EXPLORING THE RELATIONSHIPS BETWEEN THE NMW AND PRODUCTIVITY[1]

REPORT TO THE LOW PAY COMMISSION

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EXECUTIVE SUMMARY

A growing body of evidence suggests that one of the effects of the National Minimum Wage (NMW) has been to increase labour productivity amongst low-paying firms (Galindo-Rueda & Pereira, 2004; Rizov & Croucher, 2011; Riley & Rosazza Bondibene, 2013, 2015). This report explores some of the underlying mechanisms through which this positive association might have come about.

There are plenty of reasons to think that minimum wages may increase labour productivity. The established literature provides a number of possible explanations. Firms may adopt more capital intensive production processes in response to changes in relative factor costs or invest in unmeasured intangible assets that raise measured efficiency. As examples of the latter, firms may invest in training, particularly if firms are able to accrue rents from training provision, or organisational changes such as improved human resource and management practices. Minimum wages may also create incentives for firms to change the skill or occupational mix amongst their employees. At the same time, employees may feel more obliged to work hard in response to better wages or may be less likely to switch jobs, reducing firms' turnover costs.

This report examines whether the introduction of the NMW may have induced companies to implement a series of potentially productivity enhancing measures such as:

- Adopting more capital intensive production techniques
- Increasing the provision of training to low-paid and/or other workers
- Shifting the composition of the workforce towards a greater share of skilled workers
- Increasing the supervision of low-paid workers to extract greater effort
- Adopting tougher recruitment criteria
- Outsourcing some of the low-skill tasks that were previously conducted internally

It also examines whether low-paid workers affected by the NMW may have reacted to higher wages by:

- Exerting greater effort in their job
- Reducing their rate of absenteeism

We conduct our analyses on data from different sources. Firm-level information is obtained from the Financial Analysis Made Easy (FAME) dataset and from the Annual Respondents Database (ARD). Worker-level outcomes are investigated on data from the employee-level component of the Workplace Employment Relations Survey (WERS) and on Labour Force Survey (LFS) data. Workplace data are obtained from the establishment-level component of WERS.

Difference-in-differences models are used to estimate the impacts of the NMW on our outcomes of interest. We compare the evolution of the outcome variables across groups of companies and individuals that are more (the "treated") or less (the "controls") affected by the NMW. The identification of "treated" companies in the sample is based on the average wage per worker as a proxy for the proportion of low-paid workers in the firm (in FAME and in the ARD) and on the share of workers paid below £4 at the establishment level in 1998 (in WERS). The identification of treated workers is based on workers' hourly pay before the introduction and subsequent upratings of the NMW (in the LFS), and on workers' relative position in the wage distribution or occupation (in WERS).

We report sensitivity analyses where we vary the control groups against which treated groups are compared, the time periods, and the specification of the outcome variables. We also test for differential responses to the NMW across different types of firms and workers, splitting the estimation samples by standard dimensions such as firm size and worker gender.

While some results point to a significant effect of the introduction of the NMW on treated firms' capital intensity (i.e., measured as the stock of a firm's physical capital per worker), this result is not robust across different samples, data sets or estimation techniques. Further, on the whole, our regression results do not support the hypothesis that firms that were more affected by the NMW invested more heavily in physical capital assets than a comparable control group.

We find no evidence to suggest that the NMW increased the incidence of training amongst low-paid workers. Nor do we identify any significant effects of the NMW on a series of indicators capturing workers' rate of absenteeism, workers' perception of the effort exerted or the degree of discretion they have in their job.

With regard to management practices, we do not identify any effect of the NMW on establishments' outsourcing practices, recruitment criteria or on the provision of training to the establishments' main occupation group.

We find some evidence to suggest that those establishments that were more affected by the NMW experienced a greater reduction over the period 1998-2004 in the share of workers employed in routine unskilled occupations, accompanied by a greater increase in the share of workers employed in professional occupations. However, this result needs to be interpreted with caution as it is based on a relatively small sample of establishments and it is not based on administrative data but on managers' reported measures of workforce composition. We also find some evidence to suggest that the NMW was associated with an increased likelihood of low paid workers doing shift work.

With the exception of the evidence on changes in the occupational composition of the workplace and on the incidence of shift work, we do not identify significant impacts of the NMW on any of the other measures that we look at. Three alternative, but not mutually exclusive, explanations are consistent with this negative result. First, it is possible that the positive association between the NMW and labour productivity arises through changes in a series of employer and employee behaviours. This would make it more difficult to detect a statistically significant impact of the NMW on any single type of productivity enhancing behaviour, than on productivity itself. A joint analysis of the factors that might affect productivity may be more fruitful, but the relevant information to undertake such analysis is not readily available.

Second, there may be a unique most important channel through which the NMW led to increases in productivity in low-paying firms that we have not considered in this study. In particular, it is possible that firms responded to higher minimum wages by reshaping their internal structure and by adopting managerial practices to achieve efficiency gains. While this report considers the impact of the NMW on employment structure, outsourcing and recruitment practices as reported by managers in WERS, the data do not capture informal changes in the organisation of labour that may have occurred within low-paying firms.

Third, it is possible that one of the channels that we have investigated played a major role in determining the link between the NMW and productivity but that measurement error and insufficient time variation in relevant outcome variables undermine the precision of the estimators, leading us to over-reject the hypothesis of a significant impact. However, robustness tests show that the estimators performed well in picking up the positive impact of the NMW on firms' labour costs and on low-paid workers wages. This should dispel worries that a negative result is solely due to the use of weak estimators.

In sum, the evidence presented in this report does not point to a single most important mechanism through which a positive link between the NMW and productivity in low-paying firms might have arisen. Rather, it seems likely that any positive productivity impacts from the NMW are driven by a variety of behaviours.

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1. Introduction and overview

The majority of evidence on the UK National Minimum Wage (NMW) and minimum wages more generally considers the impacts of minimum wages on employment. But firms may respond in a number of ways to minimum wages and there is a growing body of evidence on the impacts of the NMW on other business outcomes. A number of studies have found evidence to suggest that firms may have increased their productivity in response to labour cost increases associated with the NMW (Galindo-Rueda & Pereira, 2004; Rizov & Croucher, 2011; Riley & Rosazza Bondibene, 2013, 2015).

The research proposed here explores some of the factors that may contribute to the empirical relationship between the NMW and the productivity performance of businesses found in recent studies. Further insight into this relationship is important for understanding the affordability of any increases in the NMW, perhaps particularly in the current environment where the productivity performance of UK businesses has been very weak and the National Living Wage is being introduced.

A number of factors may contribute to a positive link between the NMW and firms' productivity. For example, businesses may substitute capital for labour because the relative cost of labour increases with the NMW. Employers may provide more training for their employees, e.g. in monopsony labour markets where employers can obtain rents from training provision (Acemoglu and Pischke, 1999), or may implement other organisational changes aimed at enhancing the efficiency of production. Alternatively, productivity increases may arise through increased worker effort in response to a better wage (Shapiro & Stiglitz, 1984; Akerlof, 1982). Finally, the empirical relationship between productivity and the NMW found in recent studies may in part reflect changes in hours worked and firm-specific prices associated with the NMW.

This study provides new evidence on the relationship between the NMW and firms' investment in capital and employer provided training. The study also considers the link between the NMW and some measures of organisational change, and the link between the NMW and workers' discretion in performing their jobs.

Recent evidence does not clearly suggest that UK businesses substituted capital for labour in response to relative price shifts induced by the NMW. Rizov & Croucher (2011) find some evidence of capital-labour substitution for firms in some low paying sectors. Riley and Rosazza Bondibene (2015) find no evidence that the positive associations they find between firms' productivity and the NMW result from a change in the capital intensity of production (ratio of capital stock to employment). However, capital stocks change relatively slowly (they equal a cumulated sum of past, depreciated, investments) and may be measured with substantial error. For these reasons evidence of capital labour substitution may be difficult to detect by looking at capital labour ratios alone. Riley and Rosazza Bondibene (2013) examine the impact of the NMW on the ratio of investment to turnover and find no evidence of any effect. But, the ratio of investment to turnover may not change even if firms do substitute capital for labour. In this study we examine the link between the NMW and capital labour substitution, assessing changes in the ratio of capital investment to employment between high and low average labour cost firms. This analysis builds on the analysis in Riley and Rosazza Bondibene (2015).

Previous research on the impact of the NMW on employer provided training is also mixed. Analysing the British Household Panel Survey, Arulampalam, Booth and Bryan (2004) suggest that the NMW

may have led employers to increase training provision. This is particularly evident in smaller labour markets, which may be less competitive. Dickerson (2007) re-examines these issues using the Labour Force Survey (LFS) and finds no link between the NMW and training. In this study we re-examine the link between employer-provided training and the NMW using the LFS, looking beyond the introductory period. We also examine the link between employer-provided training and the NMW using the Workplace Employment Relations Survey (WERS). WERS has recently been used to study the relationship between the NMW and various workplace practices (see e.g. Bewley, Rincon-Aznar and Wilkinson, 2014).

We also use the LFS and WERS to examine organisational changes that employers may have introduced in response to the NMW and which may have implications for productivity. Using the LFS we can examine whether minimum wages have been associated with a move towards more shift work. Using WERS we examine the effect of the NMW on establishments' outsourcing and recruitment practices, as well as the skill composition of firms' workforce.

It is difficult to find direct evidence on the relationship between minimum wages and worker effort. Experimental evidence in Owens and Kagel (2010) suggests there may be a positive link between worker effort and minimum wages. Using a natural experiment design Georgiadis (2013) finds that the NMW led to a reduction in the level of worker supervision required in the care homes sector and argues that the NMW may have functioned as an efficiency wage. In this study we investigate the relationship between the NMW and worker effort using information in WERS on establishments' use of NMW workers and measures of worker discretion and control. We investigate this in the cross-section and in the panel elements of the survey using relatively standard difference-in-difference techniques. Using the LFS we examine the relationship between the NMW absenteeism. Absenteeism may reduce with the NMW if the NMW increases workers' job satisfaction.

Taking these analyses together this study aims to provide a better understanding of the links between the NMW and firms' productivity performance.

2. Data sources and measurement issues

The productivity-enhancing channels that we investigate are mapped into a series of outcome variables measured at the level of the firm, the workplace or of the individual worker. As a consequence, the information that is necessary to construct these variables cannot be extracted from a unique dataset. We use instead a combination of datasets with different units of analysis (i.e., firms, establishment or workers), different collection methodologies (i.e., administrative data, surveys), and different time coverage. Here we describe each of these in turn.

2.1 Employee-level data from WERS

The employee-level component of the Workplace Employment Relations Survey (WERS) is a rich source of information on a series of outcomes that are relevant to identify whether the introduction of the NMW might have boosted firm-level productivity via an "efficiency-wage" effect. Survey questions cover employees' job satisfaction, the level of effort required to perform their tasks, and the level of discretion granted to the worker.

We conduct the employee-level analysis on the 1998 and the 2004 waves of WERS. While interviews with managers took place between July and October each year, we cannot observe the exact point in time at which employees completed the questionnaire. However, we are confident that all the answers provided to the 1998 and the 2004 questionnaires refer to the pre-NMW introduction and the post-NMW introduction periods, respectively. We focus on WERS years around the introduction of the NMW rather than later surveys (there is also a WERS 2011) because the introduction of the NMW provides the more stringent natural experiment.

In both waves establishments are the main sampling unit and they are selected according to a stratified sampling scheme. Establishments' minimum size for inclusion in the sample is lowered from 10 employees in 1998 to 5 employees in 2004. To compare the 1998 and the 2004 cross-sections it is therefore necessary to eliminate establishments with less than 5 employees in 2004 and to apply corrected survey weights to the establishments and employees appearing in 1998.

While information on establishments is obtained during structured interviews with managers, employee data are collected via a self-completed questionnaire. Up to 25 employees for each establishment are randomly selected to participate in the survey; because workers' participation in the survey is voluntary it is possible that there is some response bias in the survey.

The sample obtained after pooling the 1998 and the 2004 cross-sections of employee data includes 50,691 employees from 3,516 establishments. Table 1 reports the number of employees covered by each of the two waves and the weighted distribution across establishments of different size. Note that we are not looking at a panel of employees as this cannot be constructed. Rather we use the data as repeated cross-sections.

Table 1-WERS employee sample composition

	1998		2004	
	Employees	**% (weighted)**	**Employees**	**% (weighted)**
Small workplaces (<50)	7,655	31.42	7,246	37.27
Medium workplaces (50-249)	12,407	34.39	8,305	29.78
Large workplaces (>249)	8,178	34.19	6,882	32.94

Note. The Table splits the sample of workplaces according to the Low Pay commission firm-size groups. The table reports the number of employees covered in the 1998 and in the 2004 waves (unweighted) and their distribution across establishments of different size (weighted).

The identification of employees eligible for the adult rate of the NMW requires information on workers' age. Unfortunately, WERS provides employees' age within bands. In WERS 2004 it is easy to identify eligible workers because we observe a wage band starting at age 22. On the contrary, in WERS 1998 we observe a unique age band 20-24 that includes both eligible and ineligible workers. To increase the comparability of our working definition of eligible workers across waves, we assign to the group of eligible workers all individuals aged 20 or older, because this age is a lower bound in both waves.[2]

[2] An alternative way of achieving consistent age groups over time is to focus on individuals age 30+, which leaves out a large proportion of workers affected by the NMW.

2.2 Establishment-level data from WERS

The establishment-level analysis based on WERS is conducted on the subsample of establishments for which managers are interviewed both in 1998 and in 2004. In 2004, WERS does not include employee data for these establishments and it is impossible to compare over time the outcomes of interest for groups of employees from the same workplaces. Nevertheless, we are able to observe in both years for the same workplace the set of outcome variables based on managers' answers. This limited panel allows us to compare outcome changes over time across establishments that are more or less affected by the introduction of the NMW. We measure individual establishments' exposure to the policy as the reported share of employees that are paid below £4 per hour in 1998. Although the £4 threshold is greater than the adult rate of the NMW at the introduction it captures the impact of the 2000 and the 2001 upratings.

Table 2 reports the number of workplaces included in the estimation sample by major industry group and share of low-paid workers in the total workforce. After retaining in the sample only those establishments that were covered in both years, our sample is composed of 1,912 observations for 956 establishments. The small dimension of the sample is reflected in small industry-cells. Nevertheless, in some industries (such as manufacturing; wholesale and retail; education and health) there is a sufficient dispersion across establishments in the 1998 proportion of low-paid workers. This variation can be exploited to construct a continuous treatment variable and to identify the heterogeneous impact of the policy across workplaces operating in the same industry.

Table 2 - WERS establishment interviewed both in 1998 and in 2004 by industry and low-paid employment share

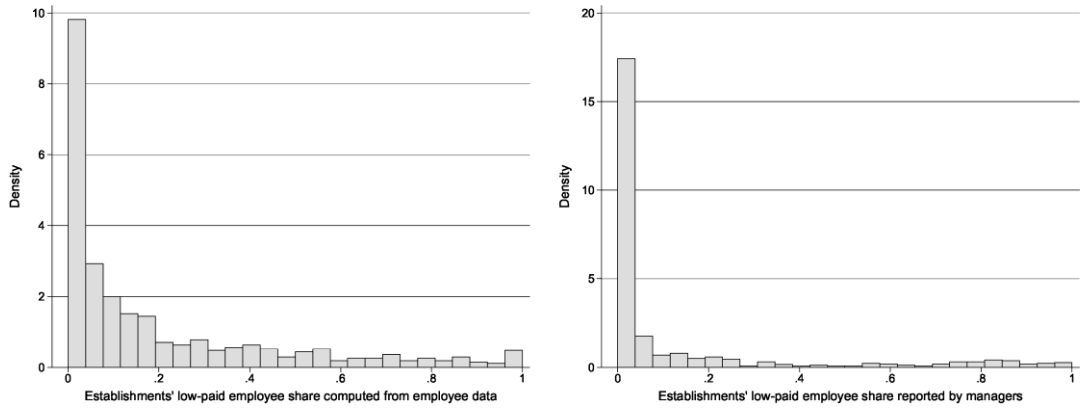
	num. of sampled establishments			weighted industry shares		
Industry	0%	less than 20%	more than 20%	0%	less than 20%	more than 20%
manufacturing	64	55	15	0.32	0.41	0.27
electricity, gas, water	18	3	2	0.74	0.03	0.23
construction	19	21	2	0.53	0.44	0.03
wholesale and retail	24	33	44	0.19	0.21	0.60
hotels and restaurants	8	6	42	0.16	0.03	0.81
transport and communication	35	19	4	0.73	0.12	0.15
financial services	35	4	0	0.94	0.06	0.00
other business services	47	22	15	0.76	0.18	0.06
public administration	64	29	11	0.73	0.23	0.04
education	80	27	17	0.64	0.22	0.14
health	47	36	33	0.51	0.21	0.28
other services	34	7	16	0.59	0.06	0.35

Note. The table reports the number of establishments sampled in 1998 by industry and share of workers paid below £4 (first three columns), and the weighted shares at the industry level.

A possible concern with this analysis arises from managers' systematic misreporting of the proportion of low-paid workers in the workplace. To check the quality of the reported shares we compare this variable with the shares of low-paid workers computed from the 1998 employee questionnaire. Figure 1 shows the distribution of establishments by the share of low-paid workers reported by managers (right-hand panel), and the distribution based on the share computed from employee data (left-hand panel). The figure suggests that managers may tend to underreport the share of low paid workers. For instance, according to managers' reporting, over 17% of the

establishments covered in both years do not have any low-paid workers, while this share falls to less than 10% when it is based on employees' reporting.[3] We conduct sensitivity tests using the computed share measure. Despite the difference in the distribution of these variables, we find that their correlation is high: 71% when considering all establishments, and 74% when considering only establishments with a non-zero share of low-paid workers.

Figure 1- Distribution of establishments by share of low-paid employees



2.3 Employee-level data from LFS

We use the LFS October 1996 – October 2014 to evaluate the impact of the NMW on the incidence of training, the incidence of shift work (a measure of organisational change) and the rate of absenteeism (an indicator of an efficiency wage effect) amongst low paid workers. The LFS analysis complements the analysis conducted on WERS in three main aspects.

First, the LFS covers a longer period of time with greater frequency than WERS. Hence, this dataset is used to investigate both the introduction of the NMW in 1999 and the impact of subsequent upratings. Second, because WERS lacks a panel component at the employee-level, the analysis conducted on the pooled 1998 and 2004 WERS cross-sections identifies the impact of the policy by comparing the outcome variables across groups of different individuals in 1998 and in 2004. In the LFS comparisons over time also involve different individuals, but we can track individuals affected by each uprating before and after its implementation allowing us to identify treatment and control groups ex-ante rather than ex-post. Third, LFS includes more precise measures of individuals' hourly wage and age than in WERS. This allows us to identify with greater confidence groups of treated individuals. We retain in the sample only employees that are eligible for the adult rate (i.e., age greater than 21).

2.4 Firm-level data from FAME and the ARD

Business micro-data allow us to investigate the relationship between the NMW and capital labour substitution. As in Riley & Rosazza Bondibene (2013, 2015) we use two business datasets for our analysis: company accounts data available in FAME and the Annual Respondents Database (ARD) (Annual Business Survey and its predecessors), an Office for National Statistics (ONS) business survey collecting information on firms' inputs and outputs. We use information on companies in FAME over

[3] This difference may be caused by differences in the measures of hours worked reported by employers and employees.

the period 1993-2012, reconstructing annual snapshots of the business population from past vintages of FAME. The ARD is not available for major sectors of the economy before 1997; we use information to 2012.

FAME contains financial data on the population of UK registered companies, but for many companies data items are missing as there are very light reporting requirements for smaller firms. We retain for our main sample company accounts that cover employment, remuneration and profits, fixed and tangible assets.[4] This main sample is different to the sample used in Riley & Rosazza Bondibene (2015) in that we do not restrict our attention to firms that report turnover[5]; a common restriction in using companies accounts data for productivity analysis. This boosts the sample size by around 50%.

The ARD holds information on the nature of production in British businesses and is a census of larger establishments and a stratified (by industry, region and employment size) random sample of establishments with fewer than 250 employees (SMEs). It covers businesses in the non-financial non-agriculture market sectors.[6] We undertake our analysis at the level of the enterprise as in Riley & Rosazza Bondibene (2015) where these data are described in further detail.

We exploit the longitudinal element of the ARD to evaluate NMW effects on businesses. Due to the rotating sampling strategy we are unable to create a balanced panel of firms with annual observations as we do with the FAME data (except for large firms). Instead we create a panel of firms observed for two years at four year intervals. The longitudinal sample is insufficient to support analysis of firms with less than 10 employees and therefore we exclude these firms; we also exclude micro firms in the FAME analysis.

3. Research methods

To investigate the different productivity-enhancing channels, we design difference-in-differences estimators that exploit the features of individual datasets. For simplicity of exposition, we split this methodological section by dataset and level of analysis.

3.1 Employee-level analysis in WERS

In the absence of a panel of employees, the main empirical challenge is to identify treated workers in the 2004 cross-section, and to compare their outcomes with those of individuals paid below the NMW in 1998. We propose two alternative strategies to match treated and control workers across the two cross-sections.

[4] We include consolidated and unconsolidated accounts, excluding subsidiaries with a UK owner (where the UK parent has at least 50% control). As in our analysis of the ARD data we exclude companies with less than 10 employees, and focus on market sector companies in the non-agriculture and non-financial industries.

[5] The sample also differs because we need a longer span of data on capital assets to construct investment proxies, and because we require firms to report tangible assets.

[6] The ARD includes partial coverage of the agricultural sector (we exclude these businesses) as well as businesses in "non-market" service sectors such as education, health and social work. We exclude businesses in these latter sectors where inputs and outputs are thought not to be directly comparable, making productivity analysis difficult to undertake. We also exclude businesses in the mining and quarrying, and utilities sectors (typically very large businesses with erratic patterns of output) and in the real estate sector, where output mostly reflects imputed housing rents.

3.1.1 Identification strategy based on the percentiles of the wage distribution

The first strategy exploits the relative position of individuals in the wage distribution. The assumption underpinning this strategy is that the introduction of the NMW narrows the pay-gap between low-paid individuals but it does not affect the relative position of each individual in the wage distribution. If this assumption holds, we can identify the Average Treatment effect on the Treated (*ATT*) as:

$$ATT = E\left[Y_{i,04}^{QT} - Y_{j,98}^{QT}\right] - E[Y_{h,04}^{QC} - Y_{l,98}^{QC}]$$

where the subscripts *i*, *j*, *k*, *l* denote four different groups of employees observed across the two cross-sections, the notation QT indicates individuals that belong to one of the treated percentiles of the wage distribution, while QC indicates individuals positioned in one of the `control' percentiles, the subscripts '98 and '04 indicate whether the individual is observed in the 1998 or in the 2004 cross-section. The treated and `control' percentiles are defined on the basis of the average hourly wage computed at the percentile level relative to the £3.60 NMW in 1998; percentiles with average hourly wage below this threshold are considered as treated, while `control' percentiles are selected among those within a certain range above the threshold.

Having identified the treated and control percentiles we estimate the following specification on the dataset obtained by pooling the 1998 and the 2004 WERS cross-sections of employee-level data:

$$y_{ie} = \theta(const + \sum_{n=1}^{21} \alpha_{1n} Q_n + \alpha_2 (QT_n \times D_{2004}) + X_i' \beta_1 + X_e' \beta_2 + \delta_{st} + \delta_{rt} + \varepsilon_{ie}) \quad (1)$$

where y_{ie} is the outcome variable for employee *i* from establishment *e*. $\theta(.)$ is a link function that relates the linear model in the equation to a linear or a non-linear outcome. This link function changes according to the structure of the outcome variable. When the outcome variable is continuous $\theta(.)$ is linear and Model 1 is estimated by Ordinary Least Squares (OLS). When the outcome variable is ordered categorical $\theta(.)$ is a step function and the model is an ordered probability model estimated by Maximum Likelihood (ML) methods. When the outcome variable is bivariate $\theta(.)$ is the cumulative density function of the normal probability distribution and the model is Probit.

Q_n is a dummy variable taking value one if the employee is positioned in the *n*th percentile of the wage distribution and value zero otherwise, QT_n is a dummy variable taking value one if the employee is positioned in a treated percentile and value zero otherwise. D_{2004} is a dummy variable taking value one if the observation is drawn from the 2004 cross-section and value zero otherwise. X_i' is a vector of individual-level characteristics (see Table 3 below). Finally, δ_{st} and δ_{rt} are industry-year and region-year fixed-effects respectively. Their inclusion in the model controls for diverging trends in the evolution of the outcome variables between 1998 and 2004 across different industries and across different regions.[7] In Model 1, the parameter α_2 can be interpreted as the ATT of the NMW, as it captures the differential rate of growth of the outcome variable in the `treatment' group compared with its growth within the `control' group. The control group includes employees positioned in the percentiles with average hourly wage up to 20% above the 1998 NMW.

