RAISING THE STANDARD:

MINIMUM WAGES AND FIRM PRODUCTIVITY

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Raising the standard:

Minimum wages and firm productivity[1]

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Abstract

This paper exploits the introduction of the National Minimum Wage (NMW) in Britain and subsequent increases in the NMW to identify the effects of minimum wages on productivity. We find that the NMW increased average labour costs for companies that tend to employ low paid workers, both upon the introduction of the NMW and more recently following the Great Recession when many workers experienced pay freezes or wage cuts, but the NMW continued to rise. We find evidence to suggest that companies responded to these increases in labour costs by raising labour productivity. These labour productivity changes did not come about via a reduction in firms' workforce or via capital-labour substitution. Rather they were associated with increases in total factor productivity, consistent with organisational change, training and efficiency wage responses to increased labour costs from minimum wages.

Key words: minimum wage, labour costs, productivity, firm behaviour

JEL codes: J08, J31, J38, L25

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1. Introduction

Standard neo-classical theory predicts that minimum wage floors will reduce labour demand, but to date the empirical literature continues to debate whether in practice this actually occurs (see e.g. Neumark, Salas and Wascher, 2014; Ropponen, 2011). In the UK the large number of studies examining the employment impacts of the National Minimum Wage (NMW) suggest that labour demand has remained broadly unchanged despite this legislated rise in earnings for the lowest paid (see e.g. Stewart, 2004a,b; Dickens, Riley and Wilkinson, 2012; Dolton, Rosazza Bondibene and Wadsworth 2010, 2012; Dolton, Rosazza Bondibene and Stops, 2015).[2] This paper suggests that one of the reasons the employment impacts of the NMW have been muted is that firms managed to contain unit labour costs through increases in the efficiency of production.

There are several reasons to believe that minimum wages may increase labour productivity (see e.g. Metcalf, 2008). Because minimum wages increase the cost of labour, companies might move towards more capital-intensive forms of production and may implement organisational changes or offer training in an attempt to improve efficiency. Workers may exude more effort in return for a higher wage and this higher wage might also reduce employee turnover. Regardless these potential linkages and calls for research[3], the productivity effects of minimum wages remain relatively underexplored.

The effects of minimum wages on companies' productivity is now examined in this paper. We find that the NMW increased average labour costs for companies that on average paid low wages and that tended to employ low paid workers. These effects were evident upon the introduction of the NMW in 1999, but also during the 2000s when the NMW was rising faster than average wages and after the financial crisis of 2007/8, a time when many workers experienced nominal wage cuts (Gregg, Machin and Fernández-Salgrado, 2014) and the NMW would have prevented downward wage adjustments for some workers. Our analysis also finds a positive and significant association between the NMW and firm productivity. We report evidence that suggests increases in firms' average labour costs due to the NMW were accompanied by increases in firm labour productivity and total factor productivity,

[2] There is some evidence that the introduction of the NMW led to a reduction in the average hours worked of the lowest paid, particularly for men (Stewart & Swaffield, 2008), and led to a small reduction in employment retention for female part-time workers (Dickens, Riley and Wilkinson, forthcoming).
[3] Card and Krueger (1995).

both when the NMW was introduced and later when the NMW sustained nominal wages for low paid workers during and after the Great Recession.

In the period before the NMW was introduced we find no evidence of relative labour cost or productivity increases for firms that paid low wages. Nor do we find evidence of these effects for firms that paid better wages and that therefore should have been less affected by the NMW. Our results are not driven by any effects of the NMW on company exits. This paper therefore demonstrates a credible and significant positive link between minimum wages and firm productivity. Our findings are consistent with organisational change, efficiency wage and training responses to increased labour costs from minimum wages. The effects we find during and after the recession of 2008/9 underline the importance of downward wage flexibility in allowing productivity growth to falter further than it might otherwise have done.

We examine the impacts of the NMW following in broad terms the approach in Draca *et al.* (2005, 2011). This is a difference-in-differences approach applied to firm level data. The basic idea is to look at a group of firms that were more affected by the introduction of the NMW and its subsequent up-ratings (treatment group) than a comparison set of firms (control group). By more affected we mean where labour costs rose by more due to the imposition of and increases in the wage floor. Firms are allocated to treatment and control groups according to their average labour costs before the policy change. This quasi-experimental setting enables us to compare what happened to labour costs and productivity before and after the introduction/uprating of the NMW in low wage firms to what happened to these outcomes across the same period for a comparison group of firms whose labour costs were not affected by the NMW.

The paper is structured as follows. Section 2 reviews theory and evidence concerning minimum wages and productivity. Section 3 describes our research methods. Section 4 details the data we use. Section 5 then explores the relationship between the NMW and firms' average labour costs, which underpins our evaluation methodology. Results are presented in section 6. Section 7 summarises and concludes.

2. Theory and evidence on minimum wages and productivity

Minimum wages may increase labour productivity if they cause firms to substitute away from labour towards other factors of production such as capital because the relative cost of labour increases with the policy. They may also increase total factor productivity (TFP) if they induce firms to invest in unmeasured intangible assets such as training and organisational capital. For example, minimum wages may create incentives for firms to offer more training for their employees, particularly in monopsony labour markets where employers accrue rents from training provision (see e.g. the discussion in Acemoglu and Pischke, 1999), and implement organisational changes such as tighter human resource practices, increased performance standards at work, and better management practices (e.g. as in Lester, 1964; and as in the institutional theory of Brosnan, 2003; Kaufman, 2010; and Osterman, 2011). Alternatively, productivity increases may simply come about through increased worker effort in response to receiving a better wage (e.g. as in the shirking model of Shapiro & Stiglitz, 1984, or the gift exchange model of Akerlof, 1982).

A number of studies consider specific channels through which an increase in a minimum wage might lead to an increase in labour productivity. In a small case study of quick service restaurants in Georgia and Alabama, Hirsch, Kaufman and Zelenska (2011) analyse detailed payroll data and also survey managers and employees about human resource practices. The authors found no negative effect from the minimum wage increase on employment or hours worked. Managers reported that they could offset the labour cost increase through operational efficiencies and human resource practices. There is some evidence to suggest minimum wages might lead to increases in employer provided training. Arulampalam *et al.* (2004), using a difference-in-difference technique with longitudinal data found no indication that the introduction of the NMW in Britain reduced the training of affected workers and some evidence that it rather increased it. It is difficult to find direct evidence on the relationship between minimum wages and worker effort. Using a standard natural experiment design Georgiadis (2013) suggests that the NMW may have operated as an efficiency wage in the care homes sector, leading to a reduction in the level of worker supervision required. Experimental evidence (Owens and Kagel, 2010) points to a positive relationship between worker effort and minimum wages. Other research finds that minimum wages can reduce the high levels of job churning that characterize low-wage labour

markets. Dube, Lester and Reich (forthcoming) find that an increase in the minimum wage results in a reduction in turnover for teens and restaurant workers in the US. Also, Dube, Naidu and Reich (2007) find an increase in the average tenure of workers in limited-service restaurants in San Francisco.

These studies provide some evidence in support of a positive link between minimum wages and labour productivity, but fall short of demonstrating this link explicitly. A few studies explore the link between productivity and minimum wages directly using larger firm level datasets; the resulting evidence is not conclusive. In a research report for the Low Pay Commission (LPC) Galindo-Rueda and Pereira (2004) studied the impact of the introduction of the NMW on firms' productivity using linked employee-employer data. They link employees in the Annual Survey of Hours and Earnings (ASHE) (by firm identifier or by sector/region) to employers in the Annual Respondents Database (ARD) and find some evidence that low-paying service sector firms reduced hiring and increased labour productivity. A key limitation of their study is that because the ASHE only ever represents 1% of a particular firm's workers, it is only possible to derive meaningful indicators of exposure to the NMW for very large firms. Linking the ASHE to the ARD by sector and region instead allowed them to generate an indicator of exposure for the business population more generally but, by construction, this cannot be a very precise measure of a firm's exposure to the NMW.

Draca *et al.* (2005, 2011) got round this issue by using average labour costs to differentiate between firms that were likely to be affected by the NMW and firms that were not. Importantly, they (Draca *et al.*, 2011) show a correlation between average wages paid by the firm and the proportion of low-paid workers in a firm's workforce, suggesting that average wages are a means of identifying NMW exposure. The Draca *et al.* papers looked at companies who filed consolidated accounts to study the impact of the introduction of the NMW and very early upratings on firms' profits. They briefly consider other outcomes such as labour productivity. They find a positive association between the policy and this variable, although it is not significant.

In another research report to the LPC Rizov and Croucher (2011) use unconsolidated company accounts data and, applying a similar methodology to that in Draca *et al*. (2005, 2011), consider the entire period 1999-2009. They find that labour costs and productivity increased substantially more amongst low paying

firms (widespread across size bands and low pay sectors) than other firms in the post NMW period. However, they provide little supporting evidence to link these effects to the NMW.

