

# THE IMPACT OF THE NATIONAL MINIMUM WAGE ON FIRM BEHAVIOUR DURING RECESSION<sup>1</sup>

Rebecca Riley and Chiara Rosazza Bondibene

National Institute of Economic and Social Research

February 2013

Report to the Low Pay Commission

## Abstract

This paper examines the impact of the National Minimum Wage (NMW) on a range of outcomes for low-paying companies in the UK. We distinguish between the impacts of the NMW on small and larger firms and on firms in the low-paying sectors. We examine how these effects have changed since the introduction of the NMW in 1999. We find that upon introduction the NMW increased average labour costs for low-paying companies. Since then, its effects on companies' labour costs have been more muted. As in some previous studies, we find evidence to suggest that companies may have adjusted to the increases in labour costs as a result of the NMW by raising labour productivity and by reducing profitability. We find no robust evidence to suggest that the NMW has changed average employment or investment rates for these companies. Nor do we find robust evidence to suggest that the NMW has had a detrimental impact on firm outcomes since the financial crisis and the recession of 2008.

---

<sup>1</sup> *Disclaimer: This work contains statistical data which is Crown Copyright; it has been made available by the Office for National Statistics (ONS) through the Secure Data Service (SDS) and has been used by permission. Neither the ONS or SDS bear any responsibility for the analysis or interpretation of the data reported here. This work uses research datasets which may not exactly reproduce National Statistics aggregates.*

Acknowledgements: The authors thank Tim Butcher, Gerry Franks, Stephen Machin, and Richard Welpton for comments and discussion.

Correspondence: National Institute of Economic and Social Research, 2 Dean Trench Street, Smith Square, London SW1P 3HE; Tel.: +44-207-222-7665; fax: +44-207-654-1900

E-mail: r.riley@niesr.ac.uk; c.rosazza\_bondibene@niesr.ac.uk

## EXECUTIVE SUMMARY

This study for the Low Pay Commission (LPC) analyses the impact of the National Minimum Wage (NMW) on UK businesses, considering outcomes such as productivity, profits, investment, employment and the probability of exit.

Specifically, the study aims to build evidence of relevance in answering the following questions:

- How has the NMW affected the behaviour of smaller and larger firms and firms in the low paying sectors?
- Has the impact of the NMW on firm behaviour changed since the 2008 recession?
- How has access to finance interacted with minimum wage policy in influencing firms' behaviour during recession?

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. We assume that 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. A firm's average labour costs per employee may be low because it pays relatively low hourly wages and/or because it employs a large share of part-time workers. The control group is made up of firms from further up the distribution of average labour costs per employee. We then evaluate the impact of minimum wages by comparing outcomes for firms who are more exposed to the NMW to outcomes for firms who are less exposed to the NMW, before and after NMW introduction; or before and after the onset of recession.

We use two business datasets: the record of UK company accounts provided in Financial Analysis Made Easy (FAME) and the Annual Respondents Database (ARD) maintained by the Office for National Statistics (ONS). The FAME data that we use covers the period 1994-2010, including company accounts for three years post-recession. The time span of the ARD data used in this report is more restrictive, covering the period 1997-2007. We distinguish between NMW impacts on small firms (less than 50 employees) and NMW impacts on firms with 50 or more employees. We also estimate impacts for firms operating in the low paying sectors (as defined by the LPC).

Looking at the three years following the introduction of the NMW we find that the NMW increased average labour costs for low-pay companies. The magnitude of this increase varies according to the identification strategy and dataset used, but usually lies between 3 and 6 per cent for low-pay companies in the low pay sectors. Central estimates are larger for smaller companies and for companies in the low pay sectors, although these are not necessarily statistically different to estimates for large companies and companies in all sectors. Perhaps unsurprisingly, our estimates of the effect of the introduction of the NMW on average labour costs per employee for low-pay firms are somewhat smaller than estimates of its effect on hourly wages for low-pay employees (see for example Dickens, Riley and Wilkinson (2012) and Swaffield (2009)).

On balance, we find no effects of the introduction of the NMW on employment. We find some evidence indicating that the introduction of the NMW was associated with an increase in labour productivity. Depending on the identification strategy used these effects are apparent for small and larger firms and in both datasets. Using FAME data we find some evidence that the introduction of the NMW was associated with a reduction in profitability (price-cost margins) amongst small firms in the low pay sectors, but these effects are not particularly robust. We find no robust evidence to indicate that the NMW changed the investment behaviour of low-paying firms. Finally, looking at these initial years of the NMW we find no evidence to suggest that the introduction of the NMW led to a change in the exit rate of companies.

Looking at the data to 2007 we find that most of the increase in average labour costs associated with the NMW occurred upon introduction rather than with subsequent upratings. Again this is consistent with the evidence on wage impacts using employee data derived elsewhere. We find no robust evidence to suggest that the NMW changed employment. Again we find some evidence to suggest that firms may have responded to the NMW policy by raising labour productivity, but evaluated over this longer period these effects are less strong. Looking at the data to 2007 we find evidence of a reduction in profitability associated with the NMW. These effects are apparent using both datasets. The estimated labour productivity and profitability effects are sensitive to the definition of the treatment and control groups. Again, we find no evidence of a change in investment behaviour associated with the NMW.

Focusing explicitly on the years since the onset of recession we compare firm outcomes over the period 2008-2010 to firm outcomes 2005-2007. The NMW impacts estimated

using this approach can be interpreted as the impacts of the NMW upratings since the 2008 recession conditional on there being a national wage floor and/or as the differential impact of a national wage floor in a period of falling rather than stable employment. We find that the average labour costs of low-paying firms (firms in our treatment group) did not rise any faster than the average labour costs of firms in our control group over this period. This is not surprising given the very small NMW upratings during these years. We still find some evidence to suggest that firms that are more likely to have been affected by the NMW have on average experienced stronger labour productivity growth. But, these productivity associations with the NMW are a lot less apparent than they are for the earlier years of the NMW policy. We find no consistent evidence to suggest that the NMW affected employment, profitability, investment or the rate of company exit differently in the years since 2008 than in previous years. Our analysis of the NMW from 2008 onwards is based on the FAME data alone.

In carrying out falsification tests over the period before the introduction of the NMW we typically do not find significant differences between changes in outcomes for low-pay firms (our treatment group) and for firms that pay slightly better wages (our control group). This allows us to be more confident that the impacts we identify during the NMW period are associated with the NMW policy.

In a more exploratory analysis we include within our models of investment simple indicators of the extent to which firms relied on external financing before the financial crisis. In these augmented models we find no robust evidence of an interaction between the impacts of the NMW on the investment behaviour of small companies over the period 2008-2010 and firms' reliance on external finance.

## 1. Introduction

This study for the Low Pay Commission (LPC) analyses the impact of the National Minimum Wage (NMW) on UK businesses. Business outcomes considered include labour costs, productivity, profitability, investment, employment and the probability of exit. Specifically, the study aims to build evidence of relevance in answering the following questions:

- How has the NMW affected the behaviour of smaller and larger firms and firms in the low paying sectors?
- Has the impact of the NMW on firm behaviour changed since the 2008 recession?
- How has access to finance interacted with minimum wage policy in influencing firms' behaviour during recession?

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 in the main 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 wages potentially rose by more due to the imposition of the wage floor. This quasi-experimental setting enables us to compare what happened to our outcomes of interest before and after introduction 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 less affected by the introduction of the NMW. We include in the sample the years since the financial crisis and the onset of recession. Differences in NMW impacts during recession are examined by the inclusion of interaction terms between the NMW treatment effect and a dummy variable for 2008-2010 or by estimating separate models for the recession and earlier minimum wage periods. We augment the models of investment with indicators of firms' vulnerability to credit constraints to explore the interaction of the NMW policy with tighter credit conditions in influencing low-paying firms' behaviour.

The report is structured as follows. Section 2 provides a very brief overview of existing evidence on the NMW and firm behaviour, motivates the analysis in this report and details its contributions. Section 3 describes our research methods and section 4

discusses the two datasets we use. Results are presented in section 5. A final section concludes.

## 2. Background

Firms may respond in a number of ways to the increases in the National Minimum Wage (NMW). Standard neo-classical theory predicts that minimum wage floors will reduce labour demand (reducing employment levels or hours), but to date the large number of studies examining the employment impacts of the 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, 2009; 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 may have led to a reduction in employment retention for female part-time workers, Dickens, Riley and Wilkinson, 2012; and more muted employment growth in low-paying service sector firms, Galindo-Rueda & Pereira, 2004).

The combination of the rise in wages and little in the way of significant negative employment effects from the NMW has led to the question of whether the NMW may have led to productivity increases, for example in response to increased worker effort or increased training provision. A couple of studies have found evidence of positive productivity effects associated with the NMW (Galindo-Rueda & Pereira, 2004; Rizov & Croucher, 2011); but some found no productivity effects (Draca *et al.*, 2005, 2011; Forth *et al.*, 2009).

Others have asked whether firms may have passed on the rise in costs in higher output prices or absorbed it in reduced price-cost margins. There is some evidence that profitability has been reduced amongst low-paying firms due to the NMW (Draca *et al.*, 2005, 2011; Forth *et al.*, 2009). If profitability is reduced by the NMW this raises the possibility that investment may have been reduced, because of reductions in returns to investing in the firm or the drop in cash flow, but as far as we are aware there are no studies of the NMW and its impacts on firms' investment behaviour.

With the exception of the studies by Rizov & Croucher (2011) and Dickens, Riley and Wilkinson (2012) there is little evidence on the impact of the NMW for different size

firms. One of the reasons to expect that the impact of the NMW may vary by size of firm is that proportionally, low paid workers tend to be concentrated in smaller firms in low-paying sectors. Therefore it is likely that the NMW imposes a larger change in labour costs for these firms. Rizov & Croucher (2011) find that potential positive productivity impacts from the NMW are more marked amongst larger firms. Dickens, Riley and Wilkinson (2012) find that the potential reduction in employment retention for female part-time workers with the NMW is concentrated amongst larger firms.

Although rises in the NMW have been much muted since the onset of recession, there is some concern that the impact of the NMW may have changed over this period. Using employee data Dickens, Riley and Wilkinson (2012) find some evidence to suggest that wage differentials between NMW workers and those paid just above the NMW were restored somewhat during the recent recession years and find no evidence of a change in NMW impacts on employment of full-time workers over the business cycle; for part-time women they find some evidence of a reduction in employment retention upon introduction of the NMW and again during the recession. Using cross-country data Dolton and Rosazza Bondibene (2012) find that negative effects of minimum wages on youth employment may be more pronounced during recession, although they also find that these results are sensitive to the estimation method used.

To date there are no firm-level studies examining whether firm behaviour in response to the NMW has changed since the onset of recession. Of particular concern is the possibility that small firms are being disproportionately disadvantaged by the pay floor imposed by the NMW and restricted access to finance in recent years. As suggested above, small firms are more likely to experience significant cost increases with the NMW. Small firms are also typically more reliant on access to bank finance than larger firms. Therefore it is likely that smaller firms have been hardest hit by the banking crisis.

This report makes several contributions to the evidence on the NMW and its impacts on UK companies. First, we update existing evidence by examining the impacts of the NMW on UK businesses in a systematic way over the entire policy period up until and including 2010. This allows us to assess how the effects of the NMW may have changed over time, in particular how the effects of the NMW may differ during a period of slow growth and rising unemployment. Second, we apply similar methods to analysing the effects of the NMW on a range of business outcomes using two different business datasets: the record of UK company accounts provided in Financial Analysis Made Easy (FAME) and the

Annual Respondents Database (ARD) held by the ONS. This allows us to assess the robustness of these types of impact estimates and to provide a comprehensive picture of the way in which the NMW policy may have affected company behaviour. Finally, we specifically assess policy impacts on small businesses, which are of key policy interest.

### 3. Methodology

To estimate the impact of the NMW on firm behaviour we follow Draca *et al.* (2005, 2011) and Galindo-Rueda & Pereira (2004) in applying a difference-in-differences estimator to firm-level data. Draca *et al.* (2005, 2011) looked at companies in FAME to study the impact of the introduction of the NMW and very early upratings on firms' profits. Galindo-Rueda & Pereira (2004) studied the impact of the NMW on productivity using the Annual Survey of Hours and Earnings (ASHE) linked (by firm identifier or by sector/region) to the ARD.

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 need to measure 'exposure' to the NMW, i.e. intensity of treatment. An obvious way of doing this would be to calculate for each firm the rise in labour costs that would occur with a change in the NMW. In practice this is typically not possible because we do not observe individual workers' wages within firms. Linking ASHE with the ARD facilitates the derivation of firm-level workforce characteristics based on data for individual employees (see e.g. Haskel *et al.*, 2005; Riley, 2010; Riley & Robinson, 2011), but only for a subset of very large firms that is not representative of the firms most exposed to the NMW (Galindo-Rueda & Pereira, 2004) and hence we do not do this.

