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ABSTRACT

Although there is a large literature on the economic effects of minimum wages on labour market outcomes (especially employment), there is much less evidence on their impact on firm performance. In this paper we consider a very under-studied area - the impact of minimum wages on firm profitability. The analysis exploits the changes induced by the introduction of a national minimum wage to the UK labour market in 1999, using pre-policy information on the distribution of wages to construct treatment and comparison groups and implement a difference in differences approach. We report evidence showing that firm profitability was significantly reduced (and wages significantly raised) by the minimum wage introduction. This emerges from separate analyses of two distinct types of firm level panel data (one on firms in a very low wage sector, UK residential care homes, and a second on firms across all sectors). We find that net entry rates have fallen, but that the changes in exit and entry rates are statistically insignificant.

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I. Introduction

In debates on the economic impact of labour market regulation, much work has focused on minimum wages. Although standard economic theory unambiguously implies that wage floors raise the wages of the low paid and have a negative impact on employment (Borjas, 2004; Brown, 1999), the existing empirical literature is not so clear. Whilst many studies have shown that minimum wages significantly affect the structure of wages by increasing the relative wages of the low paid (e.g. DiNardo et al, 1996), empirical evidence on the effect on jobs is considerably more mixed (see the recent comprehensive review by Neumark and Wascher, 2007). Some studies have found the expected negative impact on employment[1], yet others have found no impact or, in occasional cases, a positive effect of minimum wages on jobs.[2]

In the light of this, one may wonder how firms are able to sustain the higher wage costs induced by the minimum wage. One possibility is that firms simply pass on higher wage costs to consumers in the form of price increases. However, there is scant evidence on this score (exceptions are Aaronson, 2001, and Aaronson and French, 2007).[3] An alternative is that the higher wage costs are not fully passed on to consumers and the minimum wage eats directly into profit margins.[4] Since there is a complete absence of any study directly examining the impact of minimum wages on firm profitability, this is the focus of this paper.

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[1] See the discussion of time series studies in Brown, Gilroy and Kohen (1982) and Brown (1999) or the US cross-state panel evidence of Neumark and Wascher (1992) and the recent longer run analyses of Neumark and Nizalova (2007).

[2] Examples here are Dickens, Manning and Machin (1999) and Card and Krueger (1994).

[3] See also the recent survey on minimum wages and prices by Lemos (2005).

[4] A third possibility is that minimum wages may "shock" firms into reducing managerial slack and improving efficiency. We examine this hypothesis below but fail to find evidence in its favour.

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 after the election of the 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[5] and so this provides a ripe testing ground for looking at whether profitability changed. 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 fewer low paid workers in order to construct treatment and comparison groups. We then compare outcomes in terms of wages, profitability and firm exit and entry 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 specialized UK data source on workers in residential care homes (a very low wage sector) and an economy-wide firm level database FAME (Financial Analysis Made Easy) that covers all registered firms in the UK.[6] In both data sets, firm profit margins fall in relatively low wage firms following the introduction of the minimum wage. These effects correspond to about a fifteen percent fall in profit margins for the average care home and an eight to eleven 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

[5] See Machin, Manning and Rahman (2003) and Stewart (2004).
[6] 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

closure for low wage firms. Examining entry, however, there is some evidence of a small, but insignificant, negative impact of the minimum wage on net entry.

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 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.[7] When introduced at a binding level, and so long as workers keep their jobs, a

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

[7] Neoclassical exploitation occurs when wages are below the value of the worker's marginal revenue product (Hicks, 1932). Whether poverty rises or falls after the minimum wage is ambiguous. Poverty could

minimum wage results in wage gains for workers who would have otherwise been paid beneath the level of the minimum wage. There may of course be unintended effects of labour market regulations that could produce harmful side effects.

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.[8] 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%.[9]

Most UK studies have not found large negative employment effects associated with these wage gains. Stewart (2004) presents 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 much in the face of large rises in wage costs, then presumably "something else has to give" and so it is important to ask how employers do respond to the introduction of the NMW? Following Ashenfelter and Smith (1979) consider a typical profit maximizing firm employing a quantity of labour (L) at wage rate (W), using other factors at price r and selling its output at price p. Profits are maximized

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rise if many low wage workers lost their jobs. See Besley and Burgess (2004) for an example of how tougher labour market regulation in India appeared to increase poverty levels.

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

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

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 labour. 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 firm profits by $\Pi(W,r,p)-\Pi(M,r,p)$. Using a second-order Taylor series this can be approximated as:

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

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, based on their wages prior to the policy introduction.

This model focuses on the short-run responses when the number of firms is fixed, rather than in the long run when the number of firms is allowed to vary.[10] We believe that this is interesting as researchers cannot be sure how long is the long run (we look up to three years after the introduction of the minimum wage, so to our minds this is reasonably long run). When the product market is perfectly competitive, profits are zero. Firms

which employ low wage workers may well exit the market, so the relevant margin of adjustment will be more exit and less entry. We examine this explicitly in sub-section IV.I below.

When the product market is imperfectly competitive there may also be effects of the minimum wage on profitability in both the short-run and the long run. Appendix A discusses these models in some detail, but it is sufficient to note that positive price cost margins are an equilibrium phenomenon in standard industrial organization models such as Cournot or differentiated product Bertrand. For example, consider a Cournot oligopoly where firms have heterogeneous marginal costs and constant returns to scale. Introducing a minimum wage has a differential impact on the firm employing more low skilled workers causing this firm to lose market share and suffer a fall in its price cost margin. However, so long as profits do not fall below the exit threshold it will remain in the market with lower profitability.

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 related study of financial performance 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

[10] Note that the short-run negative impact on profits will be larger in competitive labour markets than monopsonistic labour markets (see Card and Krueger, 1995). In the latter model there is an offsetting

variations in shareholder wealth. Crucially, 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.[11] 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 some of the other work that looks at the impact of national minimum wages, by looking at a group of firms that were more affected by the NMW introduction than a comparison set of firms.[12] By "more affected", we mean where wages potentially rose by more due to the imposition 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

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positive effect on profitability when wages increase as worker turnover declines.

[11] 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.

[12] 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.

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.[13]

We can evaluate the impact of minimum wages on firm profitability by comparing what happens 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. For example, $\overline{w}^{T=1}_{NMW=1}$ is the mean ln(wage) for the treatment group in the post-policy period. This is just the simple difference in means unconditional on other characteristics of firms. It can easily be placed into a regression context. If T = 1 for firms with a pre-policy ln(wage), $w_{i,t-1,}$ 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:

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

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

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

NMW introduction on the ln(wage) 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 in profit margins, defined as the ratio of profits to sales Π/S, as $\left[\left(\frac{\Pi}{S}\right)^{T=1}_{NMW=1} - \left(\frac{\Pi}{S}\right)^{T=1}_{NMW=0}\right] - \left[\left(\frac{\Pi}{S}\right)^{T=0}_{NMW=1} - \left(\frac{\Pi}{S}\right)^{T=0}_{NMW=0}\right]$ and the conditional difference-in-difference, ψ_2, from the regression model:

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

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 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 constitutes a valid counterfactual. The key conditions are that there are common trends and stable composition of the two groups (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 used in recent studies of minimum wages and employment[14] 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 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 sector. As a result it offers a good testing ground for studying minimum wage effects on profitability since it is a sector that is highly vulnerable to minimum wage legislation. Table 1 shows this very clearly. Prior to the minimum wage introduction in April 1999 average hourly wages were very low in the sector (at around £4 per hour). On average, 32.2% of workers were paid below the incoming minimum wage with this figure falling to 0.4% after the introduction of the policy.

The UK care homes data was collected in 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. Finally, there was some more data collected in 2000 and 2001 for South Coast homes only. The data is in the form of an unbalanced panel so that the same homes are followed over time. The sector was chosen because it is characterized by a large concentration of non-unionized, low wage employees working in

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

small firms with an average employment level of fifteen to twenty. 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).[15] The Department of Social Security paid a capped price for beds, which were 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:

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

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

[15] The average percentage of such residents was 52.7% before the minimum wage introduction and 57.6%

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 share of labour in total costs.[16] Gross profitability is then defined as the ratio of profits to revenue.

B. The Impact of the Minimum Wage on the Structure of Wages

As already noted, the care homes sector had the potential to be heavily affected by the introduction of the NMW.[17] Table 2 presents estimates of home-level wage change equations for the period surrounding NMW introduction (1998-99) and as a falsification exercise for an earlier pre-policy period (1992-93). The upper panel of Table 2 reports results for a ln(wage) change equation that employs the initial wage gap variable as a measure of treatment intensity.

$$\Delta w_{it} = \lambda_0 + \lambda_1 GAP_{i,t-1} + \upsilon_{it} \tag{5}$$

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 0.861 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 wage experienced an 8.6% increase in average wages relative to workers in a firm already paid at least the minimum. This coefficient rises slightly to 0.886 once controls are added (specification (4))).

