I. Introduction

In the debate on the economic impact of labour market regulation much work has focused on minimum wages. Although standard economic analysis implies that wage floors should have a negative impact on employment (Borjas, 2004; Brown, 2003), the existing empirical literature poses a paradox. Many studies have shown that minimum wages have significant effects on the structure of wages, by increasing the relative wages of the lowest paid workers (e.g. DiNardo et al, 1996), but most empirical studies struggle to find a negative impact on jobs (e.g. Card and Krueger, 1994).

If wages rise, but employment is steady, how are firms able to sustain higher wage costs induced by the minimum wage? It is evident that, if employment effects are modest, then "something else has to give". One possibility is that firms earn normal returns and simply pass on higher wage costs to consumers in the form of price increases. However, there is scant evidence on this score (an exception is Aaronson, 2001).[1] An alternative is that not all the higher wage costs are passed on and that the minimum wage directly eats into profit margins.[2] Since there is a complete absence of any study directly examining the impact of minimum wages on profitability, this is the focus of our study.

Our identification strategy uses variations in wages induced by the introduction of the national minimum wage (NMW) in the UK as a quasi-experiment to examine the impact of wage floors on firm profitability. The introduction occurred in 1999, two years after the election of the New Labour government that ended seventeen years of Conservative administration. There is evidence that the NMW increased wages for the low paid, but had little impact on employment[3] (similar to evidence from the US and

[1] See also the recent survey on minimum wages and prices by Lemos (2005).
[2] A third possibility is that minimum wages may "shock" firms into reducing managerial slack and improving efficiency. We found no evidence of such productivity impacts from the minimum wage in our data (we examine this hypothesis below but find no evidence in its favour).
[3] See Machin, Manning and Rahman (2003) and Stewart (2004).

other countries). We use the fact that the intensity (or "bite") of the NMW is higher for firms with many low paid workers relative to firms with few low paid workers in order to construct treatment and comparison groups. We then compare outcomes in terms of wages, profitability and firm survival using difference in differences methods.

Our work *does* uncover a significant negative association between the minimum wage introduction and firm profitability. This association is robust across two very different panel data sources, namely a specialised UK data source on workers in residential care homes and an economy-wide firm level database FAME (Financial Analysis Made Easy) that covers all registered firms in the UK.[4] In both data sets, firm profit margins were reduced following the minimum wage introduction in low wage firms compared to margins in higher wage firms. These effects correspond to about a 23 percent fall in profit margins for the average care home and an 8 to 11 percent reduction in profit margins for the average affected firms in FAME. Moreover, for both data sources, we are not able to find any evidence that this resulted in a higher probability of closure for low wage firms. As such, it appears that wage gains accruing to low wage workers because of the introduction of the national minimum wage were financed by squeezing firm profit margins.

The rest of the paper is structured as follows. In Section II we discuss the scope for minimum wages to impact on profitability and describe our modelling approach. Section III implements this empirical strategy using data for the UK residential care homes sector. This is a competitive, low wage sector where the minimum wage "bit hard" when it was introduced to the UK labour market. Importantly our care homes data features detailed firm level measures of treatment intensity in response to this policy

[4] UK firm-level panel data on company accounts is more easily available than in the US. UK firms have to lodge accounting information centrally at Companies House even if they are not listed on the stock

change as we have data on the wage distribution within homes. Section IV reports results from analysing the other data source, FAME. This has the twin advantages that it (i) covers many small, low wage service sector firms and (ii) contains direct measures of gross profits. The disadvantage is that FAME only has a measure of the average wage within the firm. The FAME analysis therefore serves as a useful point of comparison and corroboration for the estimates based on the care homes data. Section V offers some concluding comments.

II. Motivation and Modelling Strategy

A. The Scope for Minimum Wages to Impact on Profitability

The usual rationale given for the existence of minimum wages is their ability to increase the earnings of low paid workers in order to prevent exploitation and reduce poverty. When introduced at a binding level, and so long as workers keep their jobs, a minimum wage results in wage gains for workers who would have otherwise been paid beneath the minimum wage level. There may of course be unintended effects of labour market regulations that could produce harmful side effects.[5]

Until the national minimum wage was introduced in April 1999, there had never been a nationwide wage floor in operation in the UK labour market. The system that had existed prior to then, and that was abolished in 1993, was an industry-based system known as the Wages Councils.[6] The 1999 introduction seemed to have a significant impact on wages, albeit a smaller one than was predicted ex-ante, with around 4% to 5% of workers benefiting and receiving average wage gains of the order of about 10%.[7]

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exchange. Further, this information includes profits and wages (wages are not a mandatory reporting item in US company accounts even for publicly listed firms).

[5] See Besley and Burgess (2004) for an example of how tougher labour market regulation in India appeared to increase poverty levels.

[6] See Dickens, Machin and Manning (1999) or Machin and Manning (1994).

[7] See Dickens and Manning (2004) and Metcalf (2002)

It has proven difficult to identify evidence of negative employment effects associated with these wage gains. Stewart (2004) has presented robust evidence, from a range of longitudinal data sources, appearing to show that there have been no reductions in the employment probabilities of minimum wage workers as compared to workers slightly higher up the wage distribution. Even in the very low wage labour market for care assistants, a sector one can view as being highly vulnerable to minimum wage legislation, Machin, Manning and Rahman (2003) report only moderate disemployment effects.

So, if employment does not fall and wage costs rise, then how do employers respond to the introduction of the NMW? Following Ashenfelter and Smith (1979) consider a typical profit maximizing firm employing a quantity of labor (L) at wage rate (W), using other factors at price r and selling its output at price p. Profits are maximized at $\Pi^{*}(W,r,p)$ given the values of W, r and p. The derivative of the profit function with respect to the wage rate is $\partial\Pi/\partial W = -L(W,r,p)$, the negative of the demand for labor. In turn, the second derivative is $\partial^2\Pi/\partial W^2 = -\partial L/\partial W$. The introduction of a minimum wage (M) at a level above that of the prevailing wage therefore reduces the firm's profits by $\Pi(W,r,p)-\Pi(M,r,p)$. Using a second-order Taylor series this can be approximated as:

$$\Pi(W,r,p) - \Pi(M,r,p) \cong L(M-W) - 1/2(\partial L/\partial W)(M-W)^2 \qquad (1)$$

where the terms on the right-hand side correspond to wage bill and labour demand effects on profits respectively. The wage bill effect is the direct impact of higher wages holding employment constant. The second term (labour demand) offsets this profit loss to the extent that firms can substitute away from low wage workers into other factors (e.g. capital). This illustrates the inverse relationship between a firm's initial wage and the post-policy change in its profits. That is, the lower the initial wage, then the greater the

fall in profits associated with the imposition of a minimum wage. Our difference-in-difference models operationalize this idea by defining treatment groups of more affected firms, and comparison groups of less affected firms, on the basis of their wages prior to the policy introduction.

It may be surprising that there is so little evidence on the association between minimum wages and firm profitability. Card and Krueger (1995) provide the most notable study of profitability effects. They first develop a hypothetical example of a firm heavily affected by a 15% increase in the minimum wage and show that such firms face potentially large profit effects of 45% reductions in annual profits and 5% fall in market value. The remainder of their study then focuses on the shareholder value of firms. They report mixed evidence – while no systematic relationship is apparent for the major US uprating of the federal minimum wage in 1989, subsequent news about possible minimum wage increases coincided with one to two percent variations in shareholder wealth. Crucially, it can be argued that Card and Krueger examine profitability effects indirectly through investor expectations (which are problematic if stock market prices are erratic). In contrast, our current study considers the direct impact of the minimum wage on firm profit margins.[8] Furthermore, since firms on the stock market tend to be the larger firms (especially in the UK) a focus on market value will miss out the small and medium sized firms who may be most affected by minimum wages.

B. Modelling Strategy

The approach we take to identify minimum wage effects is in line with other work that looks at the impact of national minimum wages, by looking at a group of firms that

[8] This is important since it is well known that stock market prices may not reflect the fundamental value of the firm. Bubbles can exist both at the macro or micro level (e.g. Bond and Cummins, 2000). Furthermore, it is very difficult to know when the "news" of the National Minimum Wage first reaches the market as investors will form expectations of the level of the NMW and this will be reflected in the stock price long before the official rate is announced.

was more affected by the NMW introduction than a comparison set of firms.[9] By "more affected" here we mean where wages went up by more due to the imposition, or raising, of the minimum wage floor. This quasi-experimental setting enables us to compare what happened to profitability before and after NMW introduction in low wage firms as compared to what happened to profitability across the same period for a comparison group of firms whose wages were not affected so much (or at all) by the NMW introduction.

For ease of exposition, we begin our discussion of modelling by thinking in terms of a discrete indicator of treatment by the minimum wage policy for a set of low wage firms with a pre-policy introduction wage beneath a certain wage threshold W* (where W* may or may not equal the minimum wage M). A treatment indicator variable can be coded as T = 1 for low wage firms (where $W^{pre} < W^*$) and T = 0 for a set of firms whose pre-policy wage exceeds the threshold.[10]

We can evaluate the impact of minimum wages on firm profitability by comparing what happens to profitability, π, before and after minimum wage introduction across these treatment and control firms. For this procedure to be valid, we first need to establish that our choice of affected firms behave as we would expect in response to NMW introduction. The expected response would be that wages rise by more in the T = 1 firms before and after introduction as compared to the T = 0 firms.