Instead we measure exposure to the NMW and distinguish treated from untreated firms by looking at the distribution of average labour costs (or average wages and salaries paid) per head across 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 further up the distribution of average labour costs per employee. This is akin to the approach in Draca *et al.* (2005, 2011), who, in their study of the introduction of the NMW and its effects on firms' profitability, assign firms paying average wages and salaries per employee of less than £12,000 per annum to the treatment group; control group firms

are firms paying average wages and salaries per employee between £12,000 and £20,000 per annum. Importantly, using the 1998 Workplace Employee Relations Survey (WERS) they show a negative correlation between average wages paid by the firm and the proportion of workers paid less than the NMW before the introduction of the NMW, suggesting that average wages are a means of identifying NMW exposure. They also show that the proportion of minimum wage workers falls quickly beyond an average wage of £10,000 per annum, which, they suggest, supports the idea of using an average wage of around £10,000 per annum as a means of distinguishing between firms that are more and less exposed to the NMW. We experiment with different thresholds around this level, adjusting the thresholds over time with increases in the NMW or in average earnings. The key is that we observe for the treatment group relative to the control group a rise in average labour costs per head over the period when the NMW was introduced, and that we do not observe such a change in the wage distribution before then. This latter point is a means of testing whether average wages for the treatment and control group would have behaved in a similar way in the absence of the NMW policy (a test of the common trends assumption that underlies the difference-in-differences identification strategy).

### ***Longitudinal panel models***

In the first instance we select firms for the treatment and control groups based on their characteristics in the year prior to the introduction of the NMW (following Draca *et al.* (2005, 2011) and Galindo-Rueda & Pereira (2004)), tracking outcomes for these firms in the three years after the introduction of the NMW. Having defined treatment and control groups based on thresholds for average labour costs we can 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 of the NMW and  $p=1$  refers to the period after the introduction of the NMW.

$$y_{it} = \text{constant} + \alpha \text{LOWPAY}_i + \beta D_{p=1} + \gamma \text{LOWPAY}_i * D_{p=1} + \delta X_{it} + \varepsilon_{it} \quad (1)$$

In this set-up  $y_{it}$  is the outcome of interest for firm  $i$  at time  $t$ .  $\text{LOWPAY}$  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 NMW is in place, and zero otherwise. The  $X_{it}$  are controls for firm characteristics intended to net out differences between firms unrelated to the NMW.  $\varepsilon_{it}$  is an error term and the rest are parameters to be

estimated. In this example  $\gamma$  measures the impact of the introduction of the NMW on outcome  $y$ .

In order to evaluate the more recent impacts of the NMW we could in principle 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. But, this raises a number of issues. First, as time progresses firms may move out of the treatment and control groups (they may also do this in the first three years following introduction, but this is less likely over a shorter time span), so that the treatment and comparison groups become less suitable for identifying NMW effects in the later years of the policy. Second, firms may move out of their size category, which matters because we wish to distinguish between policy effects on smaller and larger firms. Third, sample sizes become small in more recent years because an increasing proportion of the cohort of firms exits the market and the remaining group of companies becomes arguably less representative of the group of firms that are affected by the policy.

For these reasons we evaluate the later impacts of the NMW by estimating equation (1) for a new set of firms. Using the same thresholds for average labour costs, adjusted for increases over time in the NMW (or average earnings; it does not matter much for the results), we select firms for the treatment and control groups based on their characteristics in some year after the NMW was already in place. For example, we select firms on the basis of their average labour costs in the year before the recession hit. We then track these firms over the course of the three years since recession ( $p=1$ ) and compare these outcomes to those in the three years before recession ( $p=0$ ). In this model  $\gamma$  in equation (1) measures the impact of the upratings in the NMW since the recession on outcome  $y$  conditional on the existence of a wage floor (because the NMW policy is in place in both the pre- and post-policy periods). For this interpretation to be valid we are assuming that the policy effects of a given wage floor are the same in recession as in a period of stable economic growth. An alternative interpretation is that  $\gamma$  in equation (1) measures 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. Increases in the NMW since recession have been more muted (than in earlier years), and hence this latter interpretation seems quite reasonable.

In this approach falsification tests are carried out by estimating equation (1) during a period *before* the introduction of the NMW, i.e. a period where both  $p=0$  and  $p=1$  refer

to a time before the introduction of the NMW (1994-1998, when there was no NMW and there were no Wage Councils). Using the same thresholds for average labour costs, adjusted for changes over time in average earnings, we select firms for the treatment and comparison groups based on their characteristics in the year before a "fictive" policy intervention.  $D_{p=1}$  is then a dummy variable equal to one if the "fictive" policy intervention is in place, and zero otherwise. Now  $\gamma$  measures the impact of the pretend policy on outcome  $y$ , and we expect it to equal zero if we are to have any confidence in the identification strategy.

Outcome measures examined include labour costs (or wages and salaries) per head; employment; labour productivity; profitability (measured as the ratio of gross profits to turnover or value added to proxy price-cost margins as in Draca *et al.* (2005, 2011) and Forth *et al.* (2009)); capital intensity (capital per employee); investment (measured relative to a measure of output). We also examine probability of exit (business failure) in a similar approach.

### ***Repeated cross section models***

In a separate exercise we also select firms for the treatment and control groups based on their characteristics in the current year (in which case the sample of firms in the period before and after, and at different points after, the introduction of the NMW may differ). For example, firms may be allocated to the treatment group if their average labour costs lie below a particular threshold in the current year. We use a moving threshold set equal to the threshold used in the cohort model above in the base year (the year before the policy introduction) and then adjust for subsequent changes to the NMW. We then evaluate the impact of the policy on annual growth in the outcomes of interest as in equation (2).

$$\Delta y_{it} = \text{constant} + \alpha \text{LOWPAY}_{it} + \beta D_{p=1} + \gamma \text{LOWPAY}_{it} * D_{p=1} + \delta X_{it} + \varepsilon_{it} \quad (2)$$

Here  $\Delta y_{it}$  denotes a change in outcome  $y$  for firm  $i$  between year  $t$  and year  $t+1$ . In the policy on period this change spans either the introduction of the NMW or subsequent upratings of the NMW. This approach is not dissimilar to an approach taken in some employee-level studies (Swaffield, 2009; Dickens, Riley & Wilkinson, 2012). We do not evaluate individual annual NMW upratings separately or the effects of the NMW in its first year. Rather, as in the cohort model in equation (1), the policy on and off periods

indicated by  $p$  refer to several years and the policy impact  $\gamma$  is essentially a pooled or average impact estimate.

Much as in estimating equation (1), we estimate equation (2) separately for the three years either side of NMW introduction, the three years either side of recession, and carry out falsification tests in the period before the NMW was introduced.

We also augment the model above to further explore whether the impact of the NMW differed during recession from previous years. Consider a three-period model as in equation (3), where  $p=0$  refers to the period before the introduction of the NMW,  $p=1$  refers to the period after the introduction of the NMW up to 2007 (the last year before the recent recession; the financial crisis started towards the end of 2007) and  $p=2$  refers to subsequent years during which economic growth has either been negative or anaemic.

$$\begin{aligned} \Delta y_{it} = & \text{constant} + \alpha \text{LOWPAY}_{it} + \beta_1 D_{p=1} + \beta_2 D_{p=2} \\ & + \gamma_1 \text{LOWPAY}_{it} * (D_{p=1} + D_{p=2}) + \gamma_2 \text{LOWPAY}_{it} * D_{p=2} + \delta X_{it} + \varepsilon_{it} \end{aligned} \quad (3)$$

In equation (3) the period following the introduction of the NMW is split into two and we include two dummies to capture this.  $D_{p=1}$  is a dummy variable equal to one if  $p=1$ , i.e. if the NMW is in place before 2008, and zero otherwise.  $D_{p=2}$  is a dummy variable equal to one if  $p=2$ , i.e. 2008 onwards (during which the NMW is also in place), and zero otherwise. In this set-up  $\gamma_1$  measures the impact of the NMW in the period before recession. The *change* in the impact of the NMW during recession is measured by  $\gamma_2$ . The impact of the NMW before and after recession is different if  $\gamma_2 \neq 0$ . Note that identification assumes that the treated and control firms respond in the same way to recession in the absence of the NMW and we are unable to test whether this is the case.

Some of the data we use (the ARD for smaller companies) lacks the longitudinal aspects required to estimate any of the equations above. When using these data we select firms for the treatment and control groups based on their characteristics in the current year, much as we do in selecting the estimation sample for equation (2) above. But rather than evaluating the impact of the policy on annual growth in the outcomes of interest as in equation (2), we evaluate the impact of the policy on levels as in equation (4).

$$y_{it} = \text{constant} + \alpha \text{LOWPAY}_{it} + \beta D_{p=1} + \gamma \text{LOWPAY}_{it} * D_{p=1} + \delta X_{it} + \varepsilon_{it} \quad (4)$$

The disadvantage of this approach is that we define treatment and control groups at the same point in time that we measure outcomes. This puts bounds around the estimated wage impact of the policy.

### ***Exploring the role of credit constraints***

Finally, we wish to explore the role of credit constraints in influencing the impacts of the NMW during recession. To gauge the importance of credit constraints on firms' investment decisions and how these interact with the NMW we compare investment outcomes for low-paying firms who are likely to be vulnerable to credit constraints (e.g. firms with high loan to value ratios) to investment outcomes for low-paying firms who are less likely to be vulnerable to credit constraints (e.g. firms with low loan to value ratios) before and after the financial crisis (i.e. before and after recession). This is a difference-in-differences type estimate where the financial crisis (or the credit crunch) is the treatment and the treatment group is the set of low paying firms that tend to rely on external finance.<sup>2</sup> Specifically, we augment equation (1), estimated over the three years either side of recession, with a measure of vulnerability to credit constraints (*CREDIT*) in the year before recession as in equation (5) below.

$$\begin{aligned} y_{it} = & \text{constant} + \alpha \text{LOWPAY}_i + \theta \text{CREDIT}_i + \varphi \text{LOWPAY}_i * \text{CREDIT}_i \\ & + \gamma \text{LOWPAY}_i * D_{p=1} + \pi \text{CREDIT}_i * D_{p=1} + \tau \text{LOWPAY}_i * \text{CREDIT}_i * D_{p=1} \\ & + \beta D_{p=1} + \delta X_{it} + \varepsilon_{it} \end{aligned} \quad (5)$$

Here the change in the impact of the NMW during recession is measured by  $\gamma$  as described above in equation (1),  $\pi$  measures the impact of credit constraints on the outcome of interest,  $\tau$  captures the interaction of NMW policy and credit constraints, and  $y_{it}$  is a measure of investment for firm  $i$  at time  $t$ . For example,  $\tau < 0$  implies that whatever effects credit constraints have on investment and whatever effects the NMW has on investment, the impact of exposure to both the wage floor and restricted credit finance tends to reduce firms' investment. The parameter  $\varphi$  nets out the potential correlations between vulnerability to credit constraints and low pay before the financial crisis. As a falsification test we also estimate the model over the period of the introduction of the NMW, when there was no financial crisis. In this case *CREDIT* is

---

<sup>2</sup> This approach to analysing credit constraints is similar to that in Almeida *et al.* (2009) and Duygan-Bump *et al.* (2011).

defined in the year before the introduction of the NMW and the expectation is that  $\tau = 0$  and  $\pi = 0$ .

#### **4. Data**

We use two business datasets for our analysis: FAME, a UK wide commercial dataset available from Bureau van Dijk, and the Annual Respondents Database (ARD) for Great Britain.

##### **FAME**

FAME contains financial data on the population of UK registered companies. Drawbacks are that for many companies data items are missing, because there are no reporting requirements. Reporting requirements are particularly light for small companies (i.e. those with less than 50 employees)<sup>3</sup>. The main attractions of FAME in conducting this research, in comparison to other commercial datasets and/or ONS datasets, is the availability of data covering the period 1994-1998 (the period after the abolition of the Wages Councils and before the introduction of the NMW), which can be used to test the validity of the identification strategy; the coverage of non-listed companies; the availability of longitudinal data for some small companies; and the availability of company balance sheet data and financing arrangements enabling us to explore the importance of credit constraints in the context of the NMW.

We extract data on companies who at some point during April 1 1993 and 31 March 2011 filed an account including information on both the value of remuneration and the number of employees. These variables are necessary for identifying treatment and control groups. This extract yields 2.4 million accounts.

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

---

<sup>3</sup> Small companies are required to file full accounts if both their turnover and assets exceed the thresholds set out in the relevant Companies Act at the time of filing.

companies that operate in a group. Stand alone companies more typically file unconsolidated accounts. Studies based on FAME data usually exclude one or the other type of account to avoid double counting. The problem with the consolidated accounts is that these miss out a lot of smaller independent companies. Given our focus on firm size this is inappropriate for our study. Table 4.1 illustrates this point, showing the number of accounts filed in the period 1998Q2-1999Q1 in our data extract and the number of employees covered in these accounts (for particular sectors and with non-missing data on key data items as described below). Summing across the rows of Table 4.1 it is apparent that the consolidated accounts cover more employees than the unconsolidated accounts, particularly outside the low paying sectors. This is primarily because they cover very large companies. In contrast, the unconsolidated accounts cover far more employees in small and medium size firms than the consolidated accounts; indeed the number of consolidated accounts/firms that are small or medium size is quite small. This is not simply because the consolidated accounts are some amalgamation of unconsolidated accounts so that by definition they appear as larger companies; small independent companies usually do not file group accounts.

