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Minimum Wages and Firm Profitability†

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We study the impact of minimum wages on firm profitability, exploiting the changes induced by the introduction of a UK national minimum wage in 1999. We use pre-policy information on the distribution of wages to implement a difference-in-differences approach. Minimum wages raise wages, but also significantly reduce profitability (especially in industries with relatively high market power). This is consistent with a simple model where wage gains from minimum wages map directly into profit reductions. There is some suggestive evidence of longer run adjustment to the minimum wage through falls in net entry rates. (*JEL* J31, J38, L25)

In debates on the economic impact of labor market regulation, much work has focused on minimum wages. Although the textbook competitive labor market model implies that wage floors raise the wages of the low paid and have a negative impact on employment (George J. Borjas 2004; Charles Brown 1999), the empirical literature is less clear-cut. Many studies have rigorously demonstrated that minimum wages significantly affect the structure of wages by increasing the relative wages of the low paid (e.g., John DiNardo, Nicole M. Fortin, and Thomas Lemieux 1996).[1] However, in spite of the large number of studies, empirical evidence on employment effects is considerably more mixed (see the recent comprehensive review by David Neumark and William L. Wascher 2007). Some have found the expected negative impact on employment,[2] yet others have found no impact or sometimes even a positive effect of minimum wages on jobs.[3]

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† To comment on this article in the online discussion forum, or to view additional materials, visit the article page at http://www.aeaweb.org/articles.php?doi=10.1257/app.3.1.129.

[1] See also Lemieux (2006) for some recent evidence on the United States and DiNardo and Lemieux (1997) for a comparison with Canada.

[2] See the discussion of time series studies in Brown, Curtis Gilroy, and Andrew 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 Olena Nizalova (2007).

[3] Examples here are Richard Dickens, Machin, and Alan Manning (1999) and David Card and Alan B. Krueger (1994).

In light of this, it is natural to ask how firms are able to sustain higher wage costs induced by the minimum wage. This paper explores the possibility that firm profit margins are reduced. A second 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.[4] Indeed, even with some positive price response, part of the higher wage costs may not be fully passed on to consumers and the minimum wage could eat directly into profit margins. A third possibility is that minimum wages may "shock" firms into reducing managerial slack and improving efficiency. We examine this productivity story but do not find any evidence for it.

Given this discussion, it is surprising that there is almost a complete absence of any study directly examining the impact of minimum wages on firm profitability. This is the focus of this paper. We adopt an identification strategy using variations in wages induced by the introduction of the national minimum wage (NMW) in the United Kingdom as a quasi-experiment to examine the impact of wage floors on firm profitability. The introduction occurred in 1999 after the election of the Labor government that ended 18 years of the Conservative administration. To date 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.

Our work does uncover a significant negative association between the NMW introduction and firm profitability. We report evidence showing wages were significantly raised, and firm profitability was significantly reduced by the minimum wage. There is also some evidence of bigger falls in margins in industries with relatively high market power, but no significant effects on employment or productivity in any sector. Our findings can be interpreted as consistent with a simple, no behavioral response model, where wage gains from minimum wages map into profit reductions. There is a hint of a selection effect in the longer run as net entry rates fall in the most affected industries, but although the magnitude of the effect is nontrivial, it is statistically insignificant.

The rest of the paper is structured as follows. In Section I, we discuss a model of profit responsiveness to wage changes from which we derive our empirical strategy. Section II discusses the data and the characterisation of firms more likely to be affected by the minimum wage introduction. Section III gives the main results on wage and profitability effects and tests their robustness. Section IV offers some further investigations using other datasets (care homes), other outcomes, and sectoral heterogeneity. Section V concludes.

[4] This was the conclusion of the survey on minimum wages and prices by Sara Lemos (2008). For exceptions on restaurant prices see Daniel Aaronson (2001); Aaronson and Eric French (2007); and Denis Fougere, Erwan Gautier, and Herve le Bihan (2008). The only United Kingdom evidence to our knowledge is Jonathan Wadsworth (2010) who finds limited effects on prices.

[5] See Machin, Alan Manning, and Lupin Rahman (2003) and Mark B. Stewart (2004).

I. Motivation and Modelling Strategy

A. *The Scope for Minimum Wages to Impact on Profitability*

Following Orley Ashenfelter and Robert S. Smith (1979), consider a profit-maximizing firm employing a quantity of labor (L) at wage rate (W), using other factors at price R, and selling its output at price P. Profits are maximized at $\Pi(W,R,P)$given the values of W, R, and P. The derivative of the profit function with respect to the wage rate is $\partial\Pi/\partial W = -L(W,R,P)$, the negative of the demand for labor. In turn, the second derivative is $\partial^2\Pi/\partial W^2 = -\partial L/\partial W$.

In this setting, the introduction of a minimum wage (M) at a level above that of the prevailing wage reduces firm profits by $\Delta\Pi = \Pi(W,R,P) - \Pi(M,R,P)$. Using a second-order Taylor series this can be approximated as

$$\Delta\Pi \cong -L\Delta W + \frac{1}{2}\frac{\partial L}{\partial W}(\Delta W)^2, \tag{1}$$

where $\Delta W = M - W$. The terms on the right-hand side of equation (1) correspond to the "wage bill" ($-L\Delta W$) and "labor demand" ($\frac{1}{2}(\partial L/\partial W)(\Delta W)^2$) effects on profits. Note that equation (1) can be rewritten as

$$\Delta\Pi \cong -WL\left(\frac{\Delta W}{W} + \frac{\eta}{2}\left(\frac{\Delta W}{W}\right)^2\right), \tag{2}$$

where $\eta = (W/L)(\partial L/\partial W) < 0$.

