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Efficiency Wages and the Economic Effects of the Minimum Wage: Evidence from a Low-Wage Labour Market

Andreas Georgiadis

THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE

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Abstract
We exploit a natural experiment provided by the 1999 introduction of the UK National Minimum Wage (NMW) to investigate the relationship between wages and monitoring and to test for Efficiency Wages considerations in a low-wage sector, the UK residential care homes industry. Our findings seem to support the wage-supervision trade-off prediction of the shirking model, and that employers didn't dissipate minimum wage rents by increasing work intensity or effort requirements on the job. Estimation results suggest that higher wage costs were more than offset by lower monitoring costs, and thus the overall evidence imply that the NMW may have operated as an Efficiency Wage. These findings support Efficiency Wage models used to explain a non-negative employment effect of the Minimum Wage and provide an explanation of recent evidence from the care homes sector that although the wage structure was heavily affected by the NMW introduction, there were moderate employment effects.

Keywords: Efficiency Wages, National Minimum Wage, Wage-supervision trade-off
JEL Classification: J31; J38; J41

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Andreas Georgiadis is a Research Economist at the Centre for Economic Performance, London School of Economics

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1 Introduction

Efficiency Wages theory has been used to explain downward wage rigidity at the microeconomic level (Weiss, 1991) and thus involuntary unemployment as well as labour market segmentation (Bulow and Summers, 1986) and wage differentials across firms or industries (Krueger and Summers, 1988).

The essence of the theory is that wages do not only determine employment but also affect employees' productive behavior or quality[1], and that is why, under certain conditions, it is optimal for employers to set compensation above the market clearing level in order to recruit, retain or motivate employees.

The main criticism against the validity of efficiency wages has been the so-called "bonding critique" (Carmichael, 1985, 1987), according to which there are more efficient mechanisms to solve the problem of asymmetric information, as bonding, that should be preferred to wage premiums. Theoretical arguments, casual observations and even anecdotal evidence have been offered in order to support or dismiss whether these restrictions are actually the case. However, as pointed out by Dickens, Katz and Lang (1985), efficiency wages cannot be dismissed on a priory theoretical grounds and evidence is needed and therefore the validity of efficiency wages is an issue that can only be resolved empirically.

Although, there is a vast number of empirical studies of efficiency wages, there are many who view the evidence as unpersuasive and inconclusive (Manning and Thomas, 1997; Autor, 2003). This is mainly due to numerous problems that render the empirical testing of efficiency wages particularly vexing. Probably the majority of problems could be possibly summarized as related to identification[2], arising mainly because efficiency wages are by definition endogenous[3] and arise under situations of asymmetric information which makes it impossible for the econometrician to observe the outcomes of interest (as for example worker's productivity or type).

Out of the numerous empirical attempts to test some of the implications of efficiency wages models, the most credible studies to date are those that find ingenious ways to properly address the identification problem either by analysing sample of firms in sectors where there is limited concern of unobserved heterogeneity (Cappelli and Chauvin, 1991; Krueger, 1991), or by exploiting natural experiments (Groshen and Krueger, 1990; Holzer, Katz and Krueger, 1991;

[1] Under asymmetric information higher wages decrease shirking (Shapiro and Stiglitz, 1984), reduce quits and turnover costs (Salop, 1979), improve the quality of potential employees (Weiss, 1980) and workers' association with the firm (Akerloff, 1982).

[2] This is mainly a problem of empirical studies based on observational data. In recent years there has been also evidence produced by laboratory experiment providing some support to efficiency wages and in particular to the "gift-exchange" model (Fehr, Gachter and Kirchsteiger, 1993, Fehr and Falk, 1999). However, the results of these experiments have been challenged by more recent evidence (Gneezy and List, 2006) that fail to provide support to the "fair wage-effort" hypothesis (at least in the long-run) and by the criticism related to the extent that the behaviour of laboratory subjects can be a good indication of actual behavior in labour markets.

[3] This simply means that wages cannot be used as a right-hand side variable in a regression where the dependent variable is a proxy of employees' productivity (Wadhwani and Wall, 1991; Konings and Walsh, 1994).

Rebitzer, 1995). Most of these studies report evidence of a negative relationship between higher wages and alternative means of regulating employees' effort (supervision) which is consistent with a prediction of the shirking model of efficiency wages (Shapiro and Stiglitz, 1984). This evidence can be viewed as indirect evidence of positive wage effects on workers' productivity.

The main criticism of the above studies has been that the evidence produced is necessary but not sufficient for efficiency wages, as it is also consistent with other explanations[4]. The most important limitations of studies exploiting a quasi-experimental design derives from its central innovation, i.e. the exploitation of the unusual features of a specific labour market, as one cannot support that the same results would be the case in another setting or labour market. Despite the limitations and criticism there are many who believe that this evidence is as "good as it gets" (Autor, 2003).

As Rebitzer (1995) puts it "It is too early to know whether the theory of efficiency wages will survive rigorous empirical investigation. The difficult econometric problems such investigations confront make it unlikely that any single study will settle the issue decisively. The empirical fate of efficiency wage theory will more likely be determined by evidence from a variety of different investigations-each having important limitations and qualifications."