In order to retain small companies in our sample and to simultaneously include the very large firms available in FAME (and avoid analysing small subsidiary sections of these) we include both the consolidated and unconsolidated accounts, deleting all subsidiary accounts (where a single parent has at least 50% control).<sup>4</sup> The resulting number of firms and employees covered is shown in the final columns of Table 4.1. In this mixed sample the number of large firms is not very different to the number of larger consolidated accounts, but the number of small firms is much larger.

We include accounts that cover turnover, profits, employment, remuneration, and fixed capital. We use as proxies for our outcomes of interest the following:

- *Average wages:* remuneration/employment
- *Labour productivity:* we examine two measures: turnover/employment and (remuneration + profits)/employment
- *Price to cost margins:* EBIT margin (ratio of earnings before interest and tax to turnover)
- *Net investment:* (year-on-year change in fixed assets)/turnover

---

<sup>4</sup> This process is not perfect as we cannot identify all subsidiaries at each point in time.

- *Company exit:* Exit dummy coded to unity for time periods after the last observed filing date if the company is recorded as inactive (dissolved, liquidated, in receivership or non-trading)<sup>5</sup>

We use the year-on-year change in fixed assets rather than gross investment because the latter is often not reported. We exclude companies with one employee (potential Director-employee companies).<sup>6</sup> For most of the analysis we include UK companies whose ultimate owner is a non-UK based company. Our main conclusions regarding NMW impacts are not affected by the inclusion or exclusion of these companies. But, in our analysis of the relationship between access to finance and NMW impacts in recession we exclude these companies. This is because foreign owned companies are likely to have access to additional "internal" financing arrangements that are not open to domestically owned companies. We use as measures of dependence on external finance and hence vulnerability to the credit crisis the ratio of short term loans to turnover and the gearing ratio. We focus on market sector companies in the non-agriculture and non-financial industries for comparability with the analysis of the ARD data and for the reasons discussed in the next section.

### ***Annual Respondents Database***

The ARD is an ONS dataset and is extensively used by researchers to study firm behaviour. It contains the ABI/ABS, which provides financial information for most large businesses and a stratified (by industry, region and employment size) sample of smaller tax-registered businesses (for a full description see e.g. Barnes & Martin, 2002; Criscuolo et al., 2003; Harris, 2005).

The ARD is available through the Secure Data Service (SDS) and data start in 1973. However, our study focuses on the period from 1997 when most of the two-digit SIC categories are available, including the service industries which include the main low pay sectors. Therefore, our policy-off period includes 1997 and 1998 and the policy-on period starts in 1999.

---

<sup>5</sup> This is similar to the definition used in Draca *et al.* (2011).

<sup>6</sup> In the results presented we also exclude companies with two employees. This is to exclude companies essentially consisting of a Company Secretary and a Director alone and to avoid potential breaks in the data introduced with the Companies Act 2006, which stipulated that it was no longer a requirement to appoint a Company Secretary. This exclusion does not make a material difference to our main results.

We exclude from our analysis in this report the recessionary years 2008 and 2009. This is due to a structural break in the micro-data in 2008 compared with 2007 and earlier years<sup>7</sup>, which complicates the analysis<sup>8</sup>.

Industries included in the ABI account for approximately two thirds of the UK economy. We exclude the public sector and the financial sector because they are not fully covered in the ABI. Another reason for excluding the public sector is the difficulty of measuring labour productivity in this industry. We also take out the agricultural sector from the analysis because it has a minimum wage policy different from the rest of the UK economy. We also exclude companies with one employee only, thereby excluding Director-employee companies.

Although information in the ABI yields annual longitudinal information for large firms, there are large gaps in the data for small firms because of the rotating sampling strategy (small firms cannot be included in the sample in consecutive years). Since we are mostly interested in small firms, who are more likely to pay low wages, but for whom we are unable to track outcomes over time, our analysis of the ARD is implemented using equation (4) as described in the previous section.

In the ARD there are a core set of variables that are present in most sectors every year and we use these to construct our dataset. Our proxies for our outcomes of interest are:

- *Average wages*: total labour cost<sup>9</sup> /employment
- *Employment*: total average employment
- *Labour productivity*: Gross Value Added (GVA) at factor costs/employment
- *Price to cost margins*: (GVA at factor costs – total labour costs)/ GVA at factor costs
- *Investment*: (gross capital expenditure – disposals)/ GVA at factor costs

---

<sup>7</sup> In 2008 the Business Register Employment Survey replaced what was previously known as the Annual Business Inquiry 1.

<sup>8</sup> See e.g. the discussions in Field, S. and Franklin, M. (2013) 'Micro-data perspectives on the UK productivity conundrum', Office for National Statistics, and Bovill, H (2012) 'Annual Business Survey (ABS) Technical Report', Office for National Statistics.

<sup>9</sup> 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).

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 ARD has previously been used to study the impacts of the NMW on plant-level productivity, profitability and exit by Forth *et al.* (2009). They use data 1999-2006 and do not use a difference-in-differences approach. Galindo-Rueda & Pereira (2004) use the ARD to study the impact of the introduction of the NMW on productivity, employment and unit labour costs, using a difference-in-differences approach on data 1997-2001. Neither of these studies identifies exposure to the NMW using average labour costs (wages) as we do in this study.

## 5. Results

Our first stage of analysis is to identify and check that treatment and control groups can be defined such that we see bigger wage increases amongst firms in the treatment group than amongst firms in the control group following the introduction of the NMW and that wages for these two groups follow similar trends in the period before the NMW was introduced (falsification tests). Having done this we then analyse how the NMW affected firms' labour costs beyond introduction and present our estimates of NMW impacts on other firm outcomes. We present our analysis of FAME in section 5.1 and our analysis of the ARD in section 5.2.

We estimate NMW impacts for firms in all sectors<sup>10</sup> and for firms in the low paying sectors, using the Low Pay Commission definition of low paying industries<sup>11</sup>. We also distinguish between small firms employing 49 employees or less and large sized firms employing at least 50 employees. We check whether the results are robust when we exclude micro-firms with fewer than 10 employees, for whom the data tends to be more erratic.

We experiment with different cut-offs to define treated and control firms; ranging from £8,000-£12,000 per annum. These are then adjusted in line with the NMW as we move further away from NMW introduction. To give an example, using a threshold of £12,000

---

<sup>10</sup> Excluding agriculture, finance, and public sectors.

<sup>11</sup> We use the Low Pay Commission definition of low pay industries. These include: retail, hospitality, social care, food processing, leisure, travel and sport, cleaning, security, textile and clothing, hairdressing.

in the year before the introduction of the NMW, the threshold in 2004 is set to £12,000 multiplied by the ratio of the NMW in 2004 to the NMW in 1998 (the latter is set at the NMW in 1999 deflated by the average earnings index to 1998). Treatment firms are those with average wages below the threshold and control firms are firms with average wages above this threshold. We exclude firms with relatively high average labour costs and firms with average pay less than £3,000 per annum (again, both of these cut-offs are adjusted for changes in the NMW over time).

As mentioned above, the analysis of WERS 1998 in Draca *et al.* (2011) points to a cut-off of around £10,000 at introduction as a means of differentiating between treatment and control firms. Draca *et al.* (2005, 2011) use a threshold of £12,000 in their main analysis. This is at the upper end of our range of thresholds because the distribution of wages in our dataset includes a larger proportion of low-paying firms (mainly because we have more small firms). For example, in Draca *et al.* (2011), around 13 per cent of firms have an average remuneration figure less than £12,000 in the year before the introduction of the NMW. In our FAME data set this figure stands at 22 per cent; 15 per cent of firms have average wages less than £10,000 and almost 10 per cent of firms have average wages less than £8,000. In our ARD data set the number of firms paying below these thresholds is higher still. We remain agnostic about the specific threshold to use and test the robustness of our results to different wage thresholds.

## 5.1 Results using FAME

In analysing the FAME data we explore the robustness of our estimates to a range of definitions of the treatment and control groups. But, results are presented for one definition only in order to simplify the presentation. Specifically, results are presented for the case where the treatment group includes firms with average labour costs below £11,000 and the control group includes firms with average labour costs between £11,000 and £19,000. We find that the £11,000 threshold tends to work best in terms of identifying wage impacts upon introduction of the NMW and also in satisfying the common trend in wages assumption for the groups of firms (size and sector) considered. Estimated wage impacts tend to be smaller (in some models) when we use a threshold lower than £11,000. The common trends in wages assumptions are more likely to be

violated when we use a threshold above £11,000. We discuss these alternate thresholds in the text as necessary.

### ***Wages (FAME)***

Table 5.1.1 reports the results of estimating equation (1). A cohort of firms that filed accounts in 1998Q4 or 1999Q1 are allocated to the treatment and control group (or deleted from the sample). Average wages for this group of companies are then tracked for the next three years and compared to average wages in the previous three years for these same firms. The NMW effect is the difference in this change over time in wages between companies in the treatment group and companies in the control group.

From the first line in Table 5.1.1 we see that average remuneration per head increased for low paying firms upon introduction of the NMW.<sup>12</sup> Our estimates of the initial wage effect of the NMW are not significant when we look at small companies in all industries, but are positive and statistically significant in all other groups. Our central estimates of wage impacts are larger for firms in the low pay sectors. They imply that wages rose by 5 per cent for firms in the low pay sectors as a consequence of the introduction of the NMW (the dependent variable in these regressions is log average wages).

Looking at the falsification period in the same table (rows labelled 1997-1998; here we select firms from the cohort that filed annual accounts in 1996Q4 or 1997Q1 and track outcomes for two years) we observe no difference in wage changes between the treatment and comparison groups in the period prior to the NMW.

In Table 5.1.2 we show the results of estimating equation (2). Annual growth in wages in the period 1999-2001 is compared to annual growth in wages in the three years before. Companies are selected for the treatment and control groups at the start of each year. The difference-in-differences estimates in the first line of the table are then our estimates of the change in annual wage growth with the introduction of the NMW. Again the results for small companies in all sectors are not significant; neither are the results for large firms in the low paying sectors. All other results are significant. For smaller companies in the low paying sectors annual wage growth was on average in the

---

<sup>12</sup> The results In Table 5.1.1. are based on a balanced panel of firms over the six years around NMW introduction. The use of the balanced panel avoids potential biases associated with composition effects induced by firm entry and exit, which may also be affected by the NMW. We also estimate equation (1) for the much larger unbalanced panel of firms selected in the year before the policy intervention, including controls for late entry, early exit, and interactions of these with treatment status. These results are not very different to those reported here.

three years following NMW introduction around 2 percentage points higher with the NMW. Estimates are a bit smaller when we look at companies in all sectors.

The falsification test shown in Table 5.1.2 (shown as 1997-1998) suggests annual wage growth developed in a similar manner over time in the period before the NMW for firms in the treatment and control groups. Again this makes us more confident that the average wage effects we identify around 1999 are due to the NMW.

In Table 5.1.1 we also select a cohort of firms that filed accounts in 2004Q4 or 2005Q1 to assess how the impact of the NMW changed between 2005-2007 and 2002-2004. The NMW was increased by an average of 18 per cent between these two periods. We do the same for a cohort of firms that filed accounts in 2007Q4 or 2008Q1 to compare how the effects of the NMW changed during recession compared to the three years before. The NMW was increased by an average of 11 per cent between the three years before and after the onset of recession in early 2008. In both cases we find small significant impacts (average wages increased by around 2 per cent) when we look at all firms in all sectors. This effect is driven by firms in the 10-49 employees group. These wage results are not significant for firms in the low paying sectors<sup>13</sup>, and for small firms once we include micro firms. A "no change" impact suggests that the NMW upratings did not lead to large increases in average labour costs for low paying firms beyond the NMW introduction period and earlier upratings. This is not to say that these later upratings had no impact on wages relative to a situation without a NMW; this merely says that the wage floor was sustained. A "no change" effect in wages during recession is also evident when we look at the recession period using the annual growth model in Table 5.1.2.<sup>14</sup>

Finally, in Table 5.1.3, we estimate the annual growth model in equation (3) over the full period since the introduction of the NMW (1999-2010). Annual changes in average wages are compared over this period to annual changes in average wages 1994-1998. Central estimates suggest that average annual wage growth was 0.5 percentage points higher over the period 1999-2007 due to the NMW. In most cases this effect is not precisely estimated. Nevertheless, the magnitude of this average growth impact is consistent with an overall change in average wage levels upon NMW introduction of the magnitude shown in Table 5.1.1 that is subsequently sustained. The change in wage

<sup>13</sup> Note that sample sizes are quite small when we focus on small firms in the low paying sectors and this may affect our results.

<sup>14</sup> The sample of small firms drops off during these later periods. This is because the FAME extract of small firms that report full accounts gradually diminishes over time during the 2000s.

growth during recession shown in Table 5.1.3 is statistically no different from this average effect 1999-2007.

*Overall, our estimates of wage impacts suggest the NMW increased average labour costs for low paying firms by 3-5 per cent in its initial years. These effects may have been larger for firms in low paying sectors. Taken together our estimates suggest it is likely that most of this rise in average labour costs amongst low paying firms occurred in the early years of the NMW and that this level increase was then maintained relative to firms with slightly higher average labour costs. Our results do not suggest that the NMW led to excessive increases in average labour costs for low paying firms during recession.*

### ***Other outcomes (FAME)***

#### *Employment*

Looking at our estimates of the effect of the NMW on employment from model (1) in table 5.1.1 we find a fall in employment amongst small companies with the introduction of the NMW. But, this is unlikely to reflect a policy effect because we see a similar drop in employment amongst small low paying firms in the falsification period (before the introduction of the NMW).