[7] We also estimate models excluding these fixed effects, and models introducing occupation-year and education-year fixed effects. Results are qualitatively similar across models with different sets of fixed-effects.

To check for a possible heterogeneous impact of the policy on treated individuals at different points of the wage distribution, we estimate a more flexible specification that does not constrain the parameter α_2 to assume the same value across treated percentiles:

$$y_{ie} = \theta(const + \sum_{n=1}^{21} \alpha_{1n} Q_n + \sum_{n=1}^{11} \alpha_{2n} (Q_n \times D_{2004}) + X_i'\beta_1 + X_e'\beta_2 + \delta_{st} + \delta_{rt} + \varepsilon_{ie}) \quad (2)$$

Model 2 includes interactions between the post-reform indicator D_{2004} and dummy variables for each treated percentile. Each interaction captures the differential growth rate of the outcome variable for individuals falling in that percentile compared with individuals in the control group.

3.1.2 Identification strategy based on cells

Our second empirical strategy exploits the proportion of individuals paid below £3.60 in the 1998 cross-section to assign a `probability of treatment' to each individual in the 2004 cross-section. To do so, we divide the sample into different cells defined on the basis of employees' occupation, industry and region of employment. At the cell-level, we estimate the probability of treatment as the 1998 proportion of individuals with an hourly wage below £3.60. The effect of the NMW is identified by exploiting the differential evolution of the outcome variables over the period 1998-2004 across individuals assigned to cells with different probabilities of treatment. To do so, we estimate the following DiD model:

$$y_{ie} = \theta\left(const + \gamma_1 \widehat{P_c} + \gamma_2 (\widehat{P_c} \times D_{2004}\right) + X_i'\beta_1 + X_e'\beta_2 + \delta_{st} + \delta_{rt} + \varepsilon_{ie}) \quad (3)$$

where $\widehat{P_c} = \frac{T98_c}{N98_c}$ is the estimated probability of treatment computed as the ratio of 1998 individuals with hourly wage below £3.60 $T98_c$ in cell *c* over the total number of 1998 individuals in that same cell.[8] The coefficient γ_2 captures the effect of the policy. This approach is conceptually similar to performing an exact matching of individuals across waves. Because exact matching quickly exhausts the number of possible matches as the number of dimensions increases, we use few exogenous dimensions to define our cells. We use three alternative definitions of cells:

-*Cell1* : industry-occupation (171 combinations)

-*Cell2* : occupation-region (99 combinations)

-*Cell3*: industry-region (132 combinations).

We validate this strategy by conducting a placebo exercise. For each cell we construct a `wrong' estimator of the probability of treatment $\widehat{W_c}$ obtained as the ratio of individuals with an hourly wage equal to or greater than the 2004 NMW level, but within 20% above this level, over all eligible individuals. This ratio is the probability that a 2004 individual in a particular cell is `just not-treated'. We then set the estimation sample to include all individuals with wages up to 40% above the NMW.

3.1.3 Hourly wages and eligibility for the adult rate in WERS

WERS reports the number of hours worked per week H (including overtime) and the weekly wage-bands of individual employees $(W_L, W_U]$. From this information we compute the upper and lower bounds of an employee's hourly wage $HW_U = \frac{W_U}{H}$ and $HW_L = \frac{W_L}{H}$. In line with previous work on this dataset (e.g., Forth and Millward, 2002; Bryson, 2002) we estimate interval regressions on these

[8] We compute these ratios considering only weighted observations for individuals aged 20 or older.

bounds, where the probability that the wage falls at any point in the interval HW_L and HW_U is a function of the characteristics of the employee, of her job and workplace.

Table 3- Interval regressions on hourly wage bands

	(1) 1998	(2) 2004
Worker's attributes		
Male	0.150***	0.143***
	(0.011)	(0.010)
health	-0.072**	-0.025*
	(0.022)	(0.012)
depend	0.019*	0.006
	(0.009)	(0.010)
married	0.052***	0.031***
	(0.009)	(0.009)
Union Member	0.095***	0.055***
	(0.012)	(0.012)
Partime (hours<30)	0.093***	0.188***
	(0.017)	(0.018)
White Background	0.053*	0.073***
	(0.024)	(0.018)
Permanent contract	0.143***	0.062**
	(0.022)	(0.020)
Overtime/ Total hours	-0.088*	0.617***
	(0.040)	(0.056)
Worker's qualification (excluded: no qualification)		
GCSE	0.085***	0.101***
	(0.012)	(0.013)
A-levels	0.157***	0.204***
	(0.015)	(0.016)
Degree	0.280***	0.293***
	(0.017)	(0.016)
Post-graduate	0.381***	0.363***
	(0.027)	(0.025)
Worker's age band (excluded:<20)		
20-24	0.216***	0.102
	(0.030)	(0.053)
25-29	0.321***	0.189***
	(0.028)	(0.055)
30-39	0.408***	0.286***
	(0.029)	(0.047)
40-49	0.422***	0.409***
	(0.030)	(0.046)
50-59	0.419***	0.428***
	(0.029)	(0.048)
>=60	0.335***	0.364***
	(0.035)	(0.028)
Worker's tenure (excluded: less than one year)		
more than 1 year, less than 2 years	0.022	0.014
	(0.015)	(0.014)
more than 2 years, less than 5 years	0.051***	0.058***
	(0.013)	(0.012)
more than 5 years, less than 10 years	0.075***	0.075***
	(0.014)	(0.014)
more than 10 years	0.103***	0.139***
	(0.014)	(0.015)
Regions (excluded: Yorkshire & Humberside)		
East	0.032	0.017
	(0.030)	(0.029)
East Midlands	0.014	0.007
	(0.024)	(0.028)
London	0.224***	0.205***
	(0.023)	(0.024)
North East	-0.059	-0.021
	(0.032)	(0.036)
North West	0.027	-0.008
	(0.024)	(0.021)
Scotland	0.032	0.008
	(0.023)	(0.025)
South East	0.130***	0.084***
	(0.024)	(0.022)

continued	(1) 1998	(2) 2004
South West	0.059	-0.019
	(0.030)	(0.025)
Wales	-0.035	-0.011
	(0.036)	(0.036)
West Midlands	0.050	0.001
	(0.027)	(0.024)
Workers' Occupation (excluded: operative and assembly)		
Managers and senior administrators	0.624***	0.496***
	(0.021)	(0.024)
Professionals	0.514***	0.447***
	(0.020)	(0.026)
Associate professional and technical	0.339***	0.337***
	(0.019)	(0.024)
Clerical and secretarial	0.196***	0.150***
	(0.017)	(0.023)
Craft and skilled service	0.180***	0.137***
	(0.023)	(0.025)
Personal and protective service	0.046	-0.032
	(0.028)	(0.027)
Sales	0.089**	-0.021
	(0.031)	(0.029)
Other routinary unskilled	-0.079***	-0.130***
	(0.017)	(0.024)
Firm employment size (excluded: 10-24)		
25-49	0.036	0.036
	(0.024)	(0.022)
50-99	0.029	0.036
	(0.024)	(0.021)
100-199	0.050*	0.074**
	(0.024)	(0.023)
200-499	0.067**	0.071**
	(0.026)	(0.022)
500 or greater	0.129***	0.086***
	(0.025)	(0.021)
Industry (excluded: manufacturing, utilities and construction)		
Sales	-0.059**	-0.118***
	(0.023)	(0.024)
Hotels and Resturants	-0.176***	-0.131**
	(0.028)	(0.045)
Transports and Communications	-0.033	0.046
	(0.023)	(0.028)
Financial	0.011	0.043
	(0.022)	(0.030)
Business Services	-0.003	0.047*
	(0.025)	(0.023)
Education, public sector, Health	-0.067**	-0.072***
	(0.021)	(0.020)
Company's characteristics		
More than 50% workforce is partime	-0.172***	-0.190***
	(0.021)	(0.018)
Women less than 25% workforce	0.045**	0.046**
	(0.016)	(0.017)
Manual workers more than 50% workforce	-0.025	-0.110***
	(0.017)	(0.017)
Independent Business	-0.071***	-0.020
	(0.016)	(0.015)
Foreign franchise	0.169***	0.050**
	(0.027)	(0.019)
Observations	25,382	22,451
Right-Censored Obs.	901	888

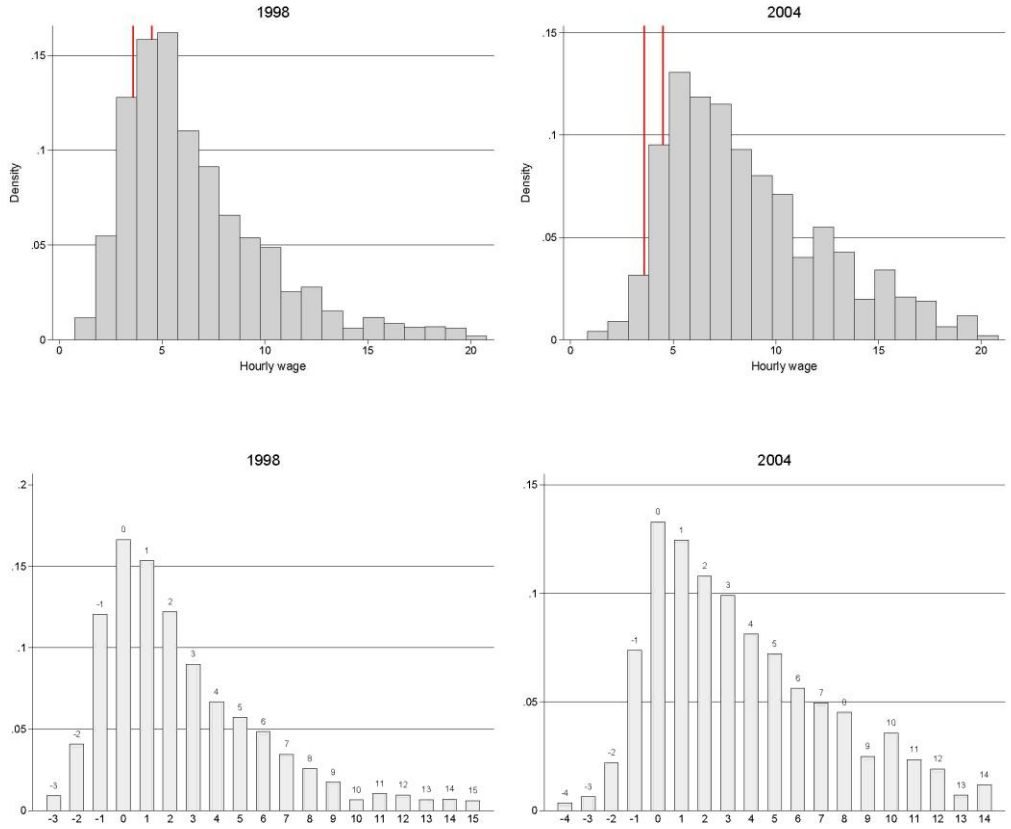
Note. The table reports estimates from interval regressions on hourly wage bands estimated separately on the 1998 and the 2004 WERS employee cross-sections. All regressors except for overtime/total hours are introduced in the model as dummy variables taking value one when the attribute applies to the workers or to her employing company. Robust weighted standard errors are reported in parentheses . Following the documentation of the 2004 wave of WERS, we apply weights revised in 2004 to the 1998 cross-section to increase the consistency of the analysis across survey waves. Significance levels: *≤0.01, **≤0.05,* ≤0.1.**

Table 3 reports the results from interval regressions separately estimated on the 1998 and the 2004 cross-sections. We then use the estimated parameters to predict the exact hourly wage of each

worker, constraining the predicted value to fall in between HW_L and HW_U. We also conduct robustness tests where we use the upper bound of an individual wage band as a proxy for its hourly wage instead of the value predicted from interval regressions.

We validate our estimates of the hourly wage by graphical analysis. Figure 2 shows weighted histograms of the predicted hourly wages for the years 1998 and 2004. In the upper panel, the bin width is set at £1, while the lower and the upper bounds of each bin depends on the minimum value of the distributions' support.[9] The two vertical lines represent respectively the 1999 and the 2004 adult rates of £3.6 and £4.5.[10] Because the two lines fall within bins, these histograms may underestimate the discontinuity in the wage distribution that we expect to see in 2004 with the NMW in place. Even so we see more bunching around the NMW in 2004 than in 1998. To better capture the discontinuity, in the following two histograms, the lower bound of `bin 0' takes value £3.6 in 1998 and £4.5 in 2004. By forcing the lower bound of `bin 0' to coincide with the NMW level we can sharply split workers that are just below or above the NMW. By comparing the histograms for 2004 with those for 1998, we can see that in 2004 the discontinuity in the distribution of wages just below the NMW level is a bit clearer.

Figure 2 -Histograms of predicted hourly wages



Note. In the top two histograms the two vertical red lines represent the 1999 and the 2004 NMW levels of £3.6 and £4.5. In the histograms in the bottom panel the lower bound of `bin 0' is set at £3.6 in 1998 and at £4.5 in 2004. In all histograms the bin width is set at £1.

[9] If *min* is the lowest observed hourly wage, the *n*th bin has lower bound min+*n,* and upper bound min+*n+1*.
[10] In WERS 2004 some workers may have answered the survey after the October NMW up-rate. Nevertheless we prefer to use the £4.5 that represents the lowest NMW among all the workers interviewed in that survey wave.

3.2 Establishment-level analysis in WERS

We estimate the following DiD model on the restricted sample of workplaces for which managers are interviewed in both waves:

$$\Delta y_{e,98-04} = \theta(const + \vartheta_1 S_{e,98} + \beta_2 \Delta EMP_{e,98-04} + X'_{e,98}\beta_2 + \varepsilon_{ie}) \quad (4)$$

where $\Delta y_{e,98-04}$ indicates that the outcome is constructed by first-differencing the values of the establishment-level variable over the two periods of time. For some outcomes it indicates that the 2004 outcome value depends on the 1998 value of an underlying variable. For example, when we look at outsourcing the dependent variable is a dummy variable taking value one if the establishment outsources some tasks in 2004 conditional on those same tasks being conducted internally in 1998. $S_{e,98}$ is either the reported or the computed share of low-paid employees (i.e., below £4) in 1998. $\Delta EMP_{e,98-04}$ is the log change in the number of a firm's employees over the period 1998-2004 and it captures diverging trends across firms with different growth opportunities. Finally, $X'_{e,98}$ is a vector of establishment-level time invariant characteristics including: industry dummies, region dummies, size dummies (see Table 3 for categories), a dummy for independent companies, and a dummy indicating whether the establishment belongs to the production sector. The parameter ϑ_1 captures the differential change in the outcome variable across establishments that are differently affected by the policy due to a different share of low-paid workers in 1998.

3.3 Employee-level analysis in LFS

Our analysis of the LFS data complements a previous study by Dickerson (2007) on the impact of the NMW on low-paid workers' training in two respects. First, we extend the timeframe of the analysis to cover both the introduction of the NMW and subsequent upratings until October 2013. Second, we estimate two specifications of the model respectively addressing one of the two major identification challenges associated with this dataset.

The first challenge is determined by the large measurement errors besetting the computed measure of hourly wages (HPAY). Measurement errors make it difficult to precisely identify a group of treated workers (i.e., those that in a particular year have wages below the upcoming NMW level). A solution to this issue is to identify treated workers on the basis of the hourly rate reported by workers (HRATE). Because this variable is only available starting from the second quarter of 1999, we cannot use it to define treated and control groups in the periods preceding the introduction of the policy. As a consequence unless we rely on HPAY, that is available since 1996, we cannot investigate the effect of the introduction of the NMW. To take advantage of the more precise variable HRATE, we implement the difference-in-differences estimator used in Dickens, Riley & Wilkinson (2009) and Bryan et al. (2013) that exploits the upratings of the NMW:

$$y_{it+1} = cons + \gamma YS_{i,t+1} + (\alpha_L + \gamma_L YS_{i,t+1})L_{it} + (\alpha_T + \gamma_T YS_{i,t+1})T_{it} + (\alpha_U + \gamma_U YS_{i,t+1})U_{it} + X'_{it}\beta + \varepsilon_{it+1} \quad (5)$$

Individuals are classified into the "treatment" group ($T_{it}=1, L_{it}=0, U_{it}=0$) if their HRATE in wave 1 (the first quarter in which the individual enters the LFS panel) is below the upcoming NMW level but above or equal to the current NMW level, in the "lower group" ($T_{it}=0, L_{it}=1, U_{it}=0$) if the HRATE is below the current NMW, or in the "upper group" ($T_{it}=0, L_{it}=0, U_{it}=1$) if the HRATE is equal to or above +20% of the upcoming NMW. The control group ($T_{it}=0, L_{it}=0, U_{it}=0$) includes workers with HRATE above the upcoming rate but within 20% above that level. The outcome y_{it+1} is measured in the third

wave the individual appears in the sample. $YS_{i,t+1}$ is a dummy variable taking value one if the transition from the first to the third wave "straddles" a NMW uprating (i.e., if the individual's wave 1 falls in quarter 2 and wave 3 falls in quarter 4, or if wave 1 is in quarter 3 and wave 3 is in quarter 1 of the next year) and value 0 otherwise. Effectively, this model identifies the impact of the policy by comparing "treated" and "control" individuals during transitions when there is no rise in the NMW and during transitions when there is a rise. The model is estimated separately for each uprating and for different groups of workers defined on the basis of their gender and firm employment-size. The coefficient γ_T measures the ATT.

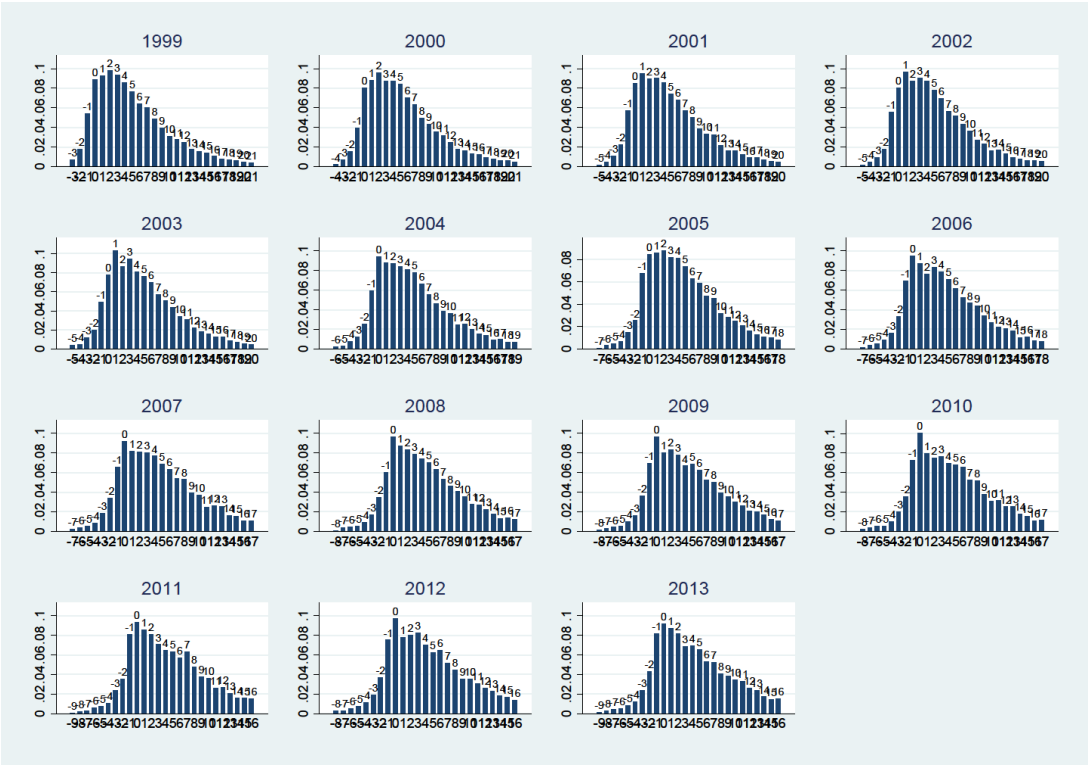
Because the variable HRATE is available for a minority of workers, an issue with this estimator is that the group of "treated" and "control" workers observed during each wave can be critically small resulting in imprecise estimates. To address this issue we implement the methodology described in Skinner et al. (2002) to impute an HRATE value for individuals for which we observe only HPAY. This methodology consists in estimating by OLS the conditional relationship between HRATE and HPAY on the sample of individuals where we observe both variables (donors), using the estimated parameter on HPAY and on other control variables to predict HRATE for those individuals with only HPAY reported (receivers). We then use the predicted HRATE to assign the HRATE value of a donor to a receiver with similar predicted HRATE. Donors maintain their own value of HRATE. This procedure allows us to expand the sample of treated and control individuals. Table 4 reports the percentage gain in sample size obtained when using the imputed HRATE instead of the reported one. Average sample gains across years for different types of cells are sizable ranging from 35% to the 71%. Figure 3 and Figure 4 show respectively the distributions of LFS employees by HPAY and the imputed HRATE. The left-tail truncation of the wage distribution due to the NMW is much more evident when using the imputed HRATE. This evidence suggests that this indicator is more precise when it comes to classifying treated and control employees.

Table 4- Increase in sample size by cell when using imputed HRATE for analysis on the LFS

	YS=0 Treated Males	YS=1 Treated Males	YS=0 Control Males	YS =1 Control Males	YS=0 Treated Females	YS =1 Treated Females	YS=0 Control Females	YS =1 Control Females
2000	47.8	37.3	94.3	52.9	46.4	19.1	69.8	26.5
2001	54.2	43.0	57.9	61.7	21.3	24.2	31.0	33.5
2002	42.4	43.1	79.8	54.8	23.4	22.5	33.8	27.8
2003	40.0	44.9	54.2	61.1	30.8	22.9	26.6	26.9
2004	52.8	44.9	60.3	57.7	26.8	25.2	44.8	39.6
2005	56.9	53.2	74.0	56.0	30.0	27.4	47.2	33.0
2006	40.5	43.8	58.0	54.2	28.2	29.2	37.5	40.0
2007	49.3	56.1	70.6	79.7	36.8	38.5	50.5	44.5
2008	44.9	57.4	68.7	69.2	38.2	34.3	43.1	48.5
2009	34.7	38.4	70.4	85.6	39.4	46.0	53.2	45.4
2010	61.4	46.8	74.9	61.2	37.7	40.8	52.7	54.7
2011	55.4	52.7	73.9	66.2	44.2	45.5	56.7	46.1
2012	38.0	58.9	78.0	76.3	43.6	53.3	50.9	54.3
2013	67.7	70.4	79.7	78.4	46.8	55.7	47.1	58.7
MEAN	49.0	49.4	71.1	65.4	35.3	34.6	46.1	41.4

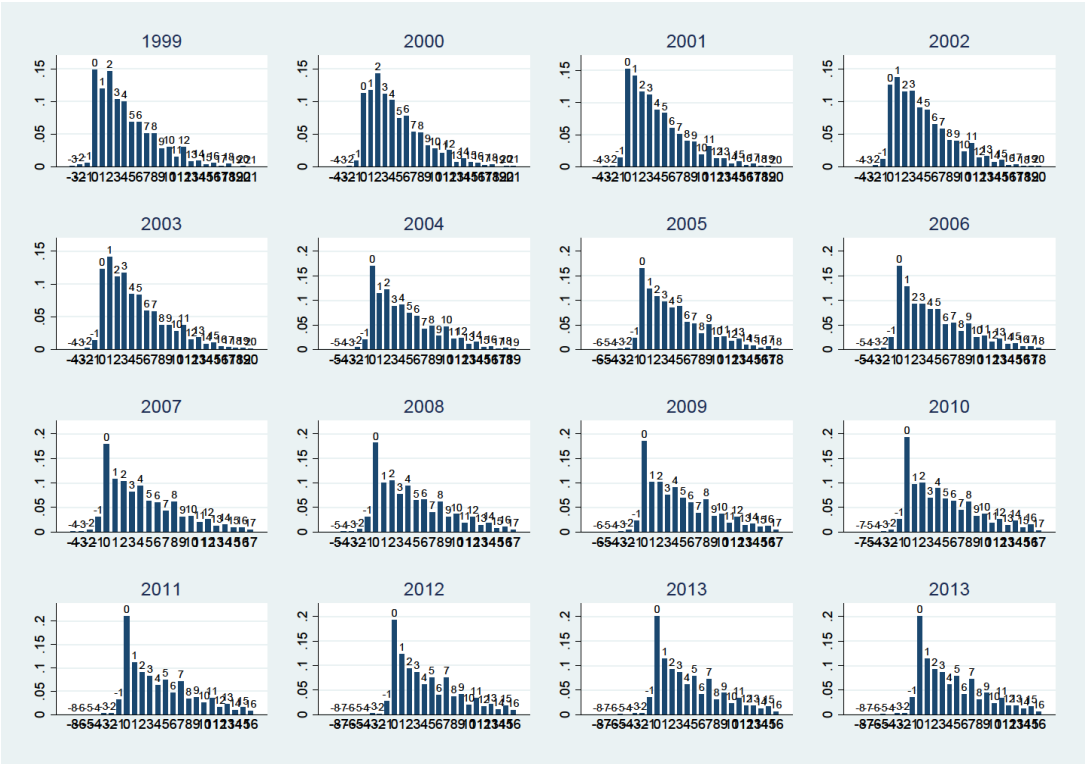
Note. The table reports the percentage gain in sample size at the cell-level (i.e., where cells are defined as a unique combination of year-transition type-treatment-gender) obtained when instead of classifying treated and control workers based on HRATE we use the imputed HRATE based on Skinner et al. (2002).