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after. We always condition on this variable in the regressions.

[16] Total sales and profits are not reported directly in the care homes data. We calculated them from the underlying home-specific components. Sales (*S*) is calculated as Occupancy Proportion* Number of Beds * Average Price (all reported in the survey). The wage bill (*WB*) and the share of labor in total costs (*SHARE*) are also reported directly in the data. We can then calculate total costs (*TC*) as the ratio of the wage bill to the labor share (*WB/SHARE*). Profits are then simply sales less total costs (*S* - *TC*). Profitability is the ratio of profits to sales, (*S* - *TC*)/*S*.

[17] 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.

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 wage distribution as the real 1999 minimum, we can compute a *GAP* measure for the earlier pre-policy time period.[18] Whilst wages *did* rise more in the pre-policy period in homes with a bigger initial wage gap the estimated relationship is much weaker, as shown by the estimated coefficient of 0.240 for the specification with controls. The gap between the two estimates is statistically significant and large in magnitude (at 0.678) 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.[19]

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 profitability. Table 3 reports estimates of regression-corrected profitability specifications. We report reduced form models that relate changes in profit margins to the initial wage gap measure as follows:

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

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. The two reported specifications

[18] Note that this is the only previous wage change information that exists, as the data was not collected in other (non-election) years.

[19] 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.

differ in whether or not they include the control variables, *Z*. In the column (2) specification with controls the coefficient is -0.492, indicating that a firm facing a 10% wage gap before the introduction of the minimum experienced a reduction in their profit margin of about 0.049. However, note that the mean of the wage gap variable is 0.037 (see Table 1). Therefore, the average firm in the sample would be facing reductions in profit margins of about 0.018 (=0.037*0.492), which given the initial average profit margin of 0.119 (see Table 1) translates into a substantial 15.1% cut in profit margins.[20]

It is important to assess the plausibility of the magnitudes of these effects. There a few ways we can do this. The first is to compare and contrast results across different data sources. We do this below, when we compare the care homes results with those from the FAME data. The second is to offer a plausibility check by carrying out a calibration exercise. In Appendix (Section A4) we detail a simple model where employment and prices do not change as a result of the minimum wage which predicts an estimate of the impact of the pre-minimum wage bill that is in line with the estimates we report. The simple exercise reported there shows that the coefficient on the initial wage gap measure should equal the ratio of the wage bill to sales. The (trend adjusted) point estimate on the wage gap term in the margins equation turns out to be -0.396 for the model with controls (and -0.343 for the non controls baseline), which in absolute terms is very close to (but just under) the wage bill to sales ratio in our sample of care homes (0.398) as reported in Table 1. Hence, the magnitude of the estimated impact is in line with what we would expect from one simple model where there is no employment or price adjustment.

[20] Using a trend adjusted version based on the past wage changes would reduce the implied effect to (lower bound) of 12.3%. (= 0.037*0.396//0.119). See Appendix A for more details of calculations.

D. Home Closures

The negative estimated profitability effects also raise the possibility that homes may have had to close because of the minimum wage. Following Machin and Wilson (2004) we look at closures over a reasonably long window by using 1998 and 2001 samples of South Coast homes. We find that, of the 487 homes with clean survey data from 1998, 126 shut down over the 1998-2001 period. This corresponds to a high closure rate of 22.6%, 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 the effects are very weak. Table 4 shows this using a probit model relating the probability of closure to the pre-introduction wage gap. Although the positive coefficient does indicate that firms most "at risk" from the NMW are more likely to close, the coefficient is insignificant and the implied magnitude of the effect is very small. Since the wage gap increased on average by 0.037 (see Table 1) the implied impact of the NMW was to increase the probability of exit by only 0.1 (= 0.037*0.393) percentage points over the base exit rate of 22.6%.

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.[21] Accounting information

[21] 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.

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.[22]

The great advantage of this data 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, the data 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[23] and do not have as clear a measure of profitability as exists in the (audited) company 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.[24]

Since FAME contains annual accounting information, we have firms reporting 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

[22] FAME is the UK part of the AMADEUS dataset of European company accounts used by many authors . See, *inter alia*, Bloom and Van Reenen (2007).
[23] 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.
[24] These firms will tend to be larger than average as the very smallest firms have the least reporting requirements.

in force. Around twenty-one 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.[25]

We use gross profits from the FAME database 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.[26] Specifically, the measure of price-cost margin we calculate is the ratio of gross profits to sales revenue, which measures profits before taking account of tax, interest or depreciation.[27] To allow for capital intensity differences we also control for firm-specific capital sales ratios.[28]

B. Defining Treatment and Comparison Groups

Unlike the care homes data, FAME does not provide 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 an average wage. 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

[25] We have checked whether the sample who report their accounts on March 31st were representative of the whole FAME sample and in Appendix Table B1 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.

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

[27] 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.

[28] 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.

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.

We have used 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 association between low pay incidence and average wages to assess the effectiveness of this empirical strategy.[29]

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 extensively experimented with the threshold cut-off and we discuss this in detail below.

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

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.[30] 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").

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. The Usefulness of Average Wages to Define Treatment

Given the issues outlined above, then it is important and necessary to ask how accurate these treatment group definitions are at identifying firms most affected firms by the minimum wage regulation. 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.

To assess the usefulness of the approach we adopt we look at segregation and wages in the 1998 cross-section of the British Workplace Employment Relations Survey

[30] 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%.

(WERS)[31]. This contains matched worker and establishment data that allows us to look at within-workplace wage distributions and explore the association between average wages and the intensity of low-wage workers. For 26,509 workers in 1,782 WERS workplaces we computed the proportion of workers paid less than £3.60 per hour (the value of the minimum wage when introduced in 1999) and the average hourly wage in the workplace. There is a strong, negative association between the two variables (a correlation coefficient of -0.61, p-value < 0.001). In Figure 1 we plot the proportion of workers paid at or below the minimum wage against the establishment's average annual wage. This proportion of minimum wage workers tapers off rapidly after an average annual wage of £10,000, supporting the idea that exposure to the minimum wage can be proxied by using an average wage threshold that is around this level. Workplaces with average annual wages of £12,000 or less (our main threshold defining the treatment group) contain 87% of all minimum wage workers. These patterns give some support to our idea that "at risk" group of minimum wage workers are concentrated in firms that pay low average wages.

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

[31] WERS is a stratified random sample of British establishments and has been conducted in several waves since 1980. It has been extensively used by economists and industrial to study a range of issues. Culley et

WERS, the minimum wage introduction should raise average firm wages by more in low wage firms. Thus, we would expect there to be larger 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). Importantly, there is no evidence of faster wage growth for the bottom decile in the pre-policy year (in fact wage growth in the bottom fifteen 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. Reassuringly, 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 definition of the treatment group is useful.

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

al. (1999) give details of the survey

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 aggregated up from the Labour Force Survey (similar to the US CPS). These are used as control variables in the analysis and include (at the three-digit industry level) the proportion of (a) part-time workers, (b) female workers and (c) union members. We also include skills 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 set of regional, one-digit industry and time dummies. Variable definitions are given in the Data Appendix. Appendix Table B2 shows the characteristics of the treatment and comparison groups for each model.[32]

Finally, the magnitude of the minimum wage increases 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%

[32] Interestingly the profitability of low wage 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.[33] Small cell sizes prevent us from estimating separate models for the 2000 and 2001 upratings.[34]

F. Descriptive Analysis

The statistical analysis of the FAME dataset begins in Table 5(a) where we present unconditional difference-in-differences in the mean ln(wage) 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[35] was higher at 21.4 percent in the low initial wage group (T = 1) as compared to wage growth of 12.3 percent in the higher initial wage group (T = 0). 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 is given for profitability in Table 5(b). The Table is structured the same way as the wages 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 0.021 between the pre- and post-NMW periods in the low wage firms (T = 1), they rose by 0.006 in the higher wage firms (T = 0). Thus, there is a negative difference-in-difference of -0.027. This difference is statistically significant and shows preliminary evidence that profit margins were squeezed in firms that were "at

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

risk" from the introduction of the minimum wage. Again, as 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).

Table 5 supports the notion that the minimum wage 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 confounding influences

G. Firm-Level Estimates of the Impact of the Minimum Wage on Wages and Profitability

Table 6(a) reports the difference-in-difference wage regression results, for three initial wage measures, the (negative of the) pre-policy average wage and 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 negative of 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

[35] Note that we are looking across the six financial years from April 1 1996 to March 31 2002 (three years before the policy and three years afterwards). In Figure 2, we simply looked one year before and after the policy introduction.