Results from estimating equation (2), the annual growth model, reported in Table 5.1.2, also show a negative employment (growth) effect for small companies in the low paying sectors. The falsification test for employment passes for this group using this model (see results labelled 1997-1998), but the negative effect at NMW introduction is only significant at the 10 per cent level and is not significant using other thresholds to define the treatment and control groups.

We find no change in the employment impact of the NMW between 2002-2004 and 2005-2007 (Table 5.1.1). Our estimates of employment impacts during recession show a statistically significant positive effect on employment in large firms (when we look at all sectors). We find no effect for small firms or for the low pay sectors. A positive change in employment during recession is also apparent for this same group in Tables 5.1.2 and 5.1.3; but for no other groups. At face value our estimates do not suggest that the NMW has been a factor contributing to reductions in staff numbers during recession. This seems plausible if we consider that the wage floor imposed by the NMW is a nominal

one; during recession companies may have been able to implement real wage cuts for low paid employees and better paid employees without cutting nominal wages.

*Taken together our estimates using the FAME data suggest that the introduction of the NMW had little effect on companies' workforce levels. Our estimates of how employment has fared during recession in low paying firms compared to slightly better paying firms does not give rise to concern about the NMW.*

### *Labour productivity*

We consider two measures of labour productivity: turnover per head and a proxy of gross value added per head equivalent to the sum of remuneration and operating surplus per head. In Table 5.1.1 our estimates of equation (1) suggest that firms responded to the rise in labour costs that occurred with the introduction of the NMW by raising labour productivity. (Note, dependent variable is in logs.) These effects are particularly evident for large firms. These effects are apparent using both measures of labour productivity and are robust to the inclusion of log capital-labour ratios in the model, suggesting these labour productivity impacts reflect increases in total factor productivity. Our falsification tests shown in Table 5.1.1 do not pass in all instances. Nevertheless, the pattern of productivity effects during the period of NMW introduction is quite different to the effects during the falsification period. We find positive effects of NMW introduction on annual labour productivity growth in Table 5.1.2 when we use the turnover measure, but no effect when we use the gross value added proxy. This time the positive effects are driven by small companies in the low paying sectors only. The falsification tests for these groups pass (labelled in Table 5.1.2 as 1997-1998).

Looking in Table 5.1.1 we see that productivity levels did not change with later upratings of the NMW or during recession. The annual growth model in Table 5.1.2 confirms this. The pooled annual growth model in Table 5.1.3 suggests labour productivity growth fell back a bit during recession amongst large low paying firms when compared to the period 1999-2007.<sup>15</sup> These are the same companies for whom we see a relative

---

<sup>15</sup> Changes in the turnover thresholds that determine Companies House filing requirements, brought about by the Companies Act 2006, came into effect during recession. It is not clear whether this may have caused a change in observed productivity between the treatment and control groups. In any case, this should not affect our recession results in the longitudinal model in Table 5.1.1, which can be compared to and are in line with the results in Table 5.1.2.

employment increase during recession in comparison to 1999-2007. In this model average annual productivity growth does not appear to be larger amongst low paying firms over the 1999-2007 period as a whole; this mirrors the average annual wage growth effect over this period, which is sufficiently small so that it is insignificant in this model.

Note that we measure productivity per employee. This means that an estimated positive effect of the NMW on productivity may reflect either an increase in productivity per hour or an increase in average hours worked; or both.

*In summary, our estimates point to either no effect or a positive effect of the NMW on productivity. Any positive effect occurs mainly during the initial years of the NMW.*

### *Profits*

In Table 5.1.1 we find a small negative effect of NMW introduction on profit rates for small companies in the low paying sectors. But, this is only significant at the 10 per cent level. Using higher thresholds to define the treatment and comparison groups this effect is statistically more significant. Our falsification tests generally pass, except in the case where we look at companies in the low paying sectors in all size groups simultaneously. In the annual change model in Table 5.1.2 we also observe some negative profitability effects in the low paying sectors upon introduction of the NMW (for small companies and where we look at all size groups together). Again these are only significant at the 10 per cent level; moreover they disappear when we use other thresholds to define the treatment and comparison groups.

Looking at the change between 2002-2004 and 2005-2007, we find in Table 5.1.1 some evidence that profitability was falling amongst low paying firms. In the pooled growth model in Table 5.1.3, looking at the period 1999-2007, we see a negative effect of the NMW on the annual change in profitability for all groups considered. These figures imply that on average profit rates rose by 0.3 percentage points less per annum amongst low paying firms since the introduction of the NMW. We find no consistent evidence of a change in the effect of the NMW on the profitability of low paying companies during recession. Our results in this section are not very different when we control for the ratio of capital to turnover in the regressions.

*Overall, our results suggest the NMW may have been associated with a small reduction in the profitability of low paying companies. These effects were not amplified during recession.*

### *Investment*

In Table 5.1.1 we generally find no effect of the introduction of the NMW on the ratio of investment to turnover. We find a negative impact for large companies in the low paying sectors, but this is only significant at the 10 per cent level. Our falsification tests pass for most, but not all groups. The negative effect on investment amongst large firms in the low paying sectors upon NMW introduction is also evident in the annual change model shown in Table 5.1.2. Like the results in Table 5.1.1, the results in Table 5.1.2 show no effects of NMW introduction on investment for any other group of firms. Again our falsification tests pass for some, but not all groups.

We find no change in the effect of the NMW on investment ratios between 2002-2004 and 2005-2007 (see Table 5.1.1). We do in this model find a reduction in investment rates amongst low paying firms during recession (for firms in all sectors; in other models these effects are apparent in low paying sectors too). However, this change in the impact of the NMW on investment during recession is not evident from the models in Tables 5.1.2 or 5.1.3.

In Table 5.1.4 we explore the impact of credit constraints on investment in the context of the NMW. We estimate the cohort model reported in Table 5.1.1 on a sample of domestically owned firms including indicators of firms' vulnerability to the credit crisis; this is the model in equation (5). We estimate this model over the recession period, selecting treatment and control firms amongst the firms that filed accounts in 2007Q4 or 2008Q1. We measure firms' vulnerability to credit constraints at this point in time. We also estimate this model over the period of NMW introduction as a means of providing a falsification test for the credit effects. If credit constraints matter for investment we should see a significant effect in the model estimated over the financial crisis and not in the model estimated during the NMW introduction period, when access to credit was not limited in general.

In Table 5.1.4 we find a negative effect of the NMW on investment during recession (see first row) and a negative effect of credit constraints in the model where we proxy firms'

vulnerability to credit constraints by the ratio of short term loans to turnover. These effects are evident in the low paying sectors, but are not significant for small companies<sup>16</sup>. We find no additional impact from being both exposed to the NMW and vulnerable to credit constraints. We do not observe these negative effects on investment in 1999-2001 when credit supply was less limited. The effects of the NMW in these models is much as in previous models (Table 5.1.1); note we are using a different sample here as we have dropped firms whose ultimate owner is foreign. (Including these firms we find no negative effects on investment of credit constraints.) When we proxy vulnerability to credit constraints with the gearing ratio we find no direct negative effect of credit constraints on investment, but we find some negative interaction with the NMW policy effect. However, this is also present in the placebo period and hence we do not place any weight on this result.

*To summarise, we find no robust evidence to suggest that the NMW has reduced investment rates amongst low paying firms. Exploratory analysis suggests that credit constraints may have hindered investment in some domestically owned firms during recession, but we have no clear evidence that these effects should have been compounded by an adverse effect on investment of the NMW.*

#### *Exit rates*

In Table 5.1.5 we show difference-in-differences estimates of the impact of the NMW on company exit rates. In these models we compare three year exit rates for a cohort of firms (treatment and controls) selected right before the introduction of the NMW or right before recession to three year exit rates for a cohort of firms selected in some earlier period. Falsification tests are not possible with our dataset. We find no robust evidence of a change in exit rates for low paying companies following the introduction of the NMW. The negative and significant (at the 10 per cent level) effect for large companies disappears when we use alternative thresholds to define treatment and control firms. For the recession cohort we find some evidence of a reduction in the exit rate amongst large low paying firms in the low paying sectors. But, this effect disappears when we benchmark on earlier periods.

*Overall, we find no evidence to suggest that the NMW changed the company exit rate.*

---

<sup>16</sup> Again we note the relatively small sample size for this group.

## 5.2 Results using the ARD

Results are shown for three different wage thresholds (£8,000, £10,000 and £12,000) to define the treatment group. Therefore, our treatment groups include firms with average wages from £3,000 to £8,000, from £3,000 to £10,000 and from £3,000 to £12,000. We experiment with different control groups, from £8,000 to £13,000, from £10,000 to £17,000 and from £12,000 to £21,000. We remove any firms with above £13,000 £17,000 and £21,000 average wages, respectively, from the main analysis because these firms are quite different in terms of their characteristics and therefore subject to different unobservable trends from the treatment group. We are careful to test the sensitivity of the results to the definitions of these thresholds by also trying different control groups (ie. £8,000-£16,000, £10,000-£18,000 and £12,000-£20,000) (not reported<sup>17</sup>).

All the difference-in-differences estimates reported are conditional on other covariates. We have estimated these models including detailed 2-digit industry and year dummies, as well as companies' ownership status (i.e. if they are foreign owned) and region.

### ***Wages (ARD)***

Table 5.2.1 reports the wage effects both at the introduction (1999-2001) and up to 2007 for each of our categories, using different treatment and control groups. It is generally evident that wages rose significantly amongst the low wage firms from when the NMW became operational. We find a NMW wage effect both when the policy was introduced as well as for the whole period of analysis up to 2007. It is apparent from the relative magnitudes of these effects (which are not very different) that most of the rise in average labour costs occurred upon introduction.

The coefficients are generally larger in small firms compared to large ones. They are also larger in low paying sectors and particularly if the firms are smaller in size. But, statistically these are not necessarily different.

If we use the £8,000 as a threshold the NMW effect for large firms is significantly different from zero. However, the coefficients become insignificant using the £10,000 and £12,000 thresholds. This could suggest that relatively large firms are in general less

---

<sup>17</sup> These results are not qualitatively different from the ones presented in the paper.

affected by the NMW legislation since they are paying on average higher wages, as we might expect.

If we compare results using the different thresholds, we notice how the wage effects are larger in scale when we focus on the £8,000 threshold rather than the higher thresholds, especially at the introduction of the policy. However, we cannot generalize this to all the specifications. To give an idea of the magnitude of the coefficients, column 6 of Table 5.2.1 suggests that wages in low paying firms were on average around 4.3% higher over the period 1999-2007 due to the NMW.

A possible concern is that our wage results are simply picking up a relationship that has nothing to do with the NMW introduction and its up-ratings. One way of checking this is by doing a falsification test where we examine an invented introduction of the NMW in a year when the policy was not yet in place and repeat our Difference-in-Differences analysis. By looking at the period before the introduction of the NMW, we expect wages to evolve in a similar way over time in the two groups. In other words, estimating (4) in the policy-off period alone, we expect the wage effect to be zero. This is a test of the common trends assumption that underlies the Difference-in-Differences identification strategy.

One of the drawbacks of the ARD data is that information for the service sector (where low paid workers are concentrated) is not available before 1997. This therefore restricts our pre-period to two years: 1997 and 1998. So our falsification tests are limited to use 1997 as a policy-off period and 1998 as an imaginary policy-on year.

The last three rows of Table 5.2.1 present these falsification tests. These results show that generally we are unable to find any difference in wages between low and high wage firms in the period when the NMW was not in place, reinforcing the conclusion that the introduction of the NMW and its up-ratings caused wages to increase in low wage firms.

*In conclusion, the NMW has raised average wages amongst low paying firms. We also find, as expected, greater wage effects in small firms and small firms in low paying sectors.*

### ***Other outcomes (ARD)***

Having reached some confidence in the selection of the treatment and control groups in our analysis we can now repeat our Difference-in-Differences analysis of (4) for the

other outcomes of interest, ie. employment, labour productivity, firm profitability and investment.

Table 5.2.2 presents our Difference-in-Differences results for these outcomes for the whole period of analysis (1997-2007), using different treatment and control groups. Table 5.2.3 mirrors Table 5.2.2 but focuses only on the years of the introduction of the policy (1999-2001). Table 5.2.4 contains our falsification tests.

Consistent with the wage results, in each of the tables results are shown for three different wage thresholds (£8,000, £10,000 and £12,000) to define the treatment group and corresponding control groups (from £8,000 to £13,000; from £10,000 to £17,000; and from £12,000 to £21,000).

Again we look separately at firms in all sectors and at firms in the low pay industries, using the Low Pay Commission definition. We distinguish between small firms employing 2-49 employees and large sized firms employing at least 50 employees. Finally, we look at whether results are robust when we include/exclude micro-firms with less than 10 employees.

### *Employment*

If we focus on all firms (column 1, Table 5.2.2), we find a positive and significant impact of the NMW on employment when we define our threshold for our treatment group at £8,000 or £10,000. This is true if we also exclude micro-firms (with less than ten employees). However, when we split the sample into small and large firms we see that this positive effect disappears in both. When we use the £12,000 threshold the positive and significant effect for all firms disappears.