In a situation of "no behavioral response," that is no impact on labor demand, the second order effect in (2), $\left((\eta/2)(\Delta W/W)^2\right)$, is zero, and the fall of profits that would result from the imposition of a minimum wage M is equal to the proportionate change in the wage multiplied by the wage bill. In the case of a labor demand effect, the second term can offset this profit loss to the extent that firms can substitute away from low-wage workers into other factors (e.g., capital).

Equation (2) also serves to illustrate the inverse relationship between a firm's initial wage and the post-policy change in its profits. It shows that the lower the initial wage, the greater the fall in profits associated with the imposition of a minimum wage. The difference-in-difference models we consider in our empirical modelling strategy (described below) will 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.

Normalizing profits on sales revenues, S, to define a profit margin shows that, for the no behavioral response model, in a statistical regression context, the coefficient on the increase in wages caused by the minimum wage ($\Delta W/W$) should simply be equal to the share of the wage bill in total revenue (WL/S):

$$\Delta(\Pi/S) = -\theta\left(\frac{\Delta W}{W}\right), \tag{3}$$

where $\theta = (WL/S)$.

More generally, to the extent there is substitution away from labor, the coefficient on the wage increase, θ, will be less (in absolute terms) than the (initial) wage bill share of revenue. Interestingly, we will show that our empirical results cannot generally reject the simple relationship in equation (3).

It is worth noting that this is consistent with the results in the rather different context of John Abowd's (1989) study of union wage increases and firm performance. Abowd (1989) estimates a version of equation (2) examining the effects of unanticipated increases in the wage bill ("union wealth") on the present discounted value of profits as reflected in changes in stock market values ("shareholder wealth"). He also finds that he cannot reject the simple model where the second order effect is zero. Abowd (1989) interprets this as evidence for strongly efficient union bargains as he focuses on a sample of unionized contracts. Strongly efficient (implicit) bargaining is also an alternative interpretation of our findings as well.[6]

It is worth focusing on some of the economic issues underlying the adjustment mechanisms implicit in the second order term of equation (1). Obviously, the magnitude of these mechanisms depend on the elasticity of the labor demand curve, η. One element of this will be the degree to which labor is substitutable for other factors. Another will be the degree to which the higher wage costs can be passed on to consumers in the form of higher prices. For example, under perfect competition price equals marginal cost, so all the wage costs are reflected in higher prices for consumers. In most oligopoly models, by contrast, mark-ups will fall as some of the wage increase is born by firms (see online Appendix A). Consequently, in our empirical work, we explicitly distinguish between industries with different degrees of product market competition as we expect heterogeneity in the minimum wage effects along this dimension (i.e., a larger effect in the less competitive industries).

The model focuses on the short-run responses of incumbent companies, rather than the long-run equilibrium when the number of firms varies.[7] We believe that the short run is still 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). Since firms that employ low-wage workers may well exit the market, the relevant margin of adjustment will be more exit and less entry. We also examine this explicitly in our empirical analysis.

Finally, 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 in Draca, Machin, and Van Reenen (2008) 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

[6] Although we find this explanation less plausible as the minimum wage mainly binds on those firms and sectors where unions are not present or, if they are, are very weak.

[7] Note that the short-run negative impact on profits will be larger in competitive labor markets than monopsonistic labor markets (see Card and Krueger 1995). In the latter model, there is an offsetting positive effect on profitability when wages increase as worker turnover declines.

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, the firm will remain in the market with lower profitability.

B. *Modelling Strategy*

The approach we take to identify minimum wage effects in the context of the above theoretical discussion is in line with the existing literature that analyzes the impact of national minimum wages. Typically, we look at a group of firms that were more affected by the NMW introduction than a comparison set of firms.[8] By "more affected," we mean those firms where wages are likely to increase due to the imposition of the minimum wage. 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 where wages were not affected as 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 treatment indicator of the minimum wage policy for a set of low-wage firms with a pre-policy introduction wage, W^{pre}, beneath the minimum wage threshold M. A treatment indicator variable can be defined as $T = 1$ for below minimum wage firms (where $W^{pre} < M$), and $T = 0$ for a set of firms whose pre-policy wage exceeds the threshold.[9]

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 $\left(\overline{w}^{T=1}_{NMW=1} - \overline{w}^{T=1}_{NMW=0}\right) - \left(\overline{w}^{T=0}_{NMW=1} - \overline{w}^{T=0}_{NMW=0}\right)$, 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 difference-in-difference estimate 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:

$$w_{it} = \alpha_1 + \beta_1 X_{\mathrm{it}} + \delta_1 Y_{\mathrm{t}} + \theta_1 I(w_{i,t-1} < mw_t) + \psi_1[I(w_{i,t-1} < mw_t)\, NMW_t] + \varepsilon_{1it}, \tag{4}$$

[8] See, among 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 United Kingdom NMW.

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

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 absorbed into the time dummies); and ε_{1it} is a random error. Here, the regression corrected difference-in-difference estimate of the impact of 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 wages in the expected manner, we move on to consider whether profitability was affected differentially between the treatment group firms ($T = 1$) and comparison group firms ($T = 0$). We look 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 $\left[\left(\overline{\Pi/S}\right)_{NMW=1}^{T=1} - \left(\overline{\Pi/S}\right)_{NMW=0}^{T=1}\right] - \left[\left(\overline{\Pi/S}\right)_{NMW=1}^{T=0} - \left(\overline{\Pi/S}\right)_{NMW=0}^{T=0}\right]$, and the conditional difference-in-difference, ψ_2, from the regression model

$$\left(\frac{\Pi}{S}\right)_{it} = \alpha_2 + \beta_2 Z_{it} + \delta_2 Y_t + \theta_2 I(w_{i,t-1} < mw_t) + \psi_2[I(w_{i,t-1} < mw_t)\, NMW_t] + \varepsilon_{2it}, \tag{5}$$

where the controls are now Z, and ε_{2it} is the error term.