A difference-in-difference estimate of the wage impact of the NMW is $(\overline{w}^{T=1}_{NMW=1} - \overline{w}^{T=1}_{NMW=0}) - (\overline{w}^{T=0}_{NMW=1} - \overline{w}^{T=0}_{NMW=0})$, where w = ln(W), NMW is a dummy variable equal to 1 for time periods when the NMW was in place (and 0 for pre-policy periods) and a bar denotes a mean. This is just the simple difference in means

[9] See, amongst others, Card's (1992) analysis of state variations in low pay incidence to identify the employment impact of the US federal minimum wage, or Stewart's (2002) similar analysis of regional variations in the UK NMW.

unconditional on other characteristics of firms. It can easily be placed into a regression context. If T = 1 for firms with pre-policy ln(wages), $w_{i,t-1,}$ less than the ln(minimum wage), mw_t, and 0 otherwise we can enter the indicator function $I(w_{i,t-1} < mw_t)$ into a ln(wage) equation for firm i in year t as follows:

$$\ln(W_{it}) = \alpha_1 + \beta_1 X_{it} + \delta_1 Y_t + \theta_1 I(w_{i,t-1} < mw_t) + \psi_1 [I(w_{i,t-1} < mw_t) * NMW_t] + \varepsilon_{1it} \quad (2)$$

where X is a set of control variables, Y denotes a set of year effects (hence a linear term in NMW_t does not enter the equation since it is controlled for by these) and ε is a random error. Here the regression corrected difference-in-difference estimate of the impact of NMW introduction on ln(wages) is the estimated coefficient on the low wage treatment dummy in the periods when the NMW was in operation, ψ_1.

After ascertaining whether the NMW impacts on wage in the expected manner we can move on to consider whether profitability was affected differentially between the treatment group firms (T = 1) and comparison group firms (T = 0). We can do so by looking at unconditional and conditional difference-in-difference estimates in an analogous way to the wage effects. Thus, we can estimate the unconditional difference-in-difference as $(\bar{\pi}^{T=1}_{NMW=1} - \bar{\pi}^{T=1}_{NMW=0}) - (\bar{\pi}^{T=0}_{NMW=1} - \bar{\pi}^{T=0}_{NMW=0})$ and the conditional difference-in-difference, ψ_2, from the regression model:

$$\pi_{it} = \alpha_2 + \beta_2 Z_{it} + \delta_2 Y_t + \theta_2 I(w_{i,t-1} < mw_t) + \psi_2 [I(w_{i,t-1} < mw_t) * NMW_t] + \varepsilon_{2it} \quad (3)$$

where the controls are now *Z* (which may perfectly overlap with *X* – see below) and ε_{2it} is the error term in the profitability equation.

C. Modelling Issues

Two main issues arise with this modelling approach. The first is potential misclassification of the treatment group; the second is whether the quasi-experimental

[10] We also consider various continuous measures of treatment intensity discussed below.

design is valid. On the first of these, it is evident that the binary treatment-control indicator risks misclassifying some of the treatment group in the comparison group (as there may be some minimum wage workers even in high average wage firms). However, we show below using a variety of methods that any such bias is likely to be small (for example, by looking at the wage impact at every percentile of the pre-policy wage distribution). In any event, it is useful to point out that such a bias will mean that we are underestimating the causal effect of the NMW on firm profits, as some of firms in the comparison group will have lower profits due to the NMW. Also, where a continuous measure of treatment intensity, say CT, is available (as it is for one of our data sources that has matched worker-firm data) we can re-specify the above framework to incorporate $CT_{i,t-1}$, a treatment indicator based on firm wages at time t-1.

On the second question, the main issue with any non-experimental evaluation of treatment effects is, of course, whether the comparison group is valid. The key conditions are that there are common trends and stable composition of the two groups over the pre-policy time period (see Blundell et al, 2004). Much of our robustness analysis below focuses on whether these two conditions are met: for example, by examining pre-policy trends and carrying out pseudo-experiments in the pre-policy period.

D. Implementation

Our analysis uses two different data sources to examine these potential wage and profitability effects of the minimum wage. Each of these data sets has different advantages and disadvantages for analysing these issues. Firstly, we use the UK care homes data analysed in recent studies of minimum wages and employment[11] to look at profitability effects in a low wage sector where the minimum wage "bit hard". An important advantage of this care homes data is that it has firm-level indicators of

[11] Machin, Manning and Rahman (2003); Machin and Wilson (2004).

treatment intensity (derived from worker level wage data) that allow us to quantify the minimum wage related shock to the cost structures of individual firms using a continuous treatment indicator.

Secondly, we use the Financial Analysis Made Easy (FAME) database of UK firms to examine the profitability effects of the minimum wage across a range of low wage industries. In this firm-level data it is only possible to define treatment-control indicators based on average wages or on matched industry-region wage data from the Labour Force Survey as we do not know the full within firm wage distribution. However, FAME does cover a larger section of the economy and contains direct measures of profit margins.

III. Minimum Wages and Profitability in UK Residential Care Homes

A. Data

In this section we look at the wage and profitability effects of the minimum wage in UK residential care homes. Historically the care homes sector has been a very low wage labour market. As a result, it provides a good testing ground for studying minimum wage effects on profitability since it is a sector that is highly vulnerable to minimum wage legislation. In particular, Table 1 shows that prior to minimum wage introduction average hourly wages were very low in the sector (at around £4 per hour) and almost 40% of its workers were paid below the incoming minimum wage prior to April 1999. Despite this, the employment effects were modest given the magnitude of wage rises following minimum wage introduction (Machin, Manning and Rahman, 2003).

The UK care homes data was collected by the Centre for Economic Performance (CEP) in three panel surveys conducted in 1992 (prior to the general election in that year) and 1993 for homes on the South Coast of England; in 1998 (before the introduction of

the NMW) and in 1999 (after the introduction of the NMW in April) for all homes across the country; and again in 2000 and 2001 (for South Coast homes only). The sector was chosen because it is characterized by a large concentration of non-unionized, low wage employees working in small firms with an average employment level of 15-20 workers. There was also product market regulation in this sector insofar that an important fraction of home residents had their care paid for by the government through the Department of Social Security (DSS). The DSS paid a capped price for beds, which was not increased when the minimum was introduced. As a result, many homes had a limited scope to increase prices in response to the minimum thereby leaving more room for employment or profitability effects to manifest themselves.

A more comprehensive account of features of the data is given in Machin, Manning and Rahman (2003). The two most important features for our analysis are the definition of the treatment variable and the characteristics of our profitability variable. The treatment indicator we mostly use is the initial firm wage gap relative to the minimum, that is the proportional increase in a firm's wage bill required to bring all of its workers up to the minimum wage. Specifically, the wage gap in home *i* is calculated as:

$$GAP_i = \frac{\sum_j h_{ji} \max(W_{ji}^{min} - W_{ji})}{\sum_j h_{ji} W_{ji}} \quad (4)$$

where h_{ji} is the weekly hours worked by worker *j* in firm *i*, W_{ji} is the hourly wage of worker *j* in firm *i*, and W_{ji}^{min} is the minimum wage relevant for worker *j* in firm *i* (this might be the age-specific or the adult minimum, although for this sector it makes little difference if only the adult minimum is used).

Importantly, this wage gap provides a continuous treatment intensity indicator that varies at the firm-level thereby quantifying the wage shock to the cost structures of individual firms. The profit variable we study is a derived one based on total revenues

less total costs. Total revenue of each home is measured directly as the product of the number of beds, the home-specific average price of beds and the home occupancy rate. Total costs are calculated by dividing the total firm wage bill by the labour cost share.[12] Gross profitability is then defined as the ratio of profits to revenue.

B. The Impact on Wages and the Structure of Wages

As already noted, the care homes sector had the potential to be heavily affected by the introduction of the NMW.[13] Table 1 shows a very marked impact on the wage structure in this sector. Approximately 38% of workers were paid below the adult minimum rate in the pre-NMW period. This was reversed once the NMW was introduced with 31% of workers paid at exactly the new minimum rate of £3.60.

Table 2 presents estimates of home-level wage change equations for the period surrounding NMW introduction (1998-99) and for an earlier pre-policy period (1992-93). The upper panel of the Table reports results for a ln(wage) change equation that employs the wage gap variable as a measure of treatment intensity.

$$\Delta \ln(W_{it}) = \lambda_0 + \lambda_1 GAP_{i,t-1} + \upsilon_{it} \quad (5)$$

The results in Table 2 show that wages clearly rose by more in homes with a larger initial wage gap when the minimum wage was introduced. The 0.8 coefficient on the Initial Wage Gap variable indicates that workers in a firm that required a 10% increase in its wage bill to comply with the minimum experienced an 8% increase in average wages relative to workers in a firm already paid at least the minimum.

The second point of note is that this correlation was much less marked in earlier time periods. Defining a counter-factual minimum wage at the same percentile of the

[12] Specifically, total revenue (TR) is calculated as TR = Occupancy Proportion* Number of Beds * Average Price. The labour cost share LC = WB/TC, where WB = firm wage bill and TC = total costs. Total costs are then derived as TC = WB/LC and profits as TR – TC.

[13] To date these data have most been used for study of minimum wage effects (e.g. Machin, Manning and Rahman, 2003) but see also Machin and Manning's (2004) test of competitive labour market theory.

wage distribution as the real 1999 minimum, we can compute a GAP measure for the earlier non-policy time period. Whilst wages *did* rise more in the non-policy period in homes with a bigger initial wage gap the effect is much weaker, as shown by the estimated coefficient of .225. The gap between the two estimates is statistically significant and large in magnitude (at .575) showing a much more marked impact, corresponding to a significant shift in the relationship between wage changes and the initial wage gap in the minimum wage introduction period.[14]

To ensure that our calculation of a counterfactual wage gap in the non-policy period is not driving this impact on wages, we also estimated wage change models including the initial period average wage as an independent variable. These equations take the form:

$$\Delta \ln(W_{it}) = \tau_0 + \tau_1 \ln(W_{i,t-1}) + \zeta_{it} \tag{6}$$

where the focus is now on estimating τ_1 , the association between wage changes and the initial average wage (ζ_{it} is an error term). Again, the reported results in Table 2 show that the link between initial pay and wage growth to be much stronger for the minimum wage period with a coefficient of -.360 compared to a coefficient of -.174 for the earlier non policy period of 1992-93. Thus, there is a very strong impact of the NMW on the structure of wages in the residential care homes sector.

