	1.35
50th percentile firm	0.67	0.81	1.25	1.6
75th percentile firm	0.76	0.87	1.4	1.99
90th percentile firm	0.83	0.92	1.6	2.51

Source: Frontier analysis of WERS 2011 data (ONS)

The differentials in absolute terms are shown in Figure 39. Here we see, for example that in the median firm, the difference in hourly pay between the employee at the 75th percentile and the employee at the 25th percentile is £5.20. We do this separately for retail and food service sectors and find smaller absolute pay differentials. The differentials are larger in the upper part of the distribution. In terms of measurement error, this would imply the source is more likely to be from observing high-paid individuals in low-pay firms than vice versa.

Figure 39 Absolute intra-firm pay differentials

	25/75 differential	10/90 differential	25/50 differential	50/75 differential
All sectors				
10th percentile	£1.80	£4.30	£0.70	£0.80
25th percentile	£3.00	£6.70	£1.10	£1.50
50th percentile	£5.20	£11.30	£1.90	£2.80
75th percentile	£8.00	£16.70	£3.30	£4.60
90th percentile	£11.20	£21.30	£5.10	£6.60
Retail and food service				
10th percentile	£1.20	£2.50	£0.40	£0.40
25th percentile	£1.50	£3.70	£0.70	£0.80
50th percentile	£2.10	£5.30	£0.90	£1.00
75th percentile	£3.30	£9.50	£1.40	£1.70
90th percentile	£5.00	£18.90	£2.30	£3.20

Source: Frontier analysis of WERS 2011 data (ONS)

ANNEX B FIRM-LEVEL IMPACTS: SENSITIVITY TESTING

This Annex includes various sensitivity tests and extensions to the analysis.

- ***Alternative employment measures.*** To address concerns around timeliness, we explore different sources of employment data.
- ***Alternative pay thresholds.*** As it is unclear how far up the pay distribution spillover effects operate, we explore a variety of different thresholds.
- ***Non-random survival.*** We use inverse probability weighting to address the potential for survival bias.
- ***Propensity score matching.*** This is an alternative estimation procedure that allows for controlling for pre-treatment trends.
- ***Multiple year analysis for turnover and survival***

Alternative employment measures

Given concerns around timeliness of employment data in the BSD, we sought the most timely indicators. Our overall choice was therefore to use PAYE data for single unit enterprises (PAYE data is not available broken down by location for multi-unit enterprises), or BRES-sourced data where there are multiple units. The BSD also contains imputed data from other sources, which we elected not to use in the main analysis.

The results using alternative employment measures are shown in Figure 40. For reference, the first row shows results using our bespoke measure. Next we consider 'headline' employment as reported directly in the BSD. This gives very similar results.

Results using PAYE data only are somewhat smaller, and statistically insignificant in the case of low-pay sectors only. Note that PAYE is not reported at the local unit level, only the enterprise level. We therefore only use the data for single-unit enterprises, where this issue does not arise. As noted, there appears to be some difference responses depending on the local unit structure of firms, with the response more apparent for larger firms. Also, PAYE-sourced data make up a only relatively small proportion of the sample, around one-fifth.

Finally we consider employment data in the BSD sourced from BRES,[39] which largely covers multi-unit enterprises. The results are very similar to the core result.

[39] This is done using the flag empsource=BRES in the BSD.

Figure 40 Treatment effect for different outcome variables

Outcome variable	All firms beta	p-value	Low-pay beta	p-value
Main approach (PAYE + BRES)	-0.030	0.00	-0.023	0.00
BSD headline	-0.029	0.00	-0.024	0.00
PAYE only	-0.019	0.04	-0.013	0.30
BRES only	-0.027	0.00	-0.020	0.00

Source: Frontier analysis of ONS data (BSD and ASHE)

Alternative pay thresholds

In our main approach we specify the treatment group as being those where the worker is being observed to be paid less than the incoming minimum wage, and the control group to be those paid at least 120% if it. The rationale is that the intervening 100% to 120% 'buffer' is affected by spillover effects, for example from maintaining pay differentials against the minimum wage.