In contrast with some previous studies, our sample includes firms in the whole market sector, comprising those most likely to be affected by the policy. We carry out a number of falsification tests to evaluate the robustness of our results and conduct sensitivity tests on alternate datasets. We consider the labour productivity effects of the NMW but also test whether the positive effects are driven by employment, capital labour substitution or total factor productivity changes. Finally, we examine these effects around the introduction of the NMW, which provides a useful experimental setting, but also examine whether firm behaviour in response to the NMW changed with the onset of recession in 2008. This is of particular interest as there is relatively little evidence available on the impact of the NMW on companies during a period of general economic weakness.

3. Methodology

To estimate the impact of the NMW on firm productivity we follow previous work in this area in applying a difference-in-differences estimator to firm-level data. This involves selecting a set of companies that were likely to be affected by the NMW, i.e. with a high share of low paid workers, from a period before the NMW change and then tracking the outcomes of these companies in the period after the policy change. The change over time in outcomes for companies in this treatment group is then compared to the same for a different set of companies (the control group) who are less likely to have been affected by the policy change. As in Draca *et al.* (2005, 2011) we use average labour costs to distinguish between treatment and control firms. Here it is crucial that average labour costs capture differences across firms in their exposure to the NMW. We return to this in section 5.

More formally we estimate the impact of the NMW in a standard difference-in-differences framework as shown in equation (1), where *p=0* refers to the period before the introduction/uprating of the NMW and *p=1* refers to the period after the introduction/uprating of the NMW.

$$y_{it} = constant + \propto LOWPAY_i + \beta D_{p=1} + \gamma LOWPAY_i * D_{p=1} + \delta X_{it} + \varepsilon_{it} \qquad (1)$$

In this set-up y_{it} is the outcome of interest for firm *i* at time *t*. $LOWPAY$ is a dummy variable equal to one if the firm is in the treatment group and zero otherwise. $D_{p=1}$ is a dummy variable equal to one if *p=1*, i.e. if the policy change has taken place, and zero otherwise. The X_{it} are controls for firm characteristics intended to net out differences between firms unrelated to the NMW. ε_{it} is an error term and the rest are parameters to be estimated. In this equation γ measures the impact of the introduction/uprating of the NMW on outcome y.

Crucial to the validity of this identification strategy is the underlying assumption that labour costs or productivity would have changed in the same way over time for the treatment and control firms in the absence of any NMW change. This is the common trends assumption. To evaluate the likelihood that γ in equation (1) is not picking up some other differential development in outcomes between the two groups of firm, unrelated to the NMW, we carry out a number of falsification tests. First, we estimate the same models on a pre-NMW period during the mid-1990s.[4] If we detect non-zero "policy effects" when the policy is not in place this casts doubt on the validity of the identification strategy. We are unable to carry out this falsification test with the ARD because these data are generally not available before 1997. Second, we choose two groups of firms from further up the distribution of average labour costs. These are chosen to be sufficiently high up the distribution that it is very unlikely that either group should be affected by the NMW. Again, if we detect "policy effects" for these groups that should be unaffected by the NMW then this casts doubt on the validity of the identification strategy.

We estimate the model in equation (1) using longitudinal information on companies in FAME (company accounts data), but also consider longitudinal information on enterprises in the ARD. The longitudinal information on firms is preferable to using repeated cross sections of firms because of the substantial heterogeneity in behaviour across firms and because selection for the treatment group *after* the NMW change, necessary in repeated cross section models, will depend on the effect of the NMW change on wages. We use a balanced panel as opposed to an unbalanced panel so that we compare the same firms before and after the policy change. This avoids biases arising from differences in business survival rates between the treatment and control firms. It does not avoid biases associated with potential NMW

[4] Draca *et al*. (2011) do this for wages and profits, but for a different sample of companies, a slightly different estimator, and not for the productivity outcomes that we consider here.

effects on business survival rates, although these are likely to be less severe in a balanced than an unbalanced panel. Therefore in assessing the robustness of our findings we examine the effect of the NMW on business and sample exit rates.

We estimate equation (1) for three separate periods: the introduction of the NMW up to 2002; an intermediate phase from 2003 to 2006 when annual increases in the NMW were relatively generous so that the "bite" of the NMW was rising (see Figure 1); and the years after the financial crisis from 2009 to 2012 when UK economic growth stagnated and nominal pay cuts and freezes became common. For the latter two periods that we consider, outcomes in the pre-policy years used to benchmark the difference in performance between the treatment and control groups after the policy change may of course be affected by the fact that the NMW is already in place. Therefore these impact estimates measure the effect on businesses of the change in the NMW between the before and after periods, rather than the effect of the NMW against a counterfactual of no NMW. During the recession period our impact estimate might also be interpreted as the difference between the impact of a given wage floor during a period of slow economic growth and its impact in a period of stable economic growth.[5]

4. Data

We use two business datasets for our analysis: FAME, a UK wide commercial dataset available from Bureau van Dijk, and the ARD for Great Britain, incorporating one of the key Office for National Statistics (ONS) business surveys used to inform aggregate estimates of production activity for the National Accounts. Here we briefly describe these data. Detailed definitions of the proxies we use for the outcomes of interest are reported in Appendix A.

Financial Analysis Made Easy (FAME)

FAME contains financial data on the population of UK registered companies. In comparison to other commercial datasets and/or ONS datasets, the availability of data covering the 1990s before the introduction of the NMW, which can be used to

[5] An alternative to estimating the impacts of the NMW over distinct time periods is to estimate equation (1) over a longer time period, tracking outcomes for the cohort of companies selected in the year prior to introduction of the NMW over this longer period. But, there are several reasons why this seems inappropriate. In particular, the sample will shrink substantially and will become less representative of the group of firms that are affected by the policy.

test the validity of the identification strategy and sample selection issues, the coverage of non-listed companies and longitudinal data for some small companies is useful. Drawbacks are that for many companies data items are missing, because there are no reporting requirements. Reporting requirements are particularly light for small companies.

We extract data on all companies who at some point during April 1 1993 and 31 March 2013 filed an account with Companies House. We retain for our main sample accounts that cover turnover, profits, employment, remuneration, and fixed capital; all of which we use to construct average labour cost and productivity related measures. In order to ensure that all active companies are included in the data at each point in time we extract this information from historical discs.[6]

FAME company data has previously been used by Draca *et al.* (2005, 2011) to study the impacts of the NMW on firm profitability (and other outcomes; in the 2011 version), using data to 2002; and by Rizov and Croucher (2011) to estimate the impact of the NMW on sectoral productivity for firms in different size groups, using data to 2009. Draca *et al.* (2005, 2011) focus on consolidated accounts only; Rizov and Croucher (2011) focus on unconsolidated accounts only. Consolidated accounts may be filed by companies that operate in a group. Stand-alone companies more typically file unconsolidated accounts. Thus, focusing on consolidated accounts only leaves out many of the smaller and medium size companies that tend to be more affected by the NMW (see discussion in Riley & Rosazza Bondibene, 2013). We include both consolidated and unconsolidated accounts in order to retain sufficient numbers of smaller companies, and use information on historical ownership structures to delete all subsidiary accounts (where a single parent has at least 50% control) to avoid double counting.

We exclude companies with less than 10 employees.[7] We focus on market sector companies in the non-agriculture and non-financial industries. Businesses in "non-market" service sectors such as education, health and social work are excluded. This

[6] This is important. The current vintage of the FAME data includes accounting records for the last decade for all companies that were active at some point during the last 4 years. Key company characteristics and ownership structures are not provided on an annual basis, but are provided as a recent snapshot. This means that the historical sample is biased towards surviving firms and makes it difficult to identify ownership structures at each point in time. Historical ownership structures are necessary in order to avoid double counting company activity (e.g. when companies file group accounts).

[7] In part for comparability with the ARD longitudinal information; data for these companies is often missing and particularly noisy.

is because inputs and outputs are thought not to be directly comparable in these sectors, making productivity analysis difficult to undertake. We also exclude businesses in the mining and quarrying, and utilities sectors (typically very large businesses with erratic patterns of output) and in the real estate sector, where output mostly reflects imputed housing rents.

Annual Respondents Database (ARD)

The Annual Respondents Database (ARD) is an establishment level business survey (or set of surveys) conducted by the ONS that is widely used in the study of firm behaviour and productivity analysis in the UK. It enables us to assess the robustness of the impact estimates obtained using FAME.

The ARD holds information on the nature of production in British businesses and is essentially a census of larger establishments and a stratified (by industry, region and employment size) random sample of establishments with less than 250 employees (SMEs). It covers businesses in the non-financial non-agriculture market sectors, including the service industries which include the main low-paying sectors, back to 1997. It is possible to use the data at both the establishment level and the enterprise level. We undertake our analysis at the level of the enterprise, which corresponds to the smallest legal unit in the ARD and hence the smallest unit with a decision making capacity. The enterprise is also more comparable than the establishment to the concept of a company that we use in FAME.

The sampling frame is the Inter-Departmental Business Register, a list of all UK incorporated businesses and other businesses registered for tax purposes (employee or sales taxes). Sampling probabilities in the ARD vary by size of firm. We focus on the sample of firms with 10 or more employees. Due to sampling rules there are very few of these micro businesses in the longitudinal data, despite their significant presence in the economy.