If we compare the econometric models (4) and (5) to the economic models of (1)–(3), we see immediately that the no behavioral response model corresponds to a restriction on the coefficients in equations (4) and (5), i.e.

$$\psi_2 = -\theta\psi_1. \tag{6}$$

We present formal tests of this restriction in the empirical section.

The main issue that arises with any nonexperimental 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 Richard 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 (or falsification tests) in the pre-policy period.

II. Data

A. *Basic Description of FAME Data*

Accounting regulations in the United Kingdom 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 United States do not have to give total employment and wage bills, whereas this is required in the United Kingdom.[10] Accounting information on UK companies is

[10] 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.

stored centrally in Companies House. It is organized into electronic databases and sold commercially by private sector data providers such as Bureau Van Dijk (BVD) from whom we obtained the FAME (Financial Analysis Made Easy) database.[11]

The great advantage of this data is that it covers a much wider range of companies than is standard in firm level analyses and, in particular, it includes firms not listed on the stock market. 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 nonmanufacturing firms where many low-wage workers are employed. By contrast, plant level databases in the United Kingdom and United States typically cover only the manufacturing sector[12] 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 subsample that do report the required information.[13]

Since FAME contains annual accounting information, we have firms reporting accounts with different year-end dates. Since the NMW was introduced on April 1, 1999, we therefore consider the subset of firms that report their end of year accounts on March 31 of each year (these are firms who report in the UK financial year). The accounting period for these firms will match exactly the period for which the NMW was in force. Around 21 percent of firms in FAME that have the accounting data we require report on this day, which corresponds to the end of the tax year in the United Kingdom.[14]

We use data on profits before interest, tax, and depreciation from the FAME database and model profitability in terms of the 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.[15] To allow for capital intensity differences, we also control for firm-specific capital to sales ratio.[16]

B. *Other Data*

We have also matched in industry-level variables aggregated up from the Labor Force Survey (similar to the US CPS). These are used as control variables in the

[11] FAME is the United Kindom's part of BVD's AMADEUS dataset of European company accounts used by many authors (e.g., Nicholas Bloom and Van Reenen 2007).

[12] The Annual Business Inquiry (ABI) database does cover nonproduction sectors, but this database is not available until the late 1990s. The US Longitudinal Research Database (LRD) only covers manufacturing.

[13] These firms will tend to be larger than average as the very smallest firms have the least stringent reporting requirements.

[14] If we estimated our basic models on the whole FAME sample irrespective of reporting month, we obtained very much the same pattern of results as our basic findings in Table 2. The estimated effects were a little smaller in magnitude, most likely because of attenuation toward zero owing to measurement error in defining treatment.

[15] For example, see Machin and Van Reenen (1993) and Margaret E. Slade (2004). Although there are many reasons why accounting and economic profits may diverge (Franklin M. Fisher and John J. 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, and that is what we do empirically in the regression-corrected difference-in-difference estimates.

[16] 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 labor toward capital equipment.

analysis and include (at the three-digit industry level) the proportion of part-time workers, female workers, and 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 region, two-digit industry and time dummies. Exact variable definitions are given in the Data Appendix. Online Appendix Table B1 shows the characteristics of the treatment and comparison groups for each model.[17]

Finally, the magnitude of the minimum wage increases over our "Policy on" period should be clarified. This period lasts from April 1, 1999 until March 31, 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 10 pence 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 percent increase in the minimum between 1999 and 2002.[18] Small cell sizes prevent us from estimating separate models for the 2000 and 2001 upratings.[19]

C. *Defining Treatment and Comparison Groups*

FAME has a total remuneration figure that can be divided by the total number of employees to calculate an average wage.[20] This creates a challenge in terms of defining our treatment and comparison groups since any given level of average wages is, in principle, compatible with a range of different within-firm wage distributions. This makes it hard to measure accurately how exposed each firm's cost structures are to the wage shock brought about by the minimum wage. Any continuous measure of treatment intensity based on the firm average wage is inevitably coarse.

We have used information from FAME, the Labor Force Survey (LFS) and the British Workplace Employment Relations Survey (WERS) to both construct and validate our treatment group indicators. Specifically, the main results use average firm wages from FAME to define our treatment and comparison groups, but we also use LFS information for the industry-level analysis of entry and exit. We 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.[21]

To investigate the impact of the minimum wage we have defined our treatment group, T, based upon average remuneration information from FAME. For our initial

[17] 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.

[18] By contrast, the consumer price index grew by 6.3 percent over the same period.

[19] For example, less than 9 percent of firms report annually on September 30 (i.e., the 12 months immediately before the October upratings).

[20] In almost all firms in the data we use, employment refers to average employment over the accounting period. Firms can report employment at the accounting year or the average over the year, but the overwhelming number of our firms report averaged employment.

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

analysis, we 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").[22] Average remuneration in the treatment group for this threshold is £8,400 which, after allowing for a deduction for nonwage 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 in our analysis. One aspect of this is that we have extensively experimented with the threshold cut-off, and we discuss this in detail below. 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.

D. *The Usefulness of Average Wages to Define Treatment*

How accurate are these treatment group definitions at identifying firms most affected by the minimum wage regulation? This hinges on how segregated low-wage workers are between firms. Our threshold-based definition will be more effective if subminimum 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 WERS.[23] 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 1,783 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 percent of all minimum wage workers. These patterns give some support to our idea that the "at risk" group of minimum wage workers is concentrated in firms that pay low average wages.

[22] In earlier versions of this paper, we also combined 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 into being. Using LFS data, we defined a low-wage industry-region cell if more than 10 percent of workers in the given firm's two-digit industry by region cell in the pre-policy period are paid below the minimum wage. In practice this made little difference to the overall pattern of results, and so we do not report this material (see Draca, Machin, and Van Reenen 2008 for all the results).