If we focus only on the NMW introduction (Table 5.2.3) we generally do not find a significant effect of the policy. We find a positive and significant effect for small firms when we use the £8,000 threshold, however, this effect disappears when we look at the £10,000 and £12,000 thresholds.

Falsification tests are reported in Table 5.2.4. Results are generally significantly different from zero as we would expect. We only find a positive and significant effect for all firms and small firms when we use the £12,000 threshold.

The last five columns of Tables 5.2.2, 5.2.3 and 5.2.4 repeat the same exercise described above for employment focusing on the low paying sectors only. We find a positive and significant impact of the NMW on employment for the whole period of analysis (Table 5.2.2). However, when we split the sample between small and large firms, the NMW effect is not significantly different from zero. This is true no matter which thresholds we are using.

If we look at the policy introduction only (last 5 columns of Table 5.2.3), we again find a positive and significant impact of the NMW on employment for low paying industries. When we split the sample between small and large firms, we find that the positive and significant effect is driven by small firms rather than larger ones. However, this positive and significant result is valid only with the £8,000 threshold and disappears when we focus on the £10,000 or £12,000 threshold.

In Table 5.2.4 we report our falsification tests. Generally, we do not find any significant NMW coefficients when our outcome is employment as we should expect.

*In conclusion, our employment results are generally insignificantly different from zero in most of our specifications. Even if sometime we find a small positive and significant effect of the NMW on employment, these results are not robust to our sensitivity tests. This leads us to conclude that there is no significant impact of the NMW on employment both at introduction and on average for whole period of analysis.*

### *Labour productivity*

Table 5.2.2 then focuses on labour productivity when we use £8,000 as a threshold for the treatment group. We do not find any significant impact of the NMW on this outcome. When we use the £10,000 as a threshold results are qualitatively similar: there is some positive and significant impact of the NMW which disappears when we split the sample into small and large firms. Results using the £12,000 as a threshold become more positive and significant in general, and these significant results are mainly for small low-paying firms.

When we focus only on the introduction of the policy, we find some positive and significant impact of the policy for all firms, however, when we split the sample into small and large firms the significant effect disappears. We also find a positive and significant effect for firms in low paying sectors, but again this effect disappears when

we split the sample into small and large firms separately. When we focus on the £10,000 threshold we find some positive and significant effect of the policy on small firms and firms in low paying sectors (both small and large). The positive and significant effect on small firms and small firms in low paying sectors is confirmed when we use the £12,000 threshold.

Generally, no matter which threshold we use in the analysis, our results pass the falsification tests (Table 5.2.4).

*In conclusion, our productivity results in general suggest little or no effect of the NMW on labour productivity. However, in some specifications, results seem to point towards a positive and significant NMW effects for small firms and small firms in low paying sectors, especially at the introduction of the policy.*

### *Profits*

We then focus on firm profitability by looking at the whole period of analysis and using the 8k threshold (Table 5.2.2), we find a negative and significant effect of the NMW on firm profits for all firms. If we split the sample between small and large firms we see that the coefficient is negative and significant only for small firms but it is not significant for large ones. Also when we focus on firms in low paying sectors, the negative coefficient is significant only for small firms. When we test the robustness of the results by checking what happens using the other thresholds, we still find qualitatively similar negative and significant results with the £10,000 thresholds; the coefficients are no longer significant when we use the £12,000 thresholds.

We then focus on the introduction of the policy only (Table 5.2.3). We still find some negative and significant coefficients for small firms and some suggestion of some negative impact for smaller firms in low paying sectors (although only significant at 10% level and only if we do not exclude micro-firms with less than ten employees) using the £8,000 threshold. These effects, however, are no longer there when we use the other wage cut-offs (£10,000 and £12,000).

Our analysis generally passes the falsification tests for our selected treatment and control groups in most of our specifications (Table 5.2.4).

*In conclusion, our profits results suggest some small negative effect of the NMW on firm profits which is mainly concentrated in small firms and small firms in low paying sectors. These results, however, are not robust to all our sensitivity tests, and are less visible upon NMW introduction.*

#### *Investment*

When we focus on our investment results, generally, in all specifications, using different thresholds, both at the introduction of the policy and for the whole period of analysis, we fail to find any significant impact of the NMW. Falsification tests are also insignificantly different from zero.

*In conclusion, these results provide no reason to suggest that the NMW has had an impact on investment either at the introduction of the policy or for the whole period of analysis.*

## **6. Conclusions**

To summarise, this report makes several contributions to the evidence on the NMW and its impacts on UK companies. First, we update existing evidence by examining the impacts of the NMW on UK businesses in a systematic way over the entire policy period up until and including 2010. This allows us to assess how the effects of the NMW may have changed over time, in particular how the effects of the NMW may differ during a period of slow growth and rising unemployment. Second, we apply similar methods to analysing the effects of the NMW on a range of business outcomes using two different business datasets, allowing us to assess the robustness of these types of impact estimates and to provide a comprehensive picture of the way in which the NMW policy may have affected company behaviour. Finally, we specifically assess policy impacts on small businesses, which are of key policy interest. We note that our analysis of the ARD data is more restricted because, due to the sampling strategy, it does not contain consistent longitudinal data for small companies.

Drawing together our results from analysing FAME and the ARD and looking at the three years following the introduction of the NMW we find that the NMW increased average labour costs for low-pay companies. The magnitude of this increase varies according to

the identification strategy and dataset used, but usually lies between 3 and 6 per cent for low-pay companies in the low pay sectors. Central estimates are larger for smaller companies and for companies in the low pay sectors, although these are not necessarily statistically different to estimates for large companies and companies in all sectors. Perhaps unsurprisingly, our estimates of the effect of the introduction of the NMW on average labour costs per employee for low-pay firms are somewhat smaller than estimates of its effect on hourly wages for low-pay employees (see for example Dickens, Riley and Wilkinson (2012) and Swaffield (2009)).

On balance, we find no effects of the introduction of the NMW on employment. We find some evidence indicating that the introduction of the NMW was associated with an increase in labour productivity. Depending on the identification strategy used these effects are apparent for small and larger firms and in both datasets. Using FAME data we find some evidence that the introduction of the NMW was associated with a reduction in profitability (price-cost margins) amongst small firms in the low pay sectors, but these effects are not particularly robust. We find no robust evidence to indicate that the NMW changed the investment behaviour of low-paying firms. Finally, looking at these initial years of the NMW we find no evidence to suggest that the introduction of the NMW led to a change in the exit rate of companies.

Looking at the data to 2007 we find that most of the increase in average labour costs associated with the NMW occurred upon introduction rather than with subsequent upratings. Again this is consistent with the evidence on wage impacts using employee data derived elsewhere. We find no robust evidence to suggest that the NMW changed employment. Again we find some evidence to suggest that firms may have responded to the NMW policy by raising labour productivity, but evaluated over this longer period these effects are less strong. Looking at the data to 2007 we find evidence of a reduction in profitability associated with the NMW. These effects are apparent using both datasets. The estimated labour productivity and profitability effects are sensitive to the definition of the treatment and control groups. Again, we find no evidence of a change in investment behaviour associated with the NMW.

Focusing explicitly on the years since the onset of recession we compare firm outcomes over the period 2008-2010 to firm outcomes 2005-2007. The NMW impacts estimated using this approach can be interpreted as the impacts of the NMW upratings since the 2008 recession conditional on there being a national wage floor and/or as the

differential impact of a national wage floor in a period of falling rather than stable employment. We find that the average labour costs of low-paying firms (firms in our treatment group) did not rise any faster than the average labour costs of firms in our control group over this period. This is not surprising given the small NMW upratings during these years. We still find some evidence to suggest that firms that are more likely to have been affected by the NMW have on average experienced stronger labour productivity growth. But, these productivity associations with the NMW are a lot less apparent than they are for the earlier years of the NMW policy. We find no consistent evidence to suggest that the NMW affected employment, profitability, investment or the rate of company exit differently in the years since 2008 than in previous years. Our analysis of the NMW from 2008 onwards is based on the FAME data alone.

In carrying out falsification tests over the period before the introduction of the NMW we typically do not find significant differences between changes in outcomes for low-pay firms (our treatment group) and for firms that pay slightly better wages (our control group). This allows us to be more confident that the impacts we identify during the NMW period are associated with the NMW policy. However, we note that the falsification tests that are possible with the ARD are somewhat limited.

In a more exploratory analysis we include within our models of investment simple indicators of the extent to which firms relied on external financing before the financial crisis. In these augmented models we find no robust evidence of an interaction between the impacts of the NMW on the investment behaviour of small companies over the period 2008-2010 and firms' reliance on external finance.

## Bibliography

- Barnes, M. and R. Martin (2002). "Business data linking; an introduction", *Economic Trends*, 581, 34–41.
- Criscuolo C., J. Haskel and R. Martin, (2003). "Building the evidence base for productivity policy using business data linking", *Economic Trends* 600, 39-51.
- Dickens, R., Riley, R. and Wilkinson, D. (2009) *The employment and hours of work effects of the National Minimum Wage*, Research Report for the Low Pay Commission.
- Dickens, R., Riley, R. and Wilkinson, D. (2012). *Re-examining the impact of the National Minimum Wage on earnings, employment and hours: The importance of recession and firm-size*, Research Report for the Low Pay Commission.
- Draca, M., Machin, S., and Van Reenen, J. (2005) *The Impact of the National Minimum Wage on Profits and Prices*, Research Report for the Low Pay Commission.
- Draca, M., Machin, S., and Van Reenen, J. (2011) 'Minimum Wages and Firm Profitability', *American Economic Journal: Applied Economics*, 3, 129-151.
- Forth, J., Rincon-Aznar, A., Robinson, C. and Harris, R. (2009) *The Impact of Recent Upratings of the National Minimum Wage on Competitiveness, Business Performance and Sector Dynamics*, Research Report for the Low Pay Commission.
- Galindo-Rueda, F. and Pereira, S. (2004) *The Impact of the National Minimum Wage on British Firms*, Research Report for the Low Pay Commission.
- Harris, R.I.D. (2005) 'Economics of the Workplace: Special Issue Editorial', *Scottish Journal of Political Economy*, 52(3), 323-343.
- Haskel, J., Hawkes, D., and Pereira, S. (2005) 'Skills, human capital and the plant productivity gap: UK evidence from matched plant, worker and workforce data', CEPR Discussion Paper No. 5334.
- Heckman, J. and Hotz, V. (1989) 'Choosing Among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training' *Journal of the American Statistical Association*, 84(408): 862-874.
- Riley, R. (2010) 'Industry knowledge spillovers: Do workers gain from their collective experience?', National Institute Discussion Paper No. 353.
- Riley, R. and Robinson, C. (2011) 'UK Economic Performance: How Far do Intangibles Count?', FP7 Innodrive Working Paper No 14.
- Rizov, M., and Croucher, R. (2011) *The impact of the UK national minimum wage on productivity by low-paying sectors and firm-size groups*, Research Report for the Low Pay Commission.
- Stewart, M. (2004a) 'The Impact of the Introduction of the UK Minimum Wage on the Employment Probabilities of Low Wage Workers', *Journal of the European Economic Association*, 2, 67-97.
- Stewart, M. (2004b) 'The Employment Effects of the National Minimum Wage', *Economic Journal*, 114, C110-C116.
- Stewart, M. and J. Swaffield (2008) 'The other margin: do minimum wages cause working hours adjustments for low-wage workers?' *Economica*, 75, 148-167.
- Swaffield, J. (2009) *Estimating the Impact of the 7th NMW Uprating on the Wage Growth of Low-Wage Workers in Britain*, Report prepared for the Low Pay Commission, November.

## TABLES

Table 4.1 FAME unconsolidated and consolidated accounts and the mixed sample

Employees	Average remuneration	Unconsolidated		Consolidated		Mixed	
		Firms	Employees	Firms	Employees	Firms	Employees
<b><i>Non Low Pay Sectors</i></b>							
3-49	3 ≤ w < 10	3358	58577	259	5996	2564	40987
3-49	10 ≤ w < 17	1188	12661	52	851	1014	10153
3-49	17 ≤ w	10942	206858	1364	32722	7902	140198
50-249	3 ≤ w < 10	1797	196682	496	65699	1194	132581
50-249	10 ≤ w < 17	263	29185	53	6519	206	22285
50-249	17 ≤ w	5943	636344	2317	289284	3859	421662
250+	3 ≤ w < 10	495	369031	448	1185014	423	926430
250+	10 ≤ w < 17	85	93475	81	265408	91	237657
250+	17 ≤ w	1900	1763569	2025	4810200	1661	3365348
<b><i>Low Pay Sectors</i></b>							
3-49	3 ≤ w < 10	1376	27993	57	1746	1038	19445
3-49	10 ≤ w < 17	984	16941	18	487	818	13029
3-49	17 ≤ w	1564	30891	145	3878	1121	20587
50-249	3 ≤ w < 10	1008	110381	299	39803	673	74469
50-249	10 ≤ w < 17	593	71077	80	11514	387	46093
50-249	17 ≤ w	887	93885	356	45260	597	63783
250+	3 ≤ w < 10	383	476332	341	1718086	305	988732
250+	10 ≤ w < 17	352	1085265	199	1192410	215	804497
250+	17 ≤ w	247	264689	227	394452	217	310509

Notes: FAME; market sectors outside agriculture and finance; accounts with non-missing information on remuneration, employees, profits, turnover, fixed assets in the current and previous years; the mixed sample includes consolidated and unconsolidated accounts less all UK subsidiaries; accounts filed 1998Q2-1999Q1.