The purpose of this paper is to offer an investigation of the shirking model by exploiting the link between efficiency wages and the minimum wage. Such link can be justified firstly by the theoretical argument that a binding minimum wage and other features of low-wage labour markets impose constraints in the implementation of first-best contracts and thus open the door to efficiency wages (Krueger, 1991; Georgiadis, 2006).

Another link is offered by the fact that efficiency wages models (Calvo and Wellisz, 1979; Manning, 1995; Rebitzer and Taylor, 1995) have been deployed to explain the striking evidence of a non-negative employment effect of the minimum wage, produced by several empirical minimum wage studies since the early 1990s (Card and Krueger, 1994, 1995).

Finally and probably most importantly the minimum wage satisfies the above market-clearing property of the efficiency wage as it creates a wedge between the wage at the current job and alternative wages[5] and provides a quasi-experimental design to study any effects of wages on worker's productive behaviour.

Our identification strategy is based on exploiting variation in wages gener-

[4] A positive relationship between productivity (effort) and wages could be also an implication of an equalising differences framework, under which employers who want their workers to work harder should pay higher wages to compensate them for the disutility of effort. The additional condition that sorts out efficiency wages from alternative explanations is that wages are set optimally so that the cost of the wage is offset by an increase in revenue or a fall in non-wage costs (Autor, 2003).

[5] Based on our theoretical model, although an economy-wide minimum wage raises also alternative wages, it creates a wedge between compensation in current job and alternative jobs, as in general equilibrium the imposition of a binding minimum wage reduces employment and increases labour force participation and thus it reduces the probability of finding a job elsewhere if one is caught shirking and being dismissed (Georgiadis, 2001, 2006).

ated by the 1999 introduction of the UK National Minimum Wage (NMW) on a sector of very low-wage firms, the residential care homes sector, to identify the relationship between wages and monitoring and thus test for the wage-supervision trade-off implication of the shirking model.

We find evidence that in care homes in which the NMW had larger impact on the wage bill, monitoring, as measured by different ratios of senior to junior employees fell by more, compared to homes that were less affected by the NMW. Our estimates suggest that wage increases induced by the NMW were on average more than offset by a fall in monitoring costs. This latter finding is further supported by evidence that the NMW had no effect on care homes profitability.

All the above evidence, combined with previous findings from the care homes sector (Machin, Manning and Rahman, 2003) that employers didn't increase effort on the job and thus didn't dissipate rents generated by the NMW, may suggest that the NMW may have operated as an efficiency wage in the care homes sector.

Overall, our paper not only provides a credible test of the shirking model, but also offers an empirical test of efficiency wage models (Calvo and Wellisz, 1979; Manning, 1995; Rebitzer; 1995) developed to explain the evidence of a non-negative minimum wage employment elasticity, which has been missing in the literature.

In this way our study provides evidence which supports an efficiency wage explanation of the recent findings from the care homes sector (Machin, Manning and Rahman, 2003; Machin and Wilson; 2004), where although the wage structure was heavily affected by the NMW introduction there were only moderate employment effects.

2 A Simple Model

In this section we extend the model of Rebitzer and Taylor[6] (1995) which was developed to explain the empirical findings of a non-negative minimum wage employment elasticity, to account for endogenously determined supervision[7].

Consider a competitive industry with a large number of identical firms, where the representative firm recruits a number of low-skilled, low-wage workers to

[6] Rebitzer and Taylor modified the Shapiro-Stiglitz model (1984) by treating the probability of detecting a shirker as inversely related to the size of the workforce (in the Shapiro-Stiglitz model the probability of detecting a shirker follows a poisson process) but assume that supervisory capacity is fixed.

[7] Rebitzer and Taylor's (RT) key result is a special prediction of a more general model presented by Calvo and Wellisz (CW) (1979), a fact that has been neglected in the literature. The two models differ only in terms of the returns to scale to production, as RT assume decreasing and CW constant returns to scale. However, their results are the same qualitatively, i.e. that a just binding minimum wage increases the employment of affected workers. By relaxing the simplified assumption of fixed supervision in RT, Georgiadis (2001, 2006) shows that the employment effect of a higher minimum wage cannot be positive although it can be zero for a just binding minimum wage. Moreover, Georgiadis (2001, 2006) also shows that the positive employment effect in CW hinges heavily on the assumption of constant returns to scale and cannot be sustained if one assumes a decreasing returns production technology.

produce a single product. Workers are homogeneous, infinitely lived and risk-neutral with instantaneous utility function given by:

$$U(w, e) = w - e \ (1)$$

For a given wage offer, the worker must decide the level of effort he/she will exert, which for simplicity is assumed to be binary, i.e. 0 if shirk and 1 if work.

$$e \in \{0, 1\} \ (2)$$

As in the Shapiro and Stiglitz)1984) model, the standard assumption here is that effort is imperfectly observed and that the only device to prevent shirking is the threat of dismissal[8]. Some monitoring is needed in partial equilibrium so that dismissal threats are non-empty, and this is why the firm employs supervisors, for whom we assume that there are no shirking considerations[9].