In using the longitudinal data we are unable to create a balanced panel of firms with annual observations as we do with the FAME data (except for large firms), because once surveyed (for two consecutive years) firms are excluded from the sample for at

least a year and may not be included thereafter. Instead, we create a panel of firms observed for two years at four year intervals[8].

Other data

The ARD and FAME financial information is published in current values. GVA deflators published by the ONS are used to construct real labour productivity values; these are available at the 2- and sometimes the 3-digit sector level. They are also used to construct a measure of real producer wages. Separately, in order to allocate firms to the treatment and control groups, we deflate average labour costs with the average earnings index, benchmarking low pay against average wages in the economy.

We use the Workplace Employment Relations Study (WERS) 1998, 2004 and 2011 to map the link between the proportion of NMW workers in the firm and the firm's average labour costs. For consistency across years, and as with the analysis of other datasets, we exclude all micro establishments.

We link the Annual Survey of Hours and Earnings (ASHE), which provides detailed and precise information on employees' wages for a sample of 1% of employees, by firm identifier to the ARD in order to evaluate the distribution of firm average labour costs for NMW workers and other workers.

5. NMW workers and firms' average labour costs

One of the main difficulties with firm-level analysis of NMW impacts is defining a suitable set of firms to allocate to the treatment group (and the control group). We follow Draca *et al.* (2005, 2011) and distinguish treated from untreated firms using firm average labour costs, exploiting the concentration of NMW workers in low average labour cost firms. We assume that those firms at the bottom of the distribution of average labour costs per employee are more exposed to the NMW and assign these to the treatment group. The control group is made up of firms from further up the distribution of average labour costs per employee.

While Draca *et al.* (2011) show that low paid workers were concentrated in firms with low average labour costs in 1998, right before the NMW was introduced, there

[8] We truncate the top and the bottom 1% of the labour productivity and total labour costs distribution within 1-digit industry sectors in each annual survey. We also truncate the longitudinal data to eliminate further outlying observations.

has been no research to verify whether this is also the case in later years. This is important in this paper, where we also wish to assess the productivity impacts of the NMW in later periods. Therefore we examine the link between minimum wage workers and workplace average labour costs in later cross sections of WERS. Furthermore, the WERS data holds information on relatively few employees per firm (up to 25 randomly selected employees), and so it is possible that some of the correlation detected between the prevalence of low paid workers and firm average labour costs, calculated from the sample of employees in the firm, arises by construction. For this reason we also look at the distribution of firm average labour costs for two groups of workers in the ASHE, employees paid the NMW and employees paid more than the NMW. If the distribution of employer average labour costs across NMW workers lies significantly to the left of the distribution of employer average labour costs across workers that are paid more than the NMW, then this further validates the use of firm average labour costs as a means of distinguishing between treatment and control firms.

In Figures 2-3 we plot the proportion of workers paid the NMW against the establishment's average annual wage in WERS 1998 and WERS 2011, illustrating thresholds £8,000, £10,000, £12,000 and £14,000 with vertical lines[9,10]. The vertical axis shows the proportion of workers paid below the NMW in the establishment. The horizontal axis shows the average annual wage at the workplace. This is divided in bins for 5 percentiles from lowest (left) to highest (right). These figures suggest that when the NMW was introduced minimum wage workers were concentrated in firms that paid low average wages and that this pattern has persisted over time. They also suggest that all 4 cut-offs shown provide a reasonable distinction between firms that are more or less likely to be affected by the NMW. In firms with average labour costs above £14,000 (in 1998 prices) less than 5% of workers are paid around the NMW. In what follows, we use £10,000, £12,000 and £14,000 thresholds to separate treatment and control firms.[11]

[9] In 2004 and 2011 we adjust the thresholds (£8,000, £10,000, £12,000 and £14,000) by the percentage increase in the average earnings index from the year of introduction to the year of analysis (equivalent to approximately £12,000, £15,000, £18,000 and £21,000 in 2008 prices). The Draca *et al*. papers use the £12,000 threshold to distinguish treatment and control firms.

[10] The 2004 figure is excluded for brevity. It looks very similar to the 1998 figure.

[11] We do not consider the £8,000 threshold as there are relatively few such businesses in the FAME and ARD panel data. This is due to light reporting requirements for small companies (defined as such by turnover, assets and/or employment) and the sampling stratification in the ARD.

Figures 4-5 use the ASHE linked to the ARD to show the distribution of average enterprise labour costs (deflated to 1998 values by annual changes in the National Minimum Wage) for two groups: employees paid at or below the minimum wage rate (blue line) and employees paid above the minimum wage rate (red line). Figures are shown for 1998 and 2010 for SMEs, but are similar for other years and the sample including larger employers.[12] For each of these figures we do a Kolmogorov-Smirnov test, which suggests that the distributions of average enterprise labour costs are different for these two groups of employees. In line with the WERS analysis, these figures confirm that workers paid at or below the NMW are concentrated in establishments with low average labour costs. This pattern again seems to persist over time.[13]

6. Results

Our main results are based on our analysis of FAME. These are reported for the three periods we consider in Table 1 (NMW introduction), Table 2 (Intermediate phase), and Table 3 (Recession period). We carry out falsification (placebo) tests (Tables 4-7), which are crucial to the interpretation of our reported impact estimates as being associated with the NMW. In Table 4 we report impact estimates from a pre-NMW period (historical placebo) and in Tables 5-7 from groups of high-paying firms that should largely be unaffected by the NMW (which we call the vertical placebo).

The dependent variable is specified in logs so that coefficients can be interpreted as the percentage change in the outcome of interest relative to the counterfactual (0.01 is equivalent to 1%). We report estimates based on OLS regression and robust regression, the latter of which adds less weight to outlying observations.

We show results for different cut-offs to define treated and control firms: £10,000, £12,000 and £14,000 per annum. These are then adjusted in line with the average earnings index (or the NMW) as we move further away from NMW introduction. In

[12] These figures are available in the research report upon which this paper is based (Riley & Rosazza Bondibene, 2015).

[13] We also run probit regressions looking at the probability of being an employee paid at or below the minimum wage. We find a statistically significant negative association between average enterprise labour costs and the probability of being a minimum wage worker. In other words, if a person works in an enterprise that pays its employees on average a low wage, then it is more likely that this person is paid at or below the NMW.

the FAME sample that we use these cut-offs correspond to broadly the 9th, 14th and 20th percentiles of the distribution of average labour costs, which at the lower end is relatively stable over time. The estimation sample that we consider includes treatment and control firms chosen in the year before the policy change (discussed above) that we can observe in each of the 3 years before the policy change and in each of the four years after.[14] In the regressions we include firm level controls: whether a firm is a start-up, young, files group accounts, exports, is foreign-owned. We also include industry specific time dummies so that performance levels are assessed relative to the industry-year mean. The number of firms in the treatment and control samples are shown for each cut-off and time period in Appendix B.[15]

It is important that we observe an increase in average labour costs associated with the NMW. If not, then there is no reason to associate any observed productivity impacts with the NMW. Looking in Tables 1-3 we see that average labour costs rose on average around 4% more for low-paying firms than for firms in the control group. These effects are evident in all NMW periods, but do not appear in the data in the period before the NMW (Table 4), nor do they appear between the groups further up the distribution of average labour costs (vertical placebo; Tables 5-7). In Tables 1-3 when we use robust regression it appears that the magnitude of the increase in labour costs associated with the NMW is diminishing over time, with the largest impacts upon introduction and the smallest impacts following the recession of 2008. This is consistent with the profile for the bite of the NMW shown in Figure 1. These results suggest that the quasi-experimental setting constructed here is able to capture NMW impacts on companies.

Productivity

We consider two measures of labour productivity: a (preferred) GVA based measure and a turnover based measure. Looking at the introduction phase (1999-2002) in Table 1 we find significant positive labour productivity impacts on both measures when we use the £12,000 and £14,000 cut-offs. In falsification tests we find no significant "policy effects" on labour productivity in the pre-NMW period (Table 4) or in the vertical placebo upon introduction of the NMW in the full sample (Table 5). These findings suggest that firms may have responded to the labour cost increases associated with the introduction of the NMW by increasing labour productivity. In

[14] We restrict the before period to 3 years because of data and policy constraints in the pre-NMW period. Results are very similar when we include (where possible) in the before period 4 years of data.
[15] Sample sizes are 7 times the number of treatment and control firms (because we track firms for 7 years).

the intermediate phase (Table 2), we observe significant positive effects on the GVA measure, but not the turnover measure. These are not apparent when we use robust regression. Moreover, we also find positive labour productivity effects further up the wage distribution (Table 6), casting doubt on the interpretation of these positive productivity effects as being related to the increases in the NMW over this period. In the recession phase we find positive labour productivity impacts on the GVA based measure of labour productivity; these are significant when we use robust regression (Table 3). Vertical placebo tests for this period (Table 7) give us some confidence in the identification strategy. We also find some evidence of labour productivity increases following recession on the turnover based measure, but these are also evident further up the wage distribution and therefore less easily interpreted as NMW effects.