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).

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, 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 also 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 -0.042 in the low wage firm and industry model compared to -0.031 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. When compared to average profits in the low-wage firms in the pre-policy period with the results for the low-wage firm model they imply a 7.7 per cent (-0.031/0.403) fall in profit margins compared to a 10.7 per cent fall for the combined model (-0.042/0.389 = 0.107).

It is important to reconcile these more modest FAME effects with the bigger percent reductions in profitability in the care homes analysis reported in the previous section of the paper. The first observation here is that results are qualitatively the same, showing negative effects on profitability in very different dataset. Second, we would expect there to be stronger effects in the care homes sector since the effects on wages are bigger and the spike in the wage distribution at exactly the minimum wage reveals that minimum wages matter more in such a vulnerable sector with large numbers of low paid

workers. This is, in fact, what we see. Third, if we carry out a calibration exercise as we did for the care homes estimates above we confirm that the magnitudes are sensible. The exercise here is a little different owing to the fact that we are not able to use a measure of the initial wage gap in the FAME data. Appendix A shows that under the assumption of no disemployment effects, the profitability effect of minimum wages should be a scaled up version of the effect of the minimum wage on average wages (where the scaling factor is the ratio of the wage bill to gross profits, which is 1.4 in our data). The results in Table 6 (column (2)) suggest a 6 percent increase in wages, which translates through to a 7.9 percent fall in profit margins (holding employment and sales constant). As noted above, the -0.031 impact on profit margins in the lower panel of Table 6 amounts to a 7.7 percent fall in margins (compared to the mean profit margin in low wage firms before minimum wage introduction). The difference (7.9% vs. 7.7%) is not large, however, suggesting (as with the care home results) that much of the effect is borne by lower profitability and the estimated magnitudes are very much in line with those predicted by a simple model.[36]

H. 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 rely on pre-policy low wage status as being a

[36] As noted in the Section II, a condition for the existence of long-run effects of minimum wages on profitability is that there is some degree of imperfect competition in the product market. To examine this idea we split industries into "high" and "low" competition industries based on a proxy for the Lerner Index (constructed as in Aghion et al, 2005). Consistently with this idea, the effects of the policy were numerically stronger in the less competitive sectors (above median value of three-digit industry Lerner index). For example, in our baseline Low Wage Firm model in column (2) of Table 6 the coefficient

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 different wage thresholds, running from an average wage of £10,000 at £1,00o 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' or falsification test 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 fall in low wage firms.

(standard error) in the industries with an above median Lerner Index was -0.042 (0.017) compared to -0.010 (0.020) in the other (below median) sectors.

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. Therefore, the positive pre-sample trend for low-wage firms seems specific to the care homes industry rather than a general feature of UK firms.

Nevertheless, we investigated this issue in 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.[37] 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 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.[38] As discussed earlier our sample seems well chosen with relatively few observations needing to be trimmed to ensure common support. More importantly, the estimated effect of the policy on wages and profitability are similar to those in column (1).[39]

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. The results show significant effect of the NMW on wages and profits in

[37] This 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.

[38] 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. We trim at the 1st percentile of the treatment group and the 99th percentile of the control group.

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

the balanced panel and, although the estimated magnitudes of the profitability effects are a little smaller, they remain strongly significant.

Column (4) of Table 9 includes a full set of three-digit industry time trends. These trends were jointly insignificant, but even so, the treatment effects appear to be slightly stronger when they are included. Column (5) includes a full set of firm fixed effects. Of course, in the case of a balanced panel with no 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 are qualitatively similar to the baseline results showing a significant effect of the introduction of a national minimum wage on both wages and profitability.

I. Entry and Exit

The FAME database identifies four categories of inactive firms, namely firms that are dissolved, liquidated, in receivership or currently non-trading.[40] Hence, we have defined 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 (Table B3 in Appendix B reports some descriptive statistics). 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

[40] 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.

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 negative of the pre-policy average wage and the other two are the two binary treatment-control comparisons. There is no evidence of any faster increase in exit rates in initially low wage firms following the minimum wage introduction.

There are two possible problems with the firm-level analysis of exit. First, we ignore the possible entry-deterring effect of the minimum wage may, and second, there may be pre-policy trends. Table 11 takes both of these into account. Obviously, we cannot fully implement this at the firm level, as entrants do not have a pre-policy wage for the entrants. However, we can examine an alternative dataset containing all entrants and exits in each three-digit sector (from the DTI's VAT Registration Database). Unlike the firm data, we cannot distinguish between exit due to takeover and exit due to bankruptcy (the focus of Table 10).

Panel A of Table 11 contains the entry results. In column (1) we examine how entry rates have changed after the policy. Perhaps surprisingly, entry rates in low wage industries appear to be *higher* following the minimum wage introduction. Column (2) repeats the analysis but for a period prior to the introduction of the policy (comparing a pseudo policy in 1998-1996 with the 1993-1994 period). We see that entry into low wage industries was already increasing prior to the policy as many low wage service sectors (retail, hospitality, etc.) have been expanding. When we control for the pre-policy trend in the final column, we estimate a *fall* in entry rates of 3.6 percentage points, as we might theoretically expect (although this is statistically insignificant).

Panel (B) conducts the same analysis but for industry exit rates with similar findings. Exit rates perversely seemed to fall as a result of the policy column (1) (as they did in Table 10). But again there was a trend towards lower exit in low wage industries, regardless of the policy (see column (2)). Consequently, the trend-adjusted difference in difference of column (3) finds an imprecisely estimated increase in exit of 1.5 percentage points. Panel (C) puts these together for net entry. The trend-adjusted difference in difference is a fall of 5.1 percentage points.

We should be cautious in over-interpreting these results as the standard errors around all these estimates are wide and the effects are not significant at conventional levels. Nevertheless, the magnitudes are not trivial. The entry rate pre-NMW was 9.3%, so the estimated fall in entry due to the minimum wage of 3.6% implies a fall of over a third in entry rates.

J. Employment and Labour Productivity

Finally, we examined other outcomes, in particular whether there was a NMW effect on firm employment. Table 12, in line with other UK evidence, suggests no significant effect of the minimum wage. The estimated effects are positive in panels A and B and negative in Panel C, but all are insignificantly different from zero.

We also looked at 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 0.054 on T*Policy On in a ln(Sales/Employment) regression). We did not therefore find any evidence of minimum wage effects on productivity.

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 there is the paradox that in some research there are not negative employment consequences. Models of imperfect competition in the product market suggest that the cost of minimum wages may be partly born by firms in terms of lower profit margins.

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 low wage care homes sector and another covering a range of private sector firms. Across both datasets our results we show robust results that that profitability was significantly reduced by the introduction of the minimum wage. With respect to other outcomes, wages rose significantly, but neither employment nor productivity seemed to change.

Finally, 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. Our analysis of an industry level panel dataset suggested that there was some fall in net entry rates following the minimum wage, hinting at a longer run negative effect on the number of firms. These

results were rather imprecise, however, and not significant at conventional levels. The dynamics effect of labour market regulations, working through entry and exit of firms, is an area which calls out for future work.

There are, of course, a number of caveats to our results. It would also be useful to have better data on prices and quality to see if this margin has also adjusted.[41] It would also be useful to have more information on the within firm distribution of workers in other sectors besides care homes. A better integration of theory and empirical work in the context of imperfect competition in both product and labour markets would be another fruitful research area. Indeed, it is somewhat of a puzzle that our results are consistent with a simple model where firms do not seem to adjust their employment and prices in response to the minimum wage in the way we would expect.

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.

[41] Although there is no evidence for these effects in the care homes sector, as it is heavily regulated (see Machin, Manning and Rahman, 2003).