We explore the impact of using alternative thresholds for assigning firms intro different groups as a robustness check for how far spillover effects run up the pay distribution (see discussed earlier in 'analytical approach' section). We now explore the results if we vary these thresholds. Across the different specifications, we see a negative effect on employment growth, ranging from 1.7 to 2.9 percentage points. The results are largest using <100 to define the minimum wage-affected firms (treatment group) and >120 to define unaffected firms (control group). This supports the hypothesis that spillover effects are most obvious up to around 20% above the minimum wage, and consistent with our prior expectation of the range over which they operate.[40]

[40] Clearly, this research is not focused on assessing the extent of spillover effects, so these results are not intended to be definitive with regard to that question.

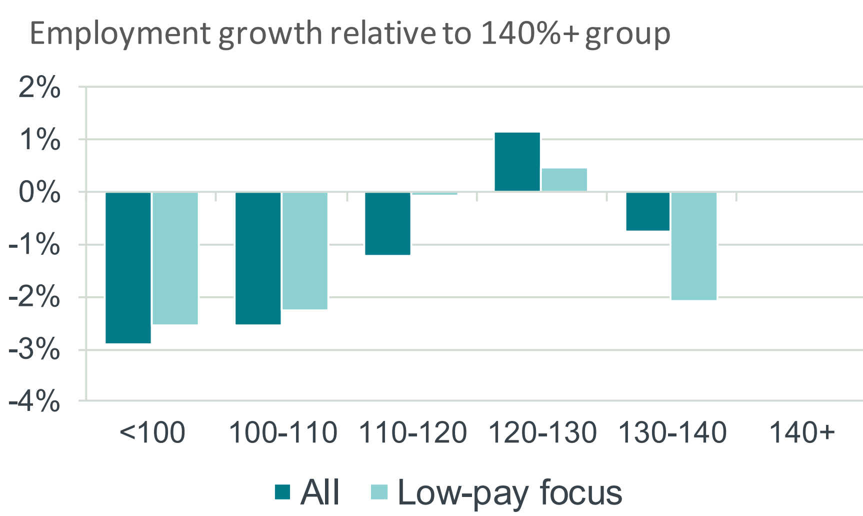
Figure 41 Treatment effect using alternative pay thresholds

Treatment group	Control group	All sectors Beta	All sectors p-value	Low-pay Beta	Low-pay p-val
<100	>100	-0.020	0.00	-0.016	0.00
<100	>110	-0.028	0.00	-0.024	0.00
<100	>120	-0.029	0.00	-0.021	0.00
<100	>130	-0.025	0.00	-0.016	0.01
<100	>140	-0.027	0.00	-0.022	0.00
<110	>110	-0.026	0.00	-0.022	0.00
<110	>120	-0.027	0.00	-0.019	0.00
<110	>130	-0.023	0.00	-0.015	0.01
<110	>140	-0.026	0.00	-0.021	0.00
<120	>120	-0.026	0.00	-0.017	0.00
<120	>130	-0.022	0.00	-0.013	0.02
<120	>140	-0.025	0.00	-0.020	0.00
<130	>140	-0.017	0.00	-0.016	0.02

Source: Frontier analysis of ONS data (BSD and ASHE)

We can also estimate the effects non-parametrically by adding dummies for the different pay bands and estimating performance relative to a base case of the control group paid in excess of 140% of the incoming minimum wage. The groups below 110 showed decline in employment relative to base case. The effect becomes more mixed in the intermediate bands, but this could also reflect smaller sample sizes for those groups, particularly in the low-pay sectors only version.

Figure 42 Change in employment for different pay bands



Source: Frontier analysis of ONS data (ASHE and BSD)

Non-random survival

The factors that predict employment growth also affect survival. This means that survival is non-random, and therefore selection into the sample will be correlated with drivers of subsequent growth. It is possible that this may bias the results. We test the sensitivity of the results to this by using inverse probability weights. In effect, this re-weights the sample so that it is representative of all firms present at the baseline year, rather than those that survive to the outcome horizon. This is done by estimating a first-stage survival regression and obtaining predicted values. These are used to form inverse probability weights, which are then used in a second-stage regression of employment growth. This estimates of the treatment effect that are very similar to the main regression results, suggesting that non-random survival effects are not introducing any strong bias.