The instantaneous probability of detecting a shirker is given by:

$$P = Min\{\frac{N}{L}, 1\} \ (3)$$

, where N and L is the number of supervisors and production workers respectively[10].

In line with the Shapiro and Stiglitz (1984) and Rebitzer and Taylor (1995) we assume that workers who are caught shirking flow to unemployment and receive an unemployment benefit μ, and that the probability of quitting, the probability of finding a job and the discount rate are q, s and r respectively.

The present discounted value (p.d.v.) of expected lifetime utility of an unemployed a worker, V^w, can then be written as:

$$V^w = w - e + \frac{(1-q)V^w + qV^u}{1+r} \ (4)$$

, where V^u is the p.d.v. of expected lifetime utility of an unemployed worker. Similarly, the p.d.v. of expected lifetime utility of a shirker is given by:

$$V^s = w + \frac{(1-P)(1-q)V^s + [1-(1-P)(1-q)]V^u}{1+r} \ (5)$$

[8] First best motivation devices, as bonding, are ruled out because of capital market imperfections, moral hazard problems on the side of the employer or because of a binding minimum wage (Weiss, 1991; Krueger, 1991).

[9] This is possible, if bonding can be implemented for supervisors but not for production workers, which can be true if one thinks of supervisors as high-skilled, high-wage workers, for whom the minimum wage does not prevent employers tilting optimally the wage-tenure profile.

[10] We assume that 1 in equation (3) is never binding, otherwise the model specialises to the standard one in the theory of the firm. Odiorne (1963) and Gordon (1990, 1994) suggest that the supervisor to staff ratio is likely to be highly correlated with the extent of monitoring.

Finally, the p.d.v. of expected lifetime utility of an unemployed worker V^u, is given by the following equation[11]:

$$V^u = \mu + \frac{sV^w + (1-s)V^u}{1+r} \quad (6)$$

A worker will shirk unless the p.d.v. of expected lifetime utility of shirking is less than or equal to that of working. This is expressed by the following equation:

$$V^w \geq V^S \quad (7)$$

Combining (4), (5), (6) and (7) we obtain:

$$w \geq \mu + e + \frac{e(r+s+q)}{P(1-q)} \quad (8)$$

Equation (8) is known as the non-shirking condition (NSC) (Shapiro and Stiglitz, 1984), and expresses the set of all wages that prevent shirking for any given value of e, μ, r, s, q and P. Under this setting it is rather intuitive that a profit maximising firm will be willing to pay the lowest possible wage associated with non-shirking. Using equation (3), (8) and the fact that the NSC is binding in equilibrium we get equation (9).

$$w^* = \mu + e + \frac{e(r+s+q)}{\frac{N}{L}(1-q)} \quad (9)$$

Equation (9) implies the prediction of the standard shirking model that in equilibrium there is a trade-off between wages and the probability of detection as expressed by the supervisor to staff ratio $\frac{N}{L}$. Equation (9) can be rearranged to express monitoring intensity as a function of the optimal wage:

$$\frac{N}{L} = \frac{e(r+s+q)}{(w-\mu-e)(1-q)} \quad (10)$$

The introduction\increase of a minimum wage under this framework will raise wages above alternative opportunities[12], which in turns increases the penalty of shirking and thus worker's propensity to shirk and this is why monitoring intensity can be relaxed.

In general equilibrium, where all firms in the sector pay the minimum wage, we need some unemployment to prevent shirking. As shown in Georgiadis (2001), under this model a binding minimum wage decreases employment at

[11] We assume throughout that once a worker chooses to shirk he/she will always shirk and will always work once he/she chooses to work.

[12] In the model this is μ which stands for the unemployment benefit or value of leisure which is equal to the market clearing wage.

the firm level[13], which in general equilibrium leads to a reduction in the probability of finding a job s[14], leading to an equilibrium outcome under which all employees are paid the minimum wage and do not shirk, but they are supervised less stringently[15].

Equation (10) is the equation of interest for our empirical analysis in the following sections.

3 Empirical Problems and Identification Strategy

This section discusses the econometric problems that arise when one attempts to estimate an empirical counterpart of equation (10), and the strategy we implement in order to tackle them.

Empirical tests of the wage-supervision trade-off have been mainly hindered by endogeneity arising from simultaneity, omitted variables and measurement error (Groshen and Krueger, 1990, Rebitzer, 1995, Brunello, 1995).

Simultaneity arises because wages and supervision intensity are motivation devices which are set optimally and simultaneously to minimise costs per efficiency unit of labour (Georgiadis, 2001). Moreover, as suggested by Rebitzer (1995), unobserved features of human resource policies that affect employees' motivation (e.g. employee screening) will be also correlated with supervision intensity and wages. The likely effect of failing to control for these factors will be a positive omitted variable bias which masks any underlying trade-off between wages and supervision (Leonard, 1987; Rebitzer, 1995).