[23] 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 relations experts to study a range of issues. Mark Culley et al. (1999) give details of the survey.

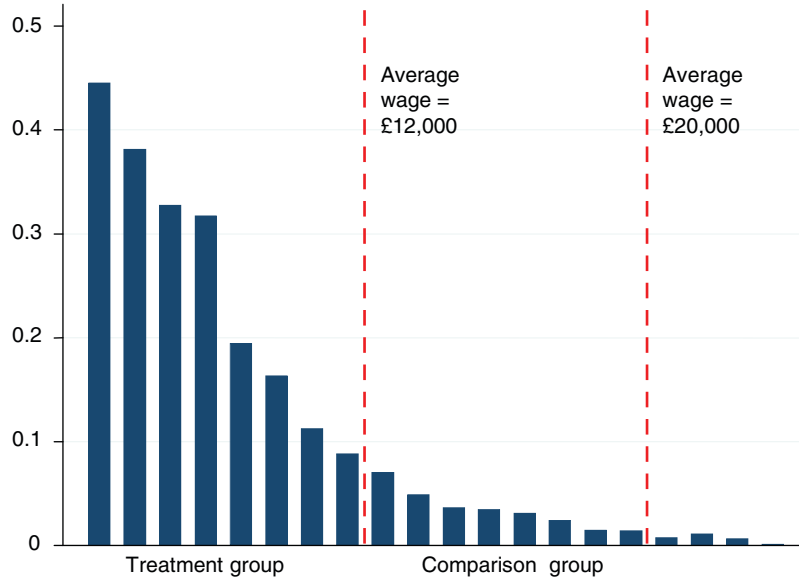

Figure 1. Validation of Average Wage Data
(*Comparison of proportion of low-wage workers and establishment average wages, WERS 1998*)

Notes: 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 5 percentiles from lowest (left) to highest (right)—a total of 20 bins up to an annualized wage of £24,000. 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.

Source: These figures are derived from the worker-establishment data (26,509 workers in 1,783 workplaces) from the 1998 Workplace Employee Relations Survey (WERS).

III. Main Results

A. *Changes in Wages Before and After the Introduction of the National Minimum Wage*

It is important to see whether we are able to observe a clear change or "twist" in the firm average wage distribution as the minimum wage was introduced. To consider this, we started our analysis by calculating the change in average wages in the year immediately before and immediately after NMW introduction for every firm at each percentile of the pre-policy firm wage distribution. If the firms in the FAME data exhibit some of the low pay patterns outlined above for WERS, 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 very clearly confirm this hypothesis. In the post-NMW introduction year from April 1, 1999 to March 31, 2000 (labeled "1999–2000 change," and denoted by the solid line), the wage change tapers off steadily beyond

FIGURE 2. CHANGE IN LN(AVERAGE WAGE) BY PERCENTILE IN THE FINANCIAL YEAR BEFORE AND AFTER NMW INTRODUCTION

Notes: 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 31, 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 1, 1998–March 31, 1999. Policy on is defined as the financial year April 1, 1999–March 31, 2000. We show the threshold for the treatment groups by hatched vertical lines. In the baseline specifications firms with average wages below £12,000 (the thirteenth percentile) are in the treatment group and firms with average wages between £20,000 (the median) and £12,000 are in the control group.

Source: The data is taken from the FAME database of company accounts.

the lowest decile of the firm average wage distribution. After the thirteenth percentile, firms appear to have had a similar increase in nominal wages of around 5.6 percent. Importantly, there is no evidence of much faster wage growth for the bottom decile in the pre-policy year (labeled "1998–1999 change," and denoted by the dotted line). In fact, wage growth in the bottom thirteen percentiles was on average 2.6 percent in the 1998–1999 financial year compared to 9.9 percent in the following year. A spike is seen for the bottom few percentiles of the wage distribution in both years, which is consistent with the notion of some transitory measurement error at the low end of the wage distribution generating mean reversion in both 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.

It is critical 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 treatment 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 quite different in terms of

Table 1—Changes in Firm Average Wages and Profitability Before and After the Introduction of the National Minimum Wage

	Pre-NMW introduction (1)	Post-NMW introduction (2)	Difference (3)
Panel A. ln(average wage), lnW			
Pre-NMW low-wage firm, $T = 1$	2.149	2.378	0.229
Pre-NMW not low-wage firm, $T = 0$	2.775	2.893	0.118
Difference-in-difference			0.111***
			(0.029)
Panel B. Π/S			
Pre-NMW low-wage firm, $T = 1$	0.128	0.089	−0.039
Pre-NMW not low-wage firm, $T = 0$	0.070	0.058	−0.012
Difference-in-difference			−0.027**
			(0.014)

Notes: Pre-NMW corresponds to the three financial years April 1, 1996–March 31, 1999 and Post-NMW refers to the three financial years April 1, 1999–March 31, 2002. $T = 1$ indicates the treatment group and $T = 0$ indicates the comparison group. Pre-NMW 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 31, 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 31, 1999. Standard errors in parentheses are clustered by firm and sample size is 4,112 (there are 951 firms).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

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.

B. *Firm-Level Estimates: Wages and Profitability*

The upper panel of Table 1 presents unconditional difference-in-differences in the mean $\ln(wage)$ for the discrete categorization of treatment and comparison groups, for the three years before and after NMW introduction.[24] It is evident that wages rose significantly faster among the low-wage firms when the minimum wage became operational. Wage growth across the pre- and post-NMW three year time period was higher at 22.9 log points in the lower initial wage group $(T = 1)$ as compared to wage growth of 11.8 log points in the higher initial wage group $(T = 0)$. The difference-in-difference of 11 percentage points is strongly significant in statistical terms. This is consistent with the hypothesis that the NMW significantly increased wages for low-wage firms.[25]

[24] 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 afterward). In Figure 2, we simply looked one year before and after the policy introduction.