THE IMPACT OF THE NATIONAL MINIMUM WAGE ON FIRM BEHAVIOUR DURING RECESSION

**Table 5.1.1 Longitudinal panel models using FAME**

Policy period:	Outcome:	Sector Size:		All Industries				Low Pay Industries			
		All	10+	<50	10-49	50+	All	10+	<50	10-49	50+
1999-2001	Remuneration per head	0.027** (0.011)	0.029*** (0.011)	0.020 (0.018)	0.016 (0.020)	0.034** (0.013)	0.050*** (0.012)	0.050*** (0.012)	0.047** (0.021)	0.049** (0.023)	0.050*** (0.015)
	Employment	-0.005 (0.020)	-0.005 (0.022)	-0.033 (0.021)	-0.072*** (0.026)	0.030 (0.028)	-0.034 (0.025)	-0.037 (0.027)	-0.060** (0.030)	-0.115*** (0.035)	-0.001 (0.033)
	Turnover per head	0.032** (0.015)	0.039** (0.016)	0.007 (0.023)	0.022 (0.027)	0.045** (0.019)	0.067*** (0.017)	0.079*** (0.018)	0.036 (0.032)	0.073** (0.036)	0.081*** (0.020)
	GVA per head	0.035** (0.017)	0.036* (0.019)	0.005 (0.029)	-0.003 (0.039)	0.052** (0.021)	0.039* (0.021)	0.055** (0.022)	-0.019 (0.043)	0.017 (0.050)	0.066*** (0.023)
	PCM	0.001 (0.003)	0.000 (0.003)	-0.005 (0.006)	-0.010 (0.007)	0.004 (0.004)	-0.003 (0.004)	-0.001 (0.004)	-0.015* (0.008)	-0.014 (0.009)	0.003 (0.005)
	Investment to turnover	-0.001 (0.009)	-0.007 (0.009)	-0.001 (0.013)	-0.010 (0.017)	-0.006 (0.011)	-0.011 (0.013)	-0.014 (0.012)	-0.002 (0.016)	0.014 (0.020)	-0.025* (0.015)
2005-2007	Remuneration per head	0.021** (0.011)	0.027** (0.011)	0.020 (0.020)	0.046* (0.025)	0.021* (0.012)	0.008 (0.013)	0.017 (0.013)	0.005 (0.028)	0.053 (0.035)	0.009 (0.014)
	Employment	0.020 (0.016)	0.022 (0.018)	0.005 (0.020)	0.012 (0.029)	0.019 (0.022)	0.020 (0.026)	0.021 (0.026)	-0.014 (0.034)	-0.046 (0.046)	0.026 (0.028)
	Turnover per head	0.007 (0.014)	0.012 (0.016)	0.002 (0.023)	0.013 (0.032)	0.012 (0.018)	0.004 (0.017)	0.021 (0.018)	-0.003 (0.035)	0.067 (0.043)	0.014 (0.020)
	GVA per head	-0.012 (0.018)	0.002 (0.020)	-0.041 (0.029)	-0.004 (0.039)	0.004 (0.023)	-0.027 (0.023)	-0.005 (0.024)	-0.066 (0.046)	0.039 (0.051)	-0.015 (0.028)
	PCM	-0.010** (0.004)	-0.009* (0.005)	-0.016** (0.008)	-0.016* (0.009)	-0.007 (0.006)	-0.013** (0.006)	-0.010* (0.006)	-0.016 (0.011)	-0.005 (0.012)	-0.011 (0.007)
	Investment to turnover	-5.293 (5.270)	-7.096 (7.095)	-0.030 (0.055)	0.038 (0.048)	-9.697 (9.687)	-0.008 (0.016)	-0.009 (0.018)	0.017 (0.035)	0.027 (0.056)	-0.018 (0.017)
2008-2010	Remuneration per head	0.019* (0.010)	0.024** (0.010)	0.028 (0.022)	0.063** (0.027)	0.017 (0.011)	0.004 (0.012)	0.005 (0.011)	0.004 (0.037)	0.013 (0.039)	0.005 (0.011)
	Employment	0.045** (0.019)	0.052*** (0.019)	0.005 (0.027)	0.007 (0.041)	0.054*** (0.018)	0.021 (0.026)	0.029 (0.025)	0.074 (0.054)	0.093 (0.073)	0.021 (0.023)
	Turnover per head	0.005 (0.014)	0.014 (0.013)	0.041 (0.032)	0.118*** (0.037)	-0.008 (0.013)	-0.012 (0.016)	-0.013 (0.016)	0.041 (0.045)	0.050 (0.056)	-0.021 (0.017)
	GVA per head	0.016 (0.021)	0.028 (0.021)	0.006 (0.042)	0.065 (0.052)	0.019 (0.023)	0.032 (0.028)	0.041 (0.028)	-0.096 (0.074)	-0.039 (0.076)	0.053* (0.030)
	PCM	-0.001 (0.005)	0.001 (0.005)	-0.012 (0.009)	-0.010 (0.011)	0.003 (0.006)	0.006 (0.006)	0.006 (0.006)	-0.016 (0.014)	-0.014 (0.016)	0.009 (0.007)
	Investment to turnover	-0.053* (0.028)	-0.060** (0.024)	-0.021 (0.056)	-0.105** (0.047)	-0.052* (0.029)	-0.051 (0.070)	-0.079 (0.072)	0.085 (0.071)	-0.002 (0.022)	-0.092 (0.083)
1997-1998	Remuneration per head	0.009 (0.009)	0.011 (0.009)	0.011 (0.015)	0.021 (0.016)	0.006 (0.010)	0.001 (0.009)	0.004 (0.009)	-0.015 (0.017)	-0.014 (0.019)	0.011 (0.010)
	Employment	-0.021 (0.016)	-0.010 (0.016)	-0.043*** (0.017)	-0.046** (0.019)	-0.003 (0.021)	-0.028 (0.023)	-0.011 (0.020)	-0.059** (0.028)	-0.053** (0.026)	0.002 (0.025)
	Turnover per head	0.018 (0.012)	0.026** (0.011)	-0.001 (0.019)	0.026 (0.018)	0.024* (0.014)	0.001 (0.015)	0.003 (0.014)	-0.039* (0.024)	-0.030 (0.022)	0.017 (0.018)
	GVA per head	0.013 (0.017)	0.025 (0.016)	-0.029 (0.028)	0.012 (0.032)	0.033* (0.018)	0.012 (0.022)	0.006 (0.020)	-0.064 (0.045)	-0.072 (0.044)	0.042** (0.021)
	PCM	0.001 (0.004)	0.003 (0.004)	-0.003 (0.006)	0.003 (0.007)	0.003 (0.004)	0.008* (0.005)	0.008** (0.004)	0.012 (0.010)	0.013 (0.008)	0.006 (0.005)
	Investment to turnover	-0.007 (0.011)	-0.016* (0.009)	0.007 (0.022)	-0.007 (0.020)	-0.019* (0.010)	-0.007 (0.013)	-0.018 (0.013)	-0.011 (0.024)	-0.042* (0.024)	-0.007 (0.016)
1999-2001	Observations	15,191	13,223	5,882	3,995	9,228	6,114	5,599	2,171	1,671	3,928
2005-2007	Observations	13,332	11,562	4,560	2,790	8,772	4,890	4,446	1,254	810	3,636
2008-2010	Observations	14,827	13,557	3,493	2,379	11,178	5,291	5,008	884	633	4,375
1997-1998	Observations	13,095	11,433	5,592	4,020	7,413	4,916	4,537	1,757	1,409	3,128

Notes: FAME; difference-in-difference estimates; market sectors excl. agriculture & finance; accounts with non-missing information on remuneration, employees, profits, turnover, fixed assets in the current and previous years; accounts filed in Q4 and Q1; separate models for each policy period; samples include 3 years before the policy period; balanced panels; regressions include 2-digit SIC dummies, regional dummies, year effects, indicator of foreign ownership, age, and indicator for exporter; equation (1).

Table 5.1.2 Repeated cross section models of annual growth using FAME

	Policy period:	Sector	All Industries					Low Pay Industries					
			Size:	All	10+	<50	10-49	50+	All	10+	<50	10-49	50+
	Annual change in:												
1999-2001	Remuneration per head		0.006*	0.009***	0.004	0.009	0.009**	0.010**	0.012***	0.015*	0.022**	0.007	
			(0.003)	(0.003)	(0.005)	(0.006)	(0.004)	(0.004)	(0.005)	(0.008)	(0.009)	(0.005)	
	Employment		0.002	-0.002	0.005	-0.000	-0.006	-0.004	-0.007	-0.001	-0.015*	-0.005	
			(0.003)	(0.004)	(0.004)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)	(0.007)	
	Turnover per head		-0.003	0.000	-0.004	0.000	0.001	0.013**	0.018***	0.016*	0.031***	0.011	
			(0.004)	(0.005)	(0.007)	(0.008)	(0.005)	(0.005)	(0.006)	(0.009)	(0.011)	(0.007)	
	GVA per head		-0.003	0.001	-0.007	-0.001	0.002	0.006	0.012	0.008	0.023	0.006	
			(0.006)	(0.007)	(0.011)	(0.013)	(0.008)	(0.008)	(0.009)	(0.015)	(0.017)	(0.010)	
	PCM		-0.001	-0.002	-0.002	-0.004*	-0.002	-0.003*	-0.003*	-0.005*	-0.005*	-0.002	
			(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	
	Investment to turnover		-0.001	-0.000	0.000	0.003	-0.003	-0.001	-0.001	-0.000	0.001	-0.004*	
			(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	
2008-2010	Remuneration per head		-0.004	-0.003	-0.004	0.003	-0.004	-0.004	-0.003	0.005	0.017	-0.005	
			(0.003)	(0.003)	(0.006)	(0.008)	(0.003)	(0.004)	(0.004)	(0.011)	(0.013)	(0.004)	
	Employment		0.007**	0.004	0.000	-0.007	0.008*	0.005	0.002	0.006	-0.004	0.001	
			(0.003)	(0.004)	(0.005)	(0.008)	(0.004)	(0.005)	(0.005)	(0.008)	(0.013)	(0.006)	
	Turnover per head		0.003	0.007	0.000	0.020	0.004	0.002	0.001	0.013	0.032*	-0.003	
			(0.004)	(0.005)	(0.009)	(0.012)	(0.005)	(0.006)	(0.006)	(0.015)	(0.018)	(0.006)	
	GVA per head		0.001	0.010	-0.001	0.030	0.006	0.002	0.006	0.005	0.036	0.002	
			(0.007)	(0.007)	(0.015)	(0.020)	(0.008)	(0.009)	(0.010)	(0.026)	(0.031)	(0.010)	
	PCM		-0.000	0.001	-0.002	0.002	0.000	-0.002	-0.000	-0.003	-0.000	-0.001	
			(0.001)	(0.001)	(0.003)	(0.004)	(0.001)	(0.002)	(0.002)	(0.005)	(0.006)	(0.002)	
	Investment to turnover		0.001	0.002	0.000	-0.000	0.002*	0.000	0.001	-0.001	0.000	0.001	
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	
1997-1998	Remuneration per head		-0.002	-0.004	-0.001	-0.004	-0.003	-0.002	-0.005	-0.002	-0.013	-0.001	
			(0.004)	(0.004)	(0.006)	(0.007)	(0.004)	(0.005)	(0.005)	(0.009)	(0.009)	(0.006)	
	Employment		0.006*	0.008*	-0.001	-0.005	0.019***	0.001	0.003	-0.003	-0.001	0.008	
			(0.004)	(0.004)	(0.005)	(0.007)	(0.006)	(0.005)	(0.006)	(0.007)	(0.009)	(0.007)	
	Turnover per head		0.007	0.010**	0.001	0.001	0.018***	0.004	0.005	-0.014	-0.017	0.019**	
			(0.005)	(0.005)	(0.008)	(0.009)	(0.006)	(0.006)	(0.006)	(0.010)	(0.011)	(0.007)	
	GVA per head		0.002	0.001	-0.001	-0.007	0.006	0.001	0.000	-0.013	-0.023	0.013	
			(0.007)	(0.007)	(0.012)	(0.013)	(0.008)	(0.009)	(0.009)	(0.016)	(0.017)	(0.010)	
	PCM		0.001	0.002	0.000	0.001	0.003*	0.002	0.002	-0.001	-0.001	0.004**	
			(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)	
	Investment to turnover		0.002**	0.002*	0.001	0.000	0.004*	0.002	0.002	0.000	-0.002	0.005*	
			(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	
1999-2001	Observations		26,181	20,463	13,583	7,865	12,598	9,896	8,354	4,519	2,977	5,377	
2008-2010	Observations		25,431	21,645	9,494	5,708	15,937	8,590	7,706	2,484	1,600	6,106	
1997-1998	Observations		20,874	16,847	11,375	7,348	9,499	7,693	6,694	3,681	2,682	4,012	

Notes: FAME; market sectors outside agriculture and finance; accounts with non-missing information on remuneration, employees, profits, turnover, fixed assets in the current and previous years; accounts filed in Q4 and Q1; separate models for each policy period; samples include 3 years before the policy period; regressions include 2-digit SIC dummies, regional dummies, year effects, indicator of foreign ownership, age, and indicator for exporter; robust regression; equation (2).