Figure 3- Distributions of employees in LFS by HPAY



Note. The lower bound of bin 0 is the current NMW. Bins width is set at £0.50

Figure 4-Distribution of employees in LFS by the imputed HRATE



Note. The lower bound of bin 0 is the current NMW. Bins width is set at £0.50

A recent paper by Brewer et al. (2015) points out that failing to account for correlations between the error terms of individuals classified in the same treatment-transition cell leads to incorrect estimated of the parameters' standard errors. This is the well-known Moulton bias. To check for this issue we implement the Donald and Lang two-stage procedure recommended by Brewer et al. (2015) to obtain consistent standard errors (note this is only possible with multiple upratings viewed on average). In the first stage, the outcome variable at the employee-level is regressed on individual-level controls and a set of dummies assigning each individual to one of 112 possible cells defined by the following dimensions:

- Year in which the transition is observed (2000-2013)
- Whether the transition straddles an uprating ($YS_{i,t+1}=0$ *or* $YS_{i,t+1}=1$)
- The assignment of the individual to one of the four possible groups ($T_{it}=1$ *or* $L_{it}=1$ *or* $U_{it}=1$ *or* $C_{it}=1$)

In the second stage we use the 112 estimated parameters on the cell dummies as dependent variables in a model including on the right-hand side a dummy indicating whether the parameter on the left-hand side refers to the treatment group and to the transition straddling the uprating. In the second stage we control also for year-transition type fixed-effects and treatment-group type fixed effects.

Focusing on the upratings allows us to use a more precise measure of employees' hourly wage, but it may lead us to underestimate the impact of the NMW on the outcomes of interest. This is because we evaluate the impact of the policy by comparing periods when the policy is always active, albeit with different rates. In addition, it is possible that individuals' and firms' response to the policy took place around the moment of its introduction. Therefore, we complement the previous analysis on upratings by implementing a difference-in-differences estimator that compares the outcome of treated and control individuals before the introduction of the NMW (in 1997 and 1998) against the outcomes in each individual policy-on year (from 1999 to 2013). We will also estimate the model on the whole time period, hence comparing the policy-off period (1997-1998) against the whole policy-on period (1999-2013). Because we cannot observe the HRATE variable in LFS before 1999, we define treated and control individuals on the basis of the imprecise HPAY variable. We estimate the following specification:

$$y_{i,t+1} = cons + \alpha_{1t+1}T_t + \alpha_{2t}C_t + \beta_{1t+1}(T_t \times POST_t) + \delta_t + X'_{it}\beta + \varepsilon_{it+1} \quad (6)$$

where the outcome variable y_{it+1} refers to the last period the individual *i* appears in the LFS panel (i.e., the fifth survey wave), while the treatment indicator T_t is based on the hourly wage during the first period in the panel. The indicator T_t takes value one if the real hourly wage falls below the upcoming NMW rate. C_t identifies employees in the control group, and takes value one if the individual earns 10% more than the upcoming NMW but below 20% above that level. By using all individuals below the upcoming NMW as a treatment group and by excluding individuals with earnings in between the upcoming NMW and +10% that level we reduce the risk of misclassifying treated and control individuals when using the imprecise HPAY indicator of hourly wages. To better capture the different extent to which individuals with different initial wages are affected by the introduction (and upratings) of the NMW, we also estimate a "wage gap" version of model (6) where for treated individuals the treatment dummy variable T_t is replaced by the difference between an individual's wage in the first wave and the upcoming NMW level.

3.4 Firm-level analysis with FAME and ARD

To investigate the impacts of the NMW on firms' investment behaviour we follow Draca *et al.* (2011) and distinguish treatment and control firms by their average labour costs, exploiting the correlation between firm average labour costs and the share of NMW workers in the firm. As in Riley & Rosazza Bondibene (2015) we use the cut offs of £10,000, £12,000 and £14,000 (measured in 1998 prices) to separate treatment and control firms. We also use a semi-continuous measure of the strength of treatment. Based on the relationship in WERS between average labour costs and the share of NMW workers in the establishment we derive a proxy measure of the share of NMW workers in firms in the FAME and ARD datasets. The greater the share of NMW workers in the firm, the greater is the intensity of treatment from the NMW.

We select firms for the treatment and control groups in 1998, 2002 and 2008 based on their average labour costs in that year.[11] We then track outcomes for these two groups of firms up to four years later, comparing the difference in performance between these two groups to the difference in performance in the years before the NMW change as in equation (7).

$$y_{it} = constant + \propto TREAT_i + \beta POST_t + \gamma TREAT_i * POST_t + \delta X_{it} + \varepsilon_{it} \quad (7)$$

In equation (7) $TREAT$ is a dummy variable equal to one if the firm is in the treatment group (low paying firm) and zero otherwise and $POST$ is a dummy variable equal to one if policy change has taken place, and zero otherwise. The X_{it} are controls for firm characteristics intended to account for differences between firms unrelated to the NMW. Importantly these also include industry-year fixed effects so that NMW effects are identified off firm differences within industry-year groups. The industry-year effects represent the influence of any variables that we observe only at this relatively aggregate level, such as deflators and the share of part-time workers, as well as industry specific trends and shocks. ε_{it} is an error term and the rest are parameters to be estimated. We estimate model (7) on a balanced panel of firms, essentially eradicating firm fixed effects. The coefficient γ captures the impact of the introduction/uprating of the NMW on outcome y. For the introduction period the counterfactual is no NMW. For later periods the counterfactual is the NMW before the uprating. For the 2008 cohort the counterfactual may also reflect the impact of the NMW in better economic circumstances (i.e. before the financial crisis).

We also estimate a double difference-in-differences model. As shown in Riley & Rosazza Bondibene (2015), falsification tests suggest γ in model (7) may capture some element of mean reversion when estimated on the longer differences we observe in the ARD longitudinal data (e.g. due to measurement error in particular years).[12] Indeed, the falsification tests in section 5 suggest this may be the case for some outcome measures even when we observe firms on an annual basis as we do in FAME. To remedy this we choose another two groups of firms from further up the distribution of average labour costs. These are chosen to be sufficiently high up the distribution that it is very unlikely that either group should be affected by the NMW. We estimate a "treatment" effect on this sample of firms using model (7), which we assume captures the mean reversion parameter. We then net this off the treatment effect estimated on our original sample to arrive at an estimate of the

[11] These are the same time periods used in Riley & Rosazza Bondibene (2015).

[12] In the ARD we observe firms for two years and then again for another two years 4 years later.

impact of the NMW that is purged of mean reversion. The assumption is that the mean reversion parameter is similar for firms at the lower and higher ends of the distribution of average labour costs.

More formally we define two groups of firm: firms from the lower end of the distribution of average labour costs and firms from the higher end of average labour costs. Each of these groups of firm has a treatment and control set of firms defined by some threshold. We then estimate a double difference-in-differences equation as in equation (8), where $L = 1$ if the firm belongs to the lower end of the distribution of average labour costs and 0 otherwise.

$$y_{it} = const + \propto TREAT_i + \beta\ POST_t + \gamma TREAT_i * POST_t$$

$$+const_L L_i + \propto_L TREAT_i * L_i + \beta_L\ POST_t * L_i + \gamma_L TREAT_i * POST_t * L_i + \delta X_{it} + \varepsilon_{it} \quad (8)$$

Model (8) is essentially a combination of the standard difference-in-differences model in (7) and a vertical difference-in-differences model. The coefficient γ_L captures the NMW treatment effect and γ measures the mean reversion parameter (common across firms with $L = 1$ and $L = 0$).

Using FAME we also estimate (7) and (8) on a pre-NMW period (we select firms for the treatment and control groups in 1995). This is a falsification test. If we detect similar "policy effects" before the NMW was implemented this casts doubt on the validity of the identification strategy.

4. Outcome and control variables

4.1 Outcome and control variables for the employee-level analysis in WERS

We investigate the impacts of the NMW on training provision and outcomes that capture efficiency wage type effects using the WERS employee-level data. Outcome variables investigated are based on answers provided by the employees to a series of questions included in both the 1998 and the 2004 survey waves:

- Training (question B2 in WERS 1998, B4 in WERS 2004). *During the last 12 months, how much training have you had, either paid for or organised by your employer?*

  Employees provide one of six possible answers to this question: none, less than 1 day, 1 to less than 2 days, 2 to less than 5 days, 5 to less than 10 days, 10 days or more. We generate two variables based on these answers: *dtrain* is a dummy variable that takes value 1 if the employee receives some training (answers different from `none') and value 0 otherwise; *ctrain* is a categorical variable taking six possible values from 1 to 6 with the score increasing in the amount of training received by the worker.

- Workers' influence about the tasks and the pace at which they work (questions A9 in WERS 1998, A7 in WERS 2004). *In general, how much influence do you have about the following?*

- *The range of tasks you do in your job*
- *The pace at which you work*

We generate two categorical variables *taskdis* and *pacedis* with values ranging from 1 to 4 according to the answer provided: 1 for `none', 2 for `a little', 3 for `some', and 4 for `a lot'.

- Effort (questions A8 in WERS 1998, A6 in WERS 2004) *Do you agree, or disagree, with the following statements about your job?*

- *My job requires that I work very hard*
- *I never seem to have enough time to get my job done*

We generate the categorical variables *hard* and *etime* with values ranging from 1 to 5 according to the answer provided on each statement: 1 `strongly disagree', 2 `disagree', 3 `neither agree nor disagree', 4 `agree', 5 `strongly agree'.

- Overtime (questions A4 in WERS 1998 and WERS 2004*) How many overtime or extra hours do you usually work each week, whether paid or unpaid?*

We generate the dummy variable *dover* taking value 1 if the individual reports some overtime and value 0 otherwise, and the continuous variable *oratio* obtained as the ratio of overtime hours over total hours worked per week.

The X_i' vector of individual-level controls including the following variables:

- *White: a* dummy variable taking value one if the employee has a white ethnic background and value zero otherwise;
- *Partime:* a dummy variable taking value one if the employee works less than 30 hours per week and value zero otherwise;
- *Male:* a dummy variable taking value one if the employee is male and values zero if the employee is female;
- *Union:* a dummy variable taking value one if the employee is member of a union and value zero otherwise;
- *Depend:* a dummy variable taking value one if the employee has dependent children and value zero otherwise;
- *Health:* a dummy variable taking value one if the employee has a long-term health conditions and value zero otherwise;
- *Married:* a dummy variable taking value one if the employee is married and value zero otherwise;
- *Temporary:* a dummy variable taking value one if the worker has a temporary contract and value zero otherwise;
- *Educ:* a set of dummies for different qualifications achieved ;
- *Tenure:* a set of dummies for different tenure within the firm;
- *Age:* a set of dummies for different age groups;
- *Occ:* a set of dummies for different occupations.

The vector X_e' includes establishment-level controls:

- *Partime*: a dummy variable taking value one if the worker's establishment employs 50% or more part-time workers and value zero otherwise;
- *Women*: a dummy variable taking value one if the worker's establishment employs 25% or less female workers and value zero otherwise;
- *Manual:* a dummy variable taking value if the worker's establishment employs 50% or more manual workers and value zero otherwise;

- *Indep:* a dummy variable taking value one if the establishment is independent and value zero otherwise;
- *Foreign*: a dummy variable taking value one if the establishment is owned by a foreign group and value zero otherwise;
- *Size:* a set of dummy variables for different employment size of the establishment.

4.2 Establishment-level outcome and control variables in WERS

We want to test whether the introduction of the NMW has induced organizational changes at the establishment level that may have promoted productivity growth. We use comparable information across survey waves to construct the following outcome variables:

- $\Delta OccX_{98-04}$: the 1998-2004 change in the share of occupational group *X* over the workforce of the establishment. We compute this variable from data on the number of employees belonging to each occupational group. *X* is one of the following occupational groups: managers and senior administration (occ1), professional (occ2), technical (occ3), clerical and secretarial (occ4), craft and skilled services (occ5), protective and personal services (occ6), sales (occ7), operative and assembly (occ8), routine unskilled (occ9) .
- $OutX_{98-04}$: a dummy variable taking value one if in 2004 the establishment outsourced task *X* that in 1998 was performed internally. *X* is one of the following tasks: cleaning (Cle), security (Sec), catering (Cat), maintenance of building (Mai), printing and publishing (Pri), payroll (Pay), transport (Tra), computing (Com), training, recruitment (Trn).
- $RecX_{98-04}$: a dummy variable taking value one if in 2004 the manager declares that workers' characteristic *X* is important for recruitment*,* and if the same characteristic was considered not important in 1998.
- ΔOff_{98-04}: is the 1998-2004 change in the proportion of employees from the company's largest occupational group to have formal off-the-job training (i.e., off-the-job training is defined as training away from the normal place of work, but either on or off the premises).
- ΔOth_{98-04}: is the 1998-2004 change in the proportion of employees from the company's largest occupational group to be formally trained to be able to do jobs other than their own.

We control in all the establishment-level regressions for the rate of growth of the establishment between 1998 and 2004 ΔEMP_{98-04}. This is obtained as the logarithmic difference in the number of employees between the two time periods. Finally, we include a vector $X'_{e,98}$ of establishment-level time invariant characteristics including: industry dummies, region dummies, size dummies (see Table 3 for categories), a dummy for independent companies, and a dummy indicating whether the establishment belongs to the production sector.

4.3 Employee-level outcome and control variables in the LFS

We investigate three types of outcomes using LFS data. First, we extend the work of Dickerson (2007) by looking at the impact of the NMW on low-paid workers' training incidence. As in that work, we use the two main training variables from the LFS:

- ED13WK: In the 3 months since [date] have you taken part in any education or training connected with your job or a job that you might be able to do in the future?
- ED4WK: ... and did you take part in any of that education or training in the 4 weeks ending Sunday the [date]

While the first question records training provision in the last quarter, the second question records training provision in the last month. Both questions are addressed to respondents aged 16-69 either in work or in receipt of education or training. Answers are recoded to take value 1 if the individual receives training and value 0 otherwise. When we estimate model (5) the sample is restricted to individuals that are in employment in both the first and third waves, while for model (6) we retain individuals that are in employment in both their first and fifth waves.[13]

A second type of outcome that we investigate is the rate of absenteeism. We construct an individual's absence rate as in Ercolani and Robson (2006):

$$abs\_rate = \frac{A_{it}}{C_{it}}$$

Where A_{it} is the number of hours of absence, obtained as the difference between usual and actual hours worked in the reference week (BUSHR – TTACHR) in the reference week due to sickness (ILLOFF=1 or SICKDAY=1) reported by the worker. C_{it} is instead the usual number of contracted hours per week (BUSHR). A_{it} takes value 0 if the number of usual hours is smaller than the number of actual hours or if the difference between the two is not motivated by sickness. To control for individuals' fixed effects we estimate the models on the change in the *abs_rate* between the first and the third wave or between the first and the fifth wave.

The third outcome that we consider is changes in the incidence of shift work among low-paid workers. This outcome is based on the original variable SHFTWK99 that records the answer to the question:

- Do you do shift work in your (main) job most of time (SHFTWK99=1), occasionally (SHFTWK99=2) or never (SHFTWK99=3)?

Based on the answers provided we construct a bivariate shift work indicator ($SHIFT_{it}$) that is equal to 1 if SHFTWK99=1 or SHFTWK99=2 and it is equal to 0 otherwise. For most of the years this variable is available only in LFS Quarter 2, making it impossible to estimate model (5) on this outcome. Hence, we investigate this outcome through model (6). To better capture changes in an individual workers' exposure to shift work we construct the variable $DSHIFT_{it,t-1}$ that is obtained as the difference in $SHIFT_{it}$ between wave one and wave five.

The vector $\mathrm{X'_{it}}$ appearing on the right-hand side of models (5) and (6) includes controls related to an individual's workplace (i.e., SIC two-digit industry, region of employment), occupation (i.e., SOC one digit occupation, dummy for temporary occupation, job tenure), qualification (i.e., major National

[13] Recall that in model (5) the pre-policy period is the time period between upratings, i.e. within the year. Therefore we look at short transitions over 6 months (between waves 1 and 3). In model (6) the pre-policy period is the pre-NMW period and we can therefore look at annual transitions (between waves 1 and 5).

Vocational Qualification groups), family (i.e., dummies for married and head of family), demography (i.e., age, age squared and dummy for white ethnic background).

4.4 Firm-level variables in FAME and the ARD

We investigate impacts of the NMW on investment using company accounts data. Ultimately we are interested in knowing whether firms substituted capital for labour in response to relative price shifts induced by the NMW. Previous studies investigated this issue using the ratio of capital to labour. Here we also look at investment and investment relative to labour. Investment is the purchase of new capital.

Using the company accounts data we measure average labour costs as the ratio of remuneration to employment. This is the variable we use to distinguish treatment and control firms. We also estimate NMW impacts on real labour costs to validate the experiment. The capital labour ratio is calculated as the ratio of fixed assets to employment. While our main interest here is in capital labour substitution this focus stems from the finding that the NMW appears to be associated with increases in firms' labour productivity. Therefore we also estimate impacts on labour productivity, calculated as the ratio of the sum of remuneration and profits (a proxy for gross value added) to employment, and a measure of efficiency, total factor productivity (TFP), derived by residual as log labour productivity less the capital labour ratio times the capital share, where the capital share is measured as one less the industry-time period average ratio of labour costs to value added.

In the company accounts direct measures of investment are relatively sparsely reported and may not reflect investment in productive capital. We measure investment as the change in the capital stock. This measure of investment is net of depreciation and, unlike gross investment, can be negative. Indeed, the change in the capital stock is roughly centered around zero in our database. This makes it difficult to calculate log measures of investment or of the ratio of investment to capital without making a significant upward adjustment to the distribution. Therefore we use as an investment proxy the annual change in log capital. We also use as an outcome variable the annual change in the capital labour ratio, measured as the change in log capital less the change in log employment. In the models we estimate a positive treatment effect on these outcomes implies that capital (or the capital labour ratio) was rising faster in low paying firms after the introduction or uprating of the NMW.

Note that fixed assets in the company accounts may include tangible (land & buildings, vehicles, plant & machinery) assets as well as intangible assets (long term resources, not cash or held for conversion into cash that do not have a physical presence e.g. brand, reputation, goodwill, supplier relationships). For the majority of firms in our sample fixed assets are equivalent to tangible assets, most likely reflecting under-reporting of intangible assets. All regressions in this report using FAME data and involving capital or growth in capital use the fixed assets measure. We have also run these same regressions using the tangible assets measure instead and these give largely similar results (not reported).

Similar to the company accounts data we measure average labour costs in the ARD as total labour costs relative to employment. The capital labour ratio is the ratio of plant & machinery equipment

capital to employment.[14] These are derived using the gross investment variables available in the ARD. In addition to the capital labour ratio we consider two measures of investment: log investment in plant and machinery; log ratio of investment in plant and machinery to employment. Labour productivity is calculated using the measure of gross value added at factor cost available in the survey. We derive TFP in much the same way that we do using FAME.

5. Results

5.1 Results from the employee-level analysis using WERS

5.1.1 Identification strategy based on the percentiles of the wage distribution

Before analysing the outcomes of interest, we validate the identification strategy underpinning models (1) and (2) by analyzing wage growth 1998-2004 within individual percentiles of the wage distribution. The aim of this exercise is to verify whether we can identify treated individuals in the 1998 and 2004 WERS cross-sections based on their relative position in the wage distribution. Figure 5 plots the mean hourly wage computed at the level of each percentile of the 1998 wage distribution.[15] We find that only the first eleven percentiles have mean hourly wage below the 1999 NMW. We define these percentiles as treated, and assign all individuals that belong to one of these percentiles to the QT group. The control group includes employees with hourly wages up to 20% above the 1998 NMW; these are positioned between the 12th and the 21st percentile of the wage distribution.

In Figure 6, we investigate the impact of the introduction of the NMW on the wage growth of the treated percentiles. We plot the normalized wage growth obtained by subtracting the average growth rate for the whole population from the growth rate of individual percentiles to emphasize the relative growth rate within each percentile compared with the average rate over the whole population. Hence, in percentiles with positive values of the normalized growth rate wages grew faster compared to the whole population. It is easy to see that wages grew faster for the treated percentiles than for the population average. Wages rose less for the treated percentiles than for the top percentiles of the wage distribution (above the 79th), but the evolution of wages at the high end of the distribution is unlikely to be related to the NMW. Consistent with the expectation that the policy had a greater `bite' for treated workers with a relatively low initial wage we see that wages for the lowest treated percentiles grew faster than for the highest percentiles amongst the treated. Therefore, it is possible that the treatment effect of the policy on other outcomes is heterogeneous across treated percentiles. The heterogeneous impact across treated percentiles will be accounted for in model (2).

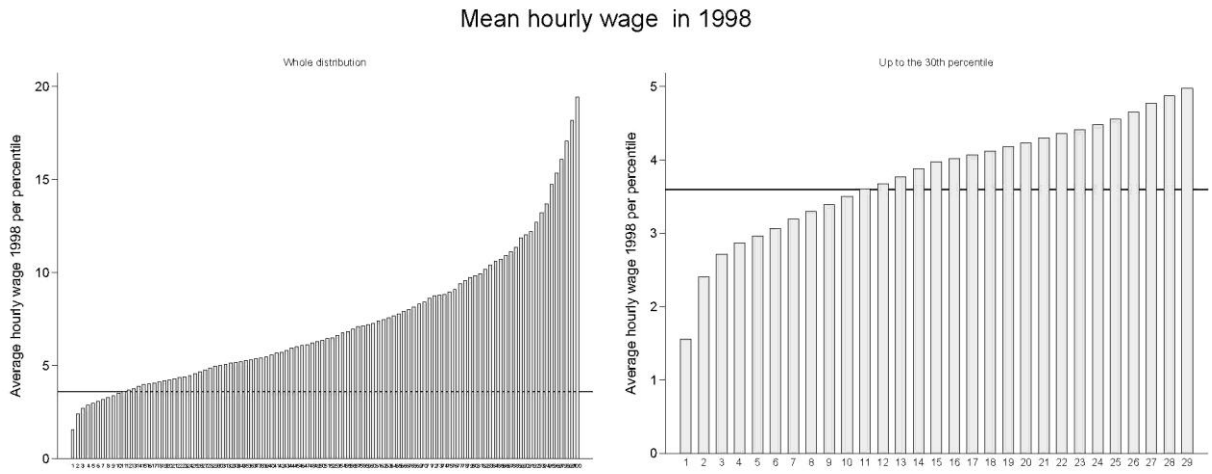
Table 5 reports the proportion of individuals assigned to each category of the outcome variables (these proportions sum to one for each outcome variable). Proportions are estimated separately for

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

[15] The distribution refers only to the individuals identified as eligible for the adult rate (aged 20 or older).

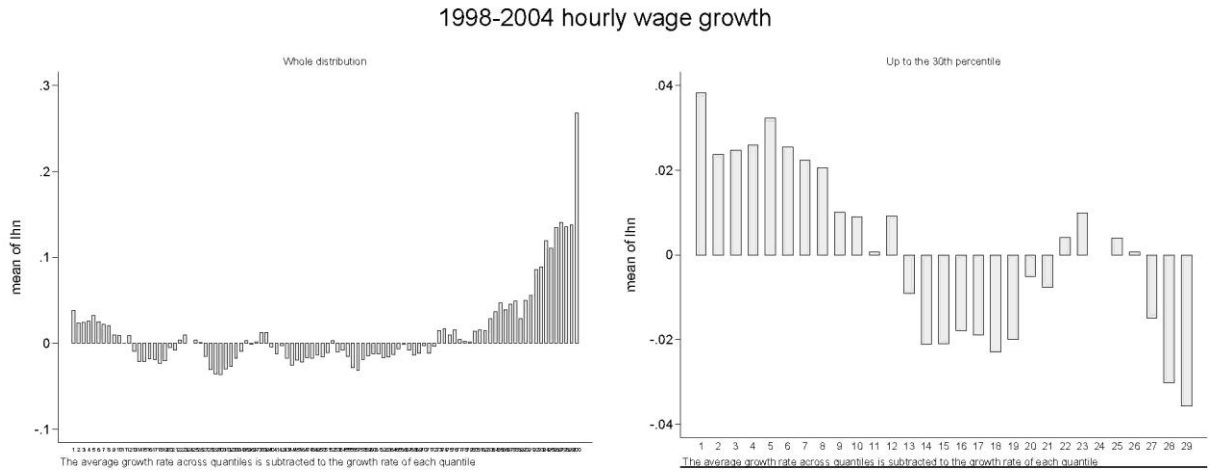
workers positioned below (or within) and above the 11th percentile of the wage distribution.[16] In both years, individuals positioned in treated percentiles report fewer days of training and less influence on the range of tasks and on the pace of work than individuals in higher percentiles. These individuals are also less likely to strongly agree that they work very hard and are less likely to agree they have insufficient time to perform their tasks. Lastly, individuals in` treated' percentiles are less likely to work overtime. Overall, this evidence is consistent with the hypothesis that low-wage workers are relatively less productive than high-wage workers.