[25] As we saw in Figure 1, in 1998 (the year prior to the introduction of the NMW in 1999), on average, 25 percent of workers in the treatment group were at or below the minimum wage compared to 3 percent in the comparison group. Based upon this 22 percentage point difference, our coefficients would have to be scaled up by a factor of 4.5 if we considered the more radical experiment of switching a firm from having *none* of its workers covered to having *all* of its workers covered by the minimum wage.

Table 2—Wages and Profitability Before and After Introduction of the National Minimum Wage (NMW), 1997–2002

	Period before and after NMW introduction, 1997–2002 ($N = 4{,}112$)	
	Change in ln(*average wage*), $\Delta \ln W$	Change in gross profit margin, $\Delta(\Pi/S)$
Panel A. Treatment = low-wage firm		
Pre-NMW low wage firm	0.090***	−0.029**
	(0.026)	(0.012)
Test of no behavioral response	p-value = 0.663	
Panel B. Treatment = –pre-policy ln(W)		
− Pre-NMW ln(W)	0.188***	−0.032**
	(0.033)	(0.015)
Test of no behavioral response	p-value = 0.144	

Notes: Coefficients estimated by ordinary least squares and standard errors in parentheses below are clustered by firm (there are 951 firms). The pre-NMW period covers the three pre-policy financial years April 1, 1996–March 31, 1999, and the post-NMW period covers the three financial years April 1, 1999–March 31, 2002. Low-wage firm pre-NMW—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 31, 1999. The comparison group is defined as firms with average wages between £12,000 and £20,000. Pre-NMW ln(W)–indicates that a continuous measure of the wage (in the pre-policy year up to March 31, 1999) is used for treatment intensity. Controls include two-digit industry dummies; 18 regional dummies; 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). "Test of no behavioral response" implements equation (3) in the text.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

An analogous set of descriptive results is presented for firm profitability in panel B of Table 1. It is clear that, while profit margins fell by 0.039 between the pre- and post-NMW periods in the pre-NMW low-wage firms, they only fell by 0.012 in the pre-NMW higher wage firms. Thus, there is a negative difference-in-difference of −0.027. This difference is statistically significant and is preliminary evidence that profit margins were squeezed in firms that were "at risk" from the introduction of the minimum wage.

Comparing these results with the simple models in Section I, we find that no behavioral response model does surprisingly well. Using the average wage bill to sales ratio of 0.27 (see Online Appendix Table B1), the implied change of profit margins using the estimated wage gains in Table 1 and equation (3) is −0.030 ($= -0.111 \times 0.27$). This is only slightly above the empirically estimated profitability reduction of −0.027 in Table 1, suggesting only minor offsetting adjustments (the second-order term in equation (2)). Below, we will see that this conclusion broadly holds up to more rigorous econometric testing.

Table 2 reports results from statistical difference-in-difference wage and profitability regressions that additionally control for firm and industry characteristics. The upper panel A shows results for the binary low-wage firm indicator, while the lower panel B uses a continuous measure, the negative of the pre-policy average wage (we report the negative in order to have signs on coefficients that are consistently defined

with the low-wage dummy). The basic pattern of results from the unconditional models of Table 1 are confirmed in these conditional specifications. For the binary indicator in the upper panel, the estimated effects show a 9.0 percentage point in wages and a 0.029 fall in profit margins (similar to Table 1). The same pattern of results is observed for the (negative of the) continuous pre-NMW wage, reported in panel B. There is a significant positive connection between wage growth and the negative of the pre-NMW wage, and a significant negative association with profitability. When compared to average profits in the low-wage firms in the pre-policy period, the results for the binary low-wage firm model imply a sizable 22.7 percent $(-0.029/0.128)$ fall in profit margins. The p-values from F-tests of the no behavioral response model are at the bottom of each panel and, again, indicate that we cannot reject the simple model underlying equation (3).

C. *Further Probing of the Baseline Results*

There are many reasons to probe these baseline results more deeply. 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 function of the average wage in the firm. This is less than ideal, even though we have (at least partially) validated its use above 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.

We therefore re-estimated the models in Table 2 for a range of different wage thresholds, running from an average wage of £10,000 at £1,000 intervals up to £15,000. The results are reassuring in that they all establish a significant NMW effect of reducing profit margins, with magnitude of the impact varying and becoming slightly larger (in absolute terms) for lower thresholds as we would expect (so there is a bigger impact on the very low-wage firms).[26]

A second possible concern is that 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. One such "placebo experiment" is reported in Table 3, where we examine an imaginary introduction of the NMW on April 1, 1996 (instead of April 1999) and repeat our analysis of wage and profitability changes. Table 3 very much reinforces the results, 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.

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 lower than average profitability growth even in the absence of the policy this would be conflated with the

[26] The profitability impacts for the different $T = 1$ thresholds were: -0.029 (0.014) for £10,000; -0.027 (0.013) for £11,000; -0.029 (0.012) for £12,000; -0.024 (0.010) for £13,000; and -0.014 (0.009) for £14,000.