Another concern in empirical studies of the trade-off arises by measurement error in supervision intensity. This is because most studies use the ratio of supervisors to supervised as a proxy for monitoring, which does not distinguish between supervisors whose primary job is regulating the activities of lower level employees and employees with supervisory job titles who nevertheless have a direct role to play in production. Thus, the supervisor to supervised ratio tends to overestimate the extent of monitoring (Kruse, 1992).

Moreover, the supervisors to staff ratio may be also problematic because it is associated with the quantity of monitoring and not the quality (Brunello, 1995). However, measurement error seems to be more of a concern in these studies, as they attempt to estimate an empirical analogue of equation (9),

[13] This result suggests that the prediction of Rebitzer and Taylor (1995) that binding minimum wage may have a positive employment effect is not robust when supervision is endogenised. The negative employment effect of the minimum wage is of second-order when the minimum wage is set infinitesimally above the initial (efficiency) wage.

[14] The probability of finding a job is expected to fall even if employment is unchanged as a result of the minimum wage imposition (this is the case in our model if the minimum wage is set infitesimally above the initial optimal wage), as labour force participation is expected to rise.

[15] This point suggests that the key prediction of the model of Rebitzer and Taylor that in partial equilibrium a just binding minimum wage increases employment, is not robust under general equilibrium.

where supervision is a right-hand side variable and thus measurement error leads to inconsistent estimates of the causal effect of interest. Alternatively, if one estimates an equation with supervision as the dependent variable (as in equation (10)) measurement error is less of a problem, although it leads to a loss in precision[16].

Another potential source of upward bias in the wage-supervision relationship may also arise because of labour demand adjustments, as an increase in the wage of supervised staff may lead to an increase in the ratio of supervisors to production workers, provided that the production function allows for some substitution between the two inputs (Groshen and Krueger, 1990).

A final problem highlighted in the empirical literature of efficiency wages, is that there are alternative theories that are consistent with a wage-supervision trade-off (Kruse, 1992). One of these theories is the "sorting by ability model" (Groshen and Krueger, 1990), which it is predicated on the assumption that more able employees are supervised less stringently because they need less co-ordination and guidance on the job.

If low-ability workers are paid lower wages, then this model also generates a prediction of the wage-supervision trade-off, as a cost-minimising firm will set wages up to the point where the marginal benefit of a fall in wages and thus in the average ability of workforce is exactly offset by the increase in supervision costs, as lower average ability of workforce will demand more supervision. Empirically this problem is translated to an omitted ability bias, which leads to a downward bias in the estimate of the wage-supervision relationship.

Our empirical strategy is based on exploiting the exogenous variation in wages generated by the 1999 introduction of the UK National Minimum Wage NMW) in a very low-pay sector, the residential care homes industry. We estimate the causal effect of the change in the wage before and after the NMW introduction on the change in supervision intensity implementing IV methods, where measures of the impact of the NMW across homes are used as instruments for the change in the wage.

In particular we are estimating the following system of equations:

$$\Delta S_{it} = \beta_0 + \beta_1 \Delta \ln W_{it} + \beta_2 \Psi_{i,t-1} + u_{it} \quad (11)$$

$$\Delta \ln W_{it} = \alpha_0 + \alpha_1 MIN_{i,t-1} + \alpha_2 \Psi_{i,t-1} + v_{it} \quad (12)$$

,where ΔS_{it} is the change in the measure of supervisors to supervised ratio for home i between the period before $(t-1)$ and after (t) the NMW introduction, $\Delta \ln W_{it}$ is the change in the natural logarithm of average hourly wage at home i in the before and after NMW introduction period, $MIN_{i,t-1}$ is a measure of the impact of the national minimum wage on home i (defined later), $\Psi_{i,t-1}$ is $(t-1)$ level home and worker characteristics and u_{it} and v_{it} are error terms.

[16] This is true provided that the measurement error has zero mean and is uncorrelated with the other regressors. Even in the case that the measurement error has no zero mean, as we suggest that the supervisor to staff proxy systematically overestimates the extent of monitoring, this only affects the estimation of the intercept (Wooldridge, 2002).

The key parameter of interest is β_1, which measures the relationship between wage changes and the change in supervision intensity after controlling for other factors such as home and workers' characteristics.

Following closely Machin, Manning and Rahman (2003) we use two measures of the impact of the UK NMW, the one is the proportion of workers at home paid below their age specific NMW before the NMW introduction and the other is the wage gap which is the proportional increase in the weekly wage bill if the wages of all workers paid below the NMW before the NMW introduction are raised at their age-specific NMW. The wage gap is defined as follows:

$$GAP_i = \frac{\sum_j h_{ji} \max(W_{ji}^{\min} - W_{ji}, 0)}{\sum_j h_{ji} W_{ji}} \quad (13)$$

,where h_{ji} is the weekly hours worked by worker j in firm i, Wji is the hourly wage of worker j in firm i, and $W_{ji}^{\min}$ is the minimum wage relevant for worker j in firm i (the adult rate or the development rate designed for those between 18 and 21 inclusive).