Figure 43 Regression for change in log employment, 2018 vs 2015, including inverse probability survival weights

	(1) All firms		(2) Low-pay	
	Beta	p-value	Beta	p-value
Foreign-owned	0.011	0.03	0.023	0.00
Firm age	-0.001	0.00	0.000	0.44
Treatment dummy	-0.027	0.00	-0.025	0.00
Log employment 2015	-0.067	0.00	-0.071	0.00
Log turnover per employee 2015	0.027	0.00	0.027	0.00
Year dummies	Included		Included	
Sector dummies	Included		Included	
Region dummies	Included		Included	
New firm	-0.014	0.00	0.022	0.00
Worker age 25-34	0.041	0.00	0.026	0.11
Worker age 35-44	0.030	0.01	0.020	0.21
Worker age 45-54	0.009	0.43	0.004	0.81
Worker age 55-64	0.005	0.66	0.005	0.75
Worker age 65+	Base		Base	
Worker age <25	0.053	0.00	0.042	0.01
Worker Male	0.028	0.00	-0.014	0.02
Worker occupation dummies	Included		Included	
Constant	-0.329	0.12	0.134	0.01
N	129887		62235	
R-squared	0.040		0.037	

Source: Frontier analysis of ONS data (BSD and ASHE)

ASHE sample size

Given concerns around measurement error, a cross-check is to filter the sample to include only workplaces for which the ASHE samples a sufficiently high proportion of the workforce. If we have sampled many individuals, this gives a more reliable measure of bite, than if this assessment is based off just one employee sampled at random. We apply thresholds of 10%, 25%, 50% and 75% respectively. This is shown in Figure 44. For example, if we only include workplaces for which we are sampling at least 75% of the workforce in ASHE, the treatment effect is -0.038 in the all sector specification. Overall, we continue to see negative impacts.

Figure 44 Results for change in log employment 2015 vs 2018 using ASHE sample size thresholds

	All sectors			Low-pay focus		
Sample threshold	**Beta**	**P-value**	**N**	**Beta**	**P-value**	**N**
10%	-0.044	0.000	63197	-0.043	0.000	30010
25%	-0.040	0.000	40980	-0.038	0.000	18508
50%	-0.039	0.000	28105	-0.043	0.003	12036
75%	-0.038	0.004	20858	-0.028	0.131	8500

Source: Frontier analysis of ONS data (BSD and ASHE)

Propensity score matching

In difference-in-difference, we estimate the impact of treatment, controlling for the covariates. An alternative procedure to use propensity score matching (PSM), where the covariates are used to estimate the propensity for treatment, and these estimated propensities used to derive a control group that is observationally similar to the treatment group. So we create a group of low-bite firms that look similar in terms of observed characteristics to the high-bite firms.

The PSM first-stage results are shown in Figure 45. The dependent variable is the treatment dummy, a binary variable set to one if the worker sampled in ASHE is paid below the incoming NLW, zero if paid above 120% of it. The variables largely have intuitive interpretation, with high bite prevalent in more northern and rural areas, in smaller workplaces with lower turnover per employee. They are more likely to employee younger and female workers.

Figure 45 First-stage propensity score matching results

Variable	All sectors		Low-pay sectors	
North East	0.165	0	base	.
North West	-0.010	0.767	-0.322	0
Yorkshire	-0.041	0.231	-0.331	0
East Midlands	0.004	0.918	-0.274	0
West Midlands	-0.047	0.166	-0.315	0
East of England	-0.286	0	-0.563	0
London	-0.795	0	-1.036	0
South East	-0.344	0	-0.625	0
South West	-0.210	0	-0.500	0
Wales	base	.	-0.193	0
Scotland	-0.183	0	-0.493	0
Foreign-owned	-0.165	0	-0.140	0
Firm age	-0.004	0	0.001	0.289
Rural	0.067	0.001	0.047	0.073
Log employment 2015	-0.085	0	-0.367	0
Log turnover per employee 2015	-0.301	0	-0.367	0
Log employment growth 2012-15	0.006	0.753	-0.033	0.225
Log turnover growth 2012-15	0.072	0	0.118	0
Sector dummies	included		included	
New firm	-0.026	0.115	0.007	0.712
Worker age 25-34	0.051	0.189	0.025	0.621
Worker age 35-44	-0.097	0.014	-0.137	0.008
Worker age 45-54	-0.082	0.037	0.029	0.575
Worker age 55-64	-0.004	0.914	0.003	0.959
Worker age 65+	base	.	base	.
Worker age <25	0.688	0	0.655	0
Worker Male	-0.341	0	-0.263	0
Worker occupation dummies	included		included	
Constant	3.759	0	3.153	0
N	92,196		44,899	
R-squared	0.46		0.28	