The results so far suggest that low-paying firms may have responded to increases in labour costs that arose with the NMW by increasing labour productivity, upon introduction and also after the financial crisis. By definition, increases in labour productivity come about either from a rise in the capital intensity of production or from a rise in total factor productivity (TFP). Therefore, we also estimate "treatment effects" on capital labour ratios and total factor productivity. These are not significant in the pre-NMW period in the full sample (Table 4) as we would expect if our identification strategy is valid. When the NMW was introduced we find positive effects on GVA per head (as discussed above), which appear to have come about due to an increase in TFP rather than via an increase in the capital labour ratio (Table 1). These effects are absent in the vertical placebo (Table 5). During the intermediate phase the positive and significant treatment effects for GVA per head in some models are mirrored in positive and significant treatment effects for TFP (Table 2). But, as with GVA per head, we also find evidence of positive treatment effects for TFP for firms that should not be affected by the NMW (Table 6). The positive labour productivity effects that we find for the recession period (Table 3) are associated with increases in TFP rather than capital labour substitution. As at introduction falsification tests further up the distribution of labour costs (Table 7) support a minimum wage interpretation.

WE also check whether the labour productivity effects that we find for low-paying firms are associated with a reduction in employment. We do not find any statistically significant employment effects for during any of the NMW periods of interest (Tables 1-3). Falsification tests suggest that we are conducting a valid

experiment. We find no employment effects in the pre-NMW period (Table 4). We find a negative employment effect in some vertical placebo models (Tables 5-7), but generally these are not statistically significant.

Business exit rates and sample selection

In order to check whether the estimated productivity impacts are driven by a possible effect of the NMW on company exit, we assess differences in business exit rates between low- and less low-paying firms in Table 8. We also assess differences in *sample* exit rates between low- and less low-paying firms. The concern is that because reporting is related to business performance (e.g. size of turnover and employment) the sample that we use to evaluate NMW impacts may depend on the impacts of the NMW on business performance (in which case we would have a sample selection issue). This is investigated in Table 9.

In Table 8, we show difference-in-differences estimates of the impact of the NMW on company exit rates. In these models we compare four year exit rates for three cohorts of firms (treatment and controls) selected right before the introduction of the NMW (1998), before the generous increases of the mid-2000s (2002), or right at the outset of recession and stagnation in UK economic performance (2008) to four year exit rates for a cohort of firms selected in the pre-NMW period (1995). For each cohort we restrict the sample to those firms that report financial variables in the three years before the policy change (as in the analysis above).

We find no evidence of a change in exit rates for low-paying companies following the introduction of the NMW for any of the cut-offs that we consider. This is generally the case for later periods (the intermediate phase and recession period) too. There is a negative and significant effect (at the 10 per cent level) when we use the £14,000 threshold, but this disappears when we use alternative thresholds to define treatment and control firms. There is in any case no evidence that the NMW should have *increased* closure rates for low-paying (and low-productivity) companies, suggesting the productivity impacts we detect do not arise because of any effect of the NMW on firm exit.

In Table 9, we show difference-in-differences estimates of the impact of the NMW on *sample* exit rates. Firms may exit the sample if they close *or* if they fail to report financial information. Companies are not obliged to report all the information that we use to study the NMW and business performance. This opens up the possibility that selection for the sample for analysis is dependent on the impacts of the NMW,

which in turn could lead to biased estimates of NMW impacts on business outcomes. To see this, note that in the regression results above we consider a (balanced) panel of firms. This has the benefit of allowing us to compare the same set of firms before and after the policy intervention, and thus our estimates are not affected by any spurious changes in sample composition. This is generally useful given the heterogeneity of firms' performance. But, if the NMW affects business (or, in particular, sample) exit and entry rates, e.g. because of the link between reporting and business scale, then our estimates in the previous section are calculated only for the sample of firms that did not shrink/exit due to the NMW and that did not expand/enter due to the NMW and that therefore we observe.[16]

The scenario that causes most concern is one where the NMW causes firm performance to deteriorate and hence exit the sample. If this were the case then our estimates of NMW effects on performance in the balanced sample of firms will be biased upwards; i.e. it might look like the NMW improves performance in part because we end up ignoring those firms whose performance worsened because of the NMW. In Table 9, we do not find that sample exit rates increased following changes in the NMW.

Robustness checks using the ARD

We check whether we also observe these effects on labour costs and productivity associated with the NMW using other datasets. Our results based on the ARD are reported in Table 10 for all three periods of interest. Vertical placebo tests are also reported in this table (we are unable to estimate the historical placebo with the ARD).

The nature of the ARD data is such that we have fewer years in our data panel. In Table 11 we estimate these "ARD style" models using FAME and report in Table 12 the equivalent impact estimates from the pre-NMW period to check the validity of this alternate model.

In Table10, we use the ARD to estimate equation (1) for the following year combinations: (1997, 1998), (2001, 2002); (2001, 2002), (2005, 2006); (2007, 2008), (2011, 2012). For each pair the first two years refer to time p=0 in equation (1) and the last two years refer to time p=1. This is by necessity slightly different to the analysis of FAME where we observe firms for seven consecutive years. As in the

[16] These issues also arise in an unbalanced panel, where we have the additional complexity that sample composition is changing over time.

FAME analysis we use cut-offs at £10,000, £12,000 and £14,000 (1998 prices). In the (unweighted) ARD sample that we use here these cut-offs correspond to broadly the 14th, 20th and 27th percentiles of the distribution of average labour costs. Also as in the previous analysis we include in the regressions 2-digit industry controls interacted with year effects. Sample sizes are reported in Appendix B.

In Table10, we show that at the introduction of the NMW average labour costs increased more amongst our treatment group of low pay firms relative to firms that paid better wages. This pattern is evident for all thresholds and estimations methods. This lends some credibility to the identification strategy used to examine NMW impacts on other outcomes, which basically attributes the difference in changes in outcomes over time between lower and higher average labour cost businesses to the NMW. Although we include industry-year controls in the ARD analysis, it is important to bear in mind that there could be other influences on business outcomes over time that affect more and less low-pay companies differently. When we are unable to take these into account in the analysis these can bias our ARD estimates of NMW impacts. Indeed, vertical placebo tests (Table10) suggest that our estimates of increases in labour costs may be biased upwards, possibly due to some dynamic adjustment (mean reversion) effect (we cannot look at the pre-NMW period with the ARD). Note also that the ARD labour cost estimates are (in some cases substantially) larger in magnitude than the FAME estimates. This is at least in part due to the fact that we can include only a few years in the ARD panel. We illustrate this in Table 11 where we estimate these same "ARD style" models using the FAME data. When we do this we generally get larger impact estimates than when using the full longitudinal panel. Moreover, we do not pass falsification tests further up the distribution of labour costs (also in Table 11) and in the pre-NMW period (Table 12). We draw two conclusions. First, it is clearly necessary to recalibrate falsification tests with (even relatively minor) changes to the model (e.g. thresholds, controls, time periods covered) to check the validity of the identification strategy. This phenomenon is also evident in employee-level studies that consider the wage and employment effects of the NMW. Second, the effects we find with the "ARD style" model do almost certainly capture some element of dynamic adjustment and are likely biased upwards.

In Table 10 we also show ARD results for the intermediate and recession periods. As expected, we generally find that average labour costs per head increased amongst

low-paying firms (the treatment group) compared to less low-paying firms (the control group). The magnitude of these average labour cost effects is generally greatest upon introduction of the NMW and smallest during the recession. This pattern was also evident when we used robust regression on the FAME data in the previous section and is in line with changes in the NMW over time giving us some confidence that we are capturing some effects that are associated with the NMW.

We also estimate the difference in 4-year changes in labour productivity (GVA per head), as well as capital labour ratios, TFP, and employment between lower and higher average labour cost businesses, using the same methodology described above. As in our FAME analysis, the impact estimates in Table 10 suggest that the increases in labour costs associated with the NMW were associated with increases in labour productivity in all three periods considered and that these arose due to increases in efficiency (TFP) rather than capital labour substitution (capital labour ratio). We find some negative and significant employment coefficients, but these are not consistent across the specifications shown and are not statistically significant when we use robust regression.

Thus, although we identify some upward biases in the analysis that is possible with the ARD, the pattern and magnitude of the estimated impacts suggest that these effects are not wholly attributable to factors unrelated to the NMW and generally support the evidence obtained using FAME.

7. Conclusions

This paper contributes evidence to a much under-explored area of research on minimum wages. We study the impact of minimum wages on firm productivity using the natural experiment setting provided by the introduction and up-ratings to the UK NMW.

Analysing company accounts data we find evidence to suggest that the NMW led to increases in labour costs amongst low-paying firms upon introduction, but also with the above average earnings increases of the mid-2000s and after the recession when NMW upratings were modest but real average wages were falling and some workers experienced nominal pay cuts. Our results suggest that these labour cost increases amongst low-paying firms may have been met by increases in labour productivity.