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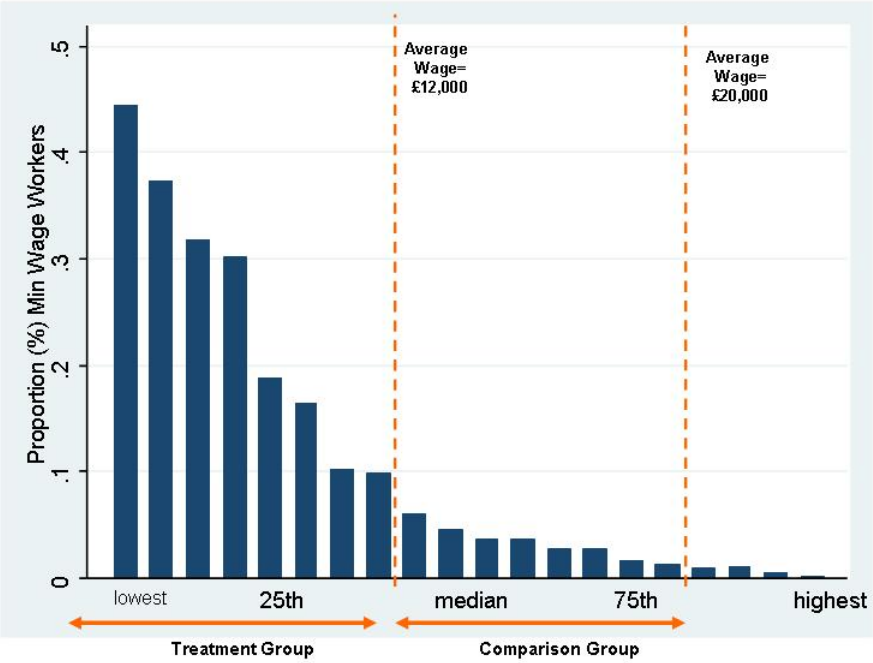
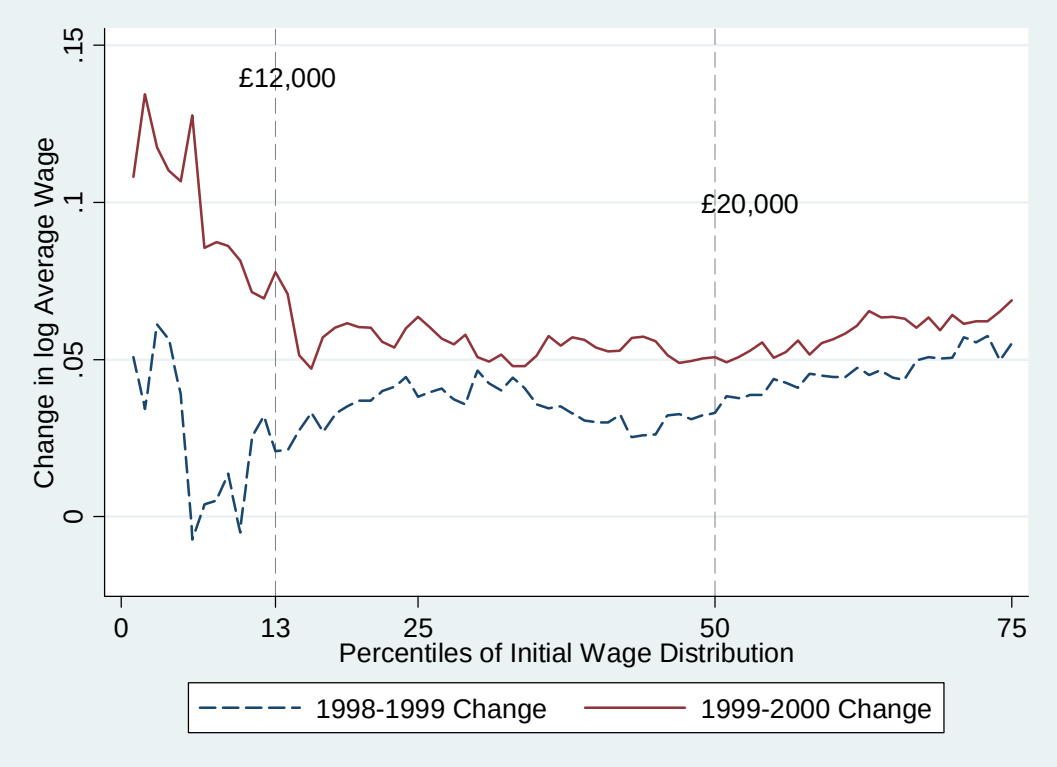
Figure 1: Proportion of Sub-Minimum Wage Workers and the Establishment's Average Annual Wage, WERS 1998 Cross Section of Matched Workers and Workplaces



Source: Workplace Employee Relations Dataset 1998 (Worker-level Survey)

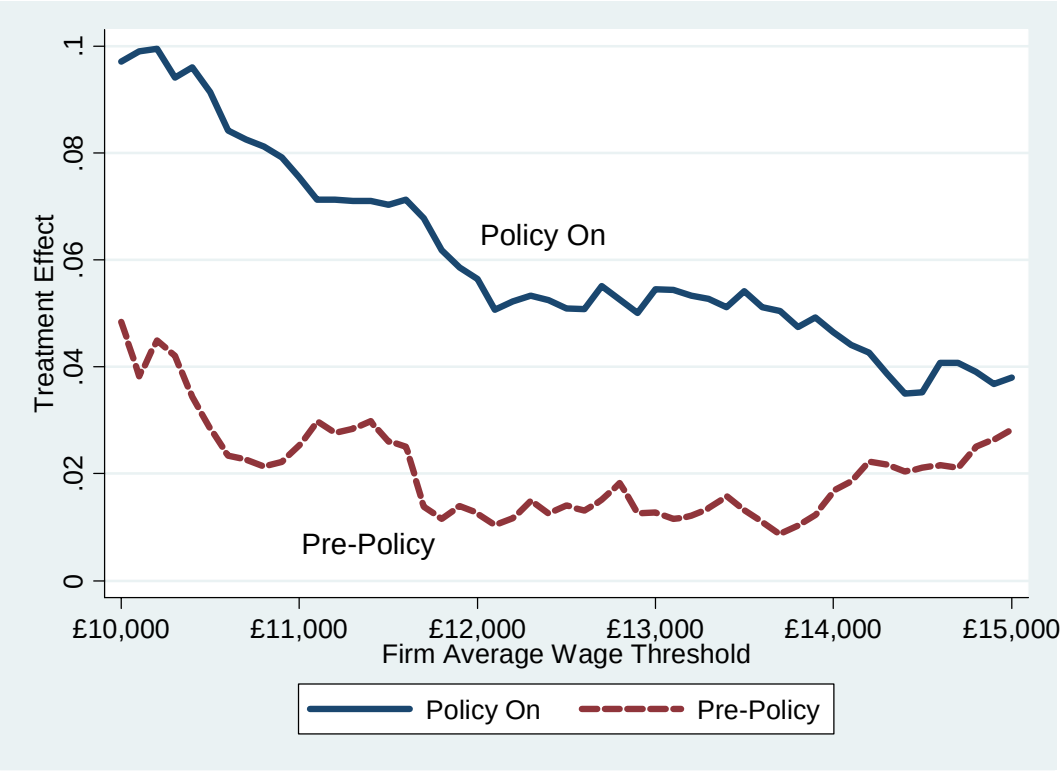
NOTES:- These figures are derived from the worker-establishment data (23,319 workers in 1,782 workplaces) from the 1998 Workplace Employee Relations Survey (WERS). The y-axis shows the proportion of workers paid below the minimum wage (£3.60 per hour) in the establishment. The x-axis shows the average annual wage at the workplace. This is divided into bins for of five percentiles from lowest (left) to highest (right) - a total of twenty bins.
We mark the relevant thresholds for our analysis with vertical lines. The £12,000 line represents the main treatment group threshold used in our analysis of the FAME data. The £20,000 line is the cut-off for the upper bound of the comparison group used in the FAME analysis.

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



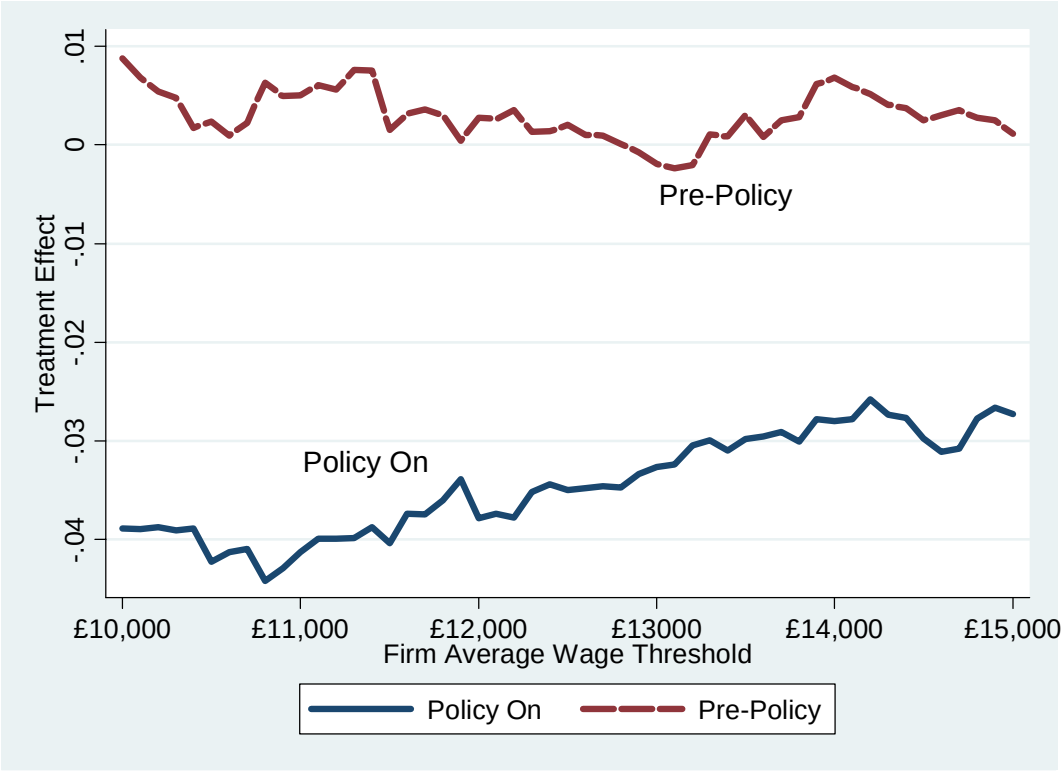
NOTES:- The data is taken from the FAME database of company accounts. The horizontal axis indicates the percentile in the firm wage distribution for a given firm in the initial period, the pre-policy financial year up to March 31st 1999. The vertical axis shows the proportionate change in average firm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