C. Care Home-Level Estimates of Profitability Effects

The previous sub-section established a significant and sizable impact of the NMW introduction on the structure of care home wages. This is a pre-requisite before studying the impact on other outcomes, in our case profitability. Table 3 thus reports estimates of

[14] This is the "trend adjusted" difference in difference estimator discussed in Blundell et al (2004). The pre-policy wage trend could be due to mean reversion and we discuss this in detail when analysing the FAME data below.

regression-corrected profitability specifications. We report reduced form models that relate changes in profit margins to the initial wage gap measure as follows:

$$\Delta(\Pi/S)_{it} = \eta_0 + \eta_1 GAP_{i,t-1} + \eta_2 Z_{i,t-1} + \xi_{it} \quad (7)$$

where ξ_{it} is the equation error.

The coefficient on the wage gap variable is estimated to be negative and significant, as shown in columns (1) and (2) of Table 3.[15] The two reported specifications differ in whether or not they include the control variables, *Z*. In the column (2) specification with controls the coefficient is -.59, indicating that a firm facing a 10% wage gap before the introduction of the minimum experienced a reduction in their profit margin of .059. However, it must be noted that the mean of the wage gap variable is approximately .04. Therefore the "average" firm in the sample would be facing reductions in profit margins of about .024, which given the initial average profit margin of .102 translates into a 23.5% cut in margins.

D. Home Closures

The negative estimated profitability effects induced by a minimum wage introduction for the lowest paying homes also raises the possibility that homes may have closed because of the minimum wage. Following Machin and Wilson (2004) we look at closures over a reasonably long window by using the 1998 and 2001 samples of South Coast homes. We find that, of the 548 homes with good survey data from 1998, 126 shut down over the 1998-2001 period. This corresponds to a fairly high closure rate (23%), as one would expect in this high turnover sector. However, when we formally test whether the firms most heavily affected by the introduction of the NMW were more likely to exit

[15] Notice that the, owing to missing data to compute margins, the number of firms falls relative to Table 2. Estimated wage equations for this sub-sample were very similar to those in Table 2 for the 1998/99 time period. For example, in a specification comparable to that in column (1) of the upper panel of Table 2, the estimated coefficient on the Initial Wage Gap was .758 with an associated standard error of .070 for the 477 firm sample.

we are unable to find any evidence in favour of this hypothesis. Table 4 shows this using a probit model to relate the probability of closure to the pre-introduction wage gap. In each case, the coefficient on the initial wage gap variable is insignificant with a t-ratio of less than one.[16]

IV. Minimum Wages and Profitability in a Firm Panel Across Many Sectors

A. Data

Accounting regulations in the UK require private firms (i.e. those unlisted on the stock market) to publicly report significantly more accounting information than their US counterparts. For example, even publicly quoted firms in the US do not have to give total employment and wage bills whereas this is required in the UK. The lack of publicly available information on private sector firms and on average remuneration may be a reason for the absence of US studies in this area. Accounting information on UK companies is stored centrally in Companies House. It is organised into electronic databases and sold commercially by private sector data providers such as Bureau Van Dijk from whom we obtained the FAME (Financial Analysis Made Easy) database.

The great advantage of FAME is that is covers a much wider range of companies than is standard in firm level analyses and, in particular, it covers non-stock market listed firms. This means we are able to include many of the smaller and medium sized firms that may be disproportionately affected by the NMW. Furthermore, FAME also covers non-manufacturing firms where many low wage workers are employed. By contrast, plant level databases in the UK and US typically cover only the manufacturing sector[17] and do not have as clear a measure of profitability as exists in the (audited) company

[16] The magnitude of the effect is also small. Since the wage gap increased on average by .04 the implied impact of the NMW was to increase the probability of exit by only .7 percentage points (= .04*.17).

accounts. However, UK accounting regulations do have reporting exemptions for some variables for the smaller firms so our analysis is confined to a sub-sample who do report the required information.[18]

Since FAME reports annual accounting information, there is one highly pertinent feature of the data, namely that firms report accounts with different year-end dates. Since the NMW was introduced on April 1st 1999, we therefore consider the sub-set of firms who report their end of year accounts on March 31st of each year (these are firms who report in the UK financial year). The accounting period for these firms will match exactly the period for which the NMW was in force. Around 21 percent of firms in FAME who have the accounting data we require report on this day, which corresponds to the end of the tax year in the UK. We have checked whether the sample who report their accounts on March 31st were representative of the whole FAME sample and in Appendix Table A1 we report the results from this exercise. It shows that our focus on firms reporting at the end of March is not likely to be a problem in that they are very similar to the full sample of FAME firms with respect to their average profit margins, wages and employment levels.

We have extracted measures of gross profits from the FAME database (i.e. including interest and depreciation) and model profitability in terms of the gross profit to sales ratio. There is a long tradition in firm-level profitability studies to use this measure, as it is probably the best approximation available in firm-level accounts data to price-cost margins.[19] Specifically, the measure of price-cost margin we calculate is the ratio of gross

[17] The Annual Business Inquiry (ABI) database does cover non-production sectors, but this database is not available until the late 1990s. The US Longitudinal Research Database (LRD) only covers manufacturing.
[18] These firms will tend to be larger than average as the very smallest firms have the least reporting requirements.
[19] For example, see Machin and Van Reenen (1993) and Slade (2004) for a more recent discussion. Although there are many reasons why accounting and economic profits may diverge (Fisher and McGowan, 1983), there is much evidence that they are on average highly positively correlated. The relationship between the profit-sales ratio and price-cost margins will also break down if there are not

profits to sales revenue, which measures profits before taking account of tax, interest or depreciation.[20] To allow for capital intensity differences we also control for firm-specific capital sales ratios.[21]

B. Defining Treatment and Comparison Groups

Unlike the care homes data, FAME does not provide any measures of the within firm distribution of wages. It has a total remuneration figure that can be divided by the total number of employees to calculate average firm remuneration. This creates a challenge in terms of defining our treatment and comparison groups since any given level of average wages is, in principle, compatible with a range of different within-firm wage distributions. This makes it hard to measure accurately how exposed each firm's cost structures are to the wage shock brought about by the minimum wage. For example, any continuous measure of treatment intensity based on the firm average wage is inevitably coarse.

To combat this potential problem we use information from FAME, the Labour Force Survey (LFS) and the Workplace Employment Relations (WERS) to both construct and validate our treatment group indicators. Specifically, we combine information on low wage industry and regions from the individual level LFS with average firm wages from FAME to define our treatment and comparison groups. We also use within-establishment information from matched worker-establishment data in WERS to consider the

constant returns to scale. In this case, controlling for capital intensity is important in allowing for differential fixed costs across firms which is what we do empirically in the regression-corrected difference in difference estimates.

[20] The apparently relatively high level of the profit margins is a function of the fact that we are using gross margins pre-tax and deductions rather than net margins.

[21] We also checked that dropping the capital sales ratio did not change the results as some of the effect of the NMW may have come from firms substituting away from more expensive labour towards capital equipment.

association between low pay incidence and average wages to assess how effective this strategy is likely to be.[22]

To investigate the impact of the minimum wage we have defined our treatment group, T, in two ways. First, we use average remuneration information from FAME and, as a starting point, define T = 1 for firms with average remuneration of less than £12,000 in the accounting year prior to minimum wage introduction ("low wage firm"). Average remuneration in the treatment group for this threshold is £8,400 which, after allowing for a deduction for non-wage costs (such as employers' payroll tax, pension contributions, etc), is equivalent to a £3.90 hourly wage for a full-time worker and is close to the NMW (introduced at £3.60 per hour). For our research purposes, the key issue is that the wages of firms beneath the threshold we choose have a significant wage boost from the NMW relative to higher wage firms and we consider this in detail. One aspect of this is that we have experimented with the threshold cutoff and we discuss this in detail below.

The second route is to combine the low wage firm information with industry-region "cell" data on the proportion of workers beneath the minimum wage in the year before it came in. Using LFS data, we define a low wage industry-region cell if more than 10% of workers in the given firm's two-digit industry by region cell in the pre-policy period are paid below the minimum wage.[23] We thus adopt a second definition of T = 1 if a firm has an average wage below £12,000 *and* is in a regional by two digit industry cell that has more than 10% of workers paid below the minimum ("low wage firm and industry").

[22] Unfortunately direct linking of data of WERS and FAME is not possible due to confidentiality restrictions.

[23] We experimented with a number of ways of defining the treatment based on cells including finer levels of industry aggregation and alternative thresholds to 10%.

As with the care homes analysis we also look at associations with the pre-policy average wage in the firm. This gives a continuous indicator that we can use to compare with the binary treatment variables based upon being beneath a particular wage threshold.

C. Validation and Usefulness of Average Wage Measures

Given the problems outlined above it is legitimate to ask how accurate these treatment group definitions are at identifying the most affected firms in our sample. This hinges on how segregated low wage workers are within firms. That is, our threshold based definition will be more effective if sub-minimum wage employees are concentrated in particular firms at the lower end of the wage distribution.