Figure 5- Mean hourly wage across percentiles of the 1998 wage distribution



Note. The figure plots the 1998 average hourly wage computed at the level of each percentile of the wage distribution for individuals aged 20 or older in 1998. The horizontal line is set at the level of the 1999 NMW (£3.6).

Figure 6- Normalized growth rate of the hourly wage between 1998 and 2004 at the level of individual percentiles



Note. The figure plots the 1998-2004 normalized growth rate of average wages at the level of individual percentiles. Normalization is achieved by subtracting the average growth rate over the whole population to the growth rate of individual percentiles.

[16] Proportions are estimated by using employee-level survey weights.

Table 5- Distribution of the respondents across outcomes of interest

	1998		2004	
	≤11th	>11th	≤11th	>11th
ctrain				
None	0.52	0.43	0.50	0.35
Less than 1 day	0.13	0.09	0.14	0.09
1 to less than 2 days	0.12	0.13	0.12	0.15
2 to less than 5 days	0.11	0.19	0.13	0.22
5 to less than 10 days	0.04	0.09	0.05	0.10
more than 10	0.08	0.08	0.07	0.08
taskdis				
none	0.22	0.16	0.17	0.11
a little	0.22	0.18	0.18	0.14
some	0.39	0.39	0.37	0.38
a lot	0.18	0.27	0.29	0.36
pacedis				
none	0.15	0.13	0.13	0.11
a little	0.17	0.16	0.17	0.15
some	0.38	0.36	0.36	0.36
a lot	0.30	0.35	0.33	0.38
hard				
strongly disagree	0.01	0.00	0.01	0.00
disagree	0.06	0.04	0.05	0.05
neither agree nor disagree	0.24	0.19	0.21	0.18
agree	0.48	0.50	0.47	0.49
strongly agree	0.21	0.26	0.26	0.28
etime				
strongly disagree	0.06	0.02	0.07	0.03
disagree	0.37	0.24	0.37	0.25
neither agree nor disagree	0.34	0.31	0.30	0.30
agree	0.17	0.27	0.18	0.27
strongly agree	0.06	0.15	0.08	0.15
dover				
no overtime	0.67	0.45	0.67	0.49
some overtime	0.33	0.55	0.33	0.51

Note. The table reports weighted proportions of employees choosing each of the possible survey answers within the group of employees with hourly wage below the 11th percentile or above the 10th percentile of the wage distribution.

Table 6 reports estimates of α_2 obtained by regressing Model 1 and 2 on the outcome variables. The first column reports OLS regressions on the hourly wage *hwage.* Model 1 estimates an average impact of NMW on wage growth of +2.5%. This coefficient is an average of the treatment effect across treated percentiles, and the estimates from Model 2 show more clearly the greater impact of the policy on the lowest percentiles (the treatment effect ranges from +3.5% to +4.1% for the 2nd to 6th percentiles and is less for the 7th to 11th percentiles). The effect on the first percentile is negative and not significant and this can be due to the noise introduced by extreme observations at the bottom of the wage distribution. Model 1 does not generate any evidence that the policy had a significant impact on the other outcomes. However, estimates from Model 2 suggest that the NMW may have had a differential impact on the hours worked by the treated individuals with the lowest wage (negative) and by the treated with the highest wage (positive). Still, Model 2 does not highlight any significant effect of the NMW on training, effort or workers' influence on their job.

Table 6-Difference-in-differences estimates percentile approach

Outcome	(1) hwage	(2) hour	(3) oratio	(4) dover	(5) dtrain	(6) ctrain	(7) taskdis	(8) pacedis	(9) hard	(10) etime
Model 1										
$QT_i \times D_{2004}$	0.025***	0.034	-0.060	0.039	-0.024	-0.018	0.070	-0.079	0.099	0.076
	(0.004)	(0.020)	(0.075)	(0.082)	(0.079)	(0.067)	(0.063)	(0.068)	(0.062)	(0.062)
D_{2004}	0.300***	0.052	-0.091	0.345	0.354	0.218	0.261	0.076	-0.102	-0.290
	(0.010)	(0.055)	(0.152)	(0.218)	(0.220)	(0.207)	(0.179)	(0.141)	(0.176)	(0.162)
Model 2										
$Q1_i \times D_{2004}$	-0.023	-0.139***	0.396	-0.175	0.211	0.105	0.720*	0.425	0.450	0.274
	(0.043)	(0.040)	(0.231)	(0.304)	(0.307)	(0.269)	(0.285)	(0.281)	(0.278)	(0.289)
$Q2_i \times D_{2004}$	0.035***	-0.105*	-0.247	0.069	0.149	0.178	0.269	-0.269	0.303	0.215
	(0.008)	(0.046)	(0.170)	(0.294)	(0.308)	(0.261)	(0.249)	(0.276)	(0.234)	(0.263)
$Q3_i \times D_{2004}$	0.035***	-0.018	0.349	-0.354	-0.254	-0.402	-0.234	-0.146	0.435	0.240
	(0.004)	(0.044)	(0.211)	(0.314)	(0.290)	(0.286)	(0.262)	(0.271)	(0.241)	(0.249)
$Q4_i \times D_{2004}$	0.035***	0.096**	-0.305	0.729*	-0.195	-0.103	-0.131	-0.341	-0.008	0.143
	(0.003)	(0.034)	(0.213)	(0.322)	(0.331)	(0.299)	(0.226)	(0.240)	(0.259)	(0.278)
$Q5_i \times D_{2004}$	0.041***	-0.053	-0.205	-0.358	-0.459	-0.408	-0.054	-0.221	-0.101	0.064
	(0.003)	(0.046)	(0.182)	(0.317)	(0.280)	(0.247)	(0.250)	(0.240)	(0.286)	(0.292)
$Q6_i \times D_{2004}$	0.036***	0.006	0.118	0.427	0.201	0.057	0.132	0.029	0.268	0.277
	(0.003)	(0.040)	(0.165)	(0.347)	(0.327)	(0.293)	(0.279)	(0.234)	(0.318)	(0.259)
$Q7_i \times D_{2004}$	0.029***	-0.045	-0.349*	0.117	-0.389	-0.333	0.316	-0.179	-0.129	-0.365
	(0.003)	(0.053)	(0.150)	(0.289)	(0.296)	(0.265)	(0.266)	(0.237)	(0.260)	(0.255)
$Q8_i \times D_{2004}$	0.031***	0.090*	-0.286	0.147	0.468	0.587*	-0.068	-0.335	-0.084	0.077
	(0.002)	(0.044)	(0.153)	(0.307)	(0.308)	(0.297)	(0.220)	(0.232)	(0.257)	(0.237)
$Q9_i \times D_{2004}$	0.025***	0.148**	0.297	0.305	0.090	-0.017	0.127	0.147	0.234	0.370
	(0.003)	(0.053)	(0.163)	(0.280)	(0.312)	(0.264)	(0.252)	(0.248)	(0.264)	(0.243)
$Q10_i \times D_{2004}$	0.017***	0.234***	-0.549***	0.019	0.248	0.161	0.489*	-0.190	0.004	0.282
	(0.003)	(0.037)	(0.158)	(0.327)	(0.311)	(0.280)	(0.228)	(0.219)	(0.245)	(0.270)
$Q11_i \times D_{2004}$	0.010**	0.141**	0.140	-0.333	-0.472	-0.420	-0.089	-0.319	0.158	0.259
	(0.003)	(0.053)	(0.147)	(0.297)	(0.285)	(0.257)	(0.254)	(0.270)	(0.289)	(0.252)
Estimator	OLS	OLS	OLS	Probit	Probit	Oprob	Oprob	Oprob	Oprob	Oprob
Obs.	7,807	7,807	3,068	7,807	7,807	7,743	7,611	7,505	7,594	7,424

Sample composition:	**1998**	**2004**
In treated percentiles	2,233	1,882
In `control' percentiles	2,030	1,710

Note. The table reports OLS, Probit and Ordered Probit estimates obtained by regressing Models 1 and 2 on the outcome variables. Individual-level and establishment-level controls are included in all models but not reported. Robust weighted standard errors are reported in parentheses (2004 adjusted employee weights are applied to the 1998 cross-section). Industry-year FE, Region-year FE and controls are included in all specifications. Significance levels: *≤0.01, **≤0.05,* ≤0.1.**

5.1.2 Identification strategy based on cells

Model 3 identifies the impact of the policy by exploiting differences in the probability of being paid below the initial NMW across occupations, regions and industries. We select these three dimensions to capture heterogeneity in the probability of treatment across individuals while still retaining a sufficient number of individuals by cell. We combine these three dimensions to obtain three cell structures. Each cell structure is based on two dimensions at a time and to identify the impact of the policy it is important that when fixing one dimension we still observe sufficient heterogeneity across the remaining dimensions. For example, the probability of treatment within each occupation should change across industries (cell 1) or across regions (cell 2).

Tables 7 to 9 report the total number of individuals and the percentage of treated in 1998 across different dimensions (Table 7: occupations and industries; Table 8: occupations and regions; Table 9: industries and regions). We use these as a proxy for the probability of treatment. As expected, some of the chosen dimensions (i.e., industry and occupation) explain most of the variation in the probability of treatment. Nevertheless, we still find sufficient variation across individuals from different cells. For example, sales employees have a very different probability of treatment across industries. Some of the cells contain a number of individuals too small to generate reliable estimates of the probability of treatment. To control for this problem, in a set of unreported robustness exercises we estimate Model 3 on a sample excluding cells with less than 20 or less than 100 individuals. Results are qualitatively very similar to the one from the baseline exercise.

Table 7-Cell 1, number of individuals and % of treated in 1998

	manuf.	utilities	construction	wholesale and retail	hotels and restaurants	transport and comm.	financial serivces	other business services	Public adm.	education	health	other comm. serv.
# managers	766	167	377	675	259	341	576	796	418	265	481	311
% treated	2%	0%	0%	0%	6%	0%	0%	1%	0%	2%	2%	4%
# professionals	530	333	261	133	38	152	185	1,025	582	2,864	878	317
% treated	1%	0%	4%	2%	12%	0%	0%	1%	0%	1%	2%	2%
# technical	618	267	274	285	37	267	391	841	799	405	1,880	381
% treated	1%	1%	0%	0%	8%	0%	1%	3%	0%	4%	5%	8%
# clerical /secret.	716	296	409	725	180	498	1,442	1,222	1,775	735	1,023	540
% treated	3%	0%	1%	7%	16%	3%	2%	9%	0%	4%	4%	3%
# craft and skilled	1,355	301	594	271	132	172	8	166	113	85	157	157
% treated	6%	1%	2%	5%	16%	2%	0%	1%	0%	14%	10%	12%
# pers. service	20	9	15	30	203	107	9	138	655	863	1,478	242
% treated	4%	0%	0%	21%	48%	11%	11%	27%	0%	18%	23%	26%
# sales	130	62	29	2,019	72	135	256	155	41	11	12	87
% treated	13%	0%	0%	29%	63%	12%	0%	4%	0%	28%	100%	42%
# operative	2,101	78	169	421	22	717	8	170	41	16	55	115
% treated	10%	0%	2%	18%	56%	9%	0%	17%	0%	6%	17%	30%
# other unskilled	479	35	195	545	506	710	21	475	147	728	648	393
% treated	20%	2%	5%	25%	64%	3%	14%	51%	0%	39%	36%	38%

Table 8-Cell 2, number of individuals and % of treated in 1998

	East	East Midlands	London	North East	North West	scotland	South East	South West	Wales	West Midlands	Yorkshire and Humbershire
# managers and senior	357	342	1,150	191	566	436	857	394	215	425	417
% treated	1%	1%	1%	0%	1%	0%	2%	0%	2%	2%	0%
# professionals	448	444	1,119	316	642	810	1,275	579	299	728	558
% treated	1%	1%	1%	0%	2%	2%	1%	1%	2%	1%	0%
# associate professionals	430	418	1,030	277	697	698	908	552	235	598	461
% treated	4%	2%	0%	5%	2%	4%	3%	6%	3%	2%	3%
# clerical and secretarial	601	763	1,565	391	1,089	980	1,342	698	403	884	717
% treated	2%	4%	0%	2%	5%	2%	2%	3%	10%	10%	5%
# craft and skilled service	284	274	220	198	481	432	413	309	183	350	298
% treated	6%	9%	1%	4%	5%	2%	13%	2%	7%	3%	5%
# personal and protective	288	221	392	260	447	403	596	261	176	314	324
% treated	15%	36%	9%	12%	18%	20%	12%	15%	11%	27%	24%
# sales	249	208	395	193	390	294	378	242	103	261	254
% treated	36%	38%	15%	23%	27%	40%	11%	26%	44%	22%	29%
# operative and assembly	270	331	225	206	506	449	380	331	256	454	398
% treated	19%	11%	5%	3%	8%	14%	7%	9%	7%	15%	13%
# other unskilled	404	412	485	242	596	463	557	380	261	491	487
% treated	31%	36%	9%	38%	29%	30%	26%	41%	39%	32%	43%

Table 9-Cell 3, number of individuals and % of treated in 1998

	East	East Midlands	London	North East	North West	Scotland	South East	South West	Wales	West Midlands	Yorkshire and Humbershire
# manufacturing	496	500	405	336	881	629	723	590	336	1023	655
% treated	7%	7%	2%	2%	4%	8%	10%	4%	3%	12%	6%
# utilities	160	66	180	91	149	273	115	236	109	107	57
% treated	1%	0%	0%	0%	1%	0%	0%	0%	3%	1%	0%
# construction	128	184	363	156	271	267	212	133	209	164	250
% treated	0%	1%	0%	1%	4%	3%	0%	2%	2%	6%	1%
# wholesale and retail	527	411	544	248	666	437	879	387	117	467	440
% treated	26%	29%	7%	18%	19%	26%	9%	20%	37%	14%	17%
# hotels and restaurants	103	105	186	60	235	155	165	136	38	100	145
% treated	55%	41%	17%	25%	29%	44%	19%	39%	56%	52%	67%
# transport and communication	253	246	486	155	326	192	461	273	104	290	354
% treated	2%	6%	1%	2%	9%	1%	3%	2%	0%	11%	5%
# financial services	111	186	762	91	333	396	455	139	114	76	241
% treated	1%	2%	0%	0%	2%	6%	1%	1%	1%	5%	0%
# other business services	210	445	1099	123	473	367	776	414	105	588	406
% treated	15%	23%	2%	29%	14%	2%	6%	12%	30%	9%	10%
# public	280	308	671	293	705	549	546	266	352	278	350
% treated	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
# education	438	380	654	205	452	577	1164	530	235	752	548
% treated	10%	11%	5%	12%	6%	20%	11%	13%	11%	11%	6%
# health	539	482	806	361	742	931	1017	630	349	430	391
% treated	23%	20%	1%	14%	15%	10%	10%	14%	9%	13%	10%
# other community services	137	188	534	193	278	295	320	71	95	303	144
% treated	13%	18%	7%	22%	30%	11%	12%	8%	32%	23%	28%

We estimate Model 3 on two different samples. In the first sample we retain only the individuals that have an hourly wage below +20% of the 2004 NMW of £4.5.

Table 10 reports the estimates obtained on this sample. Across the three cell definitions, the model correctly picks up the faster increase in wages within cells with a higher probability of treatment. By multiplying the estimated parameter $\widehat{\gamma_2}$ by the average $\widehat{P_c}$ we can obtain the ATT of the policy on wages. The ATT is +5% when defining cells as either *Cell1* or *Cell2*, while it is +3.9% when we define cells as *Cell3*. Hence, the estimated ATT from this exercise is qualitatively consistent with the one that we obtained when using percentiles to identify the treated, but it is quantitatively higher. However, both exercises fail to reveal any significant effect of the NMW on the other outcome variables.[17]

Table 10-Difference in Differences on cells (sample with individuals up to +20% of the 2004 NMW)

	(1) *hwage*	(2) *hour*	(3) *oratio*	(4) *dover*	(5) *dtrain*	(6) *ctrain*	(7) *taskdis*	(8) *pacedis*	(9) *hard*	(10) *entime*
					Cell1					
$\widehat{P_c} \times D_{2004}$	0.470***	-0.041	0.014	0.029	-0.169	-0.124	-0.184	0.179	0.238	0.040
	(0.058)	(0.082)	(0.347)	(0.300)	(0.334)	(0.270)	(0.231)	(0.245)	(0.268)	(0.238)
D_{2004}	0.078*	0.091	-0.080	0.231	0.296	0.190	0.246	-0.181	-0.081	-0.055
	(0.037)	(0.048)	(0.171)	(0.202)	(0.205)	(0.179)	(0.156)	(0.142)	(0.177)	(0.158)
$\widehat{P_c}$	-0.503***	-0.135	0.770**	-0.932**	-0.763*	-0.560	0.102	0.264	-0.272	-0.301
	(0.055)	(0.079)	(0.294)	(0.307)	(0.337)	(0.286)	(0.248)	(0.248)	(0.274)	(0.261)
Obs.	10,510	10,510	4,448	10,510	10,510	10,439	10,281	10,165	10,262	10,068
					Cell2					
$\widehat{P_c} \times D_{2004}$	0.488***	0.121	-0.587	0.730	0.104	0.300	0.014	-0.754	1.099	1.530**
	(0.115)	(0.182)	(0.620)	(0.644)	(0.761)	(0.716)	(0.608)	(0.616)	(0.575)	(0.493)
D_{2004}	0.078	0.039	0.056	0.014	0.117	-0.006	0.224	0.042	-0.235	-0.350*
	(0.041)	(0.056)	(0.197)	(0.226)	(0.234)	(0.205)	(0.180)	(0.175)	(0.194)	(0.175)
$\widehat{P_c}$	-0.420***	0.007	0.728*	-0.371	0.276	0.425	-0.185	0.144	-0.500	-0.549
	(0.068)	(0.122)	(0.368)	(0.402)	(0.482)	(0.408)	(0.355)	(0.376)	(0.367)	(0.324)
Obs.	10,510	10,510	4,448	10,510	10,510	10,439	10281	10,165	10,262	10,068
					Cell3					
$\widehat{P_c} \times D_{2004}$	0.422***	-0.102	-0.245	-0.087	-0.459	-0.450	-0.348	-0.021	0.287	0.222
	(0.057)	(0.082)	(0.358)	(0.311)	(0.329)	(0.289)	(0.248)	(0.270)	(0.280)	(0.258)
D_{2004}	0.047	0.094	0.057	0.175	0.302	0.221	0.306	-0.086	-0.108	-0.107
	(0.038)	(0.048)	(0.183)	(0.203)	(0.208)	(0.183)	(0.159)	(0.148)	(0.177)	(0.161)
$\widehat{P_c}$	-0.406***	-0.027	0.346	-0.491	-0.305	-0.140	-0.069	-0.285	-0.431	-0.694*
	(0.071)	(0.101)	(0.330)	(0.389)	(0.375)	(0.324)	(0.296)	(0.332)	(0.334)	(0.296)
Obs.	1,0510	10,510	4,448	10,510	10,510	10,439	10,281	10,165	10,262	10,068
Estimator	OLS	OLS	OLS	Probit	Probit	Oprob	Oprob	Oprob	Oprob	Oprob

Note. The table reports OLS, Probit and Ordered Probit estimates obtained by regressing Model 3 on the outcome variables. The estimation sample includes only individuals with wages up to +20% of the 2004 NMW level of £4.5. Individual-level and establishment-level controls are included in all models but not reported. Robust weighted standard errors are reported in parentheses (2004 adjusted employee weights are applied to the 1998 cross-section). Industry-year FE, Region-year FE and controls are included in all specifications. Significance levels: *≤0.01, **≤0.05,* ≤0.1.**

Table 11 reports the results obtained when Model 3 is estimated on the whole sample of individuals. In this case we see that only when using the *Cell2* definition the model correctly identifies the

[17] The only exception is the positive and significant coefficient on *etime* when we construct cells according to the *Cell2* definition. However, this result is not confirmed by the results based on alternative definitions of cells.

positive impact of the NMW on wage growth. This is likely to be caused by the presence of high-wage individuals in cells with a low probability of treatment. These are the same individuals included in the fast growing top percentiles of the wage distribution (see Figure 6- Normalized growth rate of the hourly wage between 1998 and 2004 at the level of individual percentiles). With the exception of some evidence of a significant effect on hours worked, regressions on the whole sample largely confirm the absence of a significant impact of the NMW on the outcomes of interest.

Table 11- Difference in Differences on cells (sample with all individuals)

	(1) *hwage*	(2) *hour*	(3) *oratio*	(4) *dover*	(5) *dtrain*	(6) *ctrain*	(7) *taskdis*	(8) *pacedis*	(9) *hard*	(10) *entime*
					Cell1					
$\widehat{P_c} \times D_{2004}$	0.096	0.147**	0.112	0.056	-0.116	-0.075	0.117	0.138	-0.157	-0.183
	(0.051)	(0.053)	(0.267)	(0.180)	(0.209)	(0.177)	(0.134)	(0.129)	(0.163)	(0.139)
D_{2004}	0.286***	-0.053	-0.184	-0.090	0.276*	0.086	0.264**	-0.019	0.067	0.002
	(0.040)	(0.043)	(0.102)	(0.113)	(0.120)	(0.109)	(0.087)	(0.080)	(0.097)	(0.094)
$\widehat{P_c}$	-0.346***	-0.156**	0.222	-0.590**	-0.371	-0.396*	0.083	0.048	-0.396*	-0.410**
	(0.055)	(0.060)	(0.195)	(0.184)	(0.206)	(0.189)	(0.147)	(0.141)	(0.164)	(0.149)
Obs.	37,283	44,663	21,696	44,663	44,663	44,491	44,208	43,965	44,079	43,667
					Cell2					
$\widehat{P_c} \times D_{2004}$	0.427**	0.078	0.710	0.204	0.581	0.509	0.817*	0.224	0.802	0.379
	(0.149)	(0.113)	(0.507)	(0.424)	(0.585)	(0.490)	(0.348)	(0.351)	(0.418)	(0.334)
D_{2004}	0.226***	-0.059	-0.294*	-0.139	0.126	-0.054	0.164	-0.022	-0.097	-0.119
	(0.045)	(0.049)	(0.126)	(0.131)	(0.148)	(0.131)	(0.101)	(0.093)	(0.115)	(0.105)
$\widehat{P_c}$	-0.338***	0.053	0.146	-0.245	-0.033	0.110	-0.305	-0.156	-0.430	-0.064
	(0.099)	(0.085)	(0.275)	(0.301)	(0.367)	(0.326)	(0.247)	(0.242)	(0.276)	(0.230)
Obs.	37,283	44,663	21,696	44,663	44,663	44,491	44,208	43,965	44,079	43,667
					Cell3					
$\widehat{P_c} \times D_{2004}$	0.066	0.112*	-0.030	0.039	-0.101	-0.264	-0.286	-0.003	-0.161	-0.038
	(0.054)	(0.049)	(0.234)	(0.181)	(0.199)	(0.171)	(0.148)	(0.147)	(0.164)	(0.146)
D_{2004}	0.270***	-0.055	-0.153	-0.125	0.251*	0.083	0.334***	0.008	0.044	-0.045
	(0.040)	(0.043)	(0.098)	(0.113)	(0.120)	(0.110)	(0.087)	(0.080)	(0.097)	(0.096)
$\widehat{P_c}$	-0.080	-0.055	0.242	-0.212	-0.261	-0.049	-0.313	-0.113	-0.355	-0.275
	(0.077)	(0.067)	(0.214)	(0.236)	(0.262)	(0.235)	(0.205)	(0.190)	(0.203)	(0.191)
Obs.	37,283	44,663	21,696	44,663	44,663	44,491	44,208	43,965	44,079	43,667
Estimator	OLS	OLS	OLS	Probit	Probit	Oprob	Oprob	Oprob	Oprob	Oprob

Note. The table reports OLS, Probit and Ordered Probit estimates obtained by regressing Model 3 on the outcome variables. The estimation sample includes pooled 1998 and 2004 individuals from the whole wage distribution. Individual-level and establishment-level controls are included in all models but not reported. Robust weighted standard errors are reported in parentheses (2004 adjusted employee weights are applied to the 1998 cross-section). Industry-year FE, Region-year FE and controls are included in all specifications. Significance levels: *≤0.01, **≤0.05,* ≤0.1.**

Lastly, we validate our strategy by conducting a placebo exercise. For each cell we construct a `wrong' estimator of the probability of treatment $\widehat{W_c}$ obtained as the ratio of individuals with hourly wages equal to or greater than the 2004 NMW level and smaller than +20% this level, over all eligible individuals. This ratio is the probability that a 2004 individual in a particular cell is just not-treated. We then set the estimation sample to include all individuals with up to +40% of the NMW. Results from this placebo exercise are reported in Table 12. The negative (or non-significant) coefficient on *hwage* reassures us that the positive coefficient that we found in previous estimates is not driven by issues arising from our empirical design. Hence, this placebo experiment seems to support our empirical strategy, dismissing the concern that the estimated probability of treatment is correlated with unobserved cell-level time varying factors affecting the outcome variables.