Our findings do not suggest that these increases in labour productivity arose because of reductions in employment or via capital labour substitution. The evidence suggests that these labour productivity increases may have been associated with increases in efficiency of production. This finding is consistent with organisational change, training and efficiency wage responses to increased labour costs from the NMW. This is also consistent with a decrease in employee turnover from a rise in the minimum wage.

We provide a range of supportive evidence, which lends credibility to the experimental setting we use and the robustness of our results; including analysis of the location of low paid employees relative to firm average labour costs, falsification tests, analysis of selection issues and sensitivity tests using alternate data sources.

We cannot rule out that the labour productivity increases we find are associated with increases in average hours worked. This is because we cannot control for average hours worked at the firm level (only at the industry level, which we do). The available evidence is unclear about the effects of the NMW on average hours. Dickens, Riley and Wilkinson (forthcoming) find that employers may have shifted away from part-time workers towards full-time workers in response to the NMW, which would tend to increase average hours worked. Stewart & Swaffield (2008) find that minimum wage workers' hours decreased in response to the introduction of the NMW, which would tend to reduce average hours worked. Connolly and Gregory (2002) find no significant changes in hours worked by either full- or part-time women. But, the labour productivity increases we find are associated with increases in TFP rather than changes in the capital-labour ratio, which suggests that our findings may not be driven by changes in average hours. Another issue is that we do not have information on firm level prices, and thus it is possible that the productivity effects we find reflect increases in prices rather than productivity. However, the available evidence does not suggest that the price impacts of the NMW have been very significant (Draca *et al.*, 2005; Wadsworth, 2010). Thus we suggest this paper provides an important contribution to the study of minimum wages and labour market policy to support individuals in low paid work.

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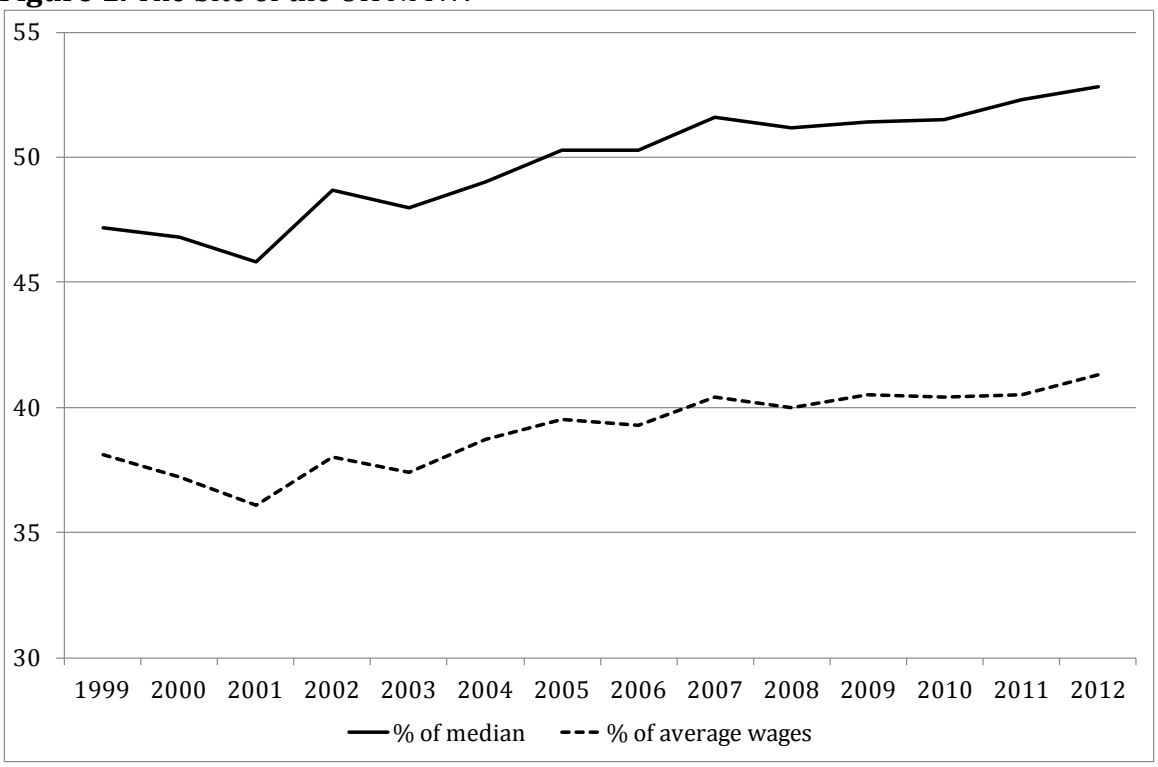
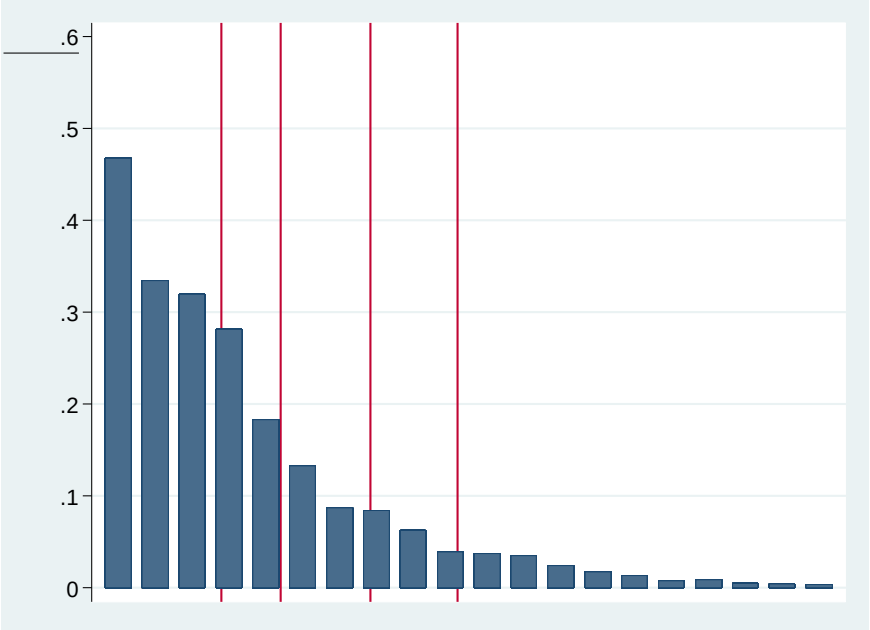
Figure 1. The bite of the UK NMW.



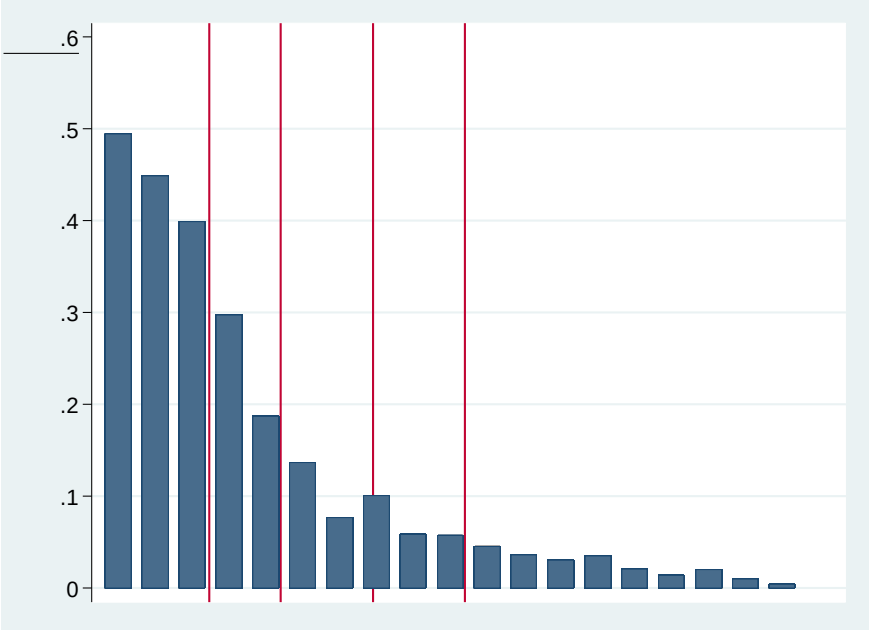
Source: Low Pay Commission Report 2015, Table 2.4; Authors' calculations.

Figure 2. Proportion of low-paid workers and establishment average wages. WERS1998.