The empirical strategy described above, which is very similar to that employed by Machin, Manning and Rahman (2003), allows us to address the identification problem that compounds the estimation of the causal relationship between wages and supervision in the literature. In particular, the NMW is expected to be a valid and strong instrument for the change in the wage that tackles any endogeneity problem due to simultaneity and omitted variables (included omitted ability)[17].

Moreover, the nature of the data is such that limits problems of unobserved heterogeneity, as the care homes sector is characterised by homogeneous occupations and workers skills and homogeneous services. Unobserved heterogeneity or omitted variables problems are further tackled by the fact that we observe outcomes at two points in time (before and after the April 1999 NMW introduction), which allows to use first differences specifications that control for time-invariant unobserved factors that may affect the relationship of interest.

Measurement error as discussed above is expected to be less of a problem compared to studies where supervision intensity is a causing variable. Moreover, under the Classical Error in Variables (CEV) framework and if the measurement error is additive and time-invariant[18], first differencing is expected to effectively eliminate it.

Another concern, as pointed out previously, arises by labour demand adjustments which are expected to have an effect in the relative employment of

[17] This holds as long as variation in $MIN_{i,t-1}$ is not driven by variation in initial wages, as the level of the NMW is the same for all regions, sectors and workers (given that they are in the same category i.e. adults). Machin, Manning and Rahman (2003) test this identifying assumption and provide evidence that supports its validity, i.e. that the relationship between the change in the wages and initial wages has shifted in the period of the introduction compared to a counterfactual period where no minimum wage was introduced.

[18] Measurement error in supervision intensity may be caused by omitted quality of supervision which may be related to time-invariant unobserved firm-specific characteristics as managerial talent.

supervisors and production workers. The direction of labour demand adjustments depends on the assumptions about the production technology.

For example, if high-skilled supervisory workers and low-skilled production employees are (gross) substitutes in production then we expect that as the minimum wage renders the latter relatively more costful and optimal adjustments will lead to an increase in the supervisor to staff ratio. In this case the labour demand effect will counteract the efficiency wage effect, and thus evidence supporting a negative relationship between wages and supervision makes the negative effect even more compelling.

4 The Data and Descriptive Statistics

The data used in our analysis were collected by the Centre for Economic Performance at LSE through postal surveys implemented before and after the April 1999 UK NMW introduction, as the main objective was to use the collected information to evaluate the economic effects of minimum wages (see Machin, Manning and Rahman, 2003 and Machin and Wilson, 2004, for details about the survey design).

Questionnaires were addressed to home managers (who are often are the home owners) asking question on home characteristics (ownership, whether home is part of larger organisation, the number of registered beds, the number of residents, etc.). Most importantly managers were also asked to provide data on job title, sex, age, length of service, possession of a nursing qualification, weekly hours and weekly wages for all workers[19].

Table 1 presents descriptive statistics on the basic characteristics of homes and of some measures of the intensity of supervision. The average home is small in size (both in terms of the number of employees, or the number of beds or residents), and the average hourly wage is quite low, around £4, suggesting that the impact of the NMW introduction which was set at £3.6 per hour[20] is expected to be significant[21].

Other prevalent characteristics of the sector are that the vast majority of employees are female (around 92% in both the full and the balanced sample), the average employee age is around 40 years, that the principal occupation is that of care assistants[22] and that only one in ten employees has a nursing qualification (the only relevant qualification/skill in the sector).

[19] Based on Machin et al. (2003) the sample of responding homes is representative of the population of homes as a whole in terms of age, hours, job tenure and wages of workers.

[20] This is the adult rate, wth the development rate (the effective minimum wage for those aged between 18 and 21 inclusive) set at £3. The adult rate is expected to be the main rate applied as employees between 18-21 years old are a very small fraction of total employment in the care homes sector and the evidence suggests that the development rate wasn't used for the majority of those people covered by the development rate (Metcalf, 2004).

[21] Machin et al (2003) and Machin and Wilson (2004) for present statistics of the "bite" of the minimum wage which suggest that the impact of the NMW introduction is very heavy.

[22] This is one of the most important reasons of why wages are quite low in the sector as the occupation of care assistants is among the lowest paid occupations in the UK (Machin et al., 2003).

The fact that the average home is small in size may suggest that monitoring problems are not important as the manager/owner can easily monitor employees effort. However, the nature of the services provided by the care homes is such that homes operate twenty four hours a day and seven days a week, which makes it impossible for the owner to monitor employees effort. This is why all managerial staff could be involved in monitoring the activities of employees in lower ranks.