Table 5.1.3 Repeated cross section models of annual growth using FAME 1994-2010

	Policy period:	Annual change in:	Sector		All Industries				Low Pay Industries				
			Size:	All	10+	<50	10-49	50+	All	10+	<50	10-49	50+
1999-2007		Remuneration per head		0.001	0.005**	-0.001	0.005	0.005**	0.004	0.005*	0.006	0.008	0.004
			(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)	(0.005)	(0.006)	(0.003)	
		Employment		0.007***	0.007***	0.006**	0.005	0.005	0.002	0.003	0.001	-0.002	0.003
			(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.006)	(0.004)	
		Turnover per head		-0.007***	-0.005	-0.006	-0.002	-0.005	0.004	0.008**	0.003	0.009	0.007
			(0.003)	(0.003)	(0.005)	(0.006)	(0.004)	(0.004)	(0.004)	(0.007)	(0.008)	(0.004)	
2008-2010 (change on 1999-2007)		GVA per head		-0.009**	-0.006	-0.009	-0.004	-0.006	-0.002	0.002	-0.004	0.007	-0.001
			(0.004)	(0.004)	(0.008)	(0.009)	(0.005)	(0.006)	(0.006)	(0.011)	(0.012)	(0.007)	
		PCM		-0.003***	-0.003***	-0.003**	-0.003*	-0.003**	-0.003***	-0.003***	-0.004**	-0.004*	-0.002*
			(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	
		Investment to turnover		-0.000	-0.000	0.000	0.001	-0.001	0.000	-0.000	0.000	-0.001	-0.000
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
Observations			82,746	67,619	37,732	22,605	45,014	29,932	26,027	11,623	7,717	18,310	

Notes: FAME; market sectors outside agriculture and finance; accounts with non-missing information on remuneration, employees, profits, turnover, fixed assets in the current and previous years; accounts filed in Q4 and Q1; regressions include 2-digit SIC dummies, regional dummies, year effects, indicator of foreign ownership, age, and indicator for exporter; robust regression; equation (3).

Table 5.1.4 Longitudinal panel models of investment and credit using FAME

	Sector Size:	All Industries					Low Pay Industries				
	All	10+	<50	10-49	50+	All	10+	<50	10-49	50+	
<b>Policy period:</b>											
2008-2010 Vulnerability to credit constraints measured by the ratio of short term loans to turnover											
	NMW DID	-0.011 (0.027)	-0.035* (0.021)	-0.004 (0.058)	-0.113** (0.054)	-0.027 (0.019)	-0.030** (0.015)	-0.032** (0.016)	-0.059* (0.032)	-0.076** (0.030)	-0.033* (0.020)
	CREDIT DID	-0.070 (0.057)	-0.017 (0.020)	-0.101 (0.125)	0.004 (0.039)	-0.043 (0.027)	-0.035* (0.021)	-0.045** (0.022)	-0.054 (0.037)	-0.062 (0.039)	-0.063** (0.030)
	Interaction	-0.087 (0.135)	-0.117 (0.109)	0.029 (0.209)	0.110 (0.089)	-0.092 (0.081)	0.101 (0.073)	0.043 (0.052)	0.205 (0.138)	0.094 (0.062)	0.047 (0.079)
	Observations	12,660	10,740	4,450	2,530	8,210	4,610	4,090	1,406	886	3,204
2008-2010 Vulnerability to credit constraints measured by the gearing ratio											
	NMW DID	0.002 (0.031)	-0.054** (0.023)	0.078 (0.071)	-0.092** (0.046)	-0.024 (0.019)	0.021 (0.026)	0.003 (0.018)	0.046 (0.075)	-0.030 (0.030)	0.005 (0.021)
	CREDIT DID	0.000 (0.003)	0.001 (0.002)	-0.000 (0.006)	-0.003 (0.006)	0.001 (0.002)	0.005* (0.003)	0.005** (0.003)	0.004 (0.006)	0.004 (0.006)	0.005* (0.002)
	Interaction	-0.014 (0.009)	-0.005 (0.004)	-0.018 (0.014)	0.005 (0.008)	-0.011* (0.006)	-0.010** (0.004)	-0.010*** (0.004)	-0.009 (0.009)	-0.004 (0.006)	-0.012** (0.005)
	Observations	12,660	10,740	4,450	2,530	8,210	4,610	4,090	1,406	886	3,204
1999-2001 Vulnerability to credit constraints measured by the ratio of short term loans to turnover											
	NMW DID	-0.005 (0.011)	-0.003 (0.012)	-0.002 (0.016)	-0.001 (0.019)	-0.010 (0.016)	0.006 (0.017)	0.006 (0.019)	0.039 (0.031)	0.058 (0.042)	-0.033* (0.018)
	CREDIT DID	-0.001 (0.021)	-0.007 (0.020)	-0.008 (0.028)	-0.017 (0.032)	0.004 (0.025)	-0.023 (0.034)	-0.026 (0.039)	-0.023 (0.046)	-0.024 (0.060)	-0.021 (0.052)
	Interaction	0.107** (0.051)	0.027 (0.060)	0.115* (0.059)	-0.002 (0.066)	0.086 (0.110)	0.052 (0.065)	0.059 (0.088)	-0.032 (0.070)	-0.074 (0.112)	0.225* (0.133)
	Observations	20,793	16,133	11,388	6,728	9,405	7,806	6,572	3,763	2,529	4,043
1999-2001 Vulnerability to credit constraints measured by the gearing ratio											
	NMW DID	0.008 (0.015)	0.010 (0.014)	0.016 (0.024)	0.016 (0.021)	0.001 (0.017)	0.029 (0.019)	0.036* (0.021)	0.054 (0.034)	0.084* (0.047)	0.002 (0.019)
	CREDIT DID	-0.001 (0.002)	-0.001 (0.002)	0.000 (0.002)	0.002 (0.002)	-0.004 (0.003)	0.001 (0.002)	0.001 (0.002)	0.005 (0.004)	0.008* (0.005)	-0.004 (0.003)
	Interaction	0.006 (0.004)	-0.002 (0.004)	0.005 (0.005)	-0.006 (0.005)	0.004 (0.006)	-0.004 (0.004)	-0.006 (0.006)	-0.009* (0.005)	-0.016* (0.009)	0.004 (0.006)
	Observations	20,793	16,133	11,388	6,728	9,405	7,806	6,572	3,763	2,529	4,043

THE IMPACT OF THE NATIONAL MINIMUM WAGE ON FIRM BEHAVIOUR DURING RECESSION

**Table 5.1.5 Business exit rates (3-year) using FAME**

		Sector Size:	All Industries					Low Pay Industries				
Policy cohort:	Compared to cohort:		All	10+	<50	10-49	50+	All	10+	<50	10-49	50+
1998	1995		-0.011 (0.008)	-0.018** (0.009)	-0.005 (0.012)	-0.019 (0.014)	-0.019* (0.010)	-0.001 (0.011)	-0.001 (0.011)	0.010 (0.026)	0.020 (0.031)	-0.009 (0.010)
	Observations		13,056	10,291	7,356	4,599	5,657	4,575	3,930	1,910	1,349	2,276
2007	2004		-0.006 (0.009)	-0.013 (0.010)	-0.002 (0.015)	-0.018 (0.018)	-0.010 (0.012)	-0.020 (0.013)	-0.027** (0.013)	0.020 (0.028)	-0.004 (0.028)	-0.031** (0.014)
	Observations		11,229	8,978	4,791	2,536	6,319	3,788	3,207	1,310	713	2,432
2007	1998		0.006 (0.011)	-0.000 (0.012)	0.001 (0.015)	-0.014 (0.018)	0.007 (0.015)	0.023 (0.018)	0.015 (0.018)	0.049 (0.032)	0.012 (0.029)	0.016 (0.022)
	Observations		12,305	9,769	5,897	3,364	6,379	4,207	3,567	1,727	1,066	2,443
2007	1995		-0.006 (0.010)	-0.020** (0.010)	-0.005 (0.015)	-0.030* (0.016)	-0.017 (0.012)	0.018 (0.018)	0.009 (0.018)	0.060* (0.035)	0.037 (0.037)	-0.006 (0.020)
	Observations		11,923	9,681	5,826	3,602	6,032	4,053	3,551	1,710	1,216	2,312

**Table 5.2.1** Difference-in-Differences, wage results, ARD.

	All					Low Pay				
	E>1	E>10	1<E<50	10<E<50	E=50+	E>1	E>10	1<E<50	10<E<50	E=50+
<b>Introduction</b>										
<u>T=3-8k; C=8-13k</u>	0.022*** (0.004)	0.018*** (0.005)	0.026*** (0.005)	0.022*** (0.006)	0.015** (0.006)	0.035*** (0.006)	0.031*** (0.007)	0.039*** (0.007)	0.034*** (0.010)	0.029*** (0.008)
<u>T=3-10k; C=10-17k</u>	0.014*** (0.004)	0.008** (0.004)	0.021*** (0.005)	0.015** (0.006)	0.006 (0.006)	0.033*** (0.007)	0.027*** (0.007)	0.041*** (0.009)	0.032*** (0.011)	0.025*** (0.009)
<u>T=3-12k; C=12-21k</u>	0.007* (0.004)	0.004 (0.004)	0.019*** (0.005)	0.014** (0.006)	0.000 (0.005)	0.028*** (0.008)	0.021*** (0.008)	0.036*** (0.010)	0.025** (0.013)	0.023*** (0.009)
<b>1999-2007</b>										
<u>T=3-8k; C=8-13k</u>	0.023*** (0.003)	0.024*** (0.004)	0.025*** (0.004)	0.032*** (0.005)	0.012** (0.005)	0.043*** (0.005)	0.040*** (0.005)	0.043*** (0.006)	0.043*** (0.008)	0.031*** (0.007)
<u>T=3-10k; C=10-17k</u>	0.019*** (0.003)	0.016*** (0.003)	0.027*** (0.004)	0.030*** (0.005)	0.001 (0.005)	0.044*** (0.006)	0.034*** (0.006)	0.051*** (0.007)	0.044*** (0.009)	0.021*** (0.007)
<u>T=3-12k; C=12-21k</u>	0.019*** (0.003)	0.016*** (0.003)	0.033*** (0.004)	0.033*** (0.005)	0.003 (0.004)	0.043*** (0.006)	0.032*** (0.006)	0.055*** (0.009)	0.045*** (0.011)	0.023*** (0.008)
<b>Falsification</b>										
<u>T=3-8k; C=8-13k</u>	-0.013** (0.006)	-0.004 (0.007)	-0.015* (0.008)	0.000 (0.011)	-0.012 (0.010)	-0.006 (0.009)	0.002 (0.010)	-0.008 (0.012)	0.008 (0.016)	-0.001 (0.013)
<u>T=3-10k; C=10-17k</u>	-0.010 (0.006)	0.000 (0.006)	-0.007 (0.008)	0.010 (0.010)	-0.008 (0.008)	-0.006 (0.010)	0.000 (0.011)	0.003 (0.014)	0.016 (0.018)	-0.007 (0.013)
<u>T=3-12k; C=12-21k</u>	0.000 (0.006)	0.008 (0.006)	0.004 (0.009)	0.020** (0.009)	0.01 (0.007)	-0.012 (0.012)	-0.005 (0.012)	-0.009 (0.017)	-0.002 (0.020)	0.008 (0.014)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: ARD, 1997-2007. Notes: Robust regression standard errors reported in brackets. \*\*\* Significant at 1% level, \*\* significant at 5% level, \* significant at 10% level.