Figure 3: Treatment Effect Coefficients From Different Thresholds and Pseudo-Experiments, Ln(AverageWage), FAME



NOTES:- Data taken from is the FAME database of company accounts. The baseline models are as per Low Wage Model, Table 6 column (2) for the Policy On period; and Table 8 column(2) for the Pre-Policy period. . The vertical axis shows the estimated treatment effects. The horizontal axis shows 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. The baseline model is then re-defined and re-estimated using 50 successive treatment group wage thresholds between £10,000 and £15,000.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.

Figure 4: Treatment Effect Coefficients From Different Thresholds and Pseudo-Experiments, Gross Profit Margins, FAME



NOTES:- Data taken from is the FAME database of company accounts. The baseline models are as per Low Wage Model, Table 6 column (2) for the Policy On period; and Table 8 column(2) for the Pre-Policy period. . The vertical axis shows the estimated treatment effects. The horizontal axis shows 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. The baseline model is then re-defined and re-estimated using 50 successive treatment group wage thresholds between £10,000 and £15,000.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.

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

	Pre-Minimum Wage Introduction, 1998	*Post-Minimum Wage Introduction,1999*
Average hourly wage (£)	3.98	4.22
% Paid Less Than Adult Minimum Wage	0.322	0.004
Initial Wage Gap	0.037	0.001
(Profits/Sales)	0.119	0.126
(Wage Bill/ Sales)	0.398	0.410
Average Age of Employees (years)	40.17	40.26
Occupancy Rate (proportion)	0.894	0.900
Female Employees (proportion)	0.927	0.928
Nursing Qualifications (proportion)	0.103	0.106
DSS (proportion)	0.527	0.576

NOTES:- These figures are based on the 454 care homes for which we have complete data on wages, profits and control variables. "DSS" is the proportion of residents who have their care paid for by the government (Department of Social Security); occupancy rate is measured by the number of beds currently in use over the total number of beds.

Table 2: The Impact of the National Minimum Wage Introduction on Average Wages in Care Homes

Dependent variable*:* $\Delta \ln W$ *, Change in Average Wage*

	Change Surrounding NMW Introduction, 1998-1999 (1)	Change in Earlier Pre-Policy Period, 1992-1993 (2)	Difference (3)	Change Surrounding NMW Introduction, 1998-1999 (4)	Change in Earlier Pre-Policy Period, 1992-1993 (5)	Difference (6)
Initial Wage Gap (t-1)	0.861 (0.045)	0.225 (0.103)	0.636 (0.111)	0.886 (0.052)	0.240 (0.104)	0.678 (0.113)
Controls	No	No	No	Yes	Yes	Yes
Number of Homes	454	225		454	225	

NOTES:- Coefficients estimated by Ordinary Least Squares, robust standard errors in parentheses underneath coefficients. The pre-policy period (1993-1992) is used because it is the only other time period for which data on care homes was collected. The sample for 1998-1999 is based on the 454 care homes for which we have complete data on wages, profits and control variables. Control variables include proportion female, mean worker age, proportion with nursing qualifications, region dummies and month dummies.

Table 3: The Impact of the National Minimum Wage Introduction on Profitability in Care Homes, 1998-1999

Dependent variable:	$\Delta(\Pi/S)$, *Change in Profitability*	
	Change Surrounding NMW Introduction, 1998-1999	Change Surrounding NMW Introduction, 1998-1999
	(1)	(2)
Initial Wage Gap (t-1)	-0.433 (0.173)	-0.492 (0.202)
Controls	No	Yes
Number of Firms	454	454

NOTES:- Coefficients estimated by Ordinary Least Squares. Robust standard errors in parentheses under coefficients. Initial pre-minimum wage period (t-1) controls include workforce characteristics (proportion female, mean worker age, proportion with nursing qualifications), the proportion of residents paid for by the government ("DSS"), region dummies and month dummies.

Table 4: The Impact of the National Minimum Wage Introduction on Care Home Closures (by August 2001)

	Pr[Home Closure], 1998-2001	
	(1)	(2)
Initial Wage Gap (t-1)	0.357 (0.356)	0.393 (0.380)
Controls	No	Yes
Number of Firms	487	487

NOTES:- The proportion of homes that were alive in 1998 (year before minimum wage) but closed by August 2001 is 0.226. We report the marginal effects from probit ML estimates with standard errors underneath in parentheses. Control variables are the proportion of female workers, and the proportion of workers with nursing qualifications, average age of employees, the proportion of DSS local authority residents, region dummies and month dummies.

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

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

	T = Low Wage Firm			*T = Low Wage Firm and Industry*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
	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

	T = Low Wage Firm			*T = Low Wage Firm and Industry*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
	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: National Minimum Wage Difference-in-Difference Regression Estimates, Change in ln(Average Wage) and Gross Profit Margins Models, FAME, 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) $\Delta \ln W$ *, Change in ln(Average Wage)*			
T*Policy On	0.139 (0.032)	0.057 (0.020)	0.054 (0.023)
T	-0.888 (0.015)	-0.527 (0.021)	-0.458 (0.029)
(b) $\Delta(\Pi/S)$ **,** *Change in Gross Profit Margin*			
T*Policy On	-0.039 (0.016)	-0.031 (0.013)	-0.042 (0.014)
T	0.013 (0.023)	0.015 (0.018)	0.002 (0.023)
Controls	Yes	Yes	Yes
Sample Size	3,820	3,820	3,820

NOTES:- Coefficients estimated by Ordinary Least Squares and standard errors in parentheses below coefficients are clustered by firm. 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. *ln(Average Wage)* - indicates that we use a continuous measure of the wage (in the pre-policy year up to March 31st 1999) is used for treatment intensity. Note that we multiply this by -1 to facilitate the comparison with estimates based on the discrete definition. *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 – Change in Gross 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 National Minimum Wage in April 1996, Change in Ln(Average Wage) and Gross Profit Margins Models, FAME, Six Financial Years From April 1 1993 to March 31 1999

	(1)	(2)	(3)
	T= Pre-Policy -ln(Average Wage)	*T = Low Wage Firm*	*T = Low Wage Firm and Industry*
(a) $\Delta \ln W$ *, Change in ln(Average Wage)*			
T*Pseudo-Policy On	0.014 (0.033)	0.013 (0.031)	0.009 (0.031)
T	-0.875 (0.020)	-0.572 (0.028)	-0.481 (0.022)
(b) $\Delta(\Pi/S)$ **,** *Change in Gross Profit Margin*			
T*Pseudo-Policy On	0.005 (0.012)	0.004 (0.010)	0.008 (0.018)
T	0.006 (0.016)	0.006 (0.015)	-0.003 (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. .Note that we multiply this by -1 to facilitate the comparison with estimates based on the discrete definition. *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 National Minimum Wage Impact, ln(Average Wage) and Profit Margins Models, FAME data, Six Financial Years From April 1st 1996 to March 1st 2002