Table 12 - Placebo exercise (sample with individuals up to +40% NMW)

	(1) *hwage*	(2) *hour*	(3) *oratio*	(4) *dover*	(5) *dtrain*	(6) *ctrain*	(7) *taskdis*	(8) *pacedis*	(9) *hard*	(10) *entime*
					Cell1					
$\widehat{W_c} \times D_{2004}$	-0.318**	0.273*	0.530	0.455	-0.033	0.365	0.764	0.448	-0.122	0.529
	(0.112)	(0.119)	(0.602)	(0.551)	(0.559)	(0.480)	(0.462)	(0.429)	(0.440)	(0.446)
D_{2004}	0.214***	-0.002	-0.162	0.071	0.206	0.078	-0.022	-0.239	-0.075	-0.228
	(0.036)	(0.046)	(0.159)	(0.194)	(0.187)	(0.160)	(0.153)	(0.143)	(0.158)	(0.149)
$\widehat{W_c}$	0.641***	0.323***	-1.567***	1.248**	0.204	-0.157	-0.674	-0.657	0.404	0.894*
	(0.085)	(0.096)	(0.419)	(0.420)	(0.422)	(0.377)	(0.361)	(0.346)	(0.360)	(0.389)
Obs.	14,224	14,224	6,194	14,224	14,224	14,134	13,952	13,818	13,922	13,699
					Cell2					
$\widehat{W_c} \times D_{2004}$	-0.294	-0.222	0.685	0.153	-0.817	-0.583	0.866	0.158	-0.136	0.616
	(0.155)	(0.180)	(0.805)	(0.844)	(0.904)	(0.794)	(0.696)	(0.628)	(0.645)	(0.736)
D_{2004}	0.200***	0.060	-0.157	0.096	0.314	0.213	-0.016	-0.189	-0.064	-0.241
	(0.037)	(0.048)	(0.161)	(0.205)	(0.195)	(0.168)	(0.156)	(0.144)	(0.162)	(0.166)
$\widehat{W_c}$	0.340***	-0.040	-0.796	-0.179	0.652	0.258	-0.567	-0.340	0.786	-0.061
	(0.097)	(0.100)	(0.418)	(0.503)	(0.567)	(0.483)	(0.391)	(0.398)	(0.405)	(0.510)
Obs.	14,224	14,224	6,194	14,224	14,224	14,134	13,952	13,818	13,922	13,699
					Cell3					
$\widehat{W_c} \times D_{2004}$	-0.043	-0.022	0.348	0.988	-0.819	-0.170	0.668	1.036*	-0.532	-0.660
	(0.121)	(0.123)	(0.601)	(0.646)	(0.660)	(0.576)	(0.478)	(0.476)	(0.535)	(0.490)
D_{2004}	0.158***	0.038	-0.088	-0.017	0.313	0.160	0.010	-0.306*	-0.022	-0.072
	(0.036)	(0.046)	(0.153)	(0.199)	(0.187)	(0.161)	(0.148)	(0.147)	(0.163)	(0.151)
$\widehat{W_c}$	0.434***	-0.115	-1.032*	-0.713	0.393	-0.088	-0.386	-0.571	-0.098	0.563
	(0.111)	(0.126)	(0.452)	(0.546)	(0.598)	(0.554)	(0.406)	(0.424)	(0.466)	(0.426)
Obs.	14,224	14,224	6,194	14,224	14,224	14,134	13,952	13,818	13,922	13,699
Estimator	OLS	OLS	OLS	Probit	Probit	Oprob	Oprob	Oprob	Oprob	Oprob

Note. The table reports OLS, Probit and Ordered Probit estimates obtained by regressing Model 3 on the outcome variables The estimation sample includes only individuals with wages up to +40% of the 2004 NMW level of £4.5. Individual-level and establishment-level controls are included in all models but not reported. Robust weighted standard errors are reported in parentheses (2004 adjusted employee weights are applied to the 1998 cross-section). Industry-year FE, Region-year FE and controls are included in all specifications. Significance levels: *≤0.01, **≤0.05,* ≤0.1.**

5.2 Results from the establishment-level analysis using WERS

This section reports estimates obtained by regressing model (4) on the restricted sample of establishments that appear in both the 1998 and 2004 WERS waves. We report results using two measures of the establishment coverage of the NMW (referred to as `bite' in the tables). One is reported by managers and the other estimated from employee-level data; see section 2.3. Results obtained by using the estimated rate of low-paid workers are reported in the lower panel of each table.

Table 13 reports estimates on occupation shares. The negative coefficient of *Bite4* in the regression on *Docc9* suggests that establishments most affected by NMW experienced a greater reduction in the share of routine unskilled workers. When regressions include measures of `bite' for full-time or part-time employees, we find that the reduction in the share of this occupational group is mostly explained by a reduction in the share of full-time workers. While the most affected establishments reduced the share of routine unskilled occupations, they increased the share of professional workers. These results point to the possibility that the NMW may have had an impact on the employment structure of companies. However, this result is not robust to the use of the `bite' measure based on employees reported wages. When using this variable to identify the most affected establishments,

the effect on *Docc9* is much smaller in absolute terms and insignificant (albeit still negative). When using this variable, we find instead a significant negative effect on the share of operative (*Docc8*) and sales (*Docc7*) workers but a positive effect on the shares of skilled (*Docc5*) and clerical workers (*Docc4*). Nevertheless, this last set of results is still consistent with the hypothesis that firms with a larger share of workers affected by the introduction of the NMW, moved toward a more skill-intensive employment structure.

Table 13 - Models on changes in occupation shares (OLS)

	(1) *Docc1*	(2) *Docc2*	(3) *Docc3*	(4) *Docc4*	(5) *Docc5*	(6) *Docc6*	(7) *Docc7*	(8) *Docc8*	(9) *Docc9*
				`Bite' on all employees (reported)					
Bite4	0.017	0.082*	0.015	0.027	-0.019	0.118	-0.124*	0.010	-0.136**
	(0.023)	(0.032)	(0.025)	(0.031)	(0.037)	(0.069)	(0.051)	(0.031)	(0.044)
ΔEMP	-0.044**	-0.014	0.030	-0.016	-0.043	0.027	0.026	0.016	0.024
	(0.014)	(0.014)	(0.016)	(0.016)	(0.026)	(0.027)	(0.015)	(0.009)	(0.026)
Obs.	860	859	859	860	858	858	857	856	857
				`Bite' on full-time employees only (reported)					
Bite4ft	0.016	0.078*	-0.001	0.040	0.008	0.073	-0.058	0.018	-0.180***
	(0.025)	(0.031)	(0.025)	(0.027)	(0.040)	(0.086)	(0.058)	(0.033)	(0.051)
ΔEMP	-0.045**	-0.015	0.027	-0.018	-0.042	0.032	0.028	0.015	0.024
	(0.014)	(0.014)	(0.016)	(0.016)	(0.026)	(0.027)	(0.015)	(0.008)	(0.026)
Obs.	864	862	864	863	861	861	860	859	861
				`Bite' on part-time employees only (reported)					
Bite4pt	-0.016	0.040	-0.004	0.004	-0.014	0.086	-0.034	-0.023	-0.051
	(0.019)	(0.026)	(0.024)	(0.030)	(0.030)	(0.047)	(0.034)	(0.025)	(0.030)
ΔEMP	-0.054***	-0.016	0.029	-0.038*	-0.031	0.032	0.030	0.024*	0.033
	(0.015)	(0.018)	(0.019)	(0.016)	(0.030)	(0.031)	(0.017)	(0.010)	(0.030)
Obs.	748	747	747	748	746	746	745	744	745
				`Bite' on all employees (estimated from employee-level data)					
Bite4pt	0.007	0.080	-0.076	0.097**	0.158**	0.067	-0.192***	-0.083*	-0.044
	(0.038)	(0.052)	(0.051)	(0.048)	(0.072)	(0.086)	(0.061)	(0.045)	(0.058)
ΔEMP	-0.043***	-0.015	0.035**	-0.024	-0.056*	0.029	0.034**	0.014	0.031
	(0.016)	(0.015)	(0.017)	(0.016)	(0.030)	(0.034)	(0.016)	(0.010)	(0.032)
Obs.	658	658	658	658	658	658	658	658	658

Note. The table reports OLS estimates of Model 4 on occupation shares. The estimation sample includes all establishments covered both by the 1998 and the 2004 WERS waves. Establishment-level controls, industry-year and sector-year FE are included in all models but not reported. Occupational groups: managers and senior administration (occ1), professional (occ2), technical (occ3), clerical and secretarial (occ4), craft and skilled services (occ5), protective and personal services (occ6), sales (occ7), operative and assembly (occ8), routine unskilled (occ9). Robust weighted standard errors are reported in parentheses (2004 adjusted establishment weights are applied to 1998 data). Significance levels: *≤0.01, **≤0.05,* ≤0.1.**

In Table 14 we present estimates from Probit models on an establishment's propensity to outsource tasks that were previously performed internally. We find a positive albeit weakly significant coefficient of *Bite4* in the regression on the outsourcing of training. Except for this effect, we do not find evidence that the NMW resulted in the outsourcing of tasks. On the contrary, we find a negative coefficient on the employee-based `bite' variable on the probability of outsourcing payroll tasks (*OutPay*). It is difficult to interpret this result in the light of a possible productivity-enhancing impact of the NMW.

Table 14 -Models on outsourcing of tasks (Probit)

	(1) *OutCle*	(2) *OutSec*	(3) *OutCat*	(4) *OutMai*	(5) *OutPri*	(6) *OutPay*	(7) *OutTra*	(8) *OutCom*	(9) *OutTrn*
					`Bite' on all employees (reported)				
Bite4	-0.669	0.177	-0.160	-0.699	0.500	-0.291	0.466	-0.426	0.766*
	(0.393)	(0.405)	(0.506)	(0.416)	(0.411)	(0.507)	(0.373)	(0.436)	(0.372)
ΔEMP	0.081	0.079	-0.051	-0.026	0.021	-0.038	0.141	-0.014	0.276
	(0.162)	(0.144)	(0.136)	(0.125)	(0.142)	(0.152)	(0.158)	(0.142)	(0.143)
Obs.	842	863	826	863	863	863	863	863	863
					`Bite' on all employees (estimated from employee-level data)				
Bite4	-0.411	0.544	-0.627	-0.377	0.639	-2.286**	0.435	-0.464	-0.310
	(0.780)	(0.644)	(0.825)	(0.622)	(0.827)	(0.956)	(0.636)	(0.893)	(0.621)
ΔEMP	0.140	-0.129	-0.038	0.003	-0.114	-0.227	0.284	-0.156	0.219
	(0.186)	(0.156)	(0.155)	(0.137)	(0.155)	(0.175)	(0.190)	(0.144)	(0.157)
Obs.	646	666	635	666	650	636	666	666	666

Note. The table reports Probit estimates of Model 4 on outsourcing dummies. The estimation sample includes all establishments covered both by the 1998 and the 2004 WERS waves. Establishment-level controls, industry-year and sector-year FE are included in all models but not reported. Robust weighted standard errors are reported in parentheses (2004 adjusted establishment weights are applied to 1998 data). The tasks reported in columns are OutCle (clerical), OutSec (Security), OutCat (Catering), OutMai (Maintainance), OutPri (Printing and Publishing), OutPay (payroll), OutTra (transportation), OutCom (IT services), OutCom (communication), OutTrn (transport). Significance levels: *≤0.01, **≤0.05,* ≤0.1.**

A possible channel through which NMW may have induced productivity gains is by encouraging firms to apply more stringent criteria for recruiting new workers. We test for this channel by estimating models on the probability of reporting each of the listed workers' attributes, deemed unimportant for recruitment in 1998 and important in 2004. In Table 15, the results based on the reported `bite' variable do not suggest any significant impact of the NMW on the recruitment of new employees. On the contrary, when using the employee-based `bite' we find some significant effects on motivation (*RecMot*) and skills (*RecSki*) but negative effects on reference (*RecRef*) and experience (*RecExp*).

Table 15- Models on recruitment criteria (Probit)

	(1) *RecRef*	(2) *RecAva*	(3) *RecExp*	(4) *RecSki*	(5) *RecAge*	(6) *RecQua*	(7) *RecExp*	(8) *RecMot*
					`Bite' on all employees (reported)			
Bite4	-0.735	0.456	0.103	0.073	0.071	0.025	0.327	0.063
	(0.435)	(0.364)	(0.414)	(0.392)	(0.513)	(0.404)	(0.281)	(0.434)
ΔEMP	0.002	0.129	0.054	0.287	0.038	0.038	0.026	0.240
	(0.166)	(0.153)	(0.172)	(0.183)	(0.174)	(0.160)	(0.141)	(0.190)
Obs.	863	863	863	842	805	842	842	803
					`Bite' on all employees (estimated from employee-level data)			
Bite4	-1.220*	0.952	-1.012*	1.145*	0.711	0.187	-0.340	1.572**
	(0.675)	(0.616)	(0.604)	(0.645)	(0.677)	(0.625)	(0.677)	(0.741)
ΔEMP	-0.018	0.301	0.065	0.031	0.099	-0.117	-0.067	0.064
	(0.183)	(0.187)	(0.195)	(0.159)	(0.216)	(0.182)	(0.161)	(0.179)
Obs.	666	666	646	646	541	646	618	635

Note. The table reports Probit estimates of Model 4 on recruitment requirements dummies. The estimation sample includes all establishments covered both by the 1998 and the 2004 WERS waves. Establishment-level controls, industry-year and sector-year FE are included in all models but not reported. Robust weighted standard errors are reported in parentheses (2004 adjusted establishment weights are applied to 1998 data).). Recruitment criteria reported in columns are RecRef (reference), RecAva (availability), RecSki (skills), RecAge (age), RecQua (qualification), RecExp (experience), RecMot (motivation). Significance levels: *≤0.01, **≤0.05,* ≤0.1.**

Lastly, we test whether establishments that were more affected by the policy increased the amount of training provided. Although, the finding from the employee-level analysis does not support this hypothesis for minimum-wage workers, it is possible that the introduction of the NMW had an impact on the amount of training received by workers positioned at a higher point of the wage distribution. Because the wording of the WERS survey question refers to the proportion of trained workers in the largest occupational group, we need to allow for the coefficient on *bite4* to vary across firms with different largest occupational groups. We do so by interacting *bite4* with dummies

identifying firms with different largest occupational group. Table 16 reports estimates from ordered probit models on changes in the proportion of trained workers.[18] Consistent with previous findings on employee-level data, we cannot find any strong evidence of differential change in the amount of training for companies that were most affected by the NMW. This negative result is consistent with the employee-based `bite' variable.

Table 16- Models on training (oprobit)

	(1) ΔOff_{98-04}	(2) ΔOth_{98-04}
`Bite' on all employees (reported)		
Bite4	0.498	1.548
	(2.151)	(2.259)
Occ1× *Bite4*	-1.214	-1.456
	(2.228)	(2.331)
Occ2× *Bite4*	-2.179	-2.986
	(2.434)	(2.440)
Occ3× *Bite4*	-1.420	-2.339
	(2.211)	(2.402)
Occ4× *Bite4*	-1.175	-1.112
	(2.279)	(2.401)
Occ5×*Bite4*	-0.070	-4.815
	(1.505)	(2.181)
Occ6× *Bite4*	-0.696	-2.497
	(2.180)	(2.230)
Occ7×*Bite4*	-0.439	-0.057
	(2.146)	(2.232)
Occ8× *Bite4*	0.253	-1.094
	(2.352)	(2.359)
Occ9× *Bite4*	-0.262	-2.179
	(2.242)	(2.297)
ΔEMP	0.117	0.118
	(0.105)	(0.094)
Obs.	835	827
`Bite' on all employees (estimated from employee-level data)		
Bite4	-1.564	-6.852*
	(1.899)	(3.534)
Occ1× Bite4	-0.830	5.957
	(2.190)	(3.714)
Occ2× *Bite4*	-2.179	-2.986
	(2.434)	(2.440)
Occ3× *Bite4*	-1.420	-2.339
	(2.211)	(2.402)
Occ4× *Bite4*	-1.175	-1.112
	(2.279)	(2.401)
Occ5×*Bite4*	-0.070	-4.815
	(1.505)	(2.181)
Occ6× *Bite4*	-0.696	-2.497
	(2.180)	(2.230)
Occ7×*Bite4*	-0.439	-0.057
	(2.146)	(2.232)
Occ8× *Bite4*	0.253	-1.094
	(2.352)	(2.359)
Occ9× *Bite4*	-0.262	-2.179
	(2.242)	(2.297)
ΔEMP	0.117	0.118
	(0.105)	(0.094)
Obs.	642	638

Note. The table reports Ordered Probit estimates of Model 4 on 1998-2004 changes in the shares of workers in largest of occupational group of the company receiving training. The estimation sample includes all establishments covered both by the 1998 and the 2004 WERS waves. Establishment-level controls, industry-year and sector-year FE are included in all models but not reported. Robust weighted standard errors are reported in parentheses (2004 adjusted establishment weights are applied to 1998 data). Significance levels: *≤0.01, **≤0.05,* ≤0.1.**

[18] Because the proportion of trained workers is reported within bands, we take the difference of the mid-point of each band in 1998 and 2004. This generates a categorical variable with a limited number of values that we analyse using Ordered Probit models.

5.3 Results from the employee-level analysis using LFS

In this section, we present the results obtained by estimating model (5) and model (6) on LFS data. We split the whole sample of employees that are eligible for the adult rate in different subsamples according to workers' gender and workplace size (small: up to 49 employees, large: at least 50 employees). By running regressions on different subsamples we allow the estimated parameters to register heterogeneous effects of the NMW for different groups of workers (e.g., Dickens et al., 2015). We compare results from the sample of individuals with reported HRATE with results from the sample with imputed HRATE (i.e., all individuals with observed HPAY). Finally, we report estimates of the effect of individual upratings and pooled estimates of the joint effect of all upratings.

We report results from two specifications of model (6). The first specification is a classical difference-in-difference model where treated individuals are identified by means of a bivariate treatment variable. The second specification is the `wage-gap' estimator where we also capture heterogeneity in the treatment across treated individuals with different initial wages.

Table 17-The effect of individual upratings on training incidence (by gender and hourly wage variable)

	Male (HRATE)		Female (HRATE)		Male (IMPUTED)		Female(IMPUTED)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
$YS_i * T_i$ (2000)	0.024	-0.065	-0.096	-0.110	-0.071	-0.093	-0.021	-0.020
	(0.13)	(0.12)	(0.09)	(0.09)	(0.07)	(0.07)	(0.05)	(0.04)
$YS_i * T_i$ (2001)	-0.002	0.012	-0.019	-0.026	0.015	0.026	-0.001	0.000
	(0.09)	(0.09)	(0.05)	(0.04)	(0.06)	(0.06)	(0.03)	(0.03)
$YS_i * T_i$ (2002)	0.020	0.024	-0.181***	-0.177***	-0.057	-0.038	-0.103**	-0.095**
	(0.11)	(0.11)	(0.06)	(0.06)	(0.06)	(0.07)	(0.04)	(0.04)
$YS_i * T_i$ (2003)	0.048	-0.034	0.045	0.041	-0.031	-0.061	0.014	0.015
	(0.10)	(0.09)	(0.05)	(0.04)	(0.06)	(0.05)	(0.03)	(0.03)
$YS_i * T_i$ (2004)	-0.133	-0.127	-0.093**	-0.092**	0.039	0.033	-0.014	-0.006
	(0.08)	(0.08)	(0.04)	(0.04)	(0.05)	(0.05)	(0.03)	(0.03)
$YS_i * T_i$ (2005)	-0.046	-0.068	0.001	-0.007	0.042	0.016	-0.014	-0.013
	(0.07)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.02)
$YS_i * T_i$ (2006)	0.092	0.087	-0.037	-0.057	-0.022	-0.021	0.002	-0.009
	(0.07)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)
$YS_i * T_i$ (2007)	-0.048	-0.027	0.057	0.043	-0.049	-0.028	-0.007	-0.012
	(0.06)	(0.06)	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)
$YS_i * T_i$ (2008)	0.117	0.116	0.063	0.047	0.055	0.036	0.045*	0.042
	(0.07)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
$YS_i * T_i$ (2009)	0.021	-0.027	0.032	-0.013	-0.019	-0.035	0.005	-0.007
	(0.09)	(0.08)	(0.05)	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)
$YS_i * T_i$ (2010)	0.054	0.012	0.018	-0.003	-0.046	-0.065	-0.021	-0.028
	(0.09)	(0.08)	(0.05)	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)
$YS_i * T_i$ (2011)	0.104	0.091	0.029	-0.008	0.054	0.049	0.038	0.027
	(0.07)	(0.07)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
$YS_i * T_i$ (2012)	-0.127	-0.120	-0.102**	-0.078	-0.015	-0.011	0.013	0.014
	(0.08)	(0.08)	(0.05)	(0.05)	(0.05)	(0.05)	(0.03)	(0.03)
$YS_i * T_i$ (2013)	0.120	0.157*	-0.019	-0.036	0.008	0.039	0.007	-0.003
	(0.09)	(0.09)	(0.06)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)
Obs.	28,901	28,678	43,988	43,737	63,112	62,698	85,819	85,408

Note. The table reports OLS estimates on a dummy for training incidence in the last three months conditional on employment status. Each line reports a year-specific DiD coefficient on the interaction between the "treat" dummy and the "YS" dummy for transitions spanning the up-rating. Robust standard errors reported in parentheses. The estimation sample includes groups L, T, U. Significance levels: ***0.01, **0.05, *0.1 .

Table 18- The effect of individual upratings on changes in absenteeism (by gender and hourly wage variable)

	Male (HRATE)		Female (HRATE)		Male (IMPUTED)		Female(IMPUTED)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
$YS_i * T_i$ (2000)	0.002	-0.002	-0.069	-0.070	-0.005	-0.006	-0.039	-0.039
	(0.02)	(0.03)	(0.06)	(0.06)	(0.04)	(0.04)	(0.04)	(0.04)
$YS_i * T_i$ (2001)	-0.109*	-0.108*	-0.003	-0.001	-0.050	-0.049	-0.006	-0.006
	(0.06)	(0.06)	(0.03)	(0.03)	(0.05)	(0.05)	(0.02)	(0.02)
$YS_i * T_i$ (2002)	-0.080	-0.079	-0.005	-0.014	0.014	0.016	0.006	0.002
	(0.09)	(0.09)	(0.05)	(0.05)	(0.09)	(0.09)	(0.04)	(0.04)
$YS_i * T_i$ (2003)	0.124**	0.121**	-0.027	-0.029	0.054	0.051	-0.028	-0.028
	(0.06)	(0.06)	(0.03)	(0.03)	(0.05)	(0.05)	(0.02)	(0.02)
$YS_i * T_i$ (2004)	-0.086	-0.085	-0.002	-0.001	-0.063	-0.062	-0.009	-0.009
	(0.05)	(0.06)	(0.03)	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)
$YS_i * T_i$ (2005)	-0.039	-0.039	-0.048**	-0.048**	-0.039	-0.038	-0.030	-0.030
	(0.04)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
$YS_i * T_i$ (2006)	0.002	0.000	-0.007	-0.012	0.014	0.011	-0.011	-0.013
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
$YS_i * T_i$ (2007)	-0.012	-0.004	-0.026	-0.025	-0.022	-0.016	-0.028	-0.028
	(0.04)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
$YS_i * T_i$ (2008)	0.017	0.016	0.011	0.012	0.025	0.024	0.002	0.003
	(0.04)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
$YS_i * T_i$ (2009)	-0.014	-0.009	0.047	0.045	-0.001	0.002	0.039	0.037
	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
$YS_i * T_i$ (2010)	-0.027	-0.037	0.033	0.032	-0.026	-0.029	0.024	0.025
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
$YS_i * T_i$ (2011)	-0.004	-0.005	0.009	0.011	-0.018	-0.016	-0.016	-0.015
	(0.04)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
$YS_i * T_i$ (2012)	0.022	0.017	0.040	0.041	-0.004	-0.007	0.042*	0.043*
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.02)	(0.02)
$YS_i * T_i$ (2013)	-0.017	-0.013	-0.021	-0.016	-0.020	-0.023	-0.012	-0.010
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)
Obs.	28,947	28,719	44,056	43,800	63,106	62,692	85,809	85,398

Note. The table reports OLS estimates on the change in an individual's abs_rate from wave1 to wave_3. Each line reports a year-specific DiD coefficient on the interaction between the "treat" dummy and the "YS" dummy for transitions spanning the up-rating. Robust standard errors reported in parentheses. The estimation sample includes groups L, T, U. Significance levels: ***0.01, **0.05, *0.1..