Source: Frontier analysis of ONS data (BSD and ASHE)

The 'propensity scores' are then used to identify firms from the control group that are similar to the high bite firms. Specifically, we use a 'nearest neighbour' criterion that finds, for each treatment firm, the 10 control firms with the closest propensity score, and use these to construct the comparator group. We then compare the outcomes of the different groups in Figure 46 Propensity score matching second stage results: change in log employment from 2015 to 2018. The first row shows the outcome for the high-bite firms. The second row (B) show outcomes for the raw control group, and (C) the nearest neighbours as selected by the PSM algorithm. Note that in (1) the PSM is doing a lot of work moving from a broad set of firms and sectors to those that are similar to the high-bite firms, and the average employment growth for the groups is quite different. In column (2), where we have already whittled down to focus on low-pay sectors and occupations, the averages in rows (B) and (C) are much closer.

Either way, the treatment effect measured in row (D) is negative and significant, and in a range that is similar to that produced by the difference-in-difference analysis.

Figure 46 Propensity score matching second stage results: change in log employment from 2015 to 2018 by group

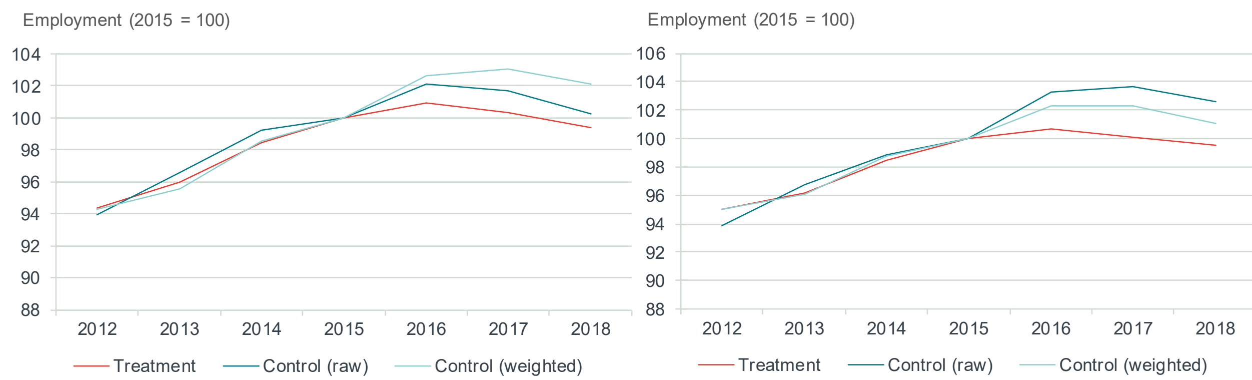
Outcome horizon	(1) All sectors	(2) Low-pay sectors
(A) Treatment	-0.006	-0.005
(B) Control (raw)	0.002	0.025
(C) Control (PSM comparator)	0.021	0.010
(D) Treatment effect ((A)-(C))	-0.027***	-0.015*

Source: Frontier analysis of ONS data (BSD and ASHE)

*Note: Significance levels are ***<0.01, **<.05, *=<0.1*

A final test of the ‘parallel trends assumption’. In order for the impact of the treatment to be interpreted as casual, we require that prior to the intervention (introduction of the NLW), the two groups were evolving similarly. The chart shows that employment growth prior to the intervention is similar, after which the groupw diverge. As stated above, for the ‘all-sector’ specification, there is some difference in between the raw control group and the PSM comparator, as we place greater weight on specific firms within the group. By contrast, very little changes in the low-pay focused specification, as the prior focus on low-pay sectors makes the groups more similar to start with.