Table 3—Wages and Profitability before and after Introduction of a Placebo National Minimum Wage (NMW), 1993–1999

	Period before and after "imaginary NMW" introduction, 1993–1999, ($N = 4{,}550$)	
	Change in ln(*average wage*), $\Delta \ln W$	Change in gross profit margin, $\Delta(\Pi/S)$
Panel A. Treatment = low-wage firm		
Pre-"imaginary NMW" low-wage firm	0.033	0.015
	(0.028)	(0.011)
Panel B. Treatment = −pre-policy ln(W)		
−Pre-"imaginary NMW" ln(W)	0.079	0.012
	(0.106)	(0.029)

Notes: Coefficients estimated by ordinary least squares and standard errors in parentheses below are clustered by firm (there are 1,047 firms). The pre-"imaginary NMW" period covers the three financial years April 1, 1993–March 31, 1996 and the post-"imaginary NMW" period covers the three financial years April 1, 1996–March 31, 1999. Low-wage firm pre-"imaginary NMW" 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 31, 1996. The comparison group is defined as firms with average wages between £12,000 and £20,000. Pre-"imaginary NMW" ln(W)–indicates that we use a continuous measure of the wage (in the Pre-"Imaginary NMW" year up to March 31, 1996) is used for treatment intensity. Controls include two-digit industry dummies; 18 regional dummies, 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).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

causal effect of the NMW impact on profits. The evidence from Table 3 suggested that there is no trend for wages or profitability in the pre-policy period. Nevertheless, we investigated this issue in more detail by estimating the profits model of Table 2 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 policy-on period there is a consistently negative effect of around 2–3 percent no matter how we draw the exact profit threshold. By contrast, in the pre-policy period, there is essentially a zero effect with the point estimates actually positive and around 1 percent.

Draca, Machin, and Van Reenen (2008) report a number of further robustness tests. First, a statistical matching technique by trimming the sample according to the propensity scores of the treatment and comparison groups did not affect the pattern of results.[27] 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 is significant and similar

[27] The basic method used is that of James J. Heckman, Hidehiko Ichimura, and Petra E. Todd (1997), where propensity scores are estimated and the sample is 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 2.

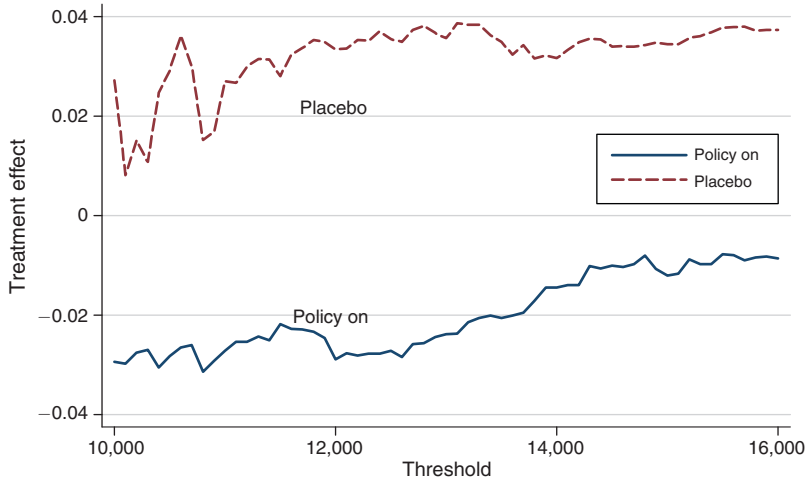

FIGURE 3. VARYING TREATMENT EFFECT COEFFICIENTS IN FAME DIFFERENCE-IN-DIFFERENCE PROFITABILITY MODELS

Notes: The baseline models are as per pre-NMW low-wage model in Table 2 (policy on period) and Table 3 (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 1, 1996 to March 31, 2002, NMW introduction on April 1, 1999. The pre-policy (pseudo-experiment) period covers the six financial years from April 1, 1993 to March 31, 1999, with an "imaginary" NMW introduction on April 1, 1996.

Source: Data taken from the FAME database of company accounts.

to those in the baseline low-wage firm specification.[28] Second, we included a full set of three-digit industry time trends. Although this is a strong test, the profitability effect was almost identical when these industry time trends were included with an estimate of −0.032 (0.015).

IV. Further Investigation of the Minimum Wage Effect

The baseline results of Section III show very clearly that low-wage firms in the FAME data experienced faster wage growth coupled with falling profit margins before and after the introduction of the UK NMW. The results also seem consistent with the no behavioral response theoretical model introduced in Section II. The model has a number of other salient features that we explore more fully in this section, in an attempt to understand the effect of minimum wages on firm profitability and mechanisms that underpin the negative effect our baseline results have uncovered.

[28] Few observations are lost under propensity score matching because the comparison group is already chosen to be of relatively low-wage firms (under £20,000 average annual wages). If we had used the entire FAME sample (including firms with average wages of over £20,000), we would have had to lose the vast majority of the sample to ensure that the comparison group had common support with the treatment group.

A. *Minimum Wages and Profitability in UK Residential Care Homes*

Here, we look at the wage and profitability effects of the minimum wage in a rather different context, UK residential care homes.[29] There are three reasons to focus on care homes to juxtapose with the FAME results. First, it is a very low-wage sector, so it offers a good testing ground for studying minimum wage effects on profitability and other economic outcomes.[30] Second, the sector is price regulated so one of the margins of adjustment (passing on higher wage costs in higher prices) is constrained. Finally, we have individual level data, so we can observe the entire within-firm wage distribution in this exercise, something we could not do in the FAME dataset.

The more sophisticated definition of treatment we are able to use is the initial firm wage gap relative to the minimum, namely the proportional increase in a firm's wage bill required to bring all of its workers up to the minimum wage. This variable, *GAP*, is defined as

$$GAP_i = \frac{\sum_j h_{ji} \max(W_{ji}^{min} - W_{ji})}{\sum_j h_{ji} W_{ji}}, \tag{7}$$

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.

For care homes, we do not have accounting data, and so the profit variable we study is a derived one based on total revenues less total costs. Total revenue of each home is measured directly as the product of the number of beds, the home-specific average price of beds, and the home occupancy rate. Total costs are calculated by dividing the total firm wage bill by the share of labor in total costs.[31] Home profitability is then defined as the ratio of profits to revenue.