Table 17 and Table 18 respectively refer to the effect of individual upratings on training incidence (in the last three months), and changes in absenteeism.[19] Samples are initially split by gender. In each table, Columns 1 to 4 refer to the sample of individuals with reported HRATE, while Columns 5 to 8 refer to the sample of individuals with imputed HRATE. We report estimates obtained with and without individual-level controls and fixed effects on the right-hand side of equation (5).

Across upratings, the point estimates on training incidence are often negative, although they are largely insignificant. Results are qualitatively similar when we use the imputed HRATE instead of the reported one. Overall, our estimates are in line with the previous results in Dickerson (2007) but they contrast with the positive effect of the NMW on training found by Arulampalam et al. (2004) with the BHPS. Similarly, regressions on absenteeism generate inconclusive results. Point estimates of the effect of individual upratings change sign over time. We mostly obtain negative estimates until the

[19] In this section we do not report estimated of model 5 and 6 on the alternative training LFS variable ED4WK (training in the last month). Results on this outcome are virtually identical to that reported for ED13WK.

2006 uprating, and more mixed effects after that point in time. With few exceptions estimates are insignificant at the usual confidence levels. In general, it is difficult to infer from these results that the policy had a statistically significant impact on the provision of training or on the rate of absenteeism. In Table 19 and Table 20 we replicate the analysis by splitting the samples according to the size of the employees' workplaces. These results do not alter our conclusions.

Table 19- The effect of individual upratings on training incidence (last three months) by workplace size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male Large		Female Large		Male Small		Female Small	
Controls	No	Yes	No	Yes	No	Yes	No	Yes
$YS_i * T_i$ (2000)	0.134	0.041	-0.159	-0.264	-0.030	-0.079	-0.083	-0.077
	(0.09)	(0.10)	(0.17)	(0.18)	(0.17)	(0.17)	(0.10)	(0.10)
$YS_i * T_i$ (2001)	0.017	0.062	-0.095	-0.028	0.010	0.007	0.033	-0.001
	(0.16)	(0.16)	(0.09)	(0.09)	(0.12)	(0.12)	(0.06)	(0.05)
$YS_i * T_i$ (2002)	-0.029	-0.089	-0.245**	-0.212**	0.026	0.075	-0.129*	-0.138**
	(0.21)	(0.20)	(0.11)	(0.11)	(0.07)	(0.08)	(0.07)	(0.07)
$YS_i * T_i$ (2003)	0.045	-0.017	0.010	0.015	0.044	-0.085	0.080	0.061
	(0.15)	(0.15)	(0.07)	(0.07)	(0.13)	(0.12)	(0.06)	(0.06)
$YS_i * T_i$ (2004)	-0.186*	-0.186*	-0.100*	-0.092	-0.022	-0.040	-0.086	-0.088
	(0.11)	(0.11)	(0.06)	(0.06)	(0.12)	(0.13)	(0.06)	(0.05)
$YS_i * T_i$ (2005)	-0.015	-0.053	-0.008	-0.024	-0.038	-0.054	0.027	0.013
	(0.10)	(0.09)	(0.06)	(0.06)	(0.11)	(0.12)	(0.06)	(0.06)
$YS_i * T_i$ (2006)	0.054	0.073	-0.040	-0.058	0.154*	0.136	-0.037	-0.049
	(0.09)	(0.10)	(0.06)	(0.05)	(0.09)	(0.09)	(0.05)	(0.05)
$YS_i * T_i$ (2007)	-0.050	-0.024	0.030	0.026	-0.047	-0.038	0.095	0.071
	(0.08)	(0.08)	(0.06)	(0.05)	(0.11)	(0.10)	(0.06)	(0.06)
$YS_i * T_i$ (2008)	0.240**	0.248**	0.028	0.037	-0.059	-0.079	0.101	0.058
	(0.10)	(0.10)	(0.06)	(0.06)	(0.12)	(0.10)	(0.06)	(0.06)
$YS_i * T_i$ (2009)	-0.008	-0.046	0.103	0.052	0.047	-0.029	-0.038	-0.077
	(0.11)	(0.11)	(0.08)	(0.08)	(0.12)	(0.12)	(0.06)	(0.06)
$YS_i * T_i$ (2010)	0.127	0.064	-0.047	-0.073	-0.045	-0.042	0.068	0.052
	(0.12)	(0.11)	(0.08)	(0.07)	(0.13)	(0.12)	(0.06)	(0.06)
$YS_i * T_i$ (2011)	0.073	0.105	0.063	0.035	0.171	0.103	-0.003	-0.049
	(0.11)	(0.10)	(0.07)	(0.07)	(0.11)	(0.10)	(0.06)	(0.06)
$YS_i * T_i$ (2012)	-0.444***	-0.411***	-0.152*	-0.152**	0.139	0.133	-0.060	-0.025
	(0.11)	(0.10)	(0.08)	(0.08)	(0.12)	(0.11)	(0.06)	(0.06)
$YS_i * T_i$ (2013)	0.091	0.096	-0.044	-0.089	0.130	0.193	-0.025	-0.021
	(0.12)	(0.12)	(0.09)	(0.08)	(0.13)	(0.12)	(0.08)	(0.07)
Obs	19,036	18,902	24,083	23,948	9,865	9,776	19,905	19,789

Note. The table reports OLS estimates on a dummy for training incidence in the last three months conditional on employment status. Each line reports a year-specific DiD coefficient on the interaction between the "treat" dummy and the "YS" dummy for transitions spanning the up-rating. Robust standard errors reported in parentheses. Small enterprises <50 employees. Large enterprises >49 employees. The estimation sample includes groups L, T, U. Significance levels: ***0.01, **0.05, *0.1.

Table 20-The effect of individual upratings on absenteeism by gender and workplace size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male Large		Female Large		Male Small		Female Small	
Controls	No	Yes	No	Yes	No	Yes	No	Yes
$YS_i * T_i$ (2000)	-0.014	-0.016	-0.156	-0.186*	0.006	0.017	-0.046	-0.043
	(0.04)	(0.05)	(0.10)	(0.11)	(0.03)	(0.03)	(0.06)	(0.06)
$YS_i * T_i$ (2001)	-0.105	-0.104	-0.020	-0.014	-0.102	-0.103	0.011	0.014
	(0.11)	(0.11)	(0.05)	(0.05)	(0.08)	(0.08)	(0.03)	(0.03)
$YS_i * T_i$ (2002)	-0.115	-0.121	-0.124	-0.127	-0.078	-0.064	0.060*	0.050*
	(0.19)	(0.19)	(0.12)	(0.12)	(0.05)	(0.05)	(0.03)	(0.03)
$YS_i * T_i$ (2003)	0.177*	0.168*	-0.025	-0.029	0.058	0.041	-0.021	-0.023
	(0.10)	(0.10)	(0.05)	(0.05)	(0.06)	(0.06)	(0.04)	(0.04)
$YS_i * T_i$ (2004)	-0.106*	-0.107*	0.024	0.022	-0.046	-0.042	-0.031	-0.030
	(0.06)	(0.06)	(0.04)	(0.04)	(0.10)	(0.10)	(0.04)	(0.04)
$YS_i * T_i$ (2005)	-0.017	-0.015	-0.062**	-0.060**	-0.043	-0.052*	-0.031	-0.029
	(0.05)	(0.06)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
$YS_i * T_i$ (2006)	0.001	-0.003	0.029	0.032	0.007	0.009	-0.042	-0.051
	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
$YS_i * T_i$ (2007)	0.016	0.031	-0.067**	-0.070**	-0.053	-0.060	0.023	0.027
	(0.05)	(0.05)	(0.03)	(0.03)	(0.05)	(0.05)	(0.03)	(0.03)
$YS_i * T_i$ (2008)	-0.015	-0.019	0.024	0.027	0.056	0.061	-0.005	-0.007
	(0.03)	(0.03)	(0.03)	(0.03)	(0.06)	(0.07)	(0.03)	(0.03)
$YS_i * T_i$ (2009)	-0.003	-0.001	0.081	0.075	-0.012	-0.013	0.025	0.026
	(0.09)	(0.09)	(0.06)	(0.06)	(0.04)	(0.04)	(0.03)	(0.03)
$YS_i * T_i$ (2010)	-0.053	-0.071	0.070	0.069	0.006	-0.003	0.006	0.007
	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)
$YS_i * T_i$ (2011)	-0.029	-0.032	0.017	0.018	0.017	0.015	0.018	0.021
	(0.06)	(0.06)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
$YS_i * T_i$ (2012)	-0.000	0.000	0.105**	0.100**	0.042**	0.027	-0.017	-0.014
	(0.08)	(0.08)	(0.05)	(0.05)	(0.02)	(0.02)	(0.04)	(0.04)
$YS_i * T_i$ (2013)	-0.003	0.008	-0.011	0.003	-0.018	-0.028	-0.035	-0.034
	(0.05)	(0.05)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)
Obs	19,064	18,929	24,131	23,993	9883	9,790	19,925	19,807

Note. The table reports OLS estimates on the change in an individual's abs_rate from wave1 to wave_3. Each line reports a year-specific DiD coefficient on the interaction between the "treat" dummy and the "YS" dummy for transitions spanning the up-rating. Robust standard errors reported in parentheses. Small enterprises <50 employees. Large enterprises >49 employees. The estimation sample includes groups L, T, U. Significance levels: ***0.01, **0.05, *0.1
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A possible explanation for these inconclusive results is the low power of the estimator when we try to identify the impact of individual upratings. Because the samples of treated and control employees across individual upratings is rather small, model (5) may fail to detect the true effect of the policy. A way to increase the power of the estimator is to jointly consider the effect of all upratings in the same regression. On the one hand, this approach does not allow for the heterogeneous effects of different upratings, on the other hand the samples of treated and control individuals are expanded considerably. Table 21 reports pooled estimates on all upratings where treated individuals are identified by the same dummy variable regardless of time. Both the pooled OLS and the Two-Stage Donald and Lang estimator identify a negative impact of the NMW on the provision of training to female workers in large establishments. Consistent with previous analysis, pooled estimates do not support the hypothesis that training provision or lower absenteeism are the main drivers of the productivity effect of the NMW.

Table 21-Pooled effect of the upratings (OLS and Two-Stage estimator)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male small		Female small		Male large		Female large	
	OLS	Two-Stage	OLS	Two-Stage	OLS	Two-Stage	OLS	Two-Stage
ED13WK	0.013	0.018	-0.019	-0.024	0.023	-0.043	-0.030*	-0.078***
	(0.029)	(0.033)	(0.014)	(0.019)	(0.029)	(0.040)	(0.017)	(0.029)
Abs_rate	-0.020	-0.013	-0.004	-0.012	-0.017	-0.026	-0.001	-0.008
	(0.014)	(0.014)	(0.009)	(0.009)	(0.017)	(0.024)	(0.011)	(0.020)

In Table 22 and Table 23 we report the results from Model 6 estimated on training and absenteeism. Each line of the two tables presents the estimates obtained by comparing the policy-off period (1997-1998) against a unique year of the policy-on period. Results are inconclusive and very similar to those reported in the previous tables. Table 24 introduces a new outcome: the change in the incidence of shift work among low-paid workers. Again, regressions comparing the policy-off period against individual policy-on periods do not identify any significant effect. Wage gap estimators of model 6 are reported in Tables 25-27. Similar to the results in Tables 22-24 these results are generally inconclusive. We note that using the wage gap estimator there appears to be a negative association between the NMW and the rate of absenteeism for men after 2007 (see Table 26). However, this association is not evident using other estimators.

Pooled estimates in Table 28, where we compare the policy-off period with the whole policy-on period, suggest that the incidence of shift work has increased faster for treated workers than for workers in the control group. This effect is very similar for both female and male workers employed in workplaces with more than 50 employees.

Table 22-Effect of the introduction and upratings on training (model 6: 'before/after' estimator)

	(1) Male	(2) Female	(3) Male Large	(4) Female Large	(5) Male Small	(6) Female Small	(7) All
did1999	-0.009	-0.005	-0.001	-0.019	-0.021	0.028	-0.006
	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)	(0.01)
Obs.	1480	4712	840	3021	640	1691	6192
did2000	-0.048*	-0.035**	-0.035	-0.071***	-0.086*	0.028	-0.040***
	(0.03)	(0.02)	(0.04)	(0.02)	(0.05)	(0.03)	(0.01)
Obs.	1232	4070	671	2582	561	1488	5302
did2001	-0.013	-0.014	0.007	-0.045*	-0.024	0.019	-0.009
	(0.03)	(0.02)	(0.03)	(0.03)	(0.07)	(0.04)	(0.02)
Obs.	1096	3513	635	2251	461	1262	4609
did2002	-0.027	-0.023	-0.049	-0.039	-0.001	0.005	-0.024
	(0.03)	(0.02)	(0.04)	(0.03)	(0.05)	(0.03)	(0.02)
Obs.	1159	3620	598	2148	561	1472	4779
did2003	0.011	-0.021	0.029	-0.054**	-0.022	0.020	-0.013
	(0.03)	(0.02)	(0.04)	(0.03)	(0.04)	(0.03)	(0.02)
Obs.	1189	3783	615	2186	574	1597	4972
did2004	0.007	-0.031	0.057	-0.043*	-0.045	-0.012	-0.023
	(0.03)	(0.02)	(0.04)	(0.03)	(0.05)	(0.03)	(0.02)
Obs.	1229	3912	623	2251	606	1661	5141
did2005	-0.012	-0.001	-0.023	-0.003	-0.010	0.014	-0.003
	(0.03)	(0.02)	(0.04)	(0.03)	(0.04)	(0.03)	(0.02)
Obs.	1323	4030	653	2254	670	1776	5353
did2006	-0.034	-0	-0.040	0.009	-0.046	-0.005	-0.010
	(0.03)	(0.02)	(0.04)	(0.02)	(0.04)	(0.03)	(0.01)
Obs.	1364	4124	686	2329	678	1795	5488
did2007	0.012	-0.016	0.001	-0.005	-0.005	-0.011	-0.009
	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)	(0.01)
Obs.	1406	4211	669	2337	737	1874	5617
did2008	-0.035	-0.021	-0.043	-0.056**	-0.060*	0.011	-0.026*
	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)	(0.01)
Obs.	1465	4162	723	2287	742	1875	5627
did2009	0.001	-0.010	-0.012	-0.024	-0.009	-0.004	-0.007
	(0.03)	(0.02)	(0.04)	(0.03)	(0.04)	(0.03)	(0.02)
Obs.	1304	3874	654	2182	650	1692	5178
did2010	0.008	-0.039**	0.054	-0.043*	-0.028	-0.025	-0.025*
	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.02)
Obs.	1296	3699	660	2060	636	1639	4995
did2011	-0.033	-0.037*	-0.044	-0.069**	-0.030	0.001	-0.035**
	(0.03)	(0.02)	(0.04)	(0.03)	(0.04)	(0.03)	(0.02)
Obs.	1318	3694	647	2121	671	1573	5012
did2012	0.029	-0.007	0.051	-0.008	0.022	0.008	0.003
	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.02)
Obs.	1404	3720	711	2092	693	1628	5124
did2013	-0.016	0.008	-0.053	-0.038	0.020	0.046	-0.003
	(0.03)	(0.02)	(0.04)	(0.03)	(0.05)	(0.03)	(0.02)
Obs.	1272	3668	659	2065	613	1603	4940
did2014	-0.027	-0.007	-0.004	-0.026	-0.085*	0.014	-0.019
	(0.03)	(0.02)	(0.05)	(0.03)	(0.05)	(0.04)	(0.02)
Obs.	1175	3313	605	1928	570	1385	4488

Note. The table reports OLS estimates on the incidence of training. Each line reports a year-specific DiD coefficient on the interaction between the "treat" dummy and the "post" dummy identifying the policy-on period. Robust standard errors reported in parentheses. Small enterprises <50 employees. Large enterprises >49 employees. The estimation sample includes only treated and control individuals. Significance levels: ***0.01, **0.05, *0.1 .

Table 23-Effects of the NMW introduction and upratings on absenteeism (model 6: 'before/after' estimator)

	(1) Male	(2) Female	(3) Male Large	(4) Female Large	(5) Male Small	(6) Female Small	(7) All
did1999	-0.013	0.013	-0.006	0.013	-0.031	0.010	0.007
	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1480	4712	840	3021	640	1691	6192
did2000	-0.019	0.008	-0.011	0.011	-0.032	0.002	0.004
	(0.02)	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1232	4070	671	2582	561	1488	5302
did2001	-0.002	-0.003	0.014	0.001	-0.046	-0.008	-0.003
	(0.02)	(0.01)	(0.02)	(0.02)	(0.05)	(0.03)	(0.01)
Obs.	1096	3513	635	2251	461	1262	4609
did2002	-0.026	-0.002	0.010	-0.003	-0.047	-0.004	-0.007
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1159	3620	598	2148	561	1472	4779
did2003	-0.016	0.007	-0.013	0.001	-0.019	0.007	0
	(0.02)	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1189	3783	615	2186	574	1597	4972
did2004	-0.022	0	-0.009	0.008	-0.043	-0.013	-0.003
	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1229	3911	623	2250	606	1661	5140
did2005	0	-0.008	0.021	-0.017	-0.037	-0.007	-0.004
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1323	4030	653	2254	670	1776	5353
did2006	-0.005	0.006	0.019	0.017	-0.033	-0.006	0.006
	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1364	4124	686	2329	678	1795	5488
did2007	-0.013	0.011	0.025	0.011	-0.046*	0.009	0.010
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Obs.	1406	4211	669	2337	737	1874	5617
did2008	-0.002	0.010	-0.007	0.016	-0.003	-0.002	0.009
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Obs.	1465	4161	723	2287	742	1874	5626
did2009	-0.026	0.010	-0.010	0.014	-0.051*	0.005	0.002
	(0.02)	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1304	3874	654	2182	650	1692	5178
did2010	0.003	0.008	0.029	0.004	-0.038	0.011	0.008
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1296	3699	660	2060	636	1639	4995
did2011	0.010	0.022**	0.013	0.011	0.001	0.026	0.019**
	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1318	3694	647	2121	671	1573	5012
did2012	-0.016	0.018	0.017	0.016	-0.039	0.013	0.009
	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1404	3719	711	2092	693	1627	5123
did2013	-0.029*	0.006	-0.017	0.015	-0.040	-0.005	-0.003
	(0.02)	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1271	3668	659	2065	612	1603	4939
did2014	0.001	-0	0.005	-0	-0.019	-0.010	0.005
	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)
Obs.	1175	3313	605	1928	570	1385	4488

Note. The table reports OLS estimates on the change in an individual's abs_rate from wave1 to wave_3. Each line reports a year-specific DiD coefficient on the interaction between the "treat" dummy and the "post" dummy identifying the policy-on period. Robust standard errors reported in parentheses. Small enterprises <50 employees. Large enterprises >49 employees The estimation sample includes only treated and control individuals. Significance levels: ***0.01, **0.05, *0.1 .

Table 24- Effects of the NMW introduction and upratings on shift work (model 6: 'before/after' estimator)

	(1) Male	(2) Female	(3) Male Large	(4) Female Large	(5) Male Small	(6) Female Small	(7) All
did1999	-0.013	0.013	-0.006	0.013	-0.031	0.010	0.007
	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1480	4712	840	3021	640	1691	6192
did2000	-0.019	0.008	-0.011	0.011	-0.032	0.002	0.004
	(0.02)	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1232	4070	671	2582	561	1488	5302
did2001	-0.002	-0.003	0.014	0.001	-0.046	-0.008	-0.003
	(0.02)	(0.01)	(0.02)	(0.02)	(0.05)	(0.03)	(0.01)
Obs.	1096	3513	635	2251	461	1262	4609
did2002	-0.026	-0.002	0.010	-0.003	-0.047	-0.004	-0.007
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1159	3620	598	2148	561	1472	4779
did2003	-0.016	0.007	-0.013	0.001	-0.019	0.007	0
	(0.02)	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1189	3783	615	2186	574	1597	4972
did2004	-0.022	0	-0.009	0.008	-0.043	-0.013	-0.003
	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1229	3911	623	2250	606	1661	5140
did2005	0	-0.008	0.021	-0.017	-0.037	-0.007	-0.004
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1323	4030	653	2254	670	1776	5353
did2006	-0.005	0.006	0.019	0.017	-0.033	-0.006	0.006
	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1364	4124	686	2329	678	1795	5488
did2007	-0.013	0.011	0.025	0.011	-0.046*	0.009	0.010
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Obs.	1406	4211	669	2337	737	1874	5617
did2008	-0.002	0.010	-0.007	0.016	-0.003	-0.002	0.009
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Obs.	1465	4161	723	2287	742	1874	5626
did2009	-0.026	0.010	-0.010	0.014	-0.051*	0.005	0.002
	(0.02)	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1304	3874	654	2182	650	1692	5178
did2010	0.003	0.008	0.029	0.004	-0.038	0.011	0.008
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1296	3699	660	2060	636	1639	4995
did2011	0.010	0.022**	0.013	0.011	0.001	0.026	0.019**
	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1318	3694	647	2121	671	1573	5012
did2012	-0.016	0.018	0.017	0.016	-0.039	0.013	0.009
	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
Obs.	1404	3719	711	2092	693	1627	5123
did2013	-0.029*	0.006	-0.017	0.015	-0.040	-0.005	-0.003
	(0.02)	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)
Obs.	1271	3668	659	2065	612	1603	4939
did2014	0.001	-0.000	0.005	-0.001	-0.019	-0.010	0.005
	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)
Obs.	1175	3313	605	1928	570	1385	4488

Note. The table reports OLS estimates on a variable taking value 1 if in wave 5 the individual does some shift work but not in wave 1, 0 if the individual does not change her exposure to shift work between thetwo waves, or -1 if the individual does some shift work in wave 1 but not in wave 5. Each line reports a year-specific DiD coefficient on the interaction between the "treat" dummy and the "post" dummy identifying the policy-on period. Robust standard errors reported in parentheses. Small enterprises <50 employees. Large enterprises >49 employees. The estimation sample includes groups L, T, U. Significance levels: ***0.01, **0.05, *0.1 .