	(1) Unbalanced Panel (Baseline)	*(2) Propensity Score*	*(3) Balanced Panel*	*(4) Industry Trends*	*(5) Fixed Effects (Unbalanced Panel)*
(1) Average Wage 1999					
(a) ΔlnW					
T = 1*Policy On	0.139	n.a.	0.107	0.157	0.128
	(0.032)	(see notes)	(0.036)	(0.034)	(0.032)
(b) Δ(Π/S)					
T = 1*Policy On	-0.039		-0.037	-0.049	-0.026
	(0.016)		(0.013)	(0.018)	(0.011)
(2) Low-Wage Firm					
(a) ΔlnW	0.056	0.061	0.065	0.063	0.075
T = 1*Policy On	(0.021)	(0.025)	(0.022)	(0.023)	(0.020)
(b) Δ(Π/S)	-0.031	-0.029	-0.028	-0.038	-0.019
T = 1*Policy On	(0.013)	(0.015)	(0.010)	(0.014)	(0.010)
(3) Low Wage Firm and Industry					
(a) ΔlnW	0.057	0.069	-0.073	0.069	0.073
T = 1*Policy On	(0.024)	(0.030)	(0.027)	(0.027)	(0.025)
(b) Δ(Π/S)	-0.044	-0.040	-0.031	-0.053	-0.022
T = 1*Policy On	(0.014)	(0.017)	(0.012)	(0.017)	(0.011)
Industry Trends	No	No	No	Yes	No
Firm Fixed Effects	No	No	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Sample Size	3,820	3,287 (Low wage firm); 3,500 (Low Wage Firm and Industry)	2,052	3,820	3,820

NOTES:- See notes to Table 6. The baseline model in column (1) is taken from Table 6 column (1). The propensity score model in column (2) trims the sample of firms using a probit model estimated using the initial firm and industry characteristics that appear as controls in the baseline model. We trim on the 99% percentile of the control and the first percentile of the treatment to obtain common support. Note that the propensity score model cannot be estimated for the continuous wage model since this is not a binary variable. Column (4) includes three digit industry-specific trends. Column (5) includes firm fixed effects.

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 of firm exit.

	(1) *T= Pre-Policy ln(Average Wage)*	(2) *T = Low Wage Firm*	(3) *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-NMW. 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.

Table 11: Firm Entry and Exit Rates by Three-Digit Industry

	(1) NMW Policy On 1996-2001	(2) Pseudo-Policy 1994-1998	(3) Trend- adjusted Difference in Differences
(A) Industry Entry Rates			
Lowpay*Policy On	0.021	0.057	-0.036
	(0.015)	(0.032)	(0.038)
Lowpay	0.010	-0.095	
	(0.066)	(0.048)	
(B) Industry Exit Rates			
Lowpay*Policy On	-0.013	-0.028	0.015
	(0.016)	(0.018)	(0.024)
Lowpay	-0.005	-0.011	
	(0.025)	(0.039)	
(C) Industry Net Entry Rate			
Lowpay*Policy On	0.034	0.085	-0.051
	(0.025)	(0.027)	(0.037)
Lowpay	0.015	-0.084	
	(0.072)	(0.060)	
Sample Size	1,020	850	

NOTES:- Data taken from Value-Added Tax (VAT) Registrations and Deregistration Data, Department of Trade and Industry (DTI). Entry rate is the proportion of firms who are newly registered in a year in a three-digit industry. Exit rate is the proportion of firms who are deregistered in the year. Net entry is entry rate – exit rate. Standard errors (in parentheses) are clustered by three-digit industry. In column (1) we define the usual definitions of policy (Policy-On = 1 for 1999-2001 and zero for 1996-1999). In column (2) we conduct a pseudo-experiment to measure the pre-policy trend. Here, Policy On = 1 for 1996-1998 and zero for 1994-1995 (we do not have consistent data for 1993). "Lowpay" is defined as the proportion of workers with an hourly wage less than £3.60 in the three-digit industry in real terms over the pre-policy period (the minimum wage threshold of £3.60 is deflated by the retail price index for the years 1994-1998). Column (3) is the trend-adjusted estimator. All columns include controls for two digit dummies, time dummies, and the proportion of employees in the three-digit industry that are female, part-time and unionized.

Table 12: Employment Effects in FAME

	(1) *Pseudo-Policy*	*(2)* *NMW Policy On*	*(3)* *Difference*
(A) Continuous Wage			
T*Policy On	0.065 (0.124)	0.191 (0.121)	0.126 (0.173)
T	0.286 (0.170)	0.488 (0.217)	-
(B) Low-Wage Firm			
T*Policy On	0.137 (0.093)	0.200 (0.090)	0.063 (0.129)
T	-0.005 (0.143)	0.389 (0.160)	-
(C) Low wage firm and industry			
T*Policy On	0.201 (0.134)	0.166 (0.106)	-0.035 (0.171)
T	0.220 (0.215)	0.701 (0.220)	-
Controls	Yes	Yes	
Sample Size	4,914	3,820	

NOTES:- As per Table 6. This table replaces wages and profits with log employment as the dependent variable in the baseline model. Column (8) reports results for the pseudo-experiment period analysed in Table 8 while Column (9) reports results for the actual NMW policy period (i.e. analogous to Table 6). A trend-adjusted difference-in-difference is reported in column (3).

Appendices

Appendix A: Theoretical Models of Profitability and Minimum Wages

A1. Introduction

In order to obtain a long-run effect of the minimum wage on profitability we need to have some degree of imperfect competition in the product market. We therefore consider several industrial organization models. Aaronson and French (2007) consider in detail the effects on the minimum wage on prices and costs in a competitive and monopsonistic labour market model. However, in these models firms do not have positive price-cost margins so profits remain zero by assumption, regardless of the minimum wage.

We separate our analysis into the short-run and long-run, where we define the short run as the period where all variables are able to change (*including* capital, labour, prices, etc) but the number of firms is held fixed. In the long run entry and exit can occur and the number of firms can change. Our analysis of exit and entry is directly applicable to the long-run results.

A2. Imperfect Competition in the Product market

Short-run effects with symmetric and asymmetric costs

Consider a two-stage game where firms pay a sunk entry cost (K) and, conditional on entering engage in competition with other firms (total number of firms in market is denoted N). The instruments of competition can be price or quantity.

We begin with the workhorse industrial organization model of an asymmetric Cournot model[42] where firms have heterogeneous marginal costs. Below we discuss alternative imperfect competition models that lead to similar qualitative results.

The non-cooperative Nash equilibrium in quantities gives a well-know expression for the price-cost margin:

$$\frac{p - c_i(q_i)}{p} = \frac{MS_i}{\eta} \tag{A1}$$

Where η is the (absolute value of the) price elasticity of product demand, p is output price, c_i is marginal cost of firm i, q_i is firm output and MS_i is the market share

[42] Cournot competition can be considered the reduced form of a two-stage game where firms set capacities in the first stage and then compete in prices in the first stage (Kreps and Scheinkman, 1983).

($MS_i = \frac{S_i}{\sum_i S_i}$) with S_i denoting firm sales. Note that equation (A1) nests the special cases of monopoly ($N = 1$). If we assume constant returns then marginal costs do not depend on output ($c_i = c_i'(q_i)$) so the price-cost margin can be characterised by the ratio of profit (Π) to sales (S):

$$\left(\frac{\Pi}{S}\right)_i = \frac{MS_i}{\eta} \tag{A2}$$

Firm *i*'s market share will depend on its marginal costs relative to the marginal costs of other firm's in the industry. If firm *i*'s marginal costs rise relative to those of other firms it will lose market share (see Tirole, 1989, Chapter 5 for example).

Consider the effect of an increase in the minimum wage. If we assume that demand is iso-elastic (we will relax this below) then the impact of the minimum wage on the firm's price-cost margins will be reflected in its market share. If a firm employs a greater proportion of minimum wage workers, it will face a larger increase in marginal costs and therefore a larger fall of its price-cost margins.

This is our key comparative static result: the introduction of a minimum wage will reduce the profitability of firms who are more "at risk" because they employ a higher share of minimum wage workers.

If we also relax the assumption the demand elasticity is constant, there will also likely be a fall in profitability. To see this clearly assume that firms are symmetric so that they all face identical marginal costs. In this case, the equilibrium condition of (A1) simplifies to

$$\frac{\Pi}{S} = \frac{1}{N}\frac{1}{\eta} \tag{A3}$$

It is clear from equation (A3) that the impact of the minimum wage will on profitability ($\frac{\Pi}{S}$) will depend on its impact on the demand elasticity (η). In particular if demand becomes more elastic, profitability will fall. For most commonly used demand curves, a minimum wage will make the demand curve more elastic because price has risen. For example, consider the case of linear industry demand (Q) for where $Q = A - bp$, $b > 0$, $A > 0$. In this case, $\eta = b\frac{p}{Q}$. Following the introduction of a minimum wage prices will be higher and quantity sold lower unless demand is perfectly elastic. The elasticity of demand is therefore higher and profitability will fall. This will reinforce the effects on market share discussed in the more general model with asymmetric firms.[43]

[43] We cannot rule out the possibility that the aggregate demand curve may become more elastic as wages rise even if the labour market is perfectly competitive. Micro-economic theory places few restrictions on

Under differentiated products Bertrand equation (A3) should be interpreted as a firm-specific elasticity.