We therefore estimate the following care homes specification

$$\Delta\left(\frac{\Pi}{S}\right)_{it} = \eta_0 + \eta_1 GAP_{i,t-1} + \eta_2 Z_{i,t-1} + \xi_{it}, \tag{8}$$

where ξ_{it} is the equation error. Under the no behavioral response model, the coefficient on *GAP* (η_1) should be equal to the wage bill share of revenues.

Table 4 presents estimates of home-level wage change and profitability change equations for the period surrounding NMW introduction (1998–1999). Panel A

[29] To date these data have mostly been used for studies of minimum wage effects on wages and jobs (e.g., Machin, Manning, and Rahman 2003), but see also Machin and Manning's (2004) test of competitive labor market theory.

[30] 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 percent of workers were paid below the incoming minimum wage with this figure falling to 0.4 percent after the introduction of the policy.

[31] 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, $(S - TC)/S$.

Table 4—National Minimum Wage Introduction and Wages and Profitability in Care Homes, 1998–1999

	Period before and after NMW introduction, 1998–1999	
Panel A. Wages	Change in ln(*average wage*), $\Delta \ln W$	
Pre-NMW wage gap	0.861***	0.886***
	(0.045)	(0.052)
Controls	No	Yes
Panel B. Profitability	$\Delta(\Pi/S)$, Change in profit margin	
Pre-NMW wage gap	−0.433***	−0.492***
	(0.173)	(0.202)
Controls	No	Yes

Notes: Coefficients estimated by ordinary least squares. Robust standard errors are in parentheses under coefficients. Sample covers 454 nursing homes in 1998 and 1999. 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.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

focuses on wages, and presents results showing that wages clearly rose by more in homes with a larger pre-NMW wage gap. Panel B shows profitability estimates, where the coefficient on the pre-NMW wage gap variable is estimated to be negative and significant. In the column 2 specification with controls, the coefficient is −0.492. Thus, there is clear evidence of profitability falls in homes that were more affected by the minimum wage introduction. This very much corroborates the FAME findings of the previous section.

There was also some evidence that wages rose more in the pre-policy period (1992−1993) in homes with a bigger initial wage gap.[32] Nevertheless, the relationship is much weaker in the earlier period, so the trend-adjusted estimate is statistically significant and large in magnitude (at 0.678). Under the no behavioral response model, the coefficient on the initial wage gap measure should equal the share of the wage bill in sales. The (trend adjusted) point estimate on the wage gap term in the profitability equation turns out to be −0.396 for the model with controls (and −0.343 for the no controls specification), which in absolute terms is very close to the wage bill to sales ratio in our sample of care homes (0.398). Hence, like the FAME results the magnitude of the estimated impact in care homes is very much in line with what we would expect from the simple no behavioral response model.

B. *Sectoral Heterogeneity: Industries with High and Low Market Power*

As noted in Section I, 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 in Table 5, we split industries into "high-" and

[32] We define a counterfactual minimum wage at the same percentile of the wage distribution as the real 1999 minimum, so we can compute a GAP measure for the earlier pre-policy time period. Note that this is the only previous wage change information that exists, as the data was not collected in other (nonelection) years.

Table 5—Splitting into High- and Low-Market Power Industries

Outcome	High-market power industries	Low-market power industries
Panel A. Wages		
Treatment = low-wage firm	0.109***	0.081**
$N = 1{,}943$ (high); $N = 2{,}169$ (low)	(0.035)	(0.038)
Panel B. Profits		
Treatment = low-wage firm	−0.037**	−0.014
$N = 1{,}943$ (high); $N = 2{,}169$ (low)	(0.018)	(0.014)
Test of no behavioral response	p-value = 0.646	p-value = 0.531
Panel C. Employment		
Treatment = low-wage firm	0.104	−0.012
$N = 1{,}943$ (high); $N = 2{,}169$ (low)	(0.142)	(0.121)
Panel D. Labor productivity		
Treatment = low-wage firm	0.075	0.113
$N = 1{,}943$ (high); $N = 2{,}169$ (low)	(0.066)	(0.090)
Panel E. Exit		
Treatment = low-wage firm	−0.023	−0.002
$N = 1{,}150$ (high); $N = 1{,}206$ (low)	(0.023)	(0.027)

Notes: This table shows the results from a series of separate regressions for the low-wage firm models (Column 1 of Table 2, panel A). The dependent variable is indicated in the first row, column 1 is on the sub-sample of firms in high-market power industries, and column 2 is the sub-sample of firms in the low market power industries. High-market power industries are defined as those with higher than the median value of the industry-level Lerner Index in the firm's three-digit industry. Low-market power industries are defined as those with below the median value of the industry-level Lerner Index in the firm's three-digit industry. Coefficients estimated by ordinary least squares and standard errors in parentheses below are clustered by firm. Employment is the ln(total number of workers in the firm). Labor productivity is ln(sales/employment). "Exit" is defined for two cohorts in 1996 (pre-NMW) and 1999 post-NMW and indicates whether the firm ceased to exist in the subsequent three years (see text). Controls include two-digit industry dummies; 18 regional dummies, 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).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

"low-" competition industries based on a proxy for the Lerner Index (constructed as in Philippe Aghion et al. 2005). Consistent with the idea of imperfect competition, the effects of the NMW policy on profitability were stronger in the less competitive sectors (defined as those with above the median value of three-digit industry Lerner index). Table 5 shows that the impact of the policy on wages was not so different (10.9 percent versus 8.1 percent). By contrast, the effect of the minimum wage on profitability was almost two-and-a-half times as large in the less competitive industries as in the more competitive sectors (as well as being significant only in the less competitive sectors).