Table 25-Incidence of training in the last year (`wage gap' estimator)

	(1) Male	(2) Female	(3) Male Large	(4) Female Large	(5) Male Small	(6) Female Small	(7) All
didgap1999	-0.003	0.013	0.009	-0.008	-0.025	0.070**	0.006
	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)	(0.01)
didgap2000	0.012	-0.022	0.034	-0.054***	-0.089**	0.041	-0.011
	(0.03)	(0.02)	(0.04)	(0.02)	(0.04)	(0.04)	(0.02)
didgap2001	0.017	-0.022	0.003	-0.027	0.074	-0.022	-0.013
	(0.05)	(0.02)	(0.05)	(0.03)	(0.08)	(0.04)	(0.02)
didgap2002	-0.016	-0.018	-0.046	-0.020	0.065	-0.003	-0.017
	(0.04)	(0.02)	(0.03)	(0.03)	(0.09)	(0.03)	(0.02)
didgap2003	-0.023	0.019	0.023	0.000	-0.095***	0.060*	0.013
	(0.03)	(0.02)	(0.05)	(0.02)	(0.03)	(0.03)	(0.02)
didgap2004	-0.029	-0.003	0.000	-0.004	-0.066*	0.001	-0.008
	(0.02)	(0.02)	(0.04)	(0.02)	(0.04)	(0.02)	(0.01)
didgap2005	0.025	0.024	-0.015	0.012	0.053	0.041*	0.024*
	(0.03)	(0.02)	(0.02)	(0.02)	(0.05)	(0.02)	(0.01)
didgap2006	-0.023	0.010	-0.034	0.022	-0.016	-0.001	0.001
	(0.02)	(0.01)	(0.03)	(0.02)	(0.04)	(0.02)	(0.01)
didgap2007	-0.001	-0.010	0.019	-0.005	-0.038	-0.008	-0.009
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.01)
didgap2008	-0.026*	-0.000	-0.037*	0.002	-0.028	-0.003	-0.005
	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
didgap2009	0.018	0.000	0.029	-0.016	0.013	0.017	0.004
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
didgap2010	0.021	-0.012	0.054*	-0.012	-0.017	-0.007	-0.004
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.01)
didgap2011	-0.036***	-0.037***	-0.041**	-0.043***	-0.037**	-0.024	-0.037***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
didgap2012	-0.001	0.000	-0.020	-0.018	0.024	0.023	0.000
	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
didgap2013	-0.003	-0.003	-0.016	-0.007	0.035	0.002	-0.003
	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
didgap2014	0.008	-0.019	-0.014	-0.015	0.031	-0.022	-0.013
	(0.02)	(0.01)	(0.02)	(0.02)	(0.04)	(0.02)	(0.01)

Table 26-rate of absenteeism (`wage gap' estimator)

	(1) Male	(2) Female	(3) Male Large	(4) Female Large	(5) Male Small	(6) Female Small	(7) All
didgap1999	-0.003	0.016	0.000	0.019	-0.007	0.011	0.012
	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
didgap2000	-0.039***	0.009	-0.029	0.014	-0.036	0.009	0.002
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
didgap2001	0.049	0.008	0.053	0.016	-0.014	-0.004	0.015
	(0.04)	(0.01)	(0.04)	(0.02)	(0.08)	(0.03)	(0.01)
didgap2002	-0.012	0.002	0.007	0.007	-0.034	-0.005	-0.000
	(0.02)	(0.01)	(0.02)	(0.01)	(0.04)	(0.02)	(0.01)
didgap2003	-0.013	-0.000	-0.010	-0.006	-0.010	0.001	-0.003
	(0.03)	(0.01)	(0.02)	(0.01)	(0.06)	(0.02)	(0.01)
didgap2004	0.011	-0.007	0.002	-0.013	0.020	-0.003	-0.002
	(0.02)	(0.01)	(0.01)	(0.01)	(0.04)	(0.02)	(0.01)
didgap2005	-0.000	-0.006	0.024	-0.017***	-0.011	0.002	-0.004
	(0.02)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
didgap2006	0.002	0.000	0.016	0.004	-0.011	-0.003	0.001
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
didgap2007	-0.012	0.006	0.014	-0.002	-0.034**	0.013	0.004
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
didgap2008	-0.016**	0.007	-0.013	0.002	-0.020	0.011	0.002
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
didgap2009	-0.026***	0.011	-0.030*	0.005	-0.026**	0.015	0.003
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
didgap2010	0.010	0.003	0.008	-0.002	0.008	0.010	0.005
	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
didgap2011	0.002	0.008	0.010	0.007	-0.005	0.005	0.007
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
didgap2012	-0.018**	0.014	0.008	0.003	-0.044***	0.025**	0.006
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
didgap2013	-0.011**	-0.001	-0.008	0.005	-0.014	-0.004	-0.004
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
didgap2014	-0.003	-0.002	-0.007	0.003	0.003	-0.011	-0.001
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)

Table 27-Change in incidence of shift working (`wage gap' estimator)

	(1) Male	(2) Female	(3) Male Large	(4) Female Large	(5) Male Small	(6) Female Small	(7) All
didgap1999	-0.036	0.024	0.007	0.077	0.061	-0.038	0.015
	(0.17)	(0.05)	(0.26)	(0.06)	(0.34)	(0.10)	(0.05)
didgap2000	0.110	0.082	0.341	0.108	-0.138	0.172	0.056
	(0.15)	(0.07)	(0.25)	(0.07)	(0.35)	(0.13)	(0.06)
didgap2001	-0.125	-0.066	-0.137	0.032	-0.924	-0.362*	-0.054
	(0.16)	(0.06)	(0.29)	(0.05)	(0.76)	(0.20)	(0.05)
didgap2002	0.075	0.014	0.072	0.077	-0.010	-0.059	0.018
	(0.12)	(0.05)	(0.13)	(0.05)	(0.26)	(0.10)	(0.05)
didgap2003	-0.155	-0.035	0.164	0.096	-0.356	-0.140	-0.052
	(0.14)	(0.06)	(0.44)	(0.08)	(0.33)	(0.10)	(0.05)
didgap2004	-0.162	0.002	0.153	0.078	-0.459*	-0.064	-0.023
	(0.13)	(0.04)	(0.24)	(0.07)	(0.25)	(0.08)	(0.04)
didgap2005	0.163	-0.047	0.105	0.089	0.522	-0.172**	-0.007
	(0.11)	(0.05)	(0.18)	(0.06)	(0.33)	(0.07)	(0.04)
didgap2006	-0.055	-0.002	0.249	0.017	-0.201	-0.001	-0.009
	(0.10)	(0.03)	(0.24)	(0.04)	(0.29)	(0.06)	(0.03)
didgap2007	0.028	0.007	0.136	0.015	-0.003	0.002	0.002
	(0.05)	(0.03)	(0.12)	(0.03)	(0.08)	(0.06)	(0.02)
didgap2008	-0.060	0.055	-0.039	0.086**	-0.118	0.019	0.031
	(0.08)	(0.04)	(0.09)	(0.04)	(0.13)	(0.06)	(0.03)
didgap2009	-0.024	-0.022	-0.132	0.049	0.012	-0.095*	-0.009
	(0.07)	(0.03)	(0.13)	(0.05)	(0.11)	(0.05)	(0.03)
didgap2010	0.116	0.020	0.293**	0.005	0.199	-0.007	0.041*
	(0.08)	(0.02)	(0.12)	(0.04)	(0.18)	(0.04)	(0.02)
didgap2011	0.053	-0.003	0.123	0.084	0.113	-0.128*	0.017
	(0.09)	(0.04)	(0.16)	(0.05)	(0.22)	(0.08)	(0.04)

Table 28- Pooled effect of the upratings (model 6: 'before/after' estimator)

	(1) Male	(2) Female	(3) Male Large	(4) Female Large	(5) Male Small	(6) Female Small	(7) All
did (training 13 weeks)	0.003	0.010	0.025	0.007	-0.001	0.007	0.007
	(0.03)	(0.02)	(0.03)	(0.02)	(0.05)	(0.02)	(0.02)
did (absenteism)	-0.010	0.007	0.003	0.009	-0.024	0.004	0.004
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
did (change shift)	0.021	0.047	0.174*	0.124***	-0.105	0.046*	0.046*
	(0.06)	(0.03)	(0.09)	(0.04)	(0.10)	(0.03)	(0.03)

5.4 Results from the firm-level analysis using FAME and the ARD

In this section we report our findings regarding capital labour substitution. The results of estimating model (7), the difference-in-differences model estimated on a balanced panel of firms from FAME, are shown in Table 29. Table 30 shows these results when we restrict the sample to SME firms (firms with less than 250 employees). The tables indicate the points at which treatment and control groups were selected (1998, for the NMW introduction; 2002, intermediate phase with large upratings; 2008, recession period). We also carry out falsification tests, reporting impact estimates from a pre-NMW period (1995, historical placebo). These are crucial to the interpretation of our reported impact estimates as being associated with the NMW. Results from the double difference-in-differences model (8) are shown in Table 31 (All firms) and Table 32 (SMEs) using the FAME dataset. We report model (8) estimated on the ARD in Table 32.

If we are to interpret the results as being associated with the NMW it is important that we observe an increase in average labour costs. Similarly, the analysis is motivated by the association between labour productivity and the NMW. We report these estimates too. We also show treatment estimates for employment, the ratio of capital to labour, TFP, and our investment in fixed capital related measures. Together these provide a picture as to whether it is increased capital investment that underlies any association between the NMW and labour productivity.

Dependent variables are specified in logs, which means that coefficients can be interpreted as the percentage change in the outcome of interest relative to the counterfactual (0.01 is equivalent to 1%). When we consider growth outcomes the treatment estimates measure the percentage point change in the annual growth rate relative to the counterfactual. When we use the share of NMW workers to measure the intensity of treatment the reported coefficient can be interpreted as the percentage change in the outcome of interest associated with moving from a workforce with no NMW workers to one with all NMW workers. Multiplying the reported coefficient by 0.1 gives the percentage change associated with a 10 percentage point increase in the proportion of NMW workers in the firm. We report OLS estimates as well as robust regression estimates, which are less influenced by outliers.

In estimating model (7) we include treatment and control firms chosen in the year before the policy change, as indicated above, that we can then observe in each of the 3 years before the policy change and in each of the four years after.[20] In estimating model (8) we use the following year combinations: (1997, 1998), (2001, 2002); (2001, 2002), (2005, 2006); (2007, 2008), (2011, 2012). For each pair the first two years refer to the before period and the last two years refer to the policy on period. This is different to the sample we use for model (7), suiting the structure of the ARD data and also the limited period for which we can observe capital growth in FAME in the pre-NMW phase.

In Table 29 we observe positive treatment estimates for average labour costs and labour productivity in the NMW periods, but nothing in the placebo period before the introduction of the NMW. The more consistent finding is that the labour productivity effects are associated with increases in TFP, but we also find some evidence of an increase in the capital labour ratio in some models; particularly around the introduction of the NMW in the robust regression model. Note that this is different to

[20] We restrict the before-period to 3 years because of data and policy constraints in the pre-NMW period. For the capital growth measures in FAME we can only observe outcomes in the two years before the policy change in the placebo experiment (1995).

Riley & Rosazza Bondibene (2015), reflecting in part the larger sample in use here (we have not made the usual restriction of including only firms that report turnover) and suggesting some heterogeneity in the way that firms may have achieved higher labour productivity in response to the NMW. We do not find much evidenec of an increase in the rate of growth of the capital stock. We do find positive and statistically significant treatment effects when the outcome measure is the rate of growth in the capital labour ratio. However, these are also present in the period before the NMW was introduced, which makes it difficult to interpret these effects as much to do with the NMW. Table 30 reports the same model estimated on the sample of SME firms only. The results are similar although the falsification tests are less strong for wages (and TFP), making this model less preferred. We find some evidence of capital labour substitution in some of these models.

In Table 31 we report estimates of model (8) using the FAME sample. The placebo is very strong. Nothing is statistically significant. In particular, we no longer detect any effects in the pre-NMW period for our capital growth outcomes. During the NMW periods we generally find evidence of wage and labour productivity effects, particularly upon introduction and during the recession, with relatively little consistent evidence of increased investment or capital growth (although there is some evidence in 2002). In Table 32 this model is estimated for the SME sample. Here the placebo estimates are again a little less convincing than when estimated on the full sample.

In Table 33 we report model (8) estimated on the ARD. The results point to increases in wages associated with increases in labour productivity and TFP. There is little consistent evidence of an increase in the capital labour ratio; we find some K/L effect in the recession period with OLS, but this does not stand up in the robust regression. We find a positive and significant treatment effect on the ratio of investment to labour in the robust regression model at the NMW introduction. But, this likely reflects the negative coefficient on employment rather than an increase in investment.

Table 29- DID FAME – All firm sample

		OLS				*Robust regression*			
T/C			Threshold (1998 prices)				Threshold (1998 prices)		
selection		Continuous	10	12	14	Continuous	10	12	14
1995	**Average labour costs**	**0.116**	**0.015**	**0.008**	**0.018 ***	**0.042**	**0.015**	**0.002**	**0.012 ***
		(0.079)	**(0.016)**	**(0.012)**	**(0.009)**	**(0.031)**	**(0.010)**	**(0.008)**	**(0.006)**
	GVA per head	0.019	0.003	0.004	0.008	0.041	0.018	0.007	0.013
		(0.093)	(0.021)	(0.015)	(0.012)	(0.057)	(0.015)	(0.012)	(0.010)
	Employment	-0.180 *	-0.023	-0.019	-0.026 *	-0.244	-0.016	-0.025	-0.038
		(0.102)	(0.023)	(0.018)	(0.015)	(0.201)	(0.048)	(0.040)	(0.034)
	K/L	0.003	-0.008	-0.015	-0.003	0.086	0.006	-0.006	0.015
		(0.119)	(0.027)	(0.022)	(0.019)	(0.178)	(0.043)	(0.035)	(0.030)
	TFP	0.024	0.001	0.005	0.015	0.080	0.009	0.010	0.011
		(0.092)	(0.021)	(0.016)	(0.013)	(0.062)	(0.016)	(0.013)	(0.011)
	K growth	0.014	0.003	-0.021	-0.025 **	-0.021	-0.007	-0.003	-0.005
		(0.077)	(0.017)	(0.015)	(0.012)	(0.030)	(0.007)	(0.006)	(0.005)
	K/L growth	0.274 ***	0.046 **	0.015	0.012	0.080 **	0.011	0.014 *	0.011 *
		(0.092)	(0.019)	(0.016)	(0.012)	(0.038)	(0.009)	(0.008)	(0.007)
1998	**Average labour costs**	**0.268 *** **	**0.053 *** **	**0.053 *** **	**0.039 *** **	**0.197 *** **	**0.042 *** **	**0.041 *** **	**0.032 *** **
		(0.068)	**(0.015)**	**(0.011)**	**(0.008)**	**(0.027)**	**(0.009)**	**(0.007)**	**(0.006)**
	GVA per head	0.150 *	0.023	0.032 *	0.033 ***	0.126 **	0.034 **	0.030 ***	0.040 ***
		(0.090)	(0.020)	(0.016)	(0.013)	(0.049)	(0.014)	(0.011)	(0.010)
	Employment	0.008	0.017	0.003	0.009	0.219	0.073 *	0.033	0.022
		(0.119)	(0.028)	(0.022)	(0.017)	(0.175)	(0.043)	(0.037)	(0.032)
	K/L	0.041	0.030	0.017	0.014	0.447 ***	0.096 **	0.054 *	0.032
		(0.132)	(0.029)	(0.023)	(0.019)	(0.155)	(0.038)	(0.033)	(0.028)
	TFP	0.144 *	0.014	0.030 *	0.029 **	0.163 ***	0.025 *	0.024 *	0.030 ***
		(0.086)	(0.020)	(0.016)	(0.012)	(0.055)	(0.015)	(0.012)	(0.010)
	K growth	0.063	0.004	0.013	0.015	0.051 **	0.003	0.004	0.007
		(0.056)	(0.015)	(0.012)	(0.011)	(0.025)	(0.006)	(0.005)	(0.005)
	K/L growth	0.146 **	0.029 *	0.036 ***	0.030 ***	0.120 ***	0.016 **	0.019 ***	0.022 ***
		(0.065)	(0.016)	(0.013)	(0.010)	(0.031)	(0.008)	(0.007)	(0.006)
2002	**Average labour costs**	**0.253 *** **	**0.047 *** **	**0.048 *** **	**0.043 *** **	**0.141 *** **	**0.030 *** **	**0.033 *** **	**0.034 *** **
		(0.079)	**(0.016)**	**(0.013)**	**(0.010)**	**(0.027)**	**(0.010)**	**(0.008)**	**(0.006)**
	GVA per head	0.292 ***	0.062 ***	0.056 ***	0.025 *	0.094 *	0.022	0.014	0.012
		(0.104)	(0.022)	(0.017)	(0.014)	(0.051)	(0.015)	(0.012)	(0.011)
	Employment	-0.110	-0.021	-0.025	-0.011	-0.137	-0.040	-0.033	-0.018
		(0.100)	(0.024)	(0.021)	(0.018)	(0.174)	(0.045)	(0.038)	(0.033)
	K/L	0.213 *	0.033	0.049 *	0.034	0.201	0.013	0.074 *	0.045
		(0.123)	(0.031)	(0.027)	(0.023)	(0.175)	(0.045)	(0.038)	(0.033)
	TFP	0.431 **	0.147 **	0.072 *	0.036	-0.044	0.008	0.000	0.001
		(0.180)	(0.059)	(0.041)	(0.022)	(0.059)	(0.017)	(0.014)	(0.012)
	K growth	-0.059	-0.023	-0.015	-0.008	-0.013	-0.009	-0.003	-0.004
		(0.066)	(0.016)	(0.013)	(0.011)	(0.026)	(0.007)	(0.006)	(0.005)
	K/L growth	0.201 ***	0.030 *	0.033 **	0.032 ***	0.076 **	0.013	0.010	0.010
		(0.068)	(0.016)	(0.014)	(0.012)	(0.032)	(0.008)	(0.007)	(0.006)
2008	**Average labour costs**	**0.403 *** **	**0.069 *** **	**0.047 *** **	**0.029 *** **	**0.272 *** **	**0.045 *** **	**0.027 *** **	**0.021 *** **
		(0.094)	**(0.017)**	**(0.013)**	**(0.009)**	**(0.028)**	**(0.010)**	**(0.008)**	**(0.006)**
	GVA per head	0.391 ***	0.059 ***	0.059 ***	0.039 ***	0.170 ***	0.047 ***	0.036 ***	0.028 ***
		(0.104)	(0.023)	(0.017)	(0.014)	(0.055)	(0.016)	(0.013)	(0.011)
	Employment	0.051	0.007	0.027	0.010	-0.013	-0.012	0.021	0.006
		(0.128)	(0.028)	(0.022)	(0.019)	(0.187)	(0.047)	(0.039)	(0.033)
	K/L	0.034	0.006	-0.005	0.006	0.183	0.033	0.003	0.014
		(0.156)	(0.035)	(0.031)	(0.026)	(0.204)	(0.051)	(0.042)	(0.036)
	TFP	0.623 ***	0.123 ***	0.106 **	0.043	0.245 ***	0.046 **	0.040 ***	0.030 **
		(0.171)	(0.040)	(0.044)	(0.035)	(0.068)	(0.019)	(0.015)	(0.013)
	K growth	-0.068	-0.005	-0.013	-0.011	-0.058 **	-0.014 **	-0.009 *	-0.007 *
		(0.065)	(0.017)	(0.015)	(0.013)	(0.025)	(0.006)	(0.005)	(0.004)
	K/L growth	0.062	0.012	0.007	0.012	-0.026	-0.005	-0.002	-0.001
		(0.068)	(0.018)	(0.015)	(0.012)	(0.032)	(0.008)	(0.007)	(0.006)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices) are excluded from the sample; balanced panel over 7 years (balanced over six years for change K growth in 1995); firms per panel 2796 (1995), 3054 (1998), 2751 (2002), 2623 (2008).

Table 30- DID FAME – SME firm sample

		OLS				*Robust regression*			
T/C selection		Continuous	Threshold (1998 prices) 10	12	14	Continuous	Threshold (1998 prices) 10	12	14
1995	**Average labour costs**	**0.174 ***	**0.022**	**0.011**	**0.028 *****	**0.056**	**0.013**	**0.002**	**0.019 *****
		(0.093)	**(0.019)**	**(0.014)**	**(0.010)**	**(0.037)**	**(0.012)**	**(0.009)**	**(0.007)**
	GVA per head	0.093	0.012	0.009	0.021	0.054	0.019	0.005	0.019
		(0.118)	(0.026)	(0.018)	(0.014)	(0.067)	(0.018)	(0.014)	(0.012)
	Employment	-0.249 **	-0.026	-0.015	-0.033 **	-0.264	-0.022	-0.017	-0.039
		(0.108)	(0.025)	(0.019)	(0.016)	(0.187)	(0.045)	(0.036)	(0.030)
	K/L	-0.079	-0.039	-0.029	-0.011	0.051	-0.013	-0.011	0.009
		(0.142)	(0.032)	(0.027)	(0.022)	(0.217)	(0.052)	(0.042)	(0.035)
	TFP	0.134	0.019	0.016	0.025 *	0.136 *	0.021	0.017	0.022 *
		(0.117)	(0.026)	(0.019)	(0.015)	(0.076)	(0.019)	(0.015)	(0.013)
	K growth	0.024	0.005	-0.025	-0.025 *	0.001	-0.001	0.000	-0.002
		(0.097)	(0.021)	(0.018)	(0.014)	(0.035)	(0.008)	(0.007)	(0.006)
	K/L growth	0.203 **	0.035	0.003	0.010	0.056	0.009	0.009	0.012
		(0.099)	(0.022)	(0.018)	(0.014)	(0.046)	(0.011)	(0.009)	(0.007)
1998	**Average labour costs**	**0.217 *****	**0.040 ****	**0.042 *****	**0.034 *****	**0.189 *****	**0.052 *****	**0.043 *****	**0.030 *****
		(0.083)	**(0.017)**	**(0.013)**	**(0.009)**	**(0.033)**	**(0.011)**	**(0.009)**	**(0.007)**
	GVA per head	0.073	0.005	0.024	0.030 **	0.089	0.037 **	0.027 **	0.041 ***
		(0.115)	(0.026)	(0.020)	(0.014)	(0.060)	(0.017)	(0.013)	(0.011)
	Employment	0.116	0.037	0.024	0.007	0.170	0.058	0.026	0.003
		(0.140)	(0.030)	(0.022)	(0.016)	(0.161)	(0.040)	(0.033)	(0.027)
	K/L	-0.102	0.000	-0.014	0.010	0.520 ***	0.109 **	0.043	0.034
		(0.167)	(0.036)	(0.027)	(0.022)	(0.191)	(0.047)	(0.040)	(0.033)
	TFP	0.125	0.008	0.030	0.029 **	0.196 ***	0.010	0.018	0.030 **
		(0.111)	(0.026)	(0.020)	(0.014)	(0.068)	(0.018)	(0.015)	(0.012)
	K growth	0.085	0.006	0.007	0.014	0.074 **	0.007	0.006	0.008 *
		(0.067)	(0.018)	(0.014)	(0.012)	(0.030)	(0.007)	(0.006)	(0.005)
	K/L growth	0.109	0.021	0.019	0.019	0.128 ***	0.018 *	0.017 **	0.022 ***
		(0.080)	(0.019)	(0.015)	(0.012)	(0.038)	(0.009)	(0.008)	(0.007)
2002	**Average labour costs**	**0.266 *****	**0.043 ****	**0.045 *****	**0.038 *****	**0.164 *****	**0.023 ***	**0.030 *****	**0.030 *****
		(0.093)	**(0.019)**	**(0.014)**	**(0.011)**	**(0.034)**	**(0.012)**	**(0.009)**	**(0.007)**
	GVA per head	0.320 **	0.063 **	0.055 ***	0.016	0.134 **	0.013	0.009	0.004
		(0.138)	(0.028)	(0.021)	(0.016)	(0.066)	(0.019)	(0.015)	(0.012)
	Employment	-0.095	-0.026	-0.031	-0.033 *	-0.128	-0.042	-0.032	-0.030
		(0.095)	(0.023)	(0.020)	(0.017)	(0.164)	(0.041)	(0.033)	(0.028)
	K/L	0.367 **	0.056	0.054 *	0.042	0.281	0.016	0.067	0.032
		(0.156)	(0.040)	(0.033)	(0.028)	(0.229)	(0.057)	(0.047)	(0.039)
	TFP	0.331 *	0.109 **	0.029	0.003	-0.027	-0.010	-0.013	-0.012
		(0.178)	(0.046)	(0.034)	(0.020)	(0.078)	(0.021)	(0.017)	(0.014)
	K growth	-0.023	-0.020	-0.009	0.000	0.008	-0.004	0.002	0.000
		(0.069)	(0.017)	(0.015)	(0.013)	(0.032)	(0.008)	(0.007)	(0.005)
	K/L growth	0.136 *	0.024	0.023	0.025 **	0.065	0.008	0.007	0.009
		(0.074)	(0.018)	(0.015)	(0.013)	(0.042)	(0.010)	(0.009)	(0.007)
2008	**Average labour costs**	**0.389 *****	**0.059 ****	**0.060 *****	**0.031 *****	**0.278 *****	**0.063 *****	**0.044 *****	**0.028 *****
		(0.104)	**(0.020)**	**(0.015)**	**(0.010)**	**(0.038)**	**(0.012)**	**(0.009)**	**(0.007)**
	GVA per head	0.414 ***	0.051 *	0.084 ***	0.052 ***	0.225 ***	0.065 ***	0.052 ***	0.029 **
		(0.135)	(0.029)	(0.023)	(0.017)	(0.077)	(0.020)	(0.016)	(0.013)
	Employment	-0.003	-0.012	0.003	0.009	0.040	0.003	0.012	0.006
		(0.105)	(0.025)	(0.021)	(0.017)	(0.173)	(0.041)	(0.033)	(0.027)
	K/L	-0.154	-0.026	-0.009	-0.003	0.150	0.017	-0.004	0.009
		(0.206)	(0.045)	(0.041)	(0.031)	(0.280)	(0.066)	(0.054)	(0.043)
	TFP	0.609 ***	0.112 **	0.101 *	0.010	0.297 ***	0.054 **	0.053 ***	0.035 **
		(0.217)	(0.050)	(0.052)	(0.031)	(0.098)	(0.025)	(0.019)	(0.016)
	K growth	-0.025	-0.009	-0.008	-0.003	-0.046	-0.016 **	-0.007	-0.003
		(0.094)	(0.024)	(0.019)	(0.016)	(0.033)	(0.008)	(0.006)	(0.005)
	K/L growth	0.045	0.008	0.003	0.007	-0.017	-0.005	-0.003	0.000
		(0.098)	(0.024)	(0.019)	(0.016)	(0.045)	(0.011)	(0.009)	(0.007)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; firms selected for neither the treatment nor the control group (with labour costs above £20,000 in 1998 prices) are excluded from the sample; balanced panel over 7 years (balanced over six years for change K growth in 1995); firms per panel 2206 (1995), 2350 (1998), 2088 (2002), 1938 (2008); SMEs.