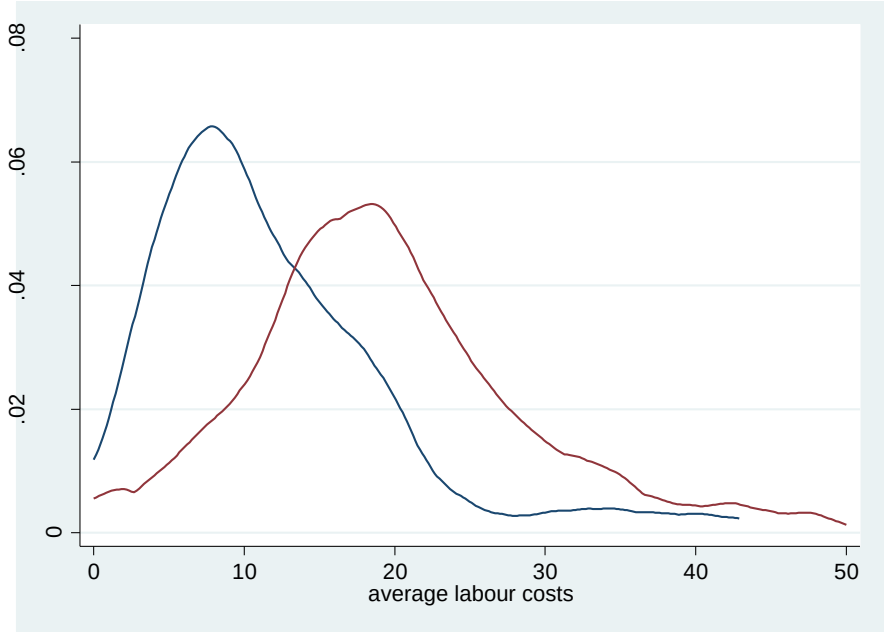
Source: WERS 1998. Authors' calculations.
Note: Vertical lines mark average wage thresholds £8,000, £10,000, £12,000 and £14,000 in 1998 prices.

Figure 3. Proportion of low-paid workers and establishment average wages. WERS2011.



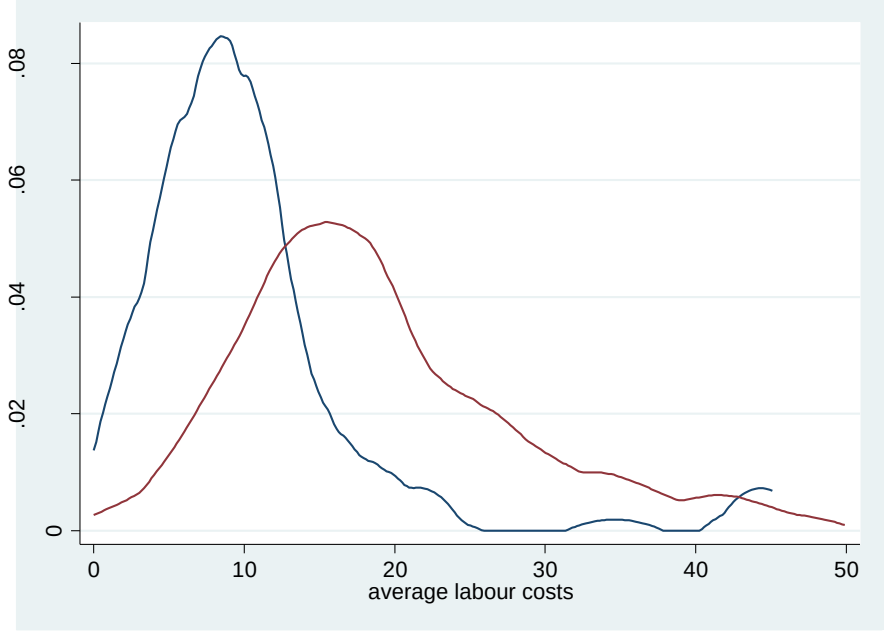
Source: WERS 2011. Authors' calculations.
Note: Vertical lines mark average wage thresholds £8,000, £10,000, £12,000 and £14,000 in 1998 prices.

Figure 4. Distribution of employer average labour costs for NMW and other employees, 1998, SMEs.



Source: ASHE linked to ARD. Authors' calculations.
Note: Not population weighted. SME enterprises have less than 250 employees.

Figure 5. Distribution of employer average labour costs for NMW and other employees, 2010, SMEs.



Source: ASHE linked to ARD. Authors' calculations.
Note: Not population weighted. SME enterprises have less than 250 employees.

Table 1. NMW introduction. Longitudinal panel models using FAME.

	OLS regression			Robust regression		
Threshold (1998 prices)	£10,000	£12,000	£14,000	£10,000	£12,000	£14,000
POLICY ON (OFF) PERIOD: **1999-2002** (1996-1998)						
Labour costs	0.039 ***	0.042 ***	0.036 ***	0.039 ***	0.036 ***	0.033 ***
	(0.014)	(0.011)	(0.009)	(0.010)	(0.008)	(0.007)
Labour productivity (GVA measure)	0.031	0.043 **	0.042 ***	0.028 *	0.037 ***	0.040 ***
	(0.022)	(0.018)	(0.014)	(0.016)	(0.013)	(0.012)
Labour productivity (turnover measure)	0.018	0.041 **	0.030 **	0.018	0.042 *	0.025
	(0.020)	(0.016)	(0.013)	(0.026)	(0.022)	(0.020)
Employment	0.019	-0.006	0.002	0.046	0.011	0.013
	(0.031)	(0.026)	(0.021)	(0.056)	(0.048)	(0.042)
Capital labour ratio	0.044	0.025	0.016	0.029	0.021	0.013
	(0.031)	(0.026)	(0.022)	(0.043)	(0.037)	(0.032)
Total factor productivity	0.014	0.032 *	0.033 **	0.018	0.027 *	0.032 ***
	(0.021)	(0.017)	(0.014)	(0.017)	(0.014)	(0.012)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; treatment and control groups selected on the basis of labour costs in the year to December 31 1998 and March 31 1999; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices) are excluded from the sample; balanced panel.

Table 2. Intermediate phase. Longitudinal panel models using FAME.

	OLS regression			Robust regression		
Threshold (1998 prices)	£10,000	£12,000	£14,000	£10,000	£12,000	£14,000
POLICY ON (OFF) PERIOD: **2003-2006** (2000-2002)						
Labour costs	0.048 ***	0.042 ***	0.036 ***	0.023 **	0.021 **	0.028 ***
	(0.018)	(0.014)	(0.011)	(0.011)	(0.009)	(0.008)
Labour productivity (GVA measure)	0.065 ***	0.046 **	0.021	0.005	0.003	0.010
	(0.025)	(0.021)	(0.017)	(0.018)	(0.015)	(0.013)
Labour productivity (turnover measure)	0.008	0.023	-0.005	-0.004	0.009	-0.008
	(0.020)	(0.019)	(0.016)	(0.029)	(0.025)	(0.021)
Employment	-0.018	-0.025	0.008	-0.031	-0.032	0.008
	(0.030)	(0.027)	(0.024)	(0.062)	(0.053)	(0.045)
Capital labour ratio	-0.014	0.008	-0.007	0.004	0.073	0.029
	(0.038)	(0.033)	(0.028)	(0.055)	(0.047)	(0.041)
Total factor productivity	0.074 ***	0.048 **	0.025	0.028	0.011	0.015
	(0.027)	(0.022)	(0.018)	(0.020)	(0.017)	(0.014)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; treatment and control groups selected on the basis of labour costs in the year to December 31 2002 and March 31 2003; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices) are excluded from the sample; balanced panel.

Table 3. Recession period. Longitudinal panel models using FAME.

	OLS regression			Robust regression		
Threshold (1998 prices)	£10,000	£12,000	£14,000	£10,000	£12,000	£14,000
POLICY ON (OFF) PERIOD: **2009-2012** (2006-2008)						
Labour costs	0.059 ***	0.043 ***	0.022 **	0.028 **	0.014	0.014 *
	(0.017)	(0.013)	(0.010)	(0.012)	(0.009)	(0.007)
Labour productivity (GVA measure)	0.013	0.030	0.021	0.031 *	0.027 *	0.028 **
	(0.025)	(0.019)	(0.016)	(0.019)	(0.015)	(0.013)
Labour productivity (turnover measure)	0.051 **	0.033 *	0.013	0.034	0.017	-0.004
	(0.023)	(0.018)	(0.015)	(0.031)	(0.025)	(0.022)
Employment	-0.005	0.016	0.001	-0.039	0.005	-0.012
	(0.032)	(0.027)	(0.023)	(0.064)	(0.052)	(0.046)
Capital labour ratio	-0.030	-0.020	0.004	-0.019	-0.020	-0.004
	(0.041)	(0.036)	(0.030)	(0.059)	(0.049)	(0.042)
Total factor productivity	0.019	0.032 *	0.016	0.036 *	0.034 **	0.030 **
	(0.024)	(0.020)	(0.016)	(0.021)	(0.017)	(0.015)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; treatment and control groups selected on the basis of labour costs in the year to December 31 2008 and March 31 2009; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices) are excluded from the sample; balanced panel.

Table 4. Falsification. Pre-NMW phase. Longitudinal panel models using FAME.