We have therefore looked at the 1998 cross-section of the Workplace Employment Relations Survey (WERS). This contains matched worker and establishment data that permits one to look at within-workplace wage distributions so as explore the associations between average wages and the extent of low wages in workplaces. For 26,509 workers in 1,782 WERS workplaces we computed the proportion of workers paid less than £3.60 per hour and the average wage in the workplace. As Figure 1a shows there is a strong, negative association between the two (correlation coefficient = -.61, p-value = .000). Looking only at workplace with an annualised average wage of less than £20,000 (Figure 1b), the cut-off we use in our FAME data, strengthens the association (correlation coefficient = -.71, p-value = .000). Overall, these patterns give support to the idea that low wage workers are concentrated in firms that pay low average wages. To the extent they are, then this acts to validate our use of average wage data to proxy low wage firms from FAME.

D. Changes in FAME Wages Before and After NMW Introduction

Following on from this, it is important to see whether changes in the firm wage distribution in the FAME data are consistent with these patterns. In particular, it is

important see whether we can pick up a clear change or "twist" in the firm average wage distribution as the minimum wage was being introduced. To this end we calculate the change in average wages in the year immediately before and immediately after the NMW was introduced for every firm at each percentile of the pre-policy firm wage distribution. If the firms in the FAME data exhibit some of the low pay patterns outlined above for WERS, and so the minimum wage introduction raises average firm wages by more in low wage firms, then we would expect there to be large changes in firm wages for the lowest percentiles of the distribution.

The results given in Figure 2 confirm this idea. The post-NMW year ("Policy On", labelled 1999-2000, for the financial year April 1 1999 to March 31 2000) the wage change tapers off steadily as we move up from the lowest decile of the firm average wage distribution. After the 15th percentile all firms appear to have had a similar increase in nominal wages of about 5% (or about 2.5% in real terms). There is no evidence of faster wage growth for the bottom decile in the pre-policy year (in fact wage growth in the bottom 15 percentiles was on average 1.8% in the 1998-1999 financial year compared to 7.7% in 1999-2000). There is a spike for the bottom first and second percentiles of the wage distribution for both years, which is consistent with the notion of some transitory measurement error at the bottom end of the wage distribution generating mean reversion in all periods. The general picture follows a similar pattern to that found for individual-level wage data (Dickens and Manning 2004) and again provides encouraging evidence that our treatment group is well defined.

The critical thing in terms of the definition of the treatment group T, as noted before, is that we identify wage effects from the treatment group definitions, so that our analysis of profitability consequences is validated by the minimum wage introduction having a bigger 'bite' on low wage firms. To make this a tighter definition we have also

defined the comparison group to be those firms with average wages above the £12,000 threshold but less than £20,000 (the median firm wage) by removing any firms with above £20,000 average wages from the main analysis. We do so since these firms are likely to be quite different in terms of their characteristics and therefore subject to different unobservable trends from the treatment group. We are careful to test for the sensitivity of the results to definitions of these thresholds.

E. Other Data

We have also matched to the firm-level FAME data various industry-level variables drawn from the Labour Force Survey. These are used as control variables in the analysis and include the proportion of part-time workers; the proportion of female workers; union density; and regional levels of human capital (proxied by the proportion of all workers who have college degrees in a particular region by two-digit industry cell). The control variables in the regression models also include a full set of industry, regional and time dummies. Variable definitions are given in the Data Appendix. Table A2 in the Appendix shows characteristics of the treatment and comparison groups for each model.[24]

Finally, the magnitude of the impact of the minimum wage over our "Policy On" period should be clarified. This period lasts from April 1st 1999 until March 31st 2002 (the end of our sample). Along with the introduction of the minimum wage, there were two upratings of the minimum during this time. The first occurred in October 2000 and saw the minimum wage rise by 10p to £3.70. The second uprating a year later was more substantial taking the minimum up to £4.10. Together these upratings constitute a 13.9%

[24] Interestingly the profitability of low wage treatment group firms is higher at the median and mean than comparison group firms. This is not true for firms as a whole where there is a positive correlation between average firm wages and profits per worker (e.g. Van Reenen, 1996). It is because we are focusing on the lower part of the wage distribution that this correlation breaks down.

increase in the minimum between 1999 and 2002.[25] Small cell sizes prevent us from estimating separate models for the 2000 and 2001 upratings.[26]

F. Descriptive Analysis

The statistical analysis of the FAME dataset begins in Table 5(a) which presents unconditional difference-in-differences in mean ln(wages) for the two different categorizations of treatment and comparison groups. It is evident from column (1) that wages rose significantly amongst the low wage firms when the minimum wage became operational. Wage growth across the pre- and post-NMW time periods (now across the six financial years from April 1 1996 to March 31 2002 as compared to the earlier one year before and after analysis) was higher at 21.4 percent in the low initial wage T = 1 group as compared to wage growth of 12.3 percent in the higher initial wage T = 0 group. The difference-in-difference of 9.1 percent is strongly significant in statistical terms. This is consistent with the hypothesis that the NMW significantly increased wages for low wage firms. Similar patterns are seen in column (2) for the combined low wage firm and low wage industry treatment group definition.

An analogous set of descriptive results are given for profitability in Table 5(b). The Table is structured the same way as the wages results in Table 5(a), showing unconditional difference-in-difference estimates of the effects of NMW on firm profitability. In column (1) it is clear that, whilst profit margins fell (by .021) between the pre- and post-NMW periods in the low wage firms, they rose in the higher wage firms (by .006). Thus, there is a negative difference-in-difference of -.027. This difference is statistically significant and shows preliminary evidence that profit margins were squeezed in firms that were more affected by the introduction of the minimum wage. Again, as

[25] By contrast, the consumer price index grew by 6.3% over the same period.
[26] For example, less than 9% of firms report annually on September 30 (i.e. the 12 months immediately before the October upratings).

column (2) shows, use of the combined low wage firm and industry treatment group definition produces a similar outturn with, if anything, the difference-in-difference profitability effect being slightly larger (in absolute terms).

Thus the Table is supportive of the notion that minimum wages introduction in the UK resulted in falls in profit margins. However, as noted above, it is important to also control for other changes that may have occurred at the same time by embedding the difference-in-difference approach into a regression framework that can factor out other coincident changes in the relevant variables.

G. Firm-Level Estimates of the Wage Impact of the Minimum Wage

Table 6(a) reports the difference-in-difference wage regression results, for three initial wage measures, the pre-policy average wage and the two binary treatment indicators. The regressions control for a range of time-varying factors (see the notes to Table 6) and it is clear that the pattern of significantly higher wage growth in pre-policy low wage firms is robust to the inclusion of the other variables in the regressions. In the first column we estimate a model that uses the average firm wage in the 1998-1999 financial year as a continuous measure of treatment intensity. According to column (1), firms with lower pre-policy average wages experienced significantly higher wage growth in the minimum wage period. In column (2) the firm level binary treatment indicator shows that growth was around 5.7 percent higher in initially low wage firms. Similarly, in column (3) the effect of being a low wage firm in a low wage industry in the pre-policy period is estimated to be significant, positive and of much the same magnitude as the estimate in column (2).

H. Firm-Level Estimates of the Profitability Impact of Minimum Wage

Having established that significant wage effects from the NMW exist in the firm-level FAME data, we next look at the evolution of firm-level profit margins in the

regression framework. The estimates are given in Table 6(b), with the same structure as for the wage models. First of all, column (1) shows that margins rose by more in firms with higher pre-policy wage levels, so that the minimum wage introduction dampened margins in the low wage firms more affected by the policy change. The low wage firm and low wage firm and industry models, in columns (2) and (3) respectively, show this very clearly. The conditional difference-in-difference estimate is negative and statistically significant for each, with there being a slightly higher impact of -.042 in the low wage firm and industry model compared to -.031 in the low wage firm model. Hence, the evidence seems to be show that profitability fell in firms that were more affected by NMW introduction. These appear to be moderate effects with the results for the low-wage firm model implying a 7.8 per cent (-.031/.400) fall in profit margins compared to a 10.7 per cent fall for the combined model (-.042/.389 = .107).

I. Robustness Tests

There are several reasons why one might want to probe these results further, and in several directions. The first, and obvious, reason is to judge the sensitivity of our definition of pre-policy low wages. Because we do not have data on the individual workers within our FAME firms, we are forced to model pre-policy low wage status as a function of the average wage in the firm. This is less than ideal, even though we have (at least partially) validated its use with the WERS data, and it is important to study whether the results are robust to alternative ways of defining the threshold between treatment and comparison groups.

Table 7 shows conditional difference-in-difference estimates of the impact of NMW introduction on profitability for a range of wage thresholds, running from an average wage of £10,000 at £1,000 intervals up to £15,000. The results are reassuring in that they all establish a significant NMW effect of reducing profit margins, with

magnitude of the impact varying and becoming slightly larger (in absolute terms) for lower thresholds as we would expect (so there is a bigger impact on the very low wage firms).

The second main reason why one may worry is if our results are simply picking up a relationship between changes in profit margins and initial low wage status that exists, but has nothing to do with the NMW introduction. We have thus looked at estimates, structured in the same way, from periods before the NMW was introduced to benchmark the findings against non-NMW periods. One such 'pseudo experiment' is reported in Table 8 where we examine an imaginary introduction of the NMW on April 1st 1996 (instead of three years later) and repeat our analysis of wage and profit changes. Table 8 reinforce the results reported to date, as we are unable to find any difference in margins between low and high wage firms in the period when the policy was not in place. This is consistent with the NMW introduction being the factor that caused margins to be reduced in relatively low wage firms.

A related issue is the possibility of pre-sample trends (possibly due to mean reversion) in the wage model. If initially low wage firms had faster than average wage growth even in the absence of the policy then this would be conflated with the NMW impact on wages. Although there was some evidence for this in the Care Homes results of Table 2, we saw there that the trend-adjusted difference in difference estimated still identified a strong and positive impact of the NMW. In the FAME data of Table 8 there is no evidence of mean reversion for wages (or profitability as discussed above) in the pre-policy period.