Figure 47 Parallel trends (all sector to left, low-pay focused to right)



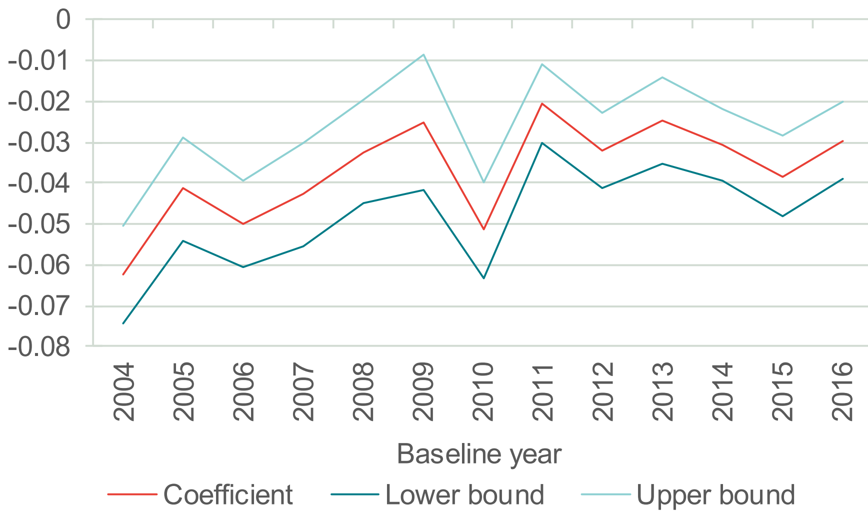
Source: Frontier analysis of ONS data (BSD and ASHE)

Analysis using multiple years – results for turnover per employee and survival

For completeness we repeat the multi-year analysis, providing results for turnover per employee and survival. This involves running both separate year-on-year regressions, as well as a pooled panel where we control for the size of the minimum wage increase in percentage terms.

As can be seen in the turnover per employee regression, we consistently see that firms in the treatment group have lower turnover growth across years. This is also borne out in the pooled panel approach. The size of uprating variable is negative and statistically insignificant.

Figure 48 Treatment effects on change in log turnover per employee, 1 year after



Source: Frontier analysis of ONS data (BSD and ASHE)

Note: results for 'all sector' specification. 'Low pay focus' gives similar results

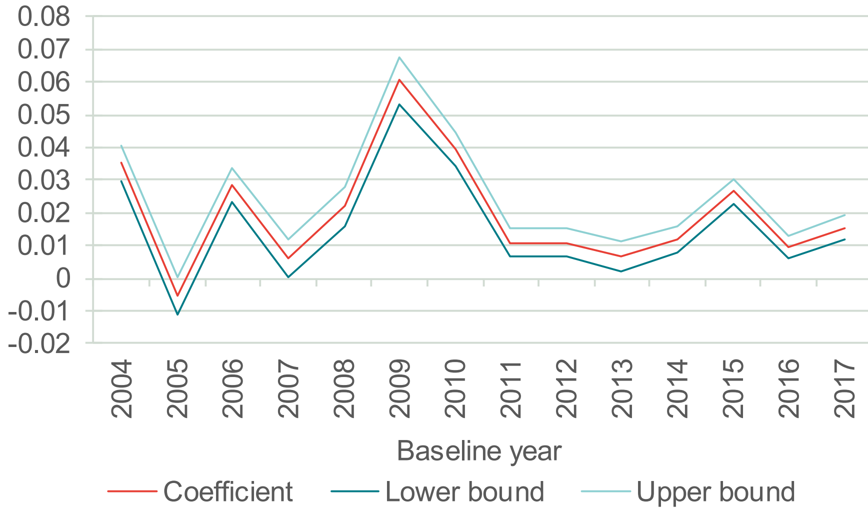
Figure 49 Multi-year regression results, change in log turnover per employee 1 year after baseline

	(1) All firms		(2) Low-pay	
	Beta	p-value	Beta	p-value
Foreign-owned	-0.029	0.000	-0.023	0.000
Firm age	0.002	0.000	0.002	0.000
Treatment dummy	-0.045	0.000	-0.042	0.000
% minimum wage increase in minimum wage firms	0.153	0.426	-0.041	0.855
Log employment 2015	0.023	0.000	0.019	0.000
Log turnover per employee 2015	-0.168	0.000	-0.165	0.000
Year dummies	Included		Included	
Sector dummies	Included		Included	
Region dummies	Included		Included	
New firm	0.004	0.225	0.000	0.923
Worker age 25-34	0.028	0.000	0.019	0.001
Worker age 35-44	0.035	0.000	0.021	0.000
Worker age 45-54	0.029	0.000	0.017	0.003
Worker age 55-64	0.017	0.000	0.015	0.010
Worker age 65+	Base		Base	
Worker age <25	0.021	0.000	0.019	0.001
Worker Male	0.018	0.000	0.023	0.000
Worker occupation dummies	Included	Included	Included	Included
Constant	1.297	0.992	0.575	0.997
N	644686		256123	
R-squared	0.083		0.081	