This result differs from Aaronson and French (2006) who consider a model of monopolistic competition. This generates an equilibrium condition like (A3). The minimum wage has no effect on price cost margins in their model because they assume that the elasticity of demand is constant. This guarantees no effect of the minimum wage on price-cost margins as all costs are passed through completely to the consumer. Additionally, the "large number of firms" assumption underlying monopolistic competition rules out strategic interactions that generate the market share effects in equation (A3)

Long-run effects

After the minimum wage is imposed, absolute profits in the industry will be lower. This will mean that there is less of an incentive to enter the industry. Consequently, we might expect to see fewer firms in the industry (from exit and/or less entry) in the long run. The short run fall in profits for the incumbent firms in the industry will therefore be greater than the long-run change as N will fall (e.g. see equation (A3)).

An important caveat to this is that the number of firms in the industry may not fall due to an "integer" effect. Since there will always be an integer number of firms in the industry all firms will usually earn some economic profit. Firms will enter and pay the sunk cost up until the point that a marginal firm entering the industry would not make a profit net of the sunk cost. For example, consider a symmetric duopoly in long-run equilibrium. If a third firm entered the industry a firm's profits (net of the sunk cost, K) would be negative i.e. $\Pi^{*(3)} - K \leq 0 \leq \Pi^{*(2)} - K$, where $\Pi^{*(3)}$ is equilibrium profits with three firms and $\Pi^{*(2)}$ equilibrium profits with two firms.

Now, except in the special case when profits in the market exactly covers the sunk cost ($\Pi^{*(3)} - K < 0$ and $\Pi^{*(2)} - K = 0$) the minimum wage could reduce $\Pi^{*(2)}$, but not by so much that $\Pi^{*(2)} - K < 0$ and one firm was forced to exit the industry. Consequently, for small increases in the minimum wage firms could have lower profits without a change in the equilibrium number of firms.

This caveat aside, in a dynamic setting we would expect that a minimum wage would increase exit and reduce the entry rate.

A3. Perfect Competition in the Product Market

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industry demand curve aggregated from consumer preferences (e.g. see Varian, 1984, chapter 3.16). Thus, it is still ultimately an empirical issue whether profitability rises or falls after the minimum wage.

Now consider the case of perfectly competitive product markets. Comparative statics of prices and factor demands following a minimum wage increase have been comprehensively analyzed by Aaronson and French (2007). Here, we will briefly contrast the usual case of perfect competition in the labour market with some alternative models. It worth emphasizing two preliminary points. First, as we discussed above that these are in the short-run as in the long run firms earn zero profits by assumption. Second, the short-run effect of the introduction of a minimum wage will be *larger* in the competitive model than in the monopsony model.

Perfect competition in the labour market

If labour markets are perfectly competitive, the short run effects of the minimum wage on profits are composed of two components (see Ashenfelter and Smith, 1979, and the main text). First, there is fall in profits due to the increased wage for the current number of workers paid below the minimum wage. This fall in profits is offset by a second effect to the degree that firms can substitute minimum wage workers for other factors of production (including non minimum wage workers). In the limiting case of perfect substitutability of minimum wage workers there will be *no effect* on profits.

Of course these are only short-run effects as there can be no economic profits under perfect competition and in equilibrium industry prices will rise and quantity will fall (so there will either be fewer firms or the average firm size will shrink).

Imperfect Competition in the labour market

There have been a variety of models proposed in recent years where firms have some power to set wages because of efficiency wages, monopsony, search or other reasons. In these models, over a certain range of values a binding minimum wage can increase employment.

Considering profits, we would expect the negative short-run effects of a minimum wage on profitability to be muted in such models. This is because, unlike the competitive model the first order effect on profits is zero as an increase in the wage has a beneficial effect on profits through making it easier to recruit, retain and/or motivate workers. There will be a second effect because the firm is being shifted away from its optimal level of the wage so overall we would still expect a decline in profits. However, this is likely to be much less severe than in the competitive model.

To see this consider a simple representation of the monopsony model. We model the firm's wage setting power in a reduced form way (following Card and Krueger, 1995) and assume that the production function, $F(W,L)$, is increasing in the wage as well as labour, L. The firm chooses wages and labour to maximize profits

$$\Pi = \max_{W,L} \; pF(W,L) - WL$$

Which lead to the standard first order condition:

$$p\frac{\partial F(W,L)}{\partial L} = W^*$$

where an asterix denotes the optimized value. We also have an additional non-standard condition from optimizing wages of:

$$p\frac{\partial F(W,L)}{\partial W} = L^* \tag{A4}$$

If we consider the effect of a small increase in wages on profits in the neighbourhood of the optimized level of wages and employment (W^*, L^*) this is given by:

$$\frac{d\Pi}{dW} = p\frac{\partial F(W^*,L^*)}{\partial W} - L^*$$

Note that this is equal to zero by the first order condition with respect to wages, equation (A4).

Long-run effects

In this setting, there are no long-run effects on profits.

Considering exit, unlike the model with imperfect competition firm size is not tied down.

In the competitive model, prices will be higher and output lower. In our constant returns set-up a zero profit equilibrium can be restored either by all firms becoming smaller or by some firms exiting.

A4. Quantification of effects assuming no disemployment or price effects

In this section, we consider the assumption is that there is no firm response after the imposition of a minimum wage except to increase workers' pay to the minimum (i.e. neither prices, employment, output, etc. change). We then take the implication of this simple behavioural assumption to the coefficients that we have estimated, first in Care Homes then in FAME (whole economy). Surprisingly, this fits the results reasonably well for care homes and is not too bad for the FAME dataset.

Care Homes

Given the definition of the wage GAP in equation (4) it immediately follows that

$$\Delta\Pi = WL * GAP$$

Where WL is the wage bill (average wages, W, multiplied by the number of workers, L) which is the denominator of the GAP. Consequently,

$$\Delta(\Pi / S) = \left(\frac{WL}{S} \right) * GAP$$

Therefore, under this model, the estimated coefficient on the GAP in the profitability equation should be equal to the ratio of the wage bill to sales.

A complication is that there is a pre-sample trend in cares home data. We do not observe profitability in the pre-sample period, so we cannot directly estimate this as we do for wages. However, we can analytically calculate an upper bound on this bias. We can write:

$$\frac{\partial[\Delta(\Pi / S)]_t}{\partial GAP_{t-1}} = \frac{\partial[\Delta(\Pi / S)]_t}{\partial \Delta \ln W_{t-1}} \frac{\partial \Delta \ln W_{t-1}}{\partial GAP_{t-1}} = \frac{\partial[\Delta(\Pi / S)]_t}{\partial \Delta \ln W_{t-1}} \beta_{\Delta \ln W, GAP}$$

Where $\beta_{\Delta \ln W, GAP}$ is the coefficient on the GAP term in the wage growth regression in the pre-sample period (see equation (5) in the text). This equals 0.240 (with covariates) or 0.225 (without covariates) according to Table 2.

The impact of this adjustment will depend on the assumptions we make over how firms respond to the wage trend (since we do not have pre-policy information on profitability). Let us continue to make the assumption that all quantities and prices are fixed when the wage changes, so this will make the profitability change as large as possible and make it hard for us to reject the null of no profit change. Under this assumption:

$$\frac{\partial[\Delta(\Pi / S)]_t}{\partial \Delta \ln W_{t-1}} = W_{t-1} \frac{\partial[(\Pi / S)]_t}{\partial W_{t-1}} = \frac{W_{t-1}}{S_t} \frac{\partial \Pi_t}{\partial W_{t-1}} = -\frac{W_{t-1} L_t}{S_t}$$

So, since L is unchanged by assumption between t and t-1:

$$\frac{\partial[\Delta(\Pi / S)]_t}{\partial GAP_{t-1}} = -\beta_{\Delta \ln W, GAP} \left(\frac{WL}{S} \right)_{t-1} \qquad \text{(A5)}$$

In the care homes data the ratio of the wage bill to sales is 0.398. So the product on the right hand side of equation (A5) is -0.096 (= 0.398*0.240) for the model with controls and -0.090 (=0.398*0.225) for the model without controls. In other words, the upper bound on the expected profitability change in the absence of the minimum wage are -0.096 (with controls) and -0.090 (without controls) respectively. Since the estimated coefficient in Table 3 is 0.492 with covariates or 0.433 without covariates, we still have a big effect. However, the trend-adjusted effects are -0.396 (= 0.096 - 0.492) and -0.343 (=-0.433 + 0.09) . This is almost the same as the wage bill share. Thus, the care home estimates are consistent with this simple model where care homes do not adjust their employment in response to a minimum wage change.