THE IMPACT OF THE NATIONAL MINIMUM WAGE ON FIRM BEHAVIOUR DURING RECESSION

**Table 5.2.2 Difference-in-Differences, other outcomes results, ARD: 1999-2007.**

	All					Low Pay				
	E>1	E>10	1<E<50	10<E<50	E=50+	E>1	E>10	1<E<50	10<E<50	E=50+
<b>Employment</b>										
<u>T=3-8k; C=8-13k</u>	0.108*** (0.021)	0.109*** (0.022)	0.001 (0.016)	0.036*** (0.012)	0.024 (0.029)	0.133*** (0.030)	0.100*** (0.031)	0.034 (0.023)	0.020 (0.016)	0.074* (0.039)
<u>T=3-10k; C=10-17k</u>	0.065*** (0.018)	0.069*** (0.019)	-0.019 (0.014)	0.023** (0.010)	-0.008 (0.024)	0.121*** (0.029)	0.090*** (0.029)	0.021 (0.023)	0.003 (0.015)	0.061* (0.035)
<u>T=3-12k; C=12-21k</u>	0.005 (0.017)	0.021 (0.017)	-0.025* (0.013)	0.019** (0.009)	-0.019 (0.020)	0.061** (0.029)	0.051* (0.029)	0.000 (0.024)	-0.002 (0.015)	0.036 (0.033)
<b>Lab prod</b>										
<u>T=3-8k; C=8-13k</u>	0.000 (0.008)	0.008 (0.009)	-0.009 (0.010)	-0.001 (0.013)	0.010 (0.012)	-0.002 (0.011)	0.009 (0.012)	-0.020 (0.014)	-0.005 (0.017)	0.012 (0.016)
<u>T=3-10k; C=10-17k</u>	0.017** (0.007)	0.011 (0.007)	0.013 (0.009)	0.014 (0.010)	0.001 (0.010)	0.011 (0.010)	0.011 (0.011)	0.000 (0.014)	0.003 (0.017)	0.023 (0.015)
<u>T=3-12k; C=12-21k</u>	0.039*** (0.006)	0.031*** (0.006)	0.039*** (0.008)	0.041*** (0.009)	0.009 (0.009)	0.034*** (0.011)	0.029*** (0.011)	0.032** (0.015)	0.034** (0.017)	0.025* (0.015)
<b>PCM</b>										
<u>T=3-8k; C=8-13k</u>	-0.013*** (0.004)	-0.011** (0.005)	-0.019*** (0.005)	-0.020*** (0.007)	-0.002 (0.007)	-0.023*** (0.006)	-0.021*** (0.007)	-0.031*** (0.007)	-0.029*** (0.010)	-0.015 (0.009)
<u>T=3-10k; C=10-17k</u>	-0.002 (0.004)	-0.005 (0.004)	-0.008* (0.004)	-0.012** (0.006)	-0.003 (0.006)	-0.019*** (0.006)	-0.020*** (0.006)	-0.029*** (0.007)	-0.031*** (0.009)	-0.012 (0.008)
<u>T=3-12k; C=12-21k</u>	0.013*** (0.003)	0.009** (0.004)	0.004 (0.004)	0.004 (0.005)	0.005 (0.005)	-0.003 (0.006)	-0.004 (0.006)	-0.013* (0.007)	-0.011 (0.009)	-0.004 (0.008)
<b>Investment</b>										
<u>T=3-8k; C=8-13k</u>	0.003 (0.003)	-0.001 (0.005)	0.003 (0.004)	-0.001 (0.005)	-0.007 (0.009)	0.001 (0.004)	0.000 (0.006)	0.000 (0.004)	-0.001 (0.006)	-0.009 (0.010)
<u>T=3-10k; C=10-17k</u>	0.006* (0.003)	0.004 (0.004)	0.005 (0.003)	0.004 (0.004)	0.001 (0.007)	0.003 (0.003)	0.000 (0.005)	0.005 (0.004)	0.006 (0.006)	-0.012 (0.008)
<u>T=3-12k; C=12-21k</u>	0.009** (0.004)	0.008** (0.003)	0.006 (0.005)	0.003 (0.004)	0.010* (0.005)	0.014** (0.007)	0.009** (0.004)	0.016 (0.004)	0.008 (0.011)	0.005 (0.006)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: ARD, 1997-2007. Notes: Robust regression standard errors reported in brackets. \*\*\* Significant at 1% level, \*\* significant at 5% level, \* significant at 10% level

THE IMPACT OF THE NATIONAL MINIMUM WAGE ON FIRM BEHAVIOUR DURING RECESSION

**Table 5.2.3** Difference-in-Differences, other outcomes results, ARD: introduction (1999-2001).

	All					Low Pay				
	E>1	E>10	1<E<50	10<E<50	E=50+	E>1	E>10	1<E<50	10<E<50	E=50+
<b>Employment</b>										
<u>T=3-8k; C=8-13k</u>	0.125*** (0.024)	0.050* (0.026)	0.052*** (0.019)	0.030** (0.014)	-0.016 (0.034)	0.121*** (0.033)	0.043 (0.035)	0.061** (0.026)	0.008 (0.018)	0.061 (0.045)
<u>T=3-10k; C=10-17k</u>	0.060*** (0.021)	0.028 (0.022)	0.015 (0.017)	0.019 (0.012)	-0.039 (0.027)	0.075** (0.033)	0.014 (0.034)	0.04 (0.026)	-0.010 (0.018)	0.009 (0.039)
<u>T=3-12k; C=12-21k</u>	-0.009 (0.020)	-0.001 (0.020)	-0.013 (0.016)	0.010 (0.011)	-0.008 (0.023)	0.014 (0.033)	0.002 (0.033)	-0.001 (0.027)	-0.012 (0.018)	0.023 (0.037)
<b>Lab prod</b>										
<u>T=3-8k; C=8-13k</u>	0.018* (0.009)	0.022** (0.011)	0.009 (0.012)	0.015 (0.015)	0.023 (0.015)	0.024* (0.013)	0.029** (0.014)	0.015 (0.017)	0.024 (0.020)	0.023 (0.019)
<u>T=3-10k; C=10-17k</u>	0.028*** (0.008)	0.015* (0.009)	0.024** (0.010)	0.017 (0.012)	0.016 (0.012)	0.034*** (0.012)	0.028** (0.013)	0.028* (0.017)	0.030 (0.020)	0.035** (0.018)
<u>T=3-12k; C=12-21k</u>	0.039*** (0.007)	0.023*** (0.007)	0.041*** (0.009)	0.034*** (0.011)	0.007 (0.010)	0.044*** (0.012)	0.031** (0.013)	0.054*** (0.017)	0.048** (0.021)	0.018 (0.017)
<b>PCM</b>										
<u>T=3-8k; C=8-13k</u>	-0.006 (0.005)	0.000 (0.006)	-0.013** (0.006)	-0.006 (0.008)	0.002 (0.009)	-0.008 (0.007)	-0.004 (0.008)	-0.015* (0.008)	-0.007 (0.011)	-0.010 (0.011)
<u>T=3-10k; C=10-17k</u>	0.006 (0.004)	0.004 (0.005)	-0.001 (0.005)	-0.002 (0.007)	0.007 (0.007)	-0.002 (0.007)	-0.003 (0.007)	-0.009 (0.008)	-0.008 (0.011)	-0.003 (0.010)
<u>T=3-12k; C=12-21k</u>	0.019*** (0.004)	0.014*** (0.004)	0.013*** (0.005)	0.013** (0.006)	0.007 (0.006)	0.01 (0.007)	0.003 (0.007)	0.009 (0.009)	0.008 (0.011)	-0.005 (0.009)
<b>Investment</b>										
<u>T=3-8k; C=8-13k</u>	0.000 (0.004)	0.000 (0.005)	0.001 (0.004)	0.002 (0.006)	-0.007 (0.010)	0.003 (0.005)	0.005 (0.006)	0.002 (0.005)	0.007 (0.006)	-0.002 (0.011)
<u>T=3-10k; C=10-17k</u>	0.000 (0.004)	-0.002 (0.004)	0.002 (0.004)	0.001 (0.005)	-0.008 (0.008)	-0.001 (0.005)	-0.007 (0.006)	0.006 (0.006)	0.006 (0.007)	-0.023** (0.010)
<u>T=3-12k; C=12-21k</u>	0.005 (0.004)	0.003 (0.004)	0.005 (0.005)	0.002 (0.004)	0.004 (0.006)	0.013 (0.008)	0.003 (0.006)	0.019 (0.006)	0.008 (0.012)	-0.001 (0.007)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: ARD, 1997-2001. Notes: Robust regression standard errors reported in brackets. \*\*\* significant at 1% level, \*\* significant at 5% level, \* significant at 10% level.

THE IMPACT OF THE NATIONAL MINIMUM WAGE ON FIRM BEHAVIOUR DURING RECESSION

**Table 5.2.4** Difference-in-Differences, other outcomes results, ARD: falsification tests (1997-1998)

	All					Low Pay				
	E>1	E>10	1<E<50	10<E<50	E=50+	E>1	E>10	1<E<50	10<E<50	E=50+
<b>Employment</b>										
<u>T=3-8k; C=8-13k</u>	0.007 (0.039)	-0.021 (0.042)	-0.025 (0.030)	0.005 (0.023)	-0.116** (0.051)	-0.01 (0.053)	-0.054 (0.055)	-0.013 (0.042)	0.000 (0.029)	-0.106 (0.065)
<u>T=3-10k; C=10-17k</u>	0.038 (0.034)	-0.034 (0.035)	0.005 (0.027)	-0.007 (0.019)	-0.005 (0.040)	-0.007 (0.051)	-0.086* (0.051)	0.02 (0.042)	0.005 (0.028)	-0.023 (0.057)
<u>T=3-12k; C=12-21k</u>	0.064** (0.031)	-0.041 (0.031)	0.055** (0.025)	0.002 (0.017)	0.011 (0.034)	0.025 (0.052)	-0.066 (0.050)	0.07 (0.043)	0.022 (0.029)	0.025 (0.053)
<b>Lab prod</b>										
<u>T=3-8k; C=8-13k</u>	-0.007 (0.015)	0.005 (0.016)	-0.015 (0.019)	0.009 (0.024)	0.014 (0.022)	-0.014 (0.020)	0.004 (0.021)	-0.027 (0.027)	-0.005 (0.033)	0.022 (0.028)
<u>T=3-10k; C=10-17k</u>	0.005 (0.012)	0.013 (0.013)	0.000 (0.017)	0.022 (0.02)	0.012 (0.018)	0.003 (0.019)	0.021 (0.020)	0.012 (0.027)	0.052 (0.032)	0.012 (0.026)
<u>T=3-12k; C=12-21k</u>	0.009 (0.011)	0.011 (0.012)	-0.005 (0.015)	-0.001 (0.018)	0.028* (0.016)	-0.012 (0.019)	-0.004 (0.020)	-0.012 (0.027)	-0.004 (0.033)	0.023 (0.026)
<b>PCM</b>										
<u>T=3-8k; C=8-13k</u>	0.012 (0.008)	0.011 (0.009)	0.010 (0.009)	0.003 (0.013)	0.021 (0.013)	0.006 (0.011)	0.010 (0.012)	-0.002 (0.014)	-0.007 (0.018)	0.018 (0.016)
<u>T=3-10k; C=10-17k</u>	0.01 (0.007)	0.009 (0.008)	0.007 (0.008)	0.007 (0.011)	0.013 (0.011)	0.017* (0.010)	0.025** (0.011)	0.014 (0.014)	0.029* (0.018)	0.017 (0.014)
<u>T=3-12k; C=12-21k</u>	0.008 (0.006)	0.002 (0.006)	-0.001 (0.008)	-0.009 (0.010)	0.007 (0.009)	0.006 (0.010)	0.003 (0.011)	0.01 (0.014)	0.009 (0.018)	-0.002 (0.013)
<b>Investment</b>										
<u>T=3-8k; C=8-13k</u>	0.002 (0.006)	0.007 (0.008)	0.002 (0.007)	0.007 (0.009)	0.015 (0.018)	0.004 (0.007)	0.005 (0.010)	0.000 (0.006)	-0.005 (0.009)	0.023 (0.020)
<u>T=3-10k; C=10-17k</u>	0.006 (0.005)	0.015** (0.007)	-0.003 (0.006)	0.004 (0.007)	0.033*** (0.013)	0.008 (0.006)	0.008 (0.008)	0.001 (0.006)	-0.006 (0.010)	0.032** (0.013)
<u>T=3-12k; C=12-21k</u>	0.001 (0.007)	0.014** (0.006)	-0.011 (0.010)	0.008 (0.007)	0.023** (0.009)	-0.007 (0.014)	0.004 (0.007)	-0.022 (0.024)	-0.007 (0.010)	0.021* (0.011)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: ARD, 1997-1998. Notes: Robust regression standard errors reported in brackets. \*\*\* significant at 1% level, \*\* significant at 5% level, \* significant at 10% level.

THE IMPACT OF THE NATIONAL MINIMUM WAGE ON FIRM BEHAVIOUR DURING RECESSION

**Table 5.2.5 Sample sizes ARD**

	All					Low Pay				
	E>1	E>10	1<E<50	10<E<50	E=50+	E>1	E>10	1<E<50	10<E<50	E=50+
<b>Introduction</b>										
<u>3-13k</u>	63,760	38,489	47,824	22,553	15,935	34,470	21,025	25,468	12,025	9,002
<u>3-17k</u>	89,702	60,058	62,185	32,540	27,517	41,595	26,776	29,259	14,439	12,335
<u>3-21k</u>	113,012	80,530	73,445	40,963	39,567	46,025	30,496	31,505	15,973	14,520
<b>1999-2007</b>										
<u>3-13k</u>	140,021	85,944	104,834	50,758	35,186	74,128	46,073	54,362	26,311	19,762
<u>3-17k</u>	198,156	134,421	138,041	74,305	60,115	89,354	58,318	62,708	31,672	26,644
<u>3-21k</u>	248,497	178,548	163,103	93,155	85,394	98,417	65,831	67,449	34,861	30,969
<b>Falsification</b>										
<u>3-13k</u>	24,560	15,125	17,989	8,555	6,569	13,525	8,301	9,841	4,617	3,683
<u>3-17k</u>	34,544	23,581	23,195	12,232	11,350	16,421	10,688	11,315	5,584	5,102
<u>3-21k</u>	43,580	31,561	27,299	15,280	16,281	18,234	12,225	12,173	6,165	6,060

Source: ARD, 1997-2007. Notes: Robust regression standard errors reported in brackets. \*\*\* significant at 1% level, \*\* significant at 5% level, \* significant at 10% level.