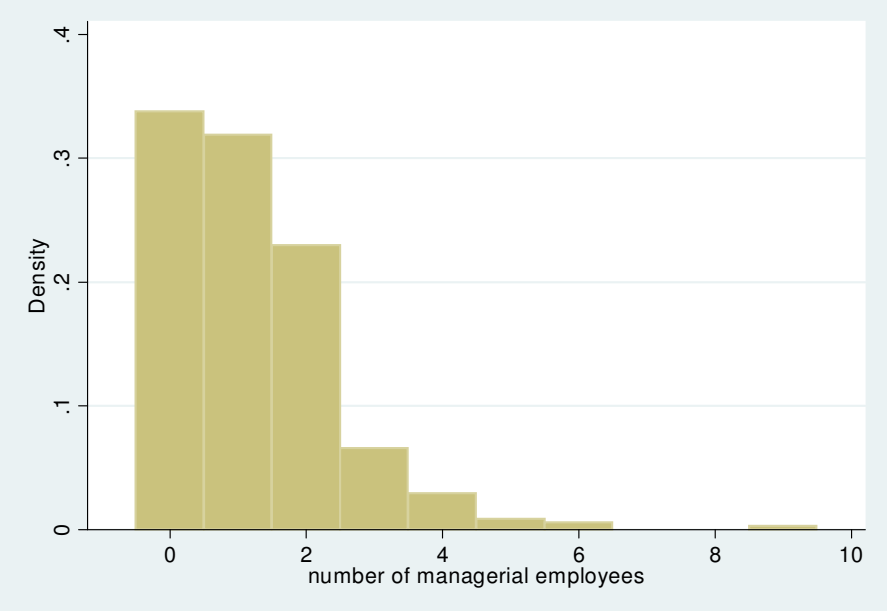
Table 1: Survey Descriptive Statistics

	All Firms		Balanced Panel	
	Pre-Minimum	Post-Minimum	Pre-Minimum	Post-Minimum
Number of Homes	1646	2366	683	683
Number of Workers	17.51	17.63	16.15	16.65
	(19.78)	(23.61)	(9.69)	(12.09)
Hourly Wage	4.04	4.24	4.01	4.27
	(0.85)	(0.81)	(0.8)	(0.8)
Proportion Female	0.91	0.92	0.92	0.92
	(0.11)	(0.11)	(0.11)	(0.11)
Average Age	40.25	40.52	40	40.58
	(6.6)	(6.8)	(6.45)	(6.8)
Proportion Care Assistants	0.62	0.61	0.62	0.63
	(0.26)	(0.27)	(0.26)	(0.26)
Proportion With Nursing Qualification	0.09	0.1	0.1	0.1
	(0.17)	(0.18)	(0.18)	(0.17)
Number of Beds	26.54	25.29	18.68	19.26
	(83.56)	(68.29)	(18.02)	(19.45)
Number of residents	22.74	22.28	16.55	17.12
	(71.67)	(60.27)	(17.34)	(18.4)
Number of managers to number of non-managers	0.13	0.14	0.14	0.13
	(0.3)	(0.37)	(0.35)	(0.29)
Weekly hours of managers to weekly hours of non-managers	0.27	0.29	0.29	0.27
	(1.03)	(1.02)	(1.02)	(0.75)
Number of employees with nursing qualification to number of those with no qualification	0.19	0.19	0.24	0.17
	(0.68)	(0.67)	(1.02)	(0.46)
Weekly hours of employees with nursing qualification to weekly hours of those with no qualification	0.27	0.25	0.39	0.22
	(1.34)	(0.99)	(2.01)	(0.71)
Number of senior care assistants to number of junior care assistants	0.29	0.28	0.28	0.28
	(0.61)	(0.54)	(0.52)	(0.47)
Weekly hours of senior care assistants to weekly hours to junior care assistants	0.42	0.39	0.36	0.37
	(1.4)	(1.06)	(0.71)	(0.65)

Notes: Standard deviations in parentheses. Pre-minimum observations refer to responses received before April 1999 and Post-minimum to responses received after March 1999. Care assistants include senior, day, and junior carers but exclude night carers and sleep-ins.

The distribution of the number of managerial employees across homes in our sample, presented in figure 1, indicates that one third of homes in the sample have no managerial employee in place, whereas one third has only one managerial employee, and 40% of homes have more than 1 managers working at home[23].

Figure 1: Sample distribution of the number of managerial employees across care homes



Thus, considering that in homes with managerial employees the one or two managers working there on average are not expected to have only monitoring responsibilities but also the fact that in a large proportion of care homes in the sample there are no managerial employees, it is expected that other senior employees, as senior care assistants and employees with nursing qualification will be those responsible for checking the activities of less senior employees.

Table 1 indicates that there are around 8 non-managerial to every managerial employee at home on average, whereas one qualified employee for every 5 non-qualified and that to every hour of work of a senior carer correspond two hours of a junior one. In our regressions, we use both bodies and hours measures of the three supervision intensity ratios.

[23] As managers are defined employees with job title; manager/head of home, matron and deputy matron, assistant manager and supervisor.

5 Results

The first stage of our empirical strategy is to estimate equation (12) where the main causing variable, the change in the log hourly wage, is regressed on the instrument (the measure of the minimum wage). This is important, as checking whether the NMW had any effect on the wage structure of the care homes sector is a necessary condition in looking at the effects of any wage change, generated by the NMW on other outcomes. Moreover, this first stage regression is a part of the 2SLS estimation method and allows us also to check the strength of the instrument.