	OLS regression			Robust regression		
Threshold (1998 prices)	£10,000	£12,000	£14,000	£10,000	£12,000	£14,000
POLICY ON (OFF) PERIOD: **1996-1999** (1993-1995)						
Labour costs	0.005	-0.003	0.011	0.004	-0.008	0.008
	(0.017)	(0.013)	(0.010)	(0.011)	(0.008)	(0.007)
Labour productivity (GVA measure)	0.000	-0.002	0.001	0.021	0.004	0.014
	(0.022)	(0.017)	(0.014)	(0.017)	(0.013)	(0.011)
Labour productivity (turnover measure)	-0.012	-0.005	0.005	-0.005	0.003	0.007
	(0.017)	(0.015)	(0.013)	(0.027)	(0.023)	(0.020)
Employment	-0.009	-0.002	-0.005	-0.010	-0.019	-0.019
	(0.025)	(0.022)	(0.018)	(0.057)	(0.048)	(0.041)
Capital labour ratio	-0.033	-0.034	-0.016	-0.014	-0.019	0.004
	(0.032)	(0.027)	(0.023)	(0.046)	(0.038)	(0.033)
Total factor productivity	0.009	0.007	0.004	0.016	0.010	0.014
	(0.022)	(0.017)	(0.014)	(0.017)	(0.014)	(0.012)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; treatment and control groups selected on the basis of labour costs in the year to December 31 1995 and March 31 1996; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices) are excluded from the sample; balanced panel.

Table 5. Falsification. NMW introduction vertical placebo. FAME.

	OLS regression			Robust regression		
Threshold (1998 prices)	£20,000	£22,000	£24,000	£20,000	£22,000	£24,000
POLICY ON (OFF) PERIOD: **1999-2002** (1996-1998)						
Labour costs	-0.002	-0.005	-0.005	0.001	0.001	0.003
	(0.006)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)
Labour productivity (GVA measure)	-0.001	0.002	-0.001	0.013	0.007	0.003
	(0.011)	(0.011)	(0.012)	(0.011)	(0.009)	(0.009)
Labour productivity (turnover measure)	-0.001	0.000	0.001	0.009	0.003	0.001
	(0.011)	(0.009)	(0.010)	(0.020)	(0.017)	(0.017)
Employment	-0.008	-0.018	-0.025 *	-0.014	-0.027	-0.034
	(0.015)	(0.013)	(0.014)	(0.040)	(0.033)	(0.033)
Capital labour ratio	0.031	0.030	0.046 **	0.030	0.034	0.047
	(0.022)	(0.020)	(0.021)	(0.035)	(0.030)	(0.030)
Total factor productivity	-0.007	-0.005	-0.010	0.001	-0.006	-0.007
	(0.012)	(0.011)	(0.012)	(0.012)	(0.010)	(0.010)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; treatment and control groups selected on the basis of labour costs in the year to December 31 1998 and March 31 1999; firms selected for neither the treatment nor the control group (with labour costs below £18,000 or above £32,000 in 1998 prices) are excluded from the sample; balanced panel.

Table 6. Falsification. Intermediate phase vertical placebo. FAME.

	OLS regression			Robust regression		
Threshold (1998 prices)	£20,000	£22,000	£24,000	£20,000	£22,000	£24,000
POLICY ON (OFF) PERIOD: **2003-2006** (2000-2002)						
Labour costs	0.007	0.007	0.006	0.007	0.010 **	0.006
	(0.008)	(0.007)	(0.007)	(0.006)	(0.005)	(0.005)
Labour productivity (GVA measure)	0.012	0.021	0.024 *	0.014	0.029 ***	0.030 ***
	(0.016)	(0.014)	(0.014)	(0.012)	(0.010)	(0.010)
Labour productivity (turnover measure)	0.014	0.007	0.010	0.022	0.015	0.007
	(0.014)	(0.013)	(0.013)	(0.023)	(0.019)	(0.019)
Employment	-0.038 *	-0.018	-0.001	-0.047	-0.035	-0.009
	(0.021)	(0.018)	(0.018)	(0.043)	(0.037)	(0.037)
Capital labour ratio	0.046 *	0.066 ***	0.073 ***	0.026	0.045	0.060
	(0.028)	(0.025)	(0.027)	(0.046)	(0.039)	(0.040)
Total factor productivity	0.004	0.009	0.018	0.011	0.022 *	0.025 **
	(0.017)	(0.014)	(0.015)	(0.015)	(0.012)	(0.013)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; treatment and control groups selected on the basis of labour costs in the year to December 31 2002 and March 31 2003; firms selected for neither the treatment nor the control group (with labour costs below £18,000 or above £32,000 in 1998 prices) are excluded from the sample; balanced panel.

Table 7. Falsification. Recession period vertical placebo. FAME.

	OLS regression			Robust regression		
Threshold (1998 prices)	£20,000	£22,000	£24,000	£20,000	£22,000	£24,000
POLICY ON (OFF) PERIOD: **2009-2012** (2006-2008)						
Labour costs	0.010	0.008	0.001	0.009	0.006	0.005
	(0.008)	(0.008)	(0.007)	(0.007)	(0.005)	(0.005)
Labour productivity (GVA measure)	0.005	0.003	-0.014	0.001	0.005	0.011
	(0.018)	(0.016)	(0.016)	(0.014)	(0.011)	(0.011)
Labour productivity (turnover measure)	0.029 *	0.023 *	0.002	0.030	0.021	0.008
	(0.016)	(0.013)	(0.013)	(0.025)	(0.021)	(0.021)
Employment	-0.045 **	-0.005	0.002	-0.031	-0.002	0.001
	(0.022)	(0.019)	(0.018)	(0.044)	(0.037)	(0.037)
Capital labour ratio	0.055	0.029	-0.005	0.044	0.032	-0.018
	(0.033)	(0.029)	(0.030)	(0.056)	(0.047)	(0.047)
Total factor productivity	-0.006	-0.005	-0.017	-0.005	-0.006	0.000
	(0.019)	(0.017)	(0.018)	(0.017)	(0.014)	(0.014)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; treatment and control groups selected on the basis of labour costs in the year to December 31 2008 and March 31 2009; firms selected for neither the treatment nor the control group (with labour costs below £18,000 or above £32,000 in 1998 prices) are excluded from the sample; balanced panel.

Table 8. Difference-in-difference estimates of business exit rates. FAME.

	Probit regression					
Threshold (1998 prices)	£10,000		£12,000		£14,000	
1998	-0.001	(0.015)	-0.011	(0.012)	-0.012	(0.010)
2002	0.003	(0.017)	0.006	(0.015)	-0.001	(0.012)
2008	-0.012	(0.017)	-0.019	(0.013)	-0.020 *	(0.012)

Notes: Probability of business exit within next 4 years; marginal effects reported; standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; treatment and control groups (as well as size or sector sub-group) selected on the basis of labour costs 1995, 1998, 2002 or 2008; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices) are excluded from the sample; balanced panel (firms reporting accounts information in the year of selection and in the two years prior to selection); SMEs have less than 250 employees; low pay sectors (broadly) as defined by the Low pay Commission.

Table 9. Difference-in-difference estimates of sample exit rates. FAME.

	Probit regression					
Threshold (1998 prices)	£10,000		£12,000		£14,000	
1998	-0.026	(0.031)	-0.013	(0.026)	-0.044 *	(0.023)
2002	-0.036	(0.034)	0.000	(0.028)	-0.013	(0.025)
2008	0.046	(0.036)	0.014	(0.030)	0.002	(0.027)

Notes: Probability of sample exit within next 4 years; marginal effects reported; standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; treatment and control groups (as well as size or sector sub-group) selected on the basis of labour costs 1995, 1998, 2002 or 2008; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices) are excluded from the sample; balanced panel (firms reporting accounts information in the year of selection and in the two years prior to selection); SMEs have less than 250 employees; low pay sectors (broadly) as defined by the Low pay Commission.

Table 10. Longitudinal panel models using the ARD.

	OLS regression				Robust regression			
Threshold (1998 prices)	£10,000	£12,000	£14,000	*£20,000 (VP)*	£10,000	£12,000	£14,000	*£20,000 (VP)*
POLICY ON (OFF) PERIOD: **2001-2002** (1997-1998)								
Labour costs	0.106 ***	0.099 ***	0.098 ***	*0.015*	0.100 ***	0.088 ***	0.098 ***	*0.022 ****
Labour productivity (GVA measure)	0.079 **	0.056 *	0.095 ***	*0.046*	0.084 **	0.065 **	0.092 ***	*0.024*
Employment	-0.096 **	-0.127 ***	-0.120 ***	*-0.049 **	-0.119	-0.118	-0.099	*-0.037*
Capital labour ratio	0.052	0.087	0.026	*0.162 ****	0.007	-0.028	-0.078	*0.095*
Total factor productivity	0.072	0.038	0.092 ***	*-0.009*	0.087 **	0.100 ***	0.115 ***	*-0.034*
POLICY ON (OFF) PERIOD: **2005-2006** (2001-2002)								
Labour costs	0.066 ***	0.070 ***	0.059 ***	*-0.003*	0.074 ***	0.080 ***	0.064 ***	*0.007*
Labour productivity (GVA measure)	0.105 ***	0.077 **	0.061 **	*-0.005*	0.074 **	0.051 *	0.028	*-0.007*
Employment	-0.108 ***	-0.089 ***	-0.104 ***	*0.040 **	-0.113	-0.110	-0.113	*0.030*
Capital labour ratio	-0.020	-0.027	-0.011	*0.010*	-0.093	-0.061	-0.012	*0.036*
Total factor productivity	0.111 **	0.085 **	0.063 **	*-0.012*	0.083 **	0.072 **	0.038	*-0.025*
POLICY ON (OFF) PERIOD: **2011-2012** (2007-2008)								
Labour costs	0.070 ***	0.071 ***	0.039 ***	*0.000*	0.088 ***	0.075 ***	0.033 **	*0.008*
Labour productivity (GVA measure)	0.089 **	0.091 **	0.006	*0.016*	0.083 **	0.122 ***	0.046	*-0.007*
Employment	-0.012	-0.007	-0.021	*-0.037*	-0.035	-0.028	-0.030	*-0.012*
Capital labour ratio	0.081	0.063	0.003	*-0.041*	0.039	-0.007	-0.008	*-0.020*
Total factor productivity	0.053	0.064	0.004	*0.029*	0.076 *	0.085 **	0.031	*0.027*