FAME

In the specifications for FAME it is easier to work with proportionate changes. Under the assumption of no change to employment or sales

$$\Delta(\Pi / S) = -\left(\frac{L}{S}\right)\Delta W = -\left(\frac{WL}{S}\right)\frac{\Delta W}{W}$$

Dividing both sides by profitability

$$\frac{\Delta(\Pi / S)}{(\Pi / S)} = -\left(\frac{WL}{\Pi}\right)\frac{\Delta W}{W}$$

Or, in natural logarithms:

$$\Delta \ln(\Pi / S) = -\left(\frac{WL}{\Pi}\right)\Delta \ln W \qquad \text{(A6)}$$

So under this model we would expect that the effects of the minimum wage on margins to be proportional to the effect on wages (scaled up by the ratio of the wage bill to profits).

In our data, the minimum wage is estimated to have increased average wages by 5.7% (Table 6, panel (a), column (2)). Given the average ratio of the wage bill to profits in the data is 1.4 this means that the implied profitability effect is 8% (= 6*1.4), slightly larger than the estimated effect of 7.7%.

A5. Summary

In models of imperfect product market competition, we would generally expect to observe negative effects on the profitability of firms where the minimum wage bites, even after firms have adjusted all factors of production. In such models, some of the increase in costs is borne by shareholders rather than just consumers and unemployed low-wage workers as in the standard competitive model.

It is worth emphasizing that employment will still fall in these models. So oligopoly could explain only why employment responses could be more muted than one would expect from a competitive model. Of course, employment changes can be positive if firms with market power in the product market also have market power in the labour market.

The final section (A4) showed a very simple model that assumes no change in sales or jobs following a minimum wage hike. This model does surprisingly well in rationalizing the results.

Additional References

Appendix B: Data

B1 Care Homes Data

The care homes data is discussed in the text and in more detail in Machin et al (2003). Homes were surveyed in the year before and after General Elections: in 1992, 1993, 1997 and 1998.

We observe individual worker data so we can construct various measures of the internal wage structure of the firm.

Total revenue and profits are not reported directly in the care homes data. We calculated them from the underlying home-specific components. Sales (*S*) is calculated as Occupancy Proportion* Number of Beds * Average Price (all reported in the survey). The wage bill (*WB*) and the labour cost share (*SHARE*) are also reported directly in the data. We can then calculate total costs (*TC*) as the ratio of the wage bill to the labour share (*WB/SHARE*). Profits are then simply sales less total costs (*S* - *TC*). Profitability is the ratio of profits to revenues, (*S* - *TC*)/*S*.

B2 FAME Data

The FAME (Financial Analysis Made Easy) dataset contains information on firm company accounts of publicly listed and private firms in the UK economy. It is supplied under licence as part of the AMADEUS database from BVD (Bureau Van Dijk). Our sample begins with data on all firms in the six financial years from April 1st 1996 to March 31st 2002 including those who had entered and exited. We select firms who report on the 31st March (see Table B1). We drop firms with missing data on our key variables (profits, wages, sales, employment, industry, and region). We use information on consolidated accounts at the lowest level that exists (i.e. we use subsidiary level information if this exists).

In the main results, we condition on the cohort of firms who were alive on March 31st 1999 when the minimum wage introduced. We also present results where we examine the impact of including firms who entered after this date (and exited before this date) including a dummy variables for entrant and exiting firms (and interactions of these dummies with the NMW policy period).

Profits/Sales: Gross profits (prior to deductions for tax, interest and dividends) over turnover (sales).

Average Wages: Total remuneration divided by total number of employees

Capital / Sales: Tangible assets over turnover (sales).

Sales / Employment: Total turnover (sales) over the number of employees.

B3 LFS Variables

The Labour Force Survey (LFS) is a large-scale household interview-based survey of individuals in the UK that has been carried out on varying bases since 1975.[44] Around 60,000 households have been interviewed per survey since 1984. Annual proportions calculated relative to firm reporting year rather than calendar year (i.e. April 1998 – March 1999).

Union membership: Defined at the three-digit UKSIC industry level, annual values 1993-2002.

Part-Time Work: Proportion of employed workforce classified as part-time, annual values 1993-2002. Defined at the three-digit industry level.

Female Workforce: Female workers as a proportion of total employed workforce, annual values 1993-2002. Defined at the three-digit industry level.

Graduate Qualifications: Proportion of graduate qualified workers per region and two-digit industry cell.

Region: Government Office Region of Workplace ("gorwk"). These include Tyne and Wear, Rest of the North East, Greater Manchester, Merseyside, Rest of the North West, South Yorkshire, West Yorkshire, Rest of Yorkshire and Humberside, West Midlands and Met Country, Rest of West Midlands, Eastern, Inner London, Outer London, South East, South West, Wales, Rest of Scotland, Northern Ireland.

B4 DTI VAT Registration and De-registration Database

The UK Department of Trade and Industry (DTI) publish data on births and deaths of companies at the three-digit level on a consistent basis from 1994 (see http://stats.berr.gov.uk/ed/vat/). These are based on Value Added Tax (VAT) Registration numbers that every incorporated firm in Britain is legally obliged to have. (This is the same as the aggregated FAME date).

We used this data to calculate for each three-digit sector the proportion of firms who entered in a year (entry rate). Entry rates calculated as the number of new VAT (Value-Added Tax) registrations as a proportion of the beginning of year stock. Exit rate calculated as the number of VAT deregistrations over the beginning-of-year stock. Net entry calculated as entry rate minus exit rate. We also calculated the net entry rate as the difference between the entry and exit rates.

[44] Between 1975 and 1983, the survey was conducted every two years. From 1984 until 1991, it was conducted annually. Since 1992, the Labour Force Survey has been conducted every three months in a five-quarter rolling panel format.

We then matched information form the LFS at the same level of aggregation to calculate the proportion of workers in each industry paid below the minimum wage in the pre-policy period.

Table B1: 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*
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.

Table B2: Characteristics of Treatment and Comparison groups (FAME), Six Financial Years from April 1 1996 to March 31 2002

	(1) T = Low Wage Firm		*(2) T = Low Wage Firm and Industry*	
	T=0	T=1	T = 0	T = 1
Average Wage (£'000s)	16.87	9.90	16.04	9.70
Profit/Sales	0.298	0.388	0.311	0.378
Capital / Sales	0.222	0.279	0.225	0.304
Wage Bill / Gross Profits	1.314	1.519	1.356	1.423
Proportion part-time employees	0.163	0.294	0.166	0.367
Proportion female employees	0.385	0.543	0.395	0.593
Proportion union members	0.208	0.179	0.213	0.159
Proportion Degree	0.145	0.124	0.132	0.095
Proportion of firms in:				
Agriculture, Forestry and Fishing	0.006	0.011	0.007	0.014
Mining	0.053	0.033	0.051	0.031
Manufacturing	0.313	0.122	0.300	0.067
Construction and Electricity	0.064	0.020	0.066	0.011
Wholesale, Retail Trade and Hospitality	0.238	0.350	0.219	0.477
Transport and Communications	0.087	0.043	0.091	0.015
Business Services	0.207	0.245	0.216	0.167
Education and Health	0.021	0.066	0.019	0.076
Personal Services	0.070	0.117	0.065	0.143
Observations	2,835	985	3,235	585

NOTES:- T= 0: Comparison group; T = 1: Treatment Group; Part-time and female employees based on Labour Force Survey (LFS) and calculated as proportion of total workers per two-digit industry by regional cell. *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 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).

Table B3: Firm Entry and Exit Rates by Three digit Industry, 1996-2001 (DTI VAT Registrations and Deregistration)

	(1) All Industries	(2) High Wage industries (below median Lowpay)	(3) Low Wage Industries (above median Lowpay)
Entry Rate	0.089	0.087	0.091
Exit Rate	0.082	0.083	0.081
Net Entry	0.007	0.003	0.011
Lowpay	0.126	0.051	0.201
Union	0.287	0.350	0.189
Female	0.343	0.274	0.411
Part-time	0.143	0.076	0.209
No. of Industries	170	85	85
No. of Observations	1,020	510	510

NOTES: Entry rates calculated as the number of new VAT (Value-Added Tax) registrations as a proportion of the beginning of year stock. Exit rate calculated as the number of VAT deregistrations over the beginning-of-year stock. Net entry calculated as entry rate minus exit rate. The variables lowpay, union, female, part-time are all sourced from the UK Labour Force survey (LFS). The "Lowpay" variable is defined as the proportion of workers with hourly wage below £3.60 in the pre-minimum wage period (1994-1998). "Below Median Lowpay" indicates all those industries where the proportion of lowpay workers ranges from 0 to 0.092. "Above Median Lowpay" indicates all of those industries where the proportion of lowpay industries ranges from 0.095 to 0.557.