Source: Frontier analysis of ONS data (BSD and ASHE)

Turning to survival, it is apparent that survival rates are higher among minimum wage firms across multiple years. But larger upratings are correlated with lower survival, but this effect is statistically insignificant.

Figure 50 Treatment effects on survival, 1 year after



Source: Frontier analysis of ONS data (BSD and ASHE)

Note: results for 'all sector' specification. 'Low pay focus' gives similar results

Figure 51 Multi-year regression results, survival 1 year after baseline

	(1) All firms		(2) Low-pay	
	Beta	p-value	Beta	p-value
Foreign-owned	0.033	0.000	0.036	0.000
Firm age	0.000	0.000	-0.001	0.000
Treatment dummy	0.020	0.004	0.025	0.028
% minimum wage increase in minimum wage firms	-0.053	0.266	-0.044	0.734
Log employment 2015	0.013	0.000	0.017	0.000
Log turnover per employee 2015	0.000	0.257	0.011	0.000
Year dummies	Included		Included	
Sector dummies	Included		Included	
Region dummies	Included		Included	
New firm	-0.050	0.000	-0.045	0.000
Worker age 25-34	-0.012	0.000	-0.008	0.001
Worker age 35-44	-0.009	0.000	-0.002	0.354
Worker age 45-54	-0.009	0.000	-0.006	0.014
Worker age 55-64	-0.001	0.540	0.001	0.672
Worker age 65+	Base		Base	
Worker age <25	-0.020	0.000	-0.017	0.000
Worker Male	-0.003	0.000	-0.006	0.000
Worker occupation dummies	Included	Included	Included	Included
Constant	0.870	.	0.837	0.000
N	745524		296349	
R-squared	0.061		0.033	

Source: Frontier analysis of ONS data (BSD and ASHE)

ANNEX C ITEM TO SECTOR CORRESPONDENCE TABLE

Item ID	Item Description	SIC	SIC Description
220106	Pub: Cold Filled Roll/Sandwich	56.30	Beverage serving activities
220107	Pub -Hot Meal	56.30/1	Licensed clubs
220111	Burger In Bun-Eat In	56.10	Restaurants and mobile food service
220116	Lemonade/Cola Draught	56.10	Restaurants and mobile food service
220117	Bottled Mineral Water	56.10	Restaurants and mobile food service
220118	Restaurant Main Course 1	56.10	Restaurants and mobile food service
220119	Restaurant Main Course 1	56.10	Restaurants and mobile food service
220120	In Store Cafeteria Meal	56.10	Restaurants and mobile food service
220121	Restaurant Cup Of Coffee	56.10	Restaurants and mobile food service
220122	Restaurant - Sweet Course	56.10	Restaurants and mobile food service
220124	Muffin/Individual Cake	56.10	Restaurants and mobile food service
220125	Fruit Juice Bottle 250-350Ml	56.10	Restaurants and mobile food service
220126	Vegetarian Main Course	56.10	Restaurants and mobile food service
220127	Pub-Roll/Sandwich Hot Or Cold	56.30	Beverage serving activities
220128	Restaurant Evening Main Course	56.10	Restaurants and mobile food service
220205	Staff Restaurant Main Course	56.29	Other food services
220208	Staff Restaurnt Hot Snack Item	56.29	Other food services
220209	Primary School- Fixed Charge	56.29	Other food services
220210	Secondary School- Cafeteria	56.29	Other food services
220211	Staff Restaurant Fizzy Drink	56.29	Other food services
220212	Staff Restaurant Sandwich	56.29	Other food services
220213	Staff Restaurant Pudding	56.29	Other food services
220214	Staff Restaurant Main Course	56.29	Other food services
220301	Fish & Chips Takeaway	56.10/3	Take-away food shops and mobile food stands
220303	Sandwich-Take-Away (Cold)	56.10/3	Take-away food shops and mobile food stands
220304	Coffee -Take-Away	56.10/3	Take-away food shops and mobile food stands
220305	Tea -Take-Away	56.10/3	Take-away food shops and mobile food stands
220310	Potato Crisps-Individual Pack	56.10/3	Take-away food shops and mobile food stands
220316	Pizza Takeaway Or Delivered	56.10/3	Take-away food shops and mobile food stands
220317	Pasty/Savoury Pie - Takeaway	56.10/3	Take-away food shops and mobile food stands
220318	Indian Takeaway	56.10/3	Take-away food shops and mobile food stands
220319	Chinese Takeaway	56.10/3	Take-away food shops and mobile food stands
220320	Takeaway Soft Drink	56.10/3	Take-away food shops and mobile food stands
220321	Takeaway Coffee Latte	56.10/3	Take-away food shops and mobile food stands
220322	Burger In Bun- Takeaway	56.10/3	Take-away food shops and mobile food stands
220323	Kebab- Takeaway	56.10/3	Take-away food shops and mobile food stands
220324	Cinema Popcorn	59.14	Motion picture projection activities
220326	Takeaway Chicken & Chips	56.10/3	Take-away food shops and mobile food stands