Nevertheless, we investigated this issue in greater detail by estimating the wage model of Table 6 with a rolling threshold from £10,000 to £15,000 for both the policy and pseudo-experiment periods. That is, we estimate the model for thresholds at each

£100 interval in this range and plot the coefficients (see Figure 3). In the pre-policy period defining the treatment group at our chosen cut-off (£12,000) has an implied effect of zero, which is what the first rows of Table 8 revealed. There is some evidence of a slight downward slope of the line before this point, however which is consistent with the idea that there was some faster wage growth for firms in the lower tail of the wage distribution consistent with some mean reversion (recall that as we move closer to the origin along the horizontal axis the treatment group is composed of increasingly low-paying firms). More importantly, however, the downward sloping line is also evident in the "Policy On" period, so the vertical distance between the two lines gives the trend-adjusted difference in difference estimate (triple differenced) of the NMW at different cut-off points. This is approximately constant over the relevant range and statistically significant.

We repeat the same exercise for profit margins in Figure 4. There is no evidence of any pre-policy trend in profitability regardless of how the thresholds are defined (which suggests that the mean reversion identified at the lowest part of the wage distribution in Figure 3 may simply be measurement error in wages, rather than genuine transitory shocks). We conclude that although there may be some evidence of mean reversion for wages in the lower part of the distribution (as is also evident in Figure 3) this generates no substantial bias in our estimate of the causal impact on the minimum wages on firm profits.

Table 9 considers a number of further robustness tests of our main results. The first column reports again the baseline estimates from Table 6 for reference. The second column implements a statistical matching technique by trimming the sample according to

the propensity scores of the treatment and comparison groups.[27] As suggested by earlier comparisons, the sample seems well chosen with only a few observations trimmed from the comparison group and both wage and profitability effects are highly similar to the estimates in column (1).[28]

Since our sample is unbalanced, it is also important to test whether our results are robust to potential compositional effects. To evaluate this issue in column (3) we estimate our model using the balanced panel (that is, for firms that report information for all six years of the panel). This sample comprises 2,052 observations that represent 54% of the overall sample of treatment and comparison group. The results show significant effect of the NMW on wages and profits in the balanced panel and, although the estimated magnitudes of the profitability effects are a little smaller, they remain strongly significant.

Finally, the specifications in the last two columns enter a set of firm fixed effects into the balanced and unbalanced panel models respectively. Of course, in the case of a balanced panel with no post-policy covariates the T*Policy On parameters would be orthogonal to the firm fixed effects. However, our models do contain covariates and we use an unbalanced panel. Nonetheless, the estimates in columns (4) and (5) are very similar to the baseline results and reassuringly show a significant wage and profitability effect of NMW introduction operating in FAME firms.

[27] The basic method used is that of Heckman, Ichimura and Todd (1997) where propensity scores are estimated and the sample then trimmed to exclude poorly matched observations without common support. To generate the propensity scores, we used a probit model that included all the control variables used in Table 6.

[28] Few observations are lost under propensity score matching because the comparison group is already chosen to be of relatively low wage firms (under £20,000 average annual wages). If we had used the entire FAME sample (including firms with average wages of over £20,000) we would have had to lose the vast majority of the sample to ensure that the comparison group had common support with the treatment group. Results are not presented for the pre-policy average wage since that is a continuous variable: if, however,

J. Firm Entry and Exits

If the NMW imposes significantly higher wage costs on firms, and they have no "excess" profits to squeeze, then all other things being equal the higher wage bill will force them out of business. We therefore need to consider the scope for the NMW to differentially affect the exit rate of low wage firms. The FAME database identifies four categories of inactive firms, namely firms that are dissolved, liquidated, in receivership or currently non-trading[29]. Hence, we define all firms in these categories as exiting firms. Furthermore, the longer period available in FAME allows us to compare exit patterns before and after the minimum wage. We do so in the same difference-in-difference setting as for the wage and profitability models based upon probit models of exit.

The results are reported in Table 10. The upper panel of the Table shows descriptive statistics on exit rates for treatment and control firms before and after minimum wage introduction. For both classifications of treatment and control firms, changes in the exit rate before and after minimum wage introduction are very similar. The difference-in-difference is statistically indistinguishable from zero in both cases.

The lower panel of the Table shows the probit model estimates. As with the wage and profitability models, there are three specifications. The first enters the pre-policy average wage and the others are the two binary treatment-control comparisons. For all three models, there is no evidence of any faster increase in exit rates in initially low wage firms following the NMW introduction. We conclude that despite the strong impact of minimum wages on profits there appears to be no discernible effects on firm exits in the FAME data over this time period.

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the specification including that variable was estimated on the trimmed sample from panels (2) or (3) this produced very similar results to the baseline estimates.

[29] So exits by takeover are not coded to be unity in this definition as takeovers may be regarded as a sign of success rather than failure. Re-defining the dependent variable to be unity if the exit is to a takeover does not change the qualitative nature of the results.

Finally we examined other outcomes, in particular whether there was a NMW effect on firm productivity to see if there were any positive "shock" effects on firm efficiency (for the Low Wage Firm and Industry definition of T we obtained a positive, but statistically insignificant, coefficient of .054 on T*Policy On in a ln(Sales/Employment) regression). We did not therefore find any evidence of productivity effects. Nor, in line with the existing empirical literature, were there any significant effects on employment.

V. Conclusions

Despite there being a large literature on the economic effects of minimum wages on labour market outcomes (especially individual's employment/unemployment and firm labour demand), there is a surprising lack of evidence on the impact of minimum wages on firm performance. In this paper we consider the impact of minimum wages on firm profitability. Studying profitability is important because the empirical evidence suggests that minimum wages do raise the earnings of low wage workers, but do not seem to have large negative employment consequences. Consequently, "something has to give" for firms to be able to sustain the increased wage costs induced by minimum wages.

Using the quasi-experiment of the introduction of a national minimum wage to the UK labour market in 1999, we considered what happened to profit margins in firms whose wages were *ex ante* more likely to be affected by the minimum wage. We examined these firms before and after introduction and compared them to a comparison group of firms whose wage costs were *ex ante* less likely to be less affected. We used two firm level panel datasets: one on the very low wage care home sector and another covering a range of private sector firms. Across both datasets our results show that that profitability was significantly reduced by the introduction of the minimum wage. We

could not find any evidence that low wage firms were forced out of business by the higher wage costs resulting from the minimum wage. One explanation for this absence of an exit effect may be because our time period is still too short and that in the very long run there is significant exit. It may also be that there is less entry into the low wage sectors as a result of the minimum wage. An alternative explanation is that firms were making profits from paying low wages prior to the minimum wage introduction and that one consequence of the introduction of the minimum wage to the UK labour market was to moderate these "excess" profits by channelling them back to the wages of low paid workers.

There are, of course, caveats to our results. It would also be useful to have better data on prices and quality to see if this margin has adjusted (although there is no evidence for these effects in the care homes sector[30], as it is heavily regulated). It would also be useful to have more information on the within firm distribution of workers in other sectors besides care homes.

Overall though, we believe this study is an important contribution to looking at the impact of labour market regulation on *firms* as well as on individuals.

[30] See Machin, Manning and Rahman (2003).