Under perfect competition, an industry facing a common increase in marginal costs will pass on the higher wage costs in the form of higher prices to consumers. In less competitive sectors, however, firms will generally adjust by reducing their profit margins, rather than just through prices. Therefore, the evidence in Table 5 is consistent with the idea that the strongest effects of the NMW on profitability will be in the less competitive sectors.

Table 6—Firm Entry and Exit (*by three-digit industry*)

	Period before and after NMW introduction, 1996–2001, ($N = 1{,}020$)	Period before and after "imaginary NMW" introduction, 1994–98, ($N = 850$)	Difference
Panel A. Change in industry entry rates			
Pre-NMW low pay proportion	0.021	0.057*	−0.036
	(0.015)	(0.032)	(0.038)
Panel B. Change in industry exit rates			
Pre-NMW low pay proportion	−0.013	−0.028	0.015
	(0.016)	(0.018)	(0.024)
Panel C. Change in industry net entry rates			
Pre-NMW low pay proportion	0.034	0.085**	−0.051
	(0.025)	(0.027)	(0.037)

Notes: 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. Pre-NMW low pay proportion is 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). All specifications include controls for two digit industry dummies, time dummies, and the proportion of employees in the three-digit industry that are female, part time, and the proportion of employees in the three-digit industry that are female, part time, and unionized.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Data taken from value-added tax (VAT) Registrations and Deregistration Data, Department of Trade and Industry (DTI).

C. *Effects of Minimum Wage on Other Outcomes: Employment, Productivity, Exit, and Entry*

We also examined the effect of the NMW policy on other firm outcomes in the lower part of Table 5, again split by high and low market power sectors. We do not find any significant negative effects on employment, consistent with some of the minimum wage literature (e.g., Card and Krueger 1994). The presence of no significant employment effect is also consistent with our tests of the no behavioral response model. Similarly, there does not appear to be any effect of the policy introduction on labor productivity (as predicted by the "shock" theory).

The FAME database identifies four categories of inactive firms, namely firms that are dissolved, liquidated, in receivership, or currently nontrading.[33] Hence, we have defined all firms in these categories as "exiting" firms. We examine three year death rates for a cohort alive April 1, 1999 (i.e., did they exit by March 31, 2002) compared to a cohort alive on April 1, 1996 (i.e., did they exit by 1999). In the final row of Table 5, there is no evidence of any faster increase in exit rates in initially low-wage firms following the minimum wage introduction either in the whole sample or

[33] 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. Redefining the dependent variable to be unity if the exit is to a takeover does not change the qualitative nature of the results.

in subsectors. The same is true in models of the probability of closure of care homes (see Machin and Joan Wilson 2004).

There are two possible problems with this firm-level analysis of exit. First, we ignore the possible entry-deterring effect of the minimum wage, and second, there may be pre-policy trends.[34] Table 6 takes both of these into account. Obviously, we cannot 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 Department of Trade and Industry's VAT Registration Database).[35]

The three panels of Table 6 show one-year entry rates, one-year exit rates, and the difference between the two ("net entry") three-digit industries. Column 1 shows estimated coefficients on a pre-NMW low-pay proportion in the period surrounding NMW introduction. Column 2 does the equivalent experiment for an imaginary/placebo policy (as in Table 3) introduced in 1996, and column 3 presents the trend-adjusted difference-in-differences. Although the first row shows that entry rates appear to perversely increase for low-wage firms after the minimum wage, there does appear to be some positive pre-policy trend in column 2, suggesting a negative trend-adjusted effect of the NMW policy on entry. Similarly, trend-adjusted exit rates in panel B are 1.5 percentage points higher after the minimum wage was introduced. The final row shows that trend-adjusted net entry rates had fallen by about 5.1 percentage points in the low-wage industries after the NMW introduction. This effect is large in magnitude, but not statistically significant. These results do hint that in the long run a margin of adjustment may be in the dimension of lower rates of net entry into the sectors most affected by the NMW.[36] There is little within firm change, but the margin of adjustment may be through the long-run number of firms.

V. Conclusions

This paper considers a very under-studied research question on the economic impact of minimum wages by looking at empirical connections between minimum wage legislation and firm profitability. Using the quasi-experiment of the introduction of a national minimum wage to the UK labor market in 1999, we utilize 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 wages were significantly raised, and firm profitability was significantly reduced by the minimum wage introduction. There is also some evidence of bigger falls in margins in industries with relatively high market power, but no effects on firm employment or productivity. Somewhat surprisingly, our findings are consistent with a simple "no behavioral

[34] Running the pseudo-policy experiment of Table 3 gave a coefficient on the policy variable of 0.021 with a standard error of 0.106 for employment and 0.077 with a standard error of (0.053) for productivity.

[35] Unlike the firm data, we cannot distinguish between exit due to takeover and exit due to bankruptcy. Online Appendix Table B2 describes some key features of these data.

[36] Our further investigations indicated that there were minimal differences in entry and exit rates between high- and low-market power industries. For example, when split by market power, the corresponding estimates for column 1, panel A, in Table 6 were 0.025 (0.022) for high and 0.019 (0.020) for low.

response" model where wage gains from minimum wages map into profit reductions. There is a hint that the long-run adjustment may be through lower rates of net entry.

There are, of course, a number of caveats to our results. It would have been useful to have data on prices and quality to see if these may also have adjusted in response to minimum wages.[37] It would also be useful to have more information on the within firm distribution of workers in other sectors besides care homes. A fuller integration of theory and empirical work in the context of imperfect competition in both product and labor markets is another fruitful research area for the future. Overall given the total sparsity of evidence of the impact of minimum wage floors on firm profitability, we believe this study is an important contribution looking at the impact of labor market regulation on *firms* as well as the more developed and extensive evidence base that exists studying the impact on individuals.

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