Table 2: Home Level Wage Effects

	Change in log Hourly Wage			
Pre-introduction NMW impact measure	(1)	(2)	(3)	(4)
Initial period low pay proportion	0.15** (0.015)	0.15** (0.015)		
Initial period wage gap			0.87** (0.11)	0.9** (0.12)
Controls	No	Yes	No	Yes
R^2	0.14	0.19	0.23	0.3
No. of observations	633	601	633	601

**significant at 1%, robust s.e. in parentheses. Controls include: proportion female,average age, proportion with nursing qualification, proportion of la/dss residents and county dummies.

Table 2 presents regressions of change in the log hourly wage before and after the 1999 NMW introduction on the two minimum wage impact measures (the proportion of workers affected and the wage gap) using specifications that include or not other controls for home and worker's characteristics. The estimates presented, which are in line with those presented by Machin et al (2003) and Machin and Wilson (2004) suggest that the NMW generated a significant boost in the average hourly wage across homes.

In particular, estimates that a workers in a care home that had 10% of workers that were paid below their age-specific minimum, experienced a 1.5% increase in the growth of average hourly wages relative to workers in a home with no affected workers in their payroll. Alternatively, workers in a firm that required 10% increase in its weekly wage bill to comply with the minimum experienced a 9% increase in the average wage growth relative to workers in a firm already paid at least the minimum.

Table 3: OLS versus 2SLS estimates of the relationship between the change in the wage and the change in the ratio of managerial employment to non-managerial (bodies/hours)

	Change in managerial to non-managerial (bodies)			Change in managerial to non-managerial (hours)		
	(1)	(2)	(3)	(1)	(2)	(3)
	OLS	2SLSa	2SLSb	OLS	2SLS	2SLSb
Change in log average wage	-0.193 (0.138)	-0.862 (0.626)	-0.623 (0.352)$^{+}$	-0.391 (0.594)	-2.309 (2.183)	-3.650 (2.660)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.23	0.07	0.16	0.15	0.08	0.11
Number of Homes	584	584	584	567	567	567

$^{+}$significant at 10% (5% for a one-tailed test alternative), robust s.e. in parentheses. 2SLSa is using the proportion low-paid as an instrument for the change in the wage, and 2SLSb is using the wage gap as an instrument for the change in the wage. Controls include proportion female, average age, proportion with nursing qualification, proportion of la/dss residents, county dummies, response-month dummies, whether part of larger organisation, ownership type, recruitment rate, quit rate, average tenure and the ratio of residents per employee.

Thus, first-stage regression results suggest that there is no concern of a weak instrument and overall, given that the NMW is introduced exogenously, that both instruments are expected to be valid.

Table 3 presents estimation results of the structural equation (11), where both OLS and 2SLS estimates, using separately each instrument are included, and where the change in the ratio of managerial to non-managerial employment (both in bodies and in hours) at home is used as the proxy for supervision intensity.

Results seem to provide some weak support to the wage-monitoring trade-off story, as a negative and significant (at 5% level for a one tail test alternative, that the coefficient of the change in log hourly wage is negative) 2SLS estimate is produced when the change in the managerial to non-managerial employees is the supervision measure and the wage gap is used as an instrument for the change in log hourly wages.

Comparing OLS and 2SLS estimates in table 3 suggests an upward OLS bias, which is consistent with the efficiency wage/shirking model that wages and supervision are substitutes in inducing employees productivity. As it is expected 2SLS estimates have higher standard errors than OLS which may be also due to the presence of measurement error in supervision intensity as discussed in the previous section.

In table 4 we present estimation results using the ratio of employment of qualified and non-qualified employees, i.e. bodies and hours of employees with nursing qualification relative to those with no nursing qualification. In this case, there is some evidence of a significant negative effect (at 5% level for a one tail test alternative, that the coefficient of the change in log hourly wage is

negative) of the change in log hourly wage on the change in supervisory intensity of employees with no nursing qualification both in bodies and hours. Again the OLS bias seems to be uniformly positive.

Furthermore, estimation results presented in table 5, where monitoring intensity is measured by the relative employment of senior to junior care assistants again both in bodies and hours provide stronger support to the wage-supervision trade-off prediction of the shirking model, as 2SLS estimates of the parameter of interest are negative and more strongly significant (at 2.5% level for a one tail test alternative, that the coefficient of the change in log hourly wage is negative).

In particular, a significant negative effect of the change in log hourly wage on relative employment of senior to junior care assistants is found when the proportion of affected workers is used as an instrument and a similar effect on relative employment of care assistants measured in hours when the wage gap is the chosen instrument. Once more comparisons of OLS with 2SLS estimates shows towards a positive bias in the coefficient of the change in hourly wage.