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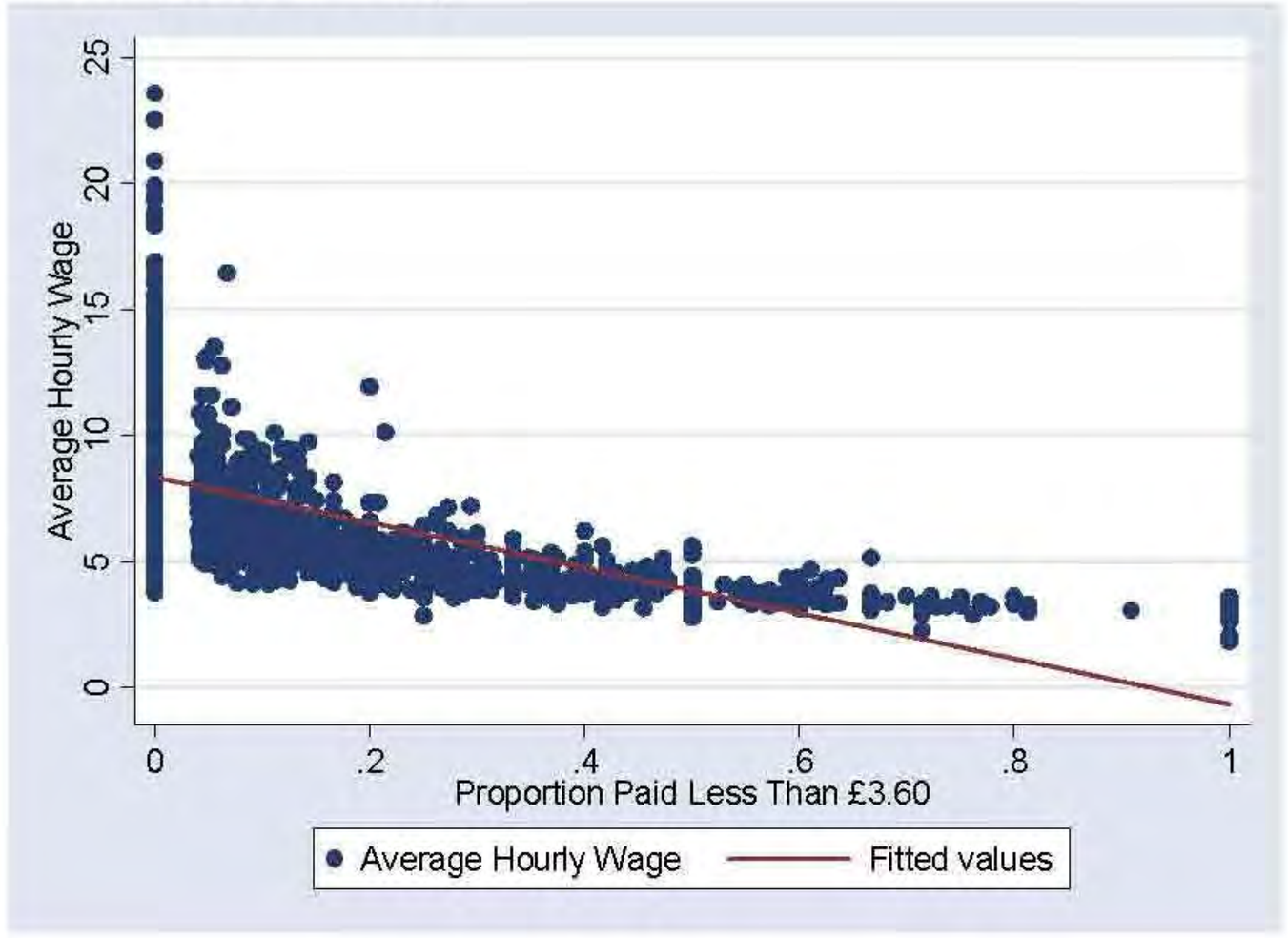
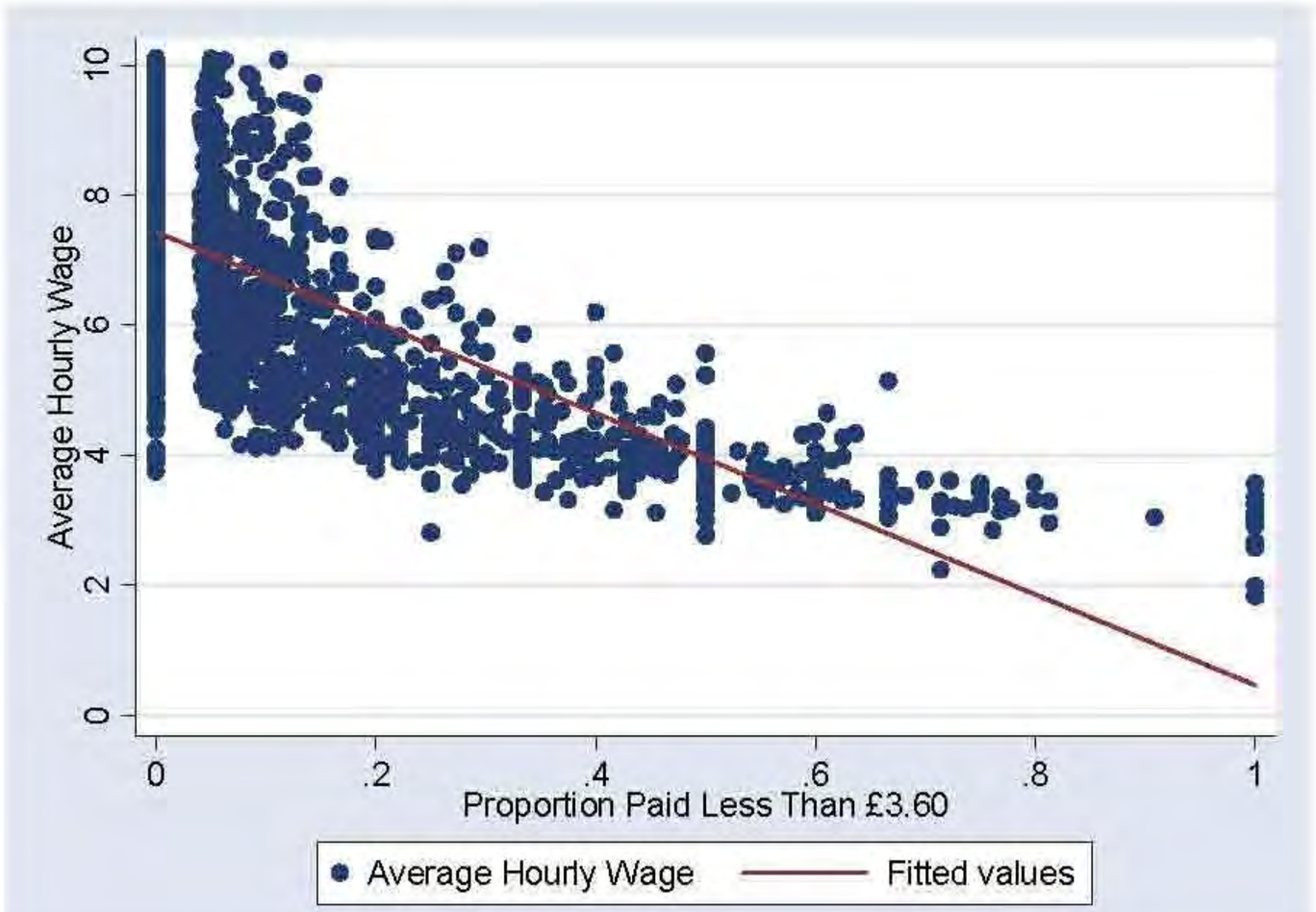
Figure 1: Proportion of Low Paid Workers by Percentile (Workplace-Level), WERS 1998 Cross Section of Matched Workers and Workplaces

(a) All WERS Establishments
(Correlation Coefficient = -.61)

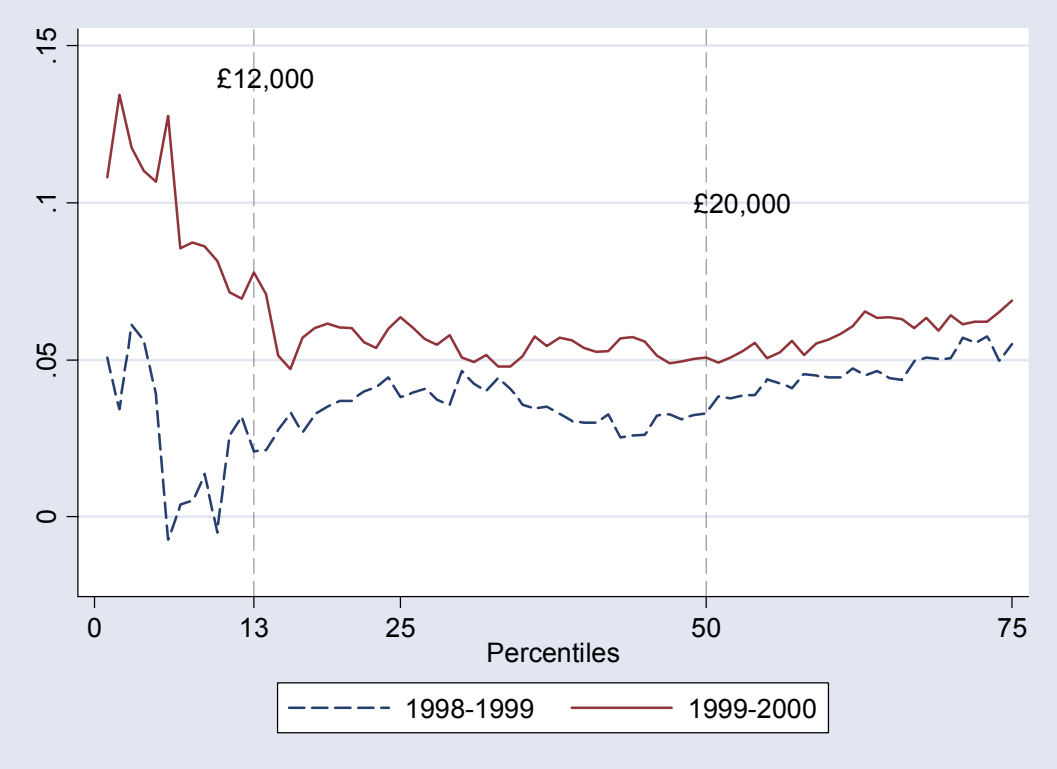


(b) WERS Establishments With Average Annualised Wages Equal to or Less than £20,000
(Correlation Coefficient = -.71)



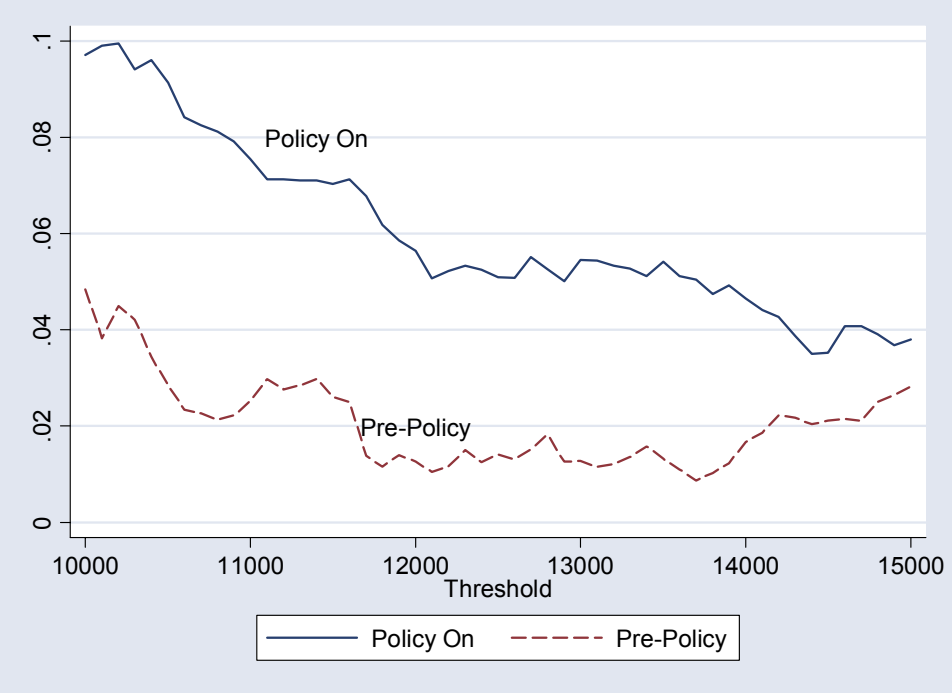
NOTES:- These figures are derived from matched worker-workplace data (26319 workers in 1782 workplaces) from the 1998 Workplace Employee Relations Survey (WERS). The horizontal axis indicates the proportion of employees in the workplace who are paid under £3.60 per hour; the vertical axis indicates the average hourly pay across the sample of workers in the same establishment.