Notes: (VP = vertical placebo); standard errors clustered by firm; statistical significance ***1%, **5%, *10%; 2-digit industry-year effects included; treatment and control groups selected on the basis of labour costs in 1998, 2002 or 2008; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices; or in the case of the vertical placebo with labour costs below £18,000 or above £32,000 in 1998 prices) are excluded from the sample; balanced panel.

Table 11. Longitudinal panel models using FAME (similar to ARD model).

	OLS regression				Robust regression			
Threshold (1998 prices)	£10,000	£12,000	£14,000	*£20,000 (VP)*	£10,000	£12,000	£14,000	*£20,000 (VP)*
POLICY ON (OFF) PERIOD: **2001-2002** (1997-1998)								
Labour costs	0.080 ***	0.073 ***	0.059 ***	*0.013 ****	0.051 ***	0.047 ***	0.045 ***	*0.018 **
Labour productivity (GVA measure)	0.089 ***	0.087 ***	0.072 ***	*0.010*	0.048 **	0.062 ***	0.062 ***	*0.034 **
Employment	0.000	-0.025	-0.015	*-0.013*	0.019	-0.013	-0.007	*-0.016*
Capital labour ratio	0.063 *	0.040	0.036	*0.043*	0.029	0.023	0.020	*0.042*
Total factor productivity	0.068 ***	0.072 ***	0.058 ***	*0.002*	0.034	0.046 **	0.048 ***	*0.019*
POLICY ON (OFF) PERIOD: **2005-2006** (2001-2002)								
Labour costs	0.098 ***	0.080 ***	0.067 ***	*0.016 ****	0.050 ***	0.041 ***	0.051 ***	*0.018 **
Labour productivity (GVA measure)	0.100 ***	0.067 **	0.048 **	*0.011*	0.029	0.023	0.028 *	*0.009*
Employment	-0.058	-0.042	-0.009	*-0.052 **	-0.083	-0.054	-0.007	*-0.067*
Capital labour ratio	0.030	0.031	0.012	*0.026*	0.069	0.121 *	0.066	*0.006*
Total factor productivity	0.102 ***	0.067 **	0.049 **	*0.003*	0.041	0.014	0.020	*0.001*
POLICY ON (OFF) PERIOD: **2011-2012** (2007-2008)								
Labour costs	0.095 ***	0.065 ***	0.041 ***	*0.015 ****	0.044 ***	0.019 *	0.025 ***	*0.011*
Labour productivity (GVA measure)	0.025	0.042 *	0.031	*0.002*	0.048 *	0.041 **	0.041 **	*0.003*
Employment	0.017	0.019	-0.013	*-0.054 ****	-0.039	-0.011	-0.043	*-0.028*
Capital labour ratio	0.001	0.009	0.016	*0.076 ****	0.038	0.027	0.021	*0.071*
Total factor productivity	0.025	0.040	0.023	*-0.012*	0.030	0.039 *	0.037 *	*-0.004*

Notes: (VP = vertical placebo); standard errors clustered by firm; statistical significance ***1%, **5%, *10%; 2-digit industry-year effects included; treatment and control groups selected on the basis of labour costs in 1998, 2002 or 2008; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices; or in the case of the vertical placebo with labour costs below £18,000 or above £32,000 in 1998 prices) are excluded from the sample; balanced panel.

Table 12. Falsification. Pre-NMW phase. Longitudinal panel models using FAME (similar to ARD model).

	OLS regression			Robust regression		
Threshold (1998 prices)	£10,000	£12,000	£14,000	£10,000	£12,000	£14,000
POLICY ON (OFF) PERIOD: **1996-1999** (1993-1995)						
Labour costs	0.047 **	0.035 **	0.041 ***	0.040 ***	0.021 *	0.030 ***
Labour productivity (GVA measure)	0.036	0.044 **	0.037 **	0.057 ***	0.042 **	0.044 ***
Employment	-0.035	-0.028	-0.036 *	-0.042	-0.045	-0.051
Capital labour ratio	0.002	-0.013	0.000	0.022	-0.002	0.019
Total factor productivity	0.036	0.048 **	0.036 **	0.043 *	0.044 **	0.043 ***

Notes: Standard errors clustered by firm; statistical significance ***1%, **5%, *10%; 2-digit industry-year effects included; treatment and control groups selected on the basis of labour costs in 1995; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices) are excluded from the sample; balanced panel.

APPENDIX A: Proxies for the outcomes of interest

FAME:

- *Average wages:* remuneration/employment
- *Labour productivity:* we examine two measures: turnover/employment and (remuneration + profits)/employment; the latter of these is a proxy for a gross-value added measure of labour productivity and is our preferred measure; the turnover based measure is used for comparability to previous studies that use FAME (turnover includes GVA and material costs).
- *Employment:* number of employees
- *Capital labour ratio:* fixed assets/employment
- *log TFP (total factor productivity):* log ((remuneration + profits)/employment) - (1-α)log (capital labour ratio), where α is the firm average labour share over the relevant time period.
- *Company exit:* exit dummy coded to unity for time periods after the last observed filing date

ARD:

- *Average wages:* total labour cost[17] /employment
- *Labour productivity:* GVA at factor costs/employment
- *Employment:* number of employees[18]
- *Capital labour ratio:* Plant & Machinery capital stock[19]/employment
- *log TFP (total factor productivity):* log (labour productivity) - (1-α)log (capital labour ratio), where α is the industry average labour share over the relevant time period.

[17] This represents amounts paid during the year to employees. This includes all overtime payments, bonuses, commissions, payments in kind, benefits in kind, holiday pay, employer's national insurance contributions, payments into pension funds by employers and redundancy payments less any amount reimbursed for this purpose from government sources. No deduction is made for income tax or employee's national insurance contributions etc. Payment to working proprietors, travelling expenses, lodging allowances, etc are excluded (ABI, Background Information, Archive Data).

[18] We use indicative employment information available in the sampling frame as we do not have a consistent series of year average or point in time employment estimates for surveyed businesses.

[19] These were made available by Richard Harris. The methodology underlying the construction of these is described in Harris (2005) "Deriving Measures of Plant-level Capital Stock in UK Manufacturing, 1973-2001", Report for the Department of Trade & Industry.

APPENDIX B: Sample sizes of the FAME and ARD regression analysis

Table B1. Sample sizes FAME.

Policy		Threshold £10,000		£12,000		£14,000	
OFF	ON	T	C	T	C	T	C
1993-1995	1996-1999	301	2057	513	1845	794	1564
1996-1998	1999-2002	352	2035	557	1830	845	1542
2000-2002	2003-2006	300	1724	470	1554	704	1320
2006-2008	2009-2012	267	1763	466	1564	726	1304

Note: These are the numbers of firms in each group. Firms are observed for 7 consecutive years and hence the number of observations is 7 times large than the numbers reported here.

Table B2. Sample sizes FAME. Vertical placebo.

Policy		Threshold £20,000		£22,000		£24,000	
OFF	ON	T	C	T	C	T	C
1996-1998	1999-2002	592	2180	1118	1654	1606	1166
2000-2002	2003-2006	497	1847	955	1389	1401	943
2006-2008	2009-2012	483	1784	942	1325	1342	925

Note: These are the numbers of firms in each group. Firms are observed for 7 consecutive years and hence the number of observations is 7 times large than the numbers reported here.

Table B3. Sample sizes ARD.

Policy		Threshold £10,000		£12,000		£14,000		*£20,000 (VP)*	
OFF	ON	T	C	T	C	T	C	*T*	*C*
1997-1998	2001-2002	356	1083	509	930	697	742	*340*	*920*
2001-2002	2005-2006	406	1339	597	1148	835	910	*396*	*1006*
2007-2008	2011-2012	406	967	555	818	741	632	*281*	*750*

Note: These are the numbers of firms in each group. Firms are observed for 4 years and hence the number of observations is 4 times large than the numbers reported here.