Table 31- Double DID FAME – All firm sample

		OLS				*Robust regression*			
T/C			Threshold (1998 prices)				Threshold (1998 prices)		
selection		Continuous	10	12	14	Continuous	10	12	14
1995	**Average labour costs**	**0.142**	**0.011**	**0.012**	**0.019**	**-0.021**	**0.005**	**-0.007**	**0.003**
		(0.092)	**(0.019)**	**(0.014)**	**(0.012)**	**(0.044)**	**(0.013)**	**(0.010)**	**(0.009)**
	GVA per head	0.003	-0.021	0.014	0.010	0.008	0.006	0.005	0.019
		(0.139)	(0.029)	(0.023)	(0.021)	(0.094)	(0.023)	(0.018)	(0.017)
	Employment	-0.073	-0.025	-0.016	-0.016	-0.136	-0.023	-0.020	-0.023
		(0.152)	(0.031)	(0.026)	(0.024)	(0.331)	(0.072)	(0.060)	(0.055)
	K/L	0.016	-0.020	-0.042	0.009	0.079	-0.005	-0.027	0.030
		(0.218)	(0.045)	(0.037)	(0.035)	(0.309)	(0.067)	(0.056)	(0.051)
	TFP	-0.070	-0.036	0.008	0.002	0.012	0.014	0.020	0.018
		(0.157)	(0.034)	(0.027)	(0.023)	(0.107)	(0.025)	(0.020)	(0.018)
	K growth	0.126	0.003	0.003	0.006	-0.035	-0.012	0.004	0.001
		(0.140)	(0.028)	(0.024)	(0.021)	(0.052)	(0.011)	(0.009)	(0.009)
	K/L growth	0.182	0.012	0.012	0.014	-0.033	-0.003	0.012	0.005
		(0.148)	(0.030)	(0.024)	(0.021)	(0.064)	(0.014)	(0.011)	(0.010)
1998	**Average labour costs**	**0.367 *** **	**0.078 *** **	**0.069 *** **	**0.051 *** **	**0.146 *** **	**0.037 *** **	**0.031 *** **	**0.024 *** **
		(0.097)	**(0.020)**	**(0.015)**	**(0.012)**	**(0.041)**	**(0.012)**	**(0.009)**	**(0.008)**
	GVA per head	0.170	0.055 **	0.043 *	0.031	0.080	0.042 **	0.040 **	0.041 ***
		(0.132)	(0.027)	(0.023)	(0.020)	(0.084)	(0.020)	(0.017)	(0.015)
	Employment	0.061	0.025	-0.002	0.020	0.235	0.055	0.024	0.028
		(0.161)	(0.035)	(0.030)	(0.025)	(0.292)	(0.064)	(0.055)	(0.050)
	K/L	-0.099	0.005	0.006	-0.013	0.344	0.069	0.047	0.013
		(0.203)	(0.040)	(0.034)	(0.033)	(0.283)	(0.062)	(0.053)	(0.048)
	TFP	0.154	0.046	0.043 *	0.031	0.163 *	0.034	0.030	0.023
		(0.148)	(0.030)	(0.024)	(0.023)	(0.096)	(0.023)	(0.019)	(0.017)
	K growth	-0.092	-0.031	-0.010	-0.014	0.030	0.000	-0.002	-0.002
		(0.107)	(0.022)	(0.019)	(0.019)	(0.047)	(0.010)	(0.009)	(0.008)
	K/L growth	-0.065	-0.017	0.008	-0.002	0.049	-0.001	0.003	0.008
		(0.109)	(0.023)	(0.020)	(0.019)	(0.056)	(0.012)	(0.010)	(0.010)
2002	**Average labour costs**	**0.485 *** **	**0.095 *** **	**0.077 *** **	**0.063 *** **	**0.113 *** **	**0.043 *** **	**0.038 *** **	**0.038 *** **
		(0.124)	**(0.024)**	**(0.018)**	**(0.015)**	**(0.044)**	**(0.013)**	**(0.010)**	**(0.009)**
	GVA per head	0.414 **	0.099 ***	0.056 **	0.014	-0.030	0.036	0.001	-0.018
		(0.173)	(0.034)	(0.028)	(0.024)	(0.094)	(0.023)	(0.019)	(0.017)
	Employment	-0.164	-0.023	-0.036	-0.015	-0.158	-0.034	-0.022	-0.009
		(0.177)	(0.036)	(0.030)	(0.028)	(0.312)	(0.069)	(0.058)	(0.053)
	K/L	0.046	0.057	0.026	0.003	0.283	0.069	0.085	0.052
		(0.243)	(0.051)	(0.043)	(0.042)	(0.352)	(0.077)	(0.065)	(0.060)
	TFP	0.668 ***	0.179 ***	0.080 *	0.039	-0.030	0.030	-0.014	-0.028
		(0.250)	(0.064)	(0.047)	(0.035)	(0.114)	(0.027)	(0.022)	(0.021)
	K growth	0.171	0.031	0.035	0.026	0.066	0.007	0.007	0.006
		(0.146)	(0.029)	(0.025)	(0.024)	(0.051)	(0.011)	(0.010)	(0.009)
	K/L growth	0.279 **	0.064 **	0.054 **	0.039	0.101	0.030 **	0.019	0.006
		(0.141)	(0.028)	(0.025)	(0.024)	(0.063)	(0.014)	(0.012)	(0.011)
2008	**Average labour costs**	**0.580 *** **	**0.112 *** **	**0.076 *** **	**0.051 *** **	**0.317 *** **	**0.055 *** **	**0.021 ** **	**0.024 *** **
		(0.127)	**(0.022)**	**(0.018)**	**(0.014)**	**(0.045)**	**(0.013)**	**(0.010)**	**(0.009)**
	GVA per head	0.568 ***	0.070 **	0.095 ***	0.090 ***	0.280 ***	0.068 ***	0.057 ***	0.044 **
		(0.177)	(0.034)	(0.028)	(0.025)	(0.102)	(0.025)	(0.020)	(0.018)
	Employment	0.092	0.056	-0.007	-0.027	-0.293	-0.030	-0.044	-0.048
		(0.217)	(0.042)	(0.036)	(0.031)	(0.335)	(0.073)	(0.061)	(0.056)
	K/L	-0.094	-0.044	-0.037	0.035	0.080	-0.022	-0.027	0.054
		(0.315)	(0.062)	(0.053)	(0.049)	(0.410)	(0.089)	(0.075)	(0.069)
	TFP	0.710 ***	0.134 ***	0.127 **	0.080	0.321 **	0.055 *	0.072 ***	0.047 **
		(0.237)	(0.051)	(0.058)	(0.049)	(0.128)	(0.029)	(0.024)	(0.022)
	K growth	-0.331 **	-0.058 **	-0.057 **	-0.033	-0.027	-0.003	-0.005	-0.003
		(0.149)	(0.029)	(0.026)	(0.025)	(0.052)	(0.011)	(0.009)	(0.009)
	K/L growth	-0.104	-0.025	-0.008	0.021	0.014	0.004	0.008	0.012
		(0.163)	(0.031)	(0.026)	(0.025)	(0.062)	(0.013)	(0.011)	(0.010)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; firms selected for neither the treatment nor the control group in neither the lower or higher labour costs groups (with labour costs between £20,000 and £27,000 or above £48,000 in 1998 prices) are excluded from the sample; balanced panel over 4 (non-consecutive) years; firms per panel 3778 (1995), 4277 (1998), 3675 (2002), 3474 (2008).

Table 32- Double DID FAME – SME firm sample

		OLS				*Robust regression*			
T/C			Threshold (1998 prices)				Threshold (1998 prices)		
selection		Continuous	10	12	14	Continuous	10	12	14
1995	**Average labour costs**	**0.193 ***	**0.018**	**0.018**	**0.028 ****	**-0.009**	**0.006**	**-0.001**	**0.013**
		(0.106)	**(0.022)**	**(0.017)**	**(0.014)**	**(0.051)**	**(0.015)**	**(0.011)**	**(0.010)**
	GVA per head	0.047	-0.020	0.019	0.018	0.020	-0.005	0.003	0.024
		(0.166)	(0.035)	(0.027)	(0.024)	(0.108)	(0.026)	(0.021)	(0.019)
	Employment	-0.069	-0.017	-0.003	-0.011	-0.163	-0.028	-0.010	-0.028
		(0.152)	(0.032)	(0.027)	(0.024)	(0.291)	(0.064)	(0.052)	(0.047)
	K/L	0.040	-0.035	-0.054	0.011	0.022	-0.024	-0.045	0.026
		(0.225)	(0.046)	(0.040)	(0.037)	(0.363)	(0.080)	(0.066)	(0.059)
	TFP	0.047	-0.013	0.029	0.011	-0.017	0.007	0.020	0.022
		(0.170)	(0.036)	(0.028)	(0.025)	(0.125)	(0.029)	(0.023)	(0.021)
	K growth	0.226	0.027	0.008	0.019	-0.017	-0.005	0.001	0.001
		(0.152)	(0.030)	(0.027)	(0.024)	(0.060)	(0.013)	(0.011)	(0.010)
	K/L growth	0.255 *	0.038	0.011	0.025	-0.020	0.004	0.003	0.010
		(0.148)	(0.031)	(0.026)	(0.023)	(0.074)	(0.016)	(0.013)	(0.012)
1998	**Average labour costs**	**0.360 ****	**0.078 ****	**0.064 ****	**0.053 ****	**0.141 ****	**0.055 ****	**0.037 ****	**0.021 ****
		(0.120)	**(0.023)**	**(0.017)**	**(0.014)**	**(0.048)**	**(0.014)**	**(0.011)**	**(0.009)**
	GVA per head	0.100	0.040	0.026	0.027	0.024	0.049 **	0.025	0.033 *
		(0.160)	(0.032)	(0.026)	(0.023)	(0.096)	(0.024)	(0.019)	(0.017)
	Employment	0.203	0.032	0.036	0.028	0.184	0.034	0.027	0.015
		(0.170)	(0.035)	(0.028)	(0.023)	(0.253)	(0.057)	(0.048)	(0.043)
	K/L	-0.189	-0.021	-0.021	-0.014	0.357	0.066	0.027	0.009
		(0.235)	(0.047)	(0.039)	(0.037)	(0.329)	(0.074)	(0.062)	(0.055)
	TFP	0.177	0.052	0.046	0.034	0.154	0.027	0.022	0.018
		(0.172)	(0.034)	(0.028)	(0.026)	(0.113)	(0.027)	(0.022)	(0.020)
	K growth	-0.117	-0.035	-0.025	-0.023	0.039	0.002	0.001	0.000
		(0.122)	(0.025)	(0.022)	(0.021)	(0.053)	(0.012)	(0.010)	(0.009)
	K/L growth	-0.064	-0.024	-0.008	-0.011	0.070	0.009	0.005	0.007
		(0.124)	(0.026)	(0.023)	(0.021)	(0.065)	(0.015)	(0.012)	(0.011)
2002	**Average labour costs**	**0.474 ****	**0.092 ****	**0.071 ****	**0.056 ****	**0.133 ****	**0.041 ****	**0.037 ****	**0.032 ****
		(0.140)	**(0.027)**	**(0.019)**	**(0.016)**	**(0.052)**	**(0.016)**	**(0.012)**	**(0.010)**
	GVA per head	0.407 **	0.101 **	0.050	-0.007	0.011	0.022	-0.011	-0.045 **
		(0.206)	(0.040)	(0.031)	(0.027)	(0.114)	(0.028)	(0.022)	(0.020)
	Employment	-0.119	-0.015	-0.033	-0.034	-0.154	-0.026	-0.028	-0.031
		(0.153)	(0.032)	(0.027)	(0.025)	(0.278)	(0.061)	(0.050)	(0.045)
	K/L	0.146	0.059	0.025	0.010	0.411	0.087	0.088	0.051
		(0.280)	(0.059)	(0.050)	(0.047)	(0.429)	(0.094)	(0.078)	(0.070)
	TFP	0.487 **	0.140 ***	0.029	-0.015	0.000	0.020	-0.029	-0.060 **
		(0.243)	(0.052)	(0.041)	(0.036)	(0.139)	(0.033)	(0.026)	(0.024)
	K growth	0.286 *	0.041	0.050 *	0.036	0.075	0.011	0.009	0.008
		(0.163)	(0.032)	(0.028)	(0.027)	(0.061)	(0.013)	(0.011)	(0.010)
	K/L growth	0.226	0.055 *	0.047 *	0.027	0.106	0.026	0.019	0.004
		(0.160)	(0.032)	(0.028)	(0.027)	(0.076)	(0.017)	(0.014)	(0.012)
2008	**Average labour costs**	**0.633 ****	**0.110 ****	**0.092 ****	**0.052 ****	**0.368 ****	**0.085 ****	**0.050 ****	**0.028 ****
		(0.130)	**(0.024)**	**(0.020)**	**(0.016)**	**(0.055)**	**(0.016)**	**(0.012)**	**(0.010)**
	GVA per head	0.784 ***	0.094 **	0.151 ***	0.111 ***	0.434 ***	0.108 ***	0.086 ***	0.050 **
		(0.214)	(0.041)	(0.036)	(0.030)	(0.130)	(0.031)	(0.024)	(0.021)
	Employment	-0.188	-0.010	-0.050	-0.040	-0.214	-0.021	-0.034	-0.042
		(0.185)	(0.036)	(0.031)	(0.027)	(0.300)	(0.064)	(0.053)	(0.047)
	K/L	-0.543	-0.141 *	-0.072	-0.025	-0.069	-0.090	-0.026	0.015
		(0.379)	(0.074)	(0.064)	(0.056)	(0.519)	(0.111)	(0.092)	(0.081)
	TFP	0.881 ***	0.154 ***	0.146 **	0.057	0.552 ***	0.084 **	0.104 ***	0.056 **
		(0.269)	(0.058)	(0.059)	(0.041)	(0.167)	(0.038)	(0.030)	(0.027)
	K growth	-0.243	-0.048	-0.052 *	-0.029	0.033	0.000	0.001	0.003
		(0.189)	(0.036)	(0.031)	(0.030)	(0.064)	(0.014)	(0.011)	(0.010)
	K/L growth	0.057	0.003	0.000	0.018	0.099	0.017	0.008	0.014
		(0.195)	(0.036)	(0.030)	(0.030)	(0.080)	(0.017)	(0.014)	(0.012)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; controls include indicators for start-up, young (less than 5 years old), group accounts, exporter, foreign ownership; 2-digit industry-year effects included; firms selected for neither the treatment nor the control group in neither the lower or higher labour costs groups (with labour costs between £20,000 and £27,000 or above £48,000 in 1998 prices) are excluded from the sample; balanced panel over 4 (non-consecutive) years; firms per panel 3049 (1995), 3434 (1998), 2889 (2002), 2685 (2008); SMEs.

Table 33- Double DID ARD – All firm sample

		OLS				*Robust regression*			
T/C selection		Continuous	Threshold (1998 prices) 10	12	14	Continuous	Threshold (1998 prices) 10	12	14
1998	**Average labour costs**	**0.634 *** **	**0.110 *** **	**0.106 *** **	**0.086 *** **	**0.508 *** **	**0.102 *** **	**0.083 *** **	**0.073 *** **
		(0.144)	**(0.032)**	**(0.028)**	**(0.025)**	**(0.090)**	**(0.025)**	**(0.020)**	**(0.019)**
	GVA per head	0.295	0.059	0.064	0.094 **	0.211	0.084 *	0.073 *	0.080 **
		(0.256)	(0.054)	(0.047)	(0.043)	(0.198)	(0.045)	(0.039)	(0.037)
	Employment	-0.273	-0.049	-0.088 **	-0.088 **	-0.214	-0.073	-0.094	-0.047
		(0.233)	(0.050)	(0.044)	(0.040)	(0.525)	(0.110)	(0.096)	(0.093)
	K/L	-0.568	-0.121	0.021	-0.016	-0.277	-0.096	-0.024	-0.028
		(0.473)	(0.100)	(0.093)	(0.075)	(0.459)	(0.097)	(0.085)	(0.081)
	TFP	0.537 *	0.110 *	0.063	0.103 **	0.569 **	0.144 ***	0.136 ***	0.129 ***
		(0.292)	(0.061)	(0.054)	(0.049)	(0.242)	(0.053)	(0.046)	(0.044)
	K growth	-0.759	-0.017	0.141	0.090	-0.678	-0.051	0.080	0.009
		(0.817)	(0.176)	(0.151)	(0.141)	(0.931)	(0.198)	(0.173)	(0.166)
	K/L growth	-0.547	0.013	0.220	0.162	0.576	0.231 *	0.282 **	0.158
		(0.792)	(0.173)	(0.149)	(0.136)	(0.602)	(0.129)	(0.112)	(0.108)
2002	**Average labour costs**	**0.696 *** **	**0.143 *** **	**0.105 *** **	**0.082 *** **	**0.445 *** **	**0.108 *** **	**0.094 *** **	**0.061 *** **
		(0.134)	**(0.027)**	**(0.023)**	**(0.020)**	**(0.082)**	**(0.022)**	**(0.018)**	**(0.017)**
	GVA per head	0.811 ***	0.182 ***	0.112 ***	0.066 *	0.452 **	0.137 ***	0.062 *	0.007
		(0.240)	(0.050)	(0.043)	(0.040)	(0.186)	(0.041)	(0.036)	(0.035)
	Employment	-0.832 ***	-0.157 ***	-0.140 ***	-0.081 **	-0.807	-0.152	-0.160	-0.079
		(0.216)	(0.046)	(0.040)	(0.034)	(0.576)	(0.120)	(0.105)	(0.101)
	K/L	0.696	0.035	0.046	0.046	-0.189	-0.088	-0.052	-0.015
		(0.668)	(0.106)	(0.102)	(0.102)	(0.429)	(0.090)	(0.078)	(0.076)
	TFP	0.598 *	0.168 ***	0.095 *	0.054	0.542 **	0.146 ***	0.090 **	0.015
		(0.308)	(0.058)	(0.052)	(0.050)	(0.224)	(0.047)	(0.042)	(0.040)
	K growth	-0.468	-0.208	0.006	0.022	-0.152	-0.180	0.054	-0.034
		(0.925)	(0.195)	(0.164)	(0.152)	(1.099)	(0.230)	(0.202)	(0.194)
	K/L growth	0.298	-0.055	0.152	0.091	0.379	-0.049	0.076	-0.082
		(0.891)	(0.188)	(0.158)	(0.146)	(0.689)	(0.145)	(0.127)	(0.122)
2008	**Average labour costs**	**0.395 *** **	**0.108 *** **	**0.094 *** **	**0.049 ** **	**0.424 *** **	**0.110 *** **	**0.078 *** **	**0.041 ** **
		(0.140)	**(0.029)**	**(0.026)**	**(0.025)**	**(0.087)**	**(0.024)**	**(0.020)**	**(0.019)**
	GVA per head	1.069 ***	0.149 **	0.190 ***	0.140 ***	0.867 ***	0.148 ***	0.191 ***	0.144 ***
		(0.280)	(0.060)	(0.053)	(0.051)	(0.245)	(0.053)	(0.048)	(0.045)
	Employment	-0.033	-0.019	-0.007	0.030	-0.332	-0.066	-0.058	-0.021
		(0.222)	(0.045)	(0.039)	(0.037)	(0.507)	(0.103)	(0.093)	(0.088)
	K/L	0.798 **	0.143 *	0.093	0.112 *	0.605	0.081	0.022	0.059
		(0.341)	(0.083)	(0.064)	(0.062)	(0.465)	(0.095)	(0.086)	(0.082)
	TFP	0.825 ***	0.095	0.156 ***	0.099 *	0.588 **	0.092	0.140 ***	0.098 *
		(0.294)	(0.066)	(0.057)	(0.054)	(0.282)	(0.059)	(0.053)	(0.050)
	K growth	0.559	0.077	-0.035	0.073	1.094	0.146	0.039	0.190
		(1.019)	(0.207)	(0.188)	(0.175)	(1.029)	(0.211)	(0.190)	(0.180)
	K/L growth	0.641	0.090	-0.046	0.034	0.951	0.145	0.003	0.060
		(1.003)	(0.200)	(0.182)	(0.170)	(0.708)	(0.145)	(0.131)	(0.124)

Notes: Standard errors clustered by firm in brackets; statistical significance ***1%, **5%, *10%; 2-digit industry-year effects included; firms selected for neither the treatment nor the control group in neither the lower or higher labour costs groups (with labour costs between £20,000 and £27,000 or above £48,000 in 1998 prices) are excluded from the sample; balanced panel over 4 (non-consecutive) years; firms per panel 2674 (1998), 3145 (2002), 2406 (2008).

6. Conclusions

This report examines whether the introduction of the NMW may have led companies to implement a series of potentially productivity enhancing measures such as:

- Adopting more capital intensive production techniques
- Increasing the provision of training to low-paid and/or other workers
- Shifting the composition of the workforce towards a greater share of skilled workers
- Increasing the supervision of low-paid workers to extract greater effort
- Adopting tougher recruitment criteria
- Outsourcing some of the low-skill tasks that were previously conducted internally

It also examines whether low-paid workers affected by the NMW may have reacted to higher wages by:

- Exerting greater effort in their job
- Reducing their rate of absenteeism

We conduct our analyses on data from different sources. Firm-level information is obtained from the Financial Analysis Made Easy (FAME) dataset and from the Annual Respondents Database (ARD). Worker-level outcomes are investigated on data from the employee-level component of the Workplace Employment Relations Survey (WERS) and on Labour Force Survey (LFS) data. Workplace data are obtained from the establishment-level component of WERS.

Difference-in-differences models are used to estimate the impacts of the NMW on our outcomes of interest. We compare the evolution of the outcome variables across groups of companies and individuals that are more (the "treated") or less (the "controls") affected by the NMW. The identification of "treated" companies in the sample is based on the average wage per worker as a proxy for the proportion of low-paid workers in the firm (in FAME and in the ARD) and on the share of workers paid below £4 at the establishment level in 1998 (in WERS). The identification of treated workers is based on workers' hourly pay before the introduction and subsequent upratings of the NMW (in the LFS), and on workers' relative position in the wage distribution or occupation (in WERS).

We report sensitivity analyses where we vary the control groups against which treated groups are compared, the time periods, and the specification of the outcome variables. We also test for differential responses to the NMW across different types of firms and workers, splitting the estimation samples on standard dimensions such as firm size and worker gender.

While some results point to a significant effect of the introduction of the NMW on treated firms' capital intensity (i.e., measured as the stock of a firm's physical capital per worker), this result is not robust across different samples, data sets or estimation techniques. Further, on the whole, our regression results do not support the hypothesis that firms that were more affected by the NMW invested more heavily in physical capital assets than a comparable control group.

We find no evidence to suggest that the NMW increased the incidence of training amongst low-paid workers. Nor do we identify any significant effects of the NMW on a series of indicators capturing workers' rate of absenteeism, workers' perception of the effort exerted or the degree of discretion they have in their job.

With regard to management practices, we do not identify any effect of the NMW on establishments' outsourcing practices, recruitment criteria or on the provision of training to the establishments' main occupation group.

We find some evidence to suggest that those establishments that were more affected by the NMW experienced a greater reduction over the period 1998-2004 in the share of workers employed in routine unskilled occupations, accompanied by a greater increase in the share of workers employed in professional occupations. However, this result needs to be interpreted with caution as it is based on a relatively small sample of establishments and it is not based on administrative data but on managers' reported measures of workforce composition. We also find some evidence to suggest that the NMW was associated with an increased likelihood of low paid workers doing shift work.

With the exception of the evidence on changes in the occupational composition of the workplace and on the incidence of shift work, we do not identify significant impacts of the NMW on any of the other measures that we look at. Three alternative, but not mutually exclusive, explanations are consistent with this negative result. First, it is possible that the positive association between the NMW and labour productivity arises through changes in a series of employer and employee behaviours. This would make it more difficult to detect a statistically significant impact of the NMW on any single type of productivity enhancing behaviour, than on productivity itself. A joint analysis of the factors that might affect productivity may be more fruitful, but the relevant information to undertake such analysis is not readily available.

Second, there may be a unique most important channel through which the NMW led to increases in productivity in low-paying firms that we have not considered in this study. In particular, it is possible that firms responded to higher minimum wages by reshaping their internal structure and by adopting managerial practices to achieve efficiency gains. While this report considers the impact of the NMW on employment structure, outsourcing and recruitment practices as reported by managers in WERS, the data do not capture informal changes in the organisation of labour that may have occurred within low-paying firms.

Third, it is possible that one of the channels that we have investigated played a major role in determining the link between the NMW and productivity but that measurement error and insufficient time variation in relevant outcome variables undermine the precision of the estimators leading us to over-reject the hypothesis of a significant impact. However, robustness tests show that the estimators performed well in picking up the positive impact of the NMW on firms' labour costs and on low-paid workers wages. This should dispel worries that a negative result is solely due to the use of weak estimators.

In sum, the evidence presented in this report does not point to a single most important mechanism from which a positive link between the NMW and productivity in low-paying firms might have arisen. Rather, it seems likely that any positive productivity impacts from the NMW are driven by a variety of behaviours.

7. References

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