Figure 2: Change in Ln(Average Wage) by Percentile, FAME.



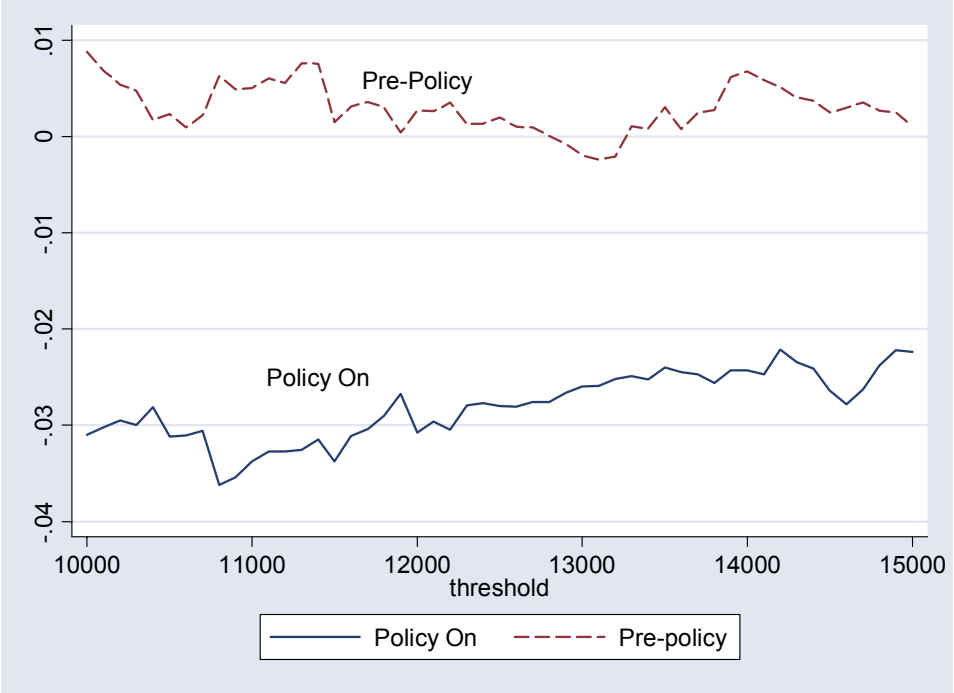
NOTES:- The data is taken from the FAME database of company accounts. The horizontal axis indicates the percentile in the firm wage distribution for a given firm in the initial period, the pre-policy financial year up to March 31st 1999. The vertical axis shows the proportionate change in average firm wages (in the pre-policy financial year and the post policy financial year) for each firm ranked by where it began in the wage distribution. Pre-Policy defined as the financial year April 1st 1998 to March 31st 1999; Policy On defined as the financial year April 1st 1999 to March 31st 2000.

Figure 3: Treatment Effect Coefficients by Threshold, Ln(AverageWage), FAME



NOTES:- Data taken from is the FAME database of company accounts. Models as per Low Wage Model, Table 6(a) column (2). The Policy On sample period covers the six financial years from April 1st 1996 to March 31st 2002, NMW introduction on April 1st 1999. The Pre-Policy (pseudo-experiment) period covers the six financial years April 1st 1993 to March 31st 1999, with an 'imaginary' NMW introduction on April 1st 1996. Thresholds are shifted in units of £100 to define treatment group (T=1) as firms with pre-policy wages of under the threshold and comparison group with firms with average wages over the threshold and under £20,000.

Figure 4: Treatment Effect Coefficients by Threshold, Gross Profit Margins, FAME



NOTES:- Data taken from is the FAME database of company accounts. Models as per Low Wage Model, Table 6(b) column (2). The Policy On sample period covers the six financial years from April 1st 1996 to March 31st 2002, NMW introduction on April 1st 1999. The Pre-Policy (pseudo-experiment) period covers the six financial years April 1st 1993 to March 31st 1999, with an 'imaginary' NMW introduction on April 1st 1996. Thresholds are shifted in units of £100 to define treatment group (T=1) as firms with pre-policy wages of under the threshold and comparison group with firms with average wages over the threshold and under £20,000.

Table 1: The "Bite" of the Minimum Wage in UK Care Homes, 1998-1999

	Pre-Minimum	*Post-Minimum*
Average hourly wage (£)	3.98	4.22
% Paid Less Than Adult Minimum Wage	37.8	4.3
Initial Wage Gap	0.041	0.003
% Paid Exactly Adult Minimum Wage	9.0	30.7
Profits/Sales	0.102	0.118

NOTES:- Based on 641 care homes for which we have wage data pre- and post-minimum wage introduction (for Profits/Sales based on 477 care homes owing to missing data).

Table 2: The Wage Impact of Minimum Wage Introduction in Care Homes

	A. Initial Wage Gap Model: $\Delta\ln(W_{it}) = \lambda_0 + \lambda_1 GAP_{i,t-1} + \upsilon_{it}$		
	(1)	(2)	(3)
	Change Surrounding NMW Introduction, 1998-1999	Change in Earlier Non-Policy Period, 1992-1993	Difference (Standard Error)
Initial Wage Gap (t-1)	.800	.225	.575
	(.070)	(.102)	(.124)
Number of Firms	641	231	
	B. Initial Ln(Wage) Model: $\Delta\ln(W_{it}) = \tau_0 + \tau_1 \ln(W_{i,t-1}) + \zeta_{it}$		
	(1)	(2)	(3)
	Change Surrounding NMW Introduction, 1998-1999	Change in Earlier Non-Policy Period, 1992-1993	Difference (Standard Error)
Initial Ln(Wage) (t-1)	-.360	-.174	-.186
	(.040)	(.057)	(.070)
Number of Firms	641	231	

NOTES:- Ordinary Least Squares estimates, robust standard errors underneath in parentheses.

Table 3: Profitability Models in Care Homes, 1998-1999

	Profitability Model: $\Delta(\Pi/S)_{it} = \eta_0 + \eta_1 GAP_{i,t-1} + \eta_2 Z_{i,t-1} + \xi_{1it}$	
	(1)	(2)
Initial Wage Gap (t-1)	-0.501 (0.218)	-0.590 (0.270)
Controls	No	Yes
Number of Firms	477	469

NOTES:- Controls include demographic characteristics (proportion female, mean age, proportion with nursing qualifications), proportion local authority residents, and county and response month dummies. Robust standard errors in parentheses.

Table 4: Models of Home Closure By August 2001

	Pr[Home Closure]	
	(1)	(2)
Initial Wage Gap (t-1)	.325 (.351)	.166 (.382)
Controls	No	Yes
Number of Firms	517	496

NOTES:- The proportion of homes that closes by August 2001 is 0.23. Marginal effects from probit estimates are reported with standard errors underneath in parentheses. Control variables are county dummies, the proportion of females, the mean age, and the proportion of workers with nursing qualifications and the proportion of local authority residents.

Table 5: Firm Wages and Profitability Before and After NMW Introduction, FAME data (Six Financial Years From April 1 1996 to March 31 2002)

(a) Difference- in-Difference - ln(Average Wage)

	(1) *T = Low Wage Firm*			(2) *T = Low Wage Firm and Industry*		
	Pre-NMW	*Post-NMW*	*Difference (Post – Pre)*	*Pre-NMW*	*Post-NMW*	*Difference (Post – Pre)*
T = 1	2.137	2.351	0.214	2.119	2.343	0.224
T = 0	2.753	2.876	0.123	2.674	2.813	0.139
Difference-in-Difference			0.091 (0.022)			0.085 (0.027)

(b) Difference-in-Difference – Gross Profit Margins

	(1) *T = Low Wage Firm*			(2) *T = Low Wage Firm and Industry*		
	Pre-NMW	*Post-NMW*	*Difference (Post – Pre)*	*Pre-NMW*	*Post-NMW*	*Difference (Post – Pre)*
T = 1	0.403	0.382	-0.021	0.389	0.359	-0.030
T = 0	0.300	0.306	0.006	0.316	0.319	0.003
Difference-in-Difference			-0.027 (0.012)			-0.033 (0.013)

NOTES:- Pre-NMW corresponds to the financial years April 1st 1996-March 31st 1999 and Post-NMW to the financial years April 1st 1999–March 31st 2002. T = 1 indicates the treatment Group and T= 0 indicates the comparison group. Standard errors in parentheses are clustered by firm. *Low Wage Firm* – the treatment group is defined as firms with an average wage equal to or below £12,000 per annum in the pre-policy financial year up to March 31st 1999; the comparison group is defined as firms with average wages between £12,000 and £20,000 in the pre-policy financial year up to March 31st 1999. *Low Wage Firm and Industry* - treatment group is defined as firms with average wages equal to or below £12,000 in the pre-policy financial year up to March 31st 1999 and being in a low wage industry (defined as a firm located in a region and three-digit industry LFS cell with 10% or more sub-minimum wage workers in the same period). The sample size is 3820.