Item ID	Item Description	SIC	SIC Description
220327	T'Away Cooked Savoury Pastry	56.10/3	Take-away food shops and mobile food stands
310102	Draught Bitter (Per Pint)	56.30	Beverage serving activities
310104	Draught Stout Per Pint	56.30	Beverage serving activities
310109	Lager - Pint 3.4-4.2%	56.30	Beverage serving activities
310110	Premium Lager - Pint 4.3-7.5%	56.30	Beverage serving activities
310111	Bottled Premium Lager 4.3-7.5%	56.30	Beverage serving activities
310112	Bottle Of Lager In Nightclub	56.30	Beverage serving activities
310114	Cider 4.5%-5.5% Abv Pint/Bottl	56.30	Beverage serving activities
310301	Whisky (Per Nip) Specify Ml	56.30	Beverage serving activities
310302	Vodka (Per Nip) Specify Ml	56.30	Beverage serving activities
310309	Spirit Based Drink 275Ml	56.30	Beverage serving activities
310310	Wine, Per 175 - 250 Ml Serving	56.30	Beverage serving activities
310314	Bottle Of Champagne	56.30	Beverage serving activities
310315	Bottle Of Wine 70-75Cl	56.30	Beverage serving activities
410508	Plumber-Daytime Hourly Rate	43.22	Plumbing, heat and air-conditioning installation
410509	Electrician-Daytime Rate/Hour	43.21	Electrical installation
410516	Gas Service Charge Local	43.22	Plumbing, heat and air-conditioning installation
410517	Decorator-Daily Rate;Spec Hrs	74.10	specialised design activities
410518	Carpenter Hourly Rate	43.32	Joinery installation
410632	Hire Of Domes Carpet Cleaner	77.29/9	Renting and leasing of other personal and household goods
430621	Annual Booster Injection	86.90	Other human health activities
430622	Dog Kennel Fees Daily Charge	93.19/9	Other sports activities
430623	Small Caged Mammal	47.76	Retail sale of flowers, plants, seeds, fertilizers, pet animals and pet food in specialised stores
440101	Domestic Cleaner Hourly Rate	81.21	General cleaning of buildings
440104	Dry Cleaning-Man'S Suit	96.01	Washing and (dry-)cleaning of textile and fur products
440105	Driving Lesson 1 Hour	85.53	Driving school activities
440113	Window-Clean 3-Bed Semi	81.22/1	Window cleaning services
440116	Washing Machine Repair	95.22	Repair of household appliances and home and garden equipment
440118	Pc Repair	95.11	Repair of computers and peripheral equipment
440120	Child Minder - Hourly Rate	88.91	Child day-care activities
440121	Catering-50 Set Menu Per Head	56.21	Event catering activities
440123	Home Removal- 1 Van	49.42	Removal services
440125	Gardener Hourly Rate	81.30	Landscape service activities
440126	Weekly Nanny Fees	88.91	Child day-care activities
440127	Monthly Self Storage Fee	68.20/9	Other letting and operating of own or leased real estate
440128	Home Care Assistant Hrly Rate	88.10	Social work activities without accommodation for the elderly and disabled
440129	Playgroup Fees - Per Session	88.91	Child day-care activities
440130	After School Club Charges	88.91	Child day-care activities