Table 6: NMW Difference-in-Difference Regression Estimates, ln(Average Wage) and Gross Profit Margins Models, FAME data, Six Financial Years From April 1 1996 to March 31 2002

	(1) *T= Pre-Policy ln(Average Wage)*	(2) *T = Low Wage Firm*	(3) *T = Low Wage Firm and Industry*
(a) ln(Average Wage)			
T*Policy On	-0.139	0.057	0.054
	(0.032)	(0.020)	(0.023)
T	0.888	-0.527	-0.458
	(0.015)	(0.021)	(0.029)
(b) Gross Profit Margin			
T*Policy On	0.039	-0.031	-0.042
	(0.016)	(0.013)	(0.014)
T	0.013	0.015	0.002
	(0.023)	(0.018)	(0.023)
Controls	Yes	Yes	Yes
Sample Size	3,820	3,820	3,820

NOTES:- Policy On = 0 for the pre-policy financial years April 1st 1996-March 31st 1999 and Policy On =1 for the financial years April 1st 1999–March 31st 2002. T = 1 indicates the treatment Group and T= 0 indicates the comparison group. Standard errors in parentheses are clustered by firm. *ln(Average Wage)* - indicates a continuous measure of the wage (in the pre-policy year up to March 31st 1999) is used for treatment intensity. *Low Wage Firm* - treatment group is defined as firms with an average wage equal to or below £12,000 per annum in the pre-policy financial year up to March 31st 1999; the comparison group is defined as firms with average wages between £12,000 and £20,000. *Low Wage Firm and Industry* - treatment group is defined as firms with average wages equal to or less than £12,000 in the pre-policy financial year up to March 31st 1999 and being in a low wage industry (defined as a firm located in a region and three-digit industry LFS cell with 10% or more sub-minimum wage workers in the same period). Controls include one digit industry division; regional dummies (18 government office regions); the firm capital-sales ratio; the proportion of workers who are graduates (by region and two-digit industry); and union membership, part-time work and female employment rates (by three-digit industry classification).

Table 7: Alternative Definitions of Treatment Group – Profit Margin Models

	T =1 if W^{pre} *<£ 10,000*	*T =1 if* W^{pre} *<£ 11,000*	*T =1 if* W^{pre} *<£ 12,000*	*T =1 if* W^{pre} *<£ 13,000*	*T =1 if* W^{pre} *<£ 14,000*	*T =1 if* W^{pre} *<£ 15,000*
T = Low Wage Firm						
T*Policy On	-0.031	-0.034	-0.031	-0.026	-0.024	-0.023
	(0.015)	(0.014)	(0.013)	(0.011)	(0.010)	(0.010)
No. of Observations in Treatment group	664	819	985	1,201	1,515	1,846
T = Low Wage Firm and Industry						
T*Policy On	-0.039	-0.041	-0.042	-0.033	-0.031	-0.026
	(0.015)	(0.015)	(0.014)	(0.013)	(0.012)	(0.011)
No. of Observations in Treatment group	423	514	585	686	825	935

NOTES:- Each column shows the results from a separate difference in differences regression where the treatment and comparison groups are defined according to different thresholds as indicated at the head of the relevant column. For example when threshold is £10,000 the treatment group (T=1) is defined as all firms with average wages below £10,000 and the comparison group (T=0) is defined as all firms with average wages equal to or above £10,000 and less than £20,000 (the £20,000 upper bound in the financial year up to March 31st 1999 is kept fixed). The *Low Wage Firm* models are directly comparable to column (2) of Table 6. The *Low Wage Firm and Industry* models are directly comparable to column (3) of Table 6. The sample size is 3820. Other notes as for Table 6.

Table 8:
Difference-in-Difference Pseudo-Experiment of Introducing a NMW in April 1996, ln(Average Wage) and Gross Profit Margins Models, FAME data, Six Financial Years From April 1 1993 to March 31 1999

	(1) *T= Pre-Policy ln(Average Wage)*	(2) *T = Low Wage Firm*	(3) *T = Low Wage Firm and Industry*
(1) ln(AverageWage)			
T*Pseudo-Policy On	-0.014	0.013	0.009
	(0.003)	(0.031)	(0.031)
T	0.875	-0.572	-0.481
	(0.020)	(0.028)	(0.022)
(2) Gross Profit Margin			
T*Pseudo-Policy On	-0.005	0.004	0.008
	(0.012)	(0.010)	(0.018)
T	-0.006	0.006	-0.003
	(0.016)	(0.015)	(0.013)
Controls	Yes	Yes	Yes
Sample Size	4,914	4,914	4,914

NOTES:- This table reports a pseudo-experiment of introducing a National Minimum Wage on April 1st 1996 for financial years April 1st 1996 and March 31st 1999, compared to a pseudo "pre-policy period" (financial years April 1st 1993 – March 31st 1996). T = 1 indicates the treatment Group and T= 0 indicates the comparison group. Standard errors in parentheses are clustered by firm. *ln(Average Wage)* - indicates a continuous measure of the wage (in the pseudo pre-policy financial year up to March 31st 1996) is used for treatment intensity. *Low Wage Firm* - treatment group is defined as firms with an average wage equal to or below £12,000 per annum in the pseudo pre-policy financial year up to March 31st 1996; the comparison group is defined as firms with average wages between £12,000 and £20,000. *Low Wage Firm and Industry* - treatment group is defined as firms with average wages equal to or less than £12,000 in the pseudo pre-policy financial year up to March 31st 1996 and being in a low wage industry (defined as a firm located in a region and three-digit industry LFS cell with 10% or more sub-minimum wage workers in the same period). Controls include one digit industry division; regional dummies (18 government office regions); the firm capital-sales ratio; the proportion of workers who are graduates (by region and two-digit industry); and union membership, part-time work and female employment rates (by three-digit industry classification).

Table 9:
Alternative Models of NMW Impact,
ln(Average Wage) and Profit Margins Models, FAME data,
Six Financial Years From April 1 1996 to March 31 2002

	(1) Unbalanced Panel (Baseline)	*(2) Propensity Score Matching*	*(3) Balanced Panel*	*(4) Balanced Panel (FE)*	*(5) Unbalanced Panel (FE)*
(1) T = ln(Average Wage 1999)					
(a) ln(Average Wage)					
T*Policy On	-0.139 (0.032)	-	-0.107 (0.036)	-0.106 (0.038)	-0.128 (0.032)
(b) Gross Profit Margin					
T*Policy On	0.039 (0.016)	-	0.037 (0.013)	0.040 (0.013)	0.026 (0.011)
(2) T = Low Wage Firm					
(a) ln(Average Wage)					
T*Policy On	0.056 (0.021)	0.056 (0.021)	0.065 (0.022)	0.070 (0.022)	0.075 (0.020)
(b) Gross Profit Margin					
T*Policy On	-0.031 (0.013)	-0.031 (0.013)	-0.028 (0.010)	-0.029 (0.011)	-0.019 (0.010)
(3) T = Low Wage Firm and Industry					
(a) ln(Average Wage)					
T*Policy On	0.057 (0.024)	0.054 (0.023)	0.073 (0.027)	0.077 (0.028)	0.073 (0.025)
(b) Gross Profit Margin					
T*Policy On	-0.044 (0.014)	-0.040 (0.014)	-0.031 (0.012)	-0.034 (0.013)	-0.022 (0.011)
Firm Fixed Effects (FE)	No	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Sample Size	3,820	See notes	2,052	2,052	3,820

NOTES:- As for Table 6. Baseline model in column (1) is taken from Table 6. Sample sizes for the propensity score model: 3,609 (low wage firm model) and 3,740 (low wage firm and industry model).

Table 10:
Firm Exits and the National Minimum Wage, FAME data,

a) Firm Exit Rates (Proportions) Before and After the National Minimum Wage.

	(1) *T = Low Wage Firm*			(2) *T = Low Wage Firm and Industry*		
	Pre-NMW	*Post-NMW*	*Difference (Post – Pre)*	*Pre-NMW*	*Post-NMW*	*Difference (Post – Pre)*
T = 1	0.036	0.053	0.017	0.032	0.040	0.008
T = 0	0.019	0.038	0.019	0.021	0.040	0.019
Difference -in- Difference			-0.002 (0.013)			-0.011 (0.016)

b) Difference-in-Difference Probit Estimates.

	Pr(Firm Exit)		
	(1)	*(2)*	*(3)*
	T= Pre-Policy ln(Average Wage)	*T = Low Wage Firm*	*T = Low Wage Firm and Industry*
T*Policy On	0.002 (0.014)	-0.005 (0.010)	-0.008 (0.010)
T	0.000 (0.008)	0.006 (0.009)	0.001 (0.010)
Controls	Yes	Yes	Yes
Sample Size	2,268	2,268	2,268

NOTES:- Definition of treatment and control groups as per Table 6. Models estimated on the Pre-NMW and POst-Standard errors clustered by firm in parentheses. Controls include: one digit industry division; eighteen government office regions; firm capital-sales ratio; firm age; firm age squared, proportion of graduate qualifications (by region and two-digit industry); and union membership, part-time work and female employment rates by three-digit industry classification.

Appendix

Table A1: FAME Sample Summary Statistics by Reporting Month, Six Financial Years From April 1 1996 to March 31 2002

	Report at End of March	*Report in Other Months*
Gross Profit/Sales	0.321	0.317
Average Wage (£'000s)	15.07	14.64
Average Employment	1,409	1,453
% Total Sample	20.7	79.3
Number of Observations	3,820	18,426

NOTES:- These are descriptive statistics from the FAME database of company accounts for all firms with average wages equal to or less than £20,000.