Item ID	Item Description	SIC	SIC Description
440132	Men'S Clothing Hire- See Help	77.29/9	Renting and leasing of other personal and household goods
440227	Funeral-Cremation	96.03	Funeral and related activities
440232	Nursery Fees: Child 0-4	88.91	Child day-care activities
440233	Newspaper Ad Non Trade 20 Word	58.13	Publishing of newspapers
440240	Basic Will For A Single Person	69.10/2	Solicitors
440254	Hourly Rate For Solicitor	69.10/2	Solicitors
520301	Man'S Haircut	96.02	Hairdressing and other beauty treatment
520303	Women'S Hrdressing-Cut/Blowdry	96.02	Hairdressing and other beauty treatment
520309	Women'S Hrdressing-Cut/Blowdry	96.02	Hairdressing and other beauty treatment
520311	Womens Highlighting	96.02	Hairdressing and other beauty treatment
520313	Non-Nhs Medicine-Physiotherapy	86.90	Other human health activities
520323	Full Leg Wax (Both Legs)	96.02	Hairdressing and other beauty treatment
520324	Residential Home	87.30	Residential care activities for the elderly and disabled
520325	Nursing Home	87.10	Residential nursing care facilities
520326	Private Dental Examination	86.23	Dental practice activities
520331	Basic Manicure	96.02	Hairdressing and other beauty treatment
520332	Non Nhs Chiropractor	86.90	Other human health activities
610227	Car Mot Test Fee, Vat Exempt	45.20	Maintenance and repair of motor vehicles
610229	Auto Car Wash	45.20	Maintenance and repair of motor vehicles
610231	Car Service- Local Garage	45.20	Maintenance and repair of motor vehicles
610232	Car Service- Main Dealer	45.20	Maintenance and repair of motor vehicles
610233	Exhaust Fitting In Fast Fit	45.20	Maintenance and repair of motor vehicles
610234	Brake Fitting In Fast Fit	45.20	Maintenance and repair of motor vehicles
610235	Car Repairs Main Dealer	45.20	Maintenance and repair of motor vehicles
610236	Car Repairs Local Garage	45.20	Maintenance and repair of motor vehicles
610238	Car Wash Hand Or Automatic	45.20	Maintenance and repair of motor vehicles
610239	Exhaust Fitting Fast Fit Cent	45.20	Maintenance and repair of motor vehicles
610240	Brake Fitting Fast Fit Centre	45.20	Maintenance and repair of motor vehicles
610241	Wheel Alignment	45.20	Maintenance and repair of motor vehicles
620303	Self-Drive Van Hire	77.11	Renting and leasing of cars and light motor vehicles
620307	Self-Drive Car Hire Basic 24Hr	77.11	Renting and leasing of cars and light motor vehicles
620308	Minicab Fare For 2 Miles	49.32	Taxi operation
620315	Car Park Charges	52.21/9	Other service activities incidental to land transportation, n.e.c.
630359	Digital Development Per Print	74.20/3	Film processing
630361	Digital Development Per Print	74.20/3	Film processing

Item ID	Item Description	SIC	SIC Description
640207	Nightclub Entry-Saturday	56.30/1	Licensed clubs
640212	Theatre Adult Eves-Front Stlls	90.04	Operation of arts facilities
640219	Swimming Pool Adm Stnd Adult	93.11	Operation of sports facilities
640222	Exercise Class Upto 1Hr	93.11	Operation of sports facilities
640224	Ten-Pin Bowling Per Game	93.11	Operation of sports facilities
640226	Private Health Club Annual Fee	93.13	Fitness facilities
640232	Private Health Club Annual Fee	93.13	Fitness facilities
640233	Private Health Club Annual Fee	93.13	Fitness facilities
640240	Livery Charges Per Week	01.43	Raising of horses and other equines
640243	Soft Play Session Time Period	93.29	Other amusement and recreation activities n.e.c.
640406	Hotel 1 Night Price	55.10	Hotels and similar accommodation

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