Table 4: OLS versus 2SLS estimates of the relationship between the change in the wage and the change in the ratio of employment of qualified to non-qualified (bodies/hours)

	Change in qualified to non-qualified (bodies)			Change in qualified to non-qualified (hours)		
	(1)	(2)	(3)	(1)	(2)	(3)
	OLS	2SLSa	2SLSb	OLS	2SLSa	2SLSb
Change in log average wage	-0.067 (0.185)	-0.285 (0.493)	-0.593 (0.330)$^{+}$	-0.280 (0.521)	-0.429 (0.940)	-1.969 (1.145)$^{+}$
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.31	0.31	0.31	0.27	0.27	0.26
Number of Homes	550	550	550	545	545	545

+significant at 10% (5% for a one-tailed test alternative), robust s.e. in parentheses. 2SLSa is using the proportion low-paid as an instrument for the change in the wage, and 2SLSb is using the wage gap as an instrument for the change in the wage. Controls include proportion female, average age, proportion with nursing qualification, proportion of la/dss residents, county dummies, response-month dummies, whether part of larger organisation, ownership type, recruitment rate, quit rate, average tenure and the ratio of residents per employee.

In particular, a significant negative effect of the change in log hourly wage on relative employment of senior to junior care assistants is found when the proportion of affected workers is used as an instrument and a similar effect on relative employment of care assistants measured in hours when the wage gap

is the chosen instrument. Once more comparisons of OLS with 2SLS estimates shows towards a positive bias in the coefficient of the change in hourly wage.

All in all we find some evidence of a negative effect of the change in the wage generated by the NMW introduction in the supervision intensity of non-managerial employees and employees with no nursing qualification and stronger evidence that higher wages relax supervision for less senior care assistants which seems to provide support to the prediction of the shirking model of a wage-supervision trade-off. Additional upward bias due to potential substitution of high-skilled employees with supervisory responsibilities for low-skilled supervised employees, and larger standard errors because of measurement error in the dependent variable make our findings of the wage-supervision trade-off more compelling.

Our findings that OLS estimates are uniformly positively biased across all specification used compared to the 2SLS estimates provide further support to the shirking model which predicts that omitted features of human resources and personnel policy that are correlated with employees' motivation/productivity tend to mask any wage-supervision that may be in operation. On the other hand a positive OLS bias is not consistent with the "sorting by ability model", where omitted ability bias is predicted to be negative as more able employees are paid higher wages and are supervised less stringently[24].

The latter evidence provides indirect support that higher wages are an employees' motivation device, which is a necessary but not sufficient condition for the shirking model to hold. This is because this condition is consistent with principal-agent models many of which do not have the efficiency wage property that the principal (employers) offers the agent (employees) a level of utility strictly above what they could get on the open labour market (Manning and Thomas, 1997).

Thus, for efficiency wages to hold, except of evidence of wage effects on workers' productivity, one needs to show also that employees are receiving rents, i.e. that employers do not dissipate the rents generated by the NMW introduction by deteriorating working conditions or increasing effort/intensity on the job, and that any employees' rents are set optimally so that the marginal cost of rents is exactly offset by the marginal benefit.

As far as the condition of rent dissipation by employers is concerned, Machin et al. (2003) report evidence that subjective effort and the intensity on the job, as measured by the number of residents per employee didn't change as a result of the NMW introduction. This evidence, combined with the fact that there are no fringe benefits or training provision in the care homes sector may be interpreted as evidence that care homes employees in minimum wage jobs receive rents by employers[25]. Rents in current jobs relative to alternative opportunities are

[24] Note also that if one controls effectively for unobserved ability, then if the sorting by ability model is true, the relationship between wages and supervision intensity should be positive, which is not the case here. This is because higher wages enable the firm to increase the average ability of the workforce and higher supervision to decrease it.

[25] Note that the existence of rents rules out, explanations of the wage-supervision trade-off, based on equalising differences, according to which higher wages serve as compensation for

justified by the fact that the NMW increases the opportunity cost of job search or decreases the probability of finding a job as it is expected to increase labour force participation. Alternatively, rents may result due to regional variation in prices, and thus regional differences in the real value of the NMW across UK regions.

Moreover, we fail to find any significant effects of the NMW on care homes profitability[26], which may be possibly explained by the fact that the increased costs induced by the NMW were offset by a fall in supervision costs. The latter explanation becomes even more compelling if one considers that Machin, Manning and Rahman (2003) failed to find evidence of other offsets, including price offsets (as prices in the care homes sector were capped by local authorities at the window of the NMW introduction), except of some moderate employment effects.

Table 5: OLS versus 2SLS estimates of the relationship between the change in the wage and the change in the ratio of employment of senior carers to junior carers (bodies/hours)

	Change in senior cares to junior carers (bodies)			Change in senior carers to junior carers (hours)		
	(1)	(2)	(3)	(1)	(2)	(3)
	OLS	2SLSa	2SLSb	OLS	2SLSa	2SLSb
Change in log average wage	-0.103 (0.155)	-1.199 (0.577)*	-0.099 (0.257)	-0.146 (0.282)	-5.351 (3.836)	-1.724 (0.775)*
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.06	0.03	0.06	0.17	0.1	0.11
Number of Homes	510	510	510	497	497	497

* significant at 5% (2.5% for a one-tailed test alternative), robust s.e. in parentheses. 2SLSa is using the proportion low-paid as an instrument for the change in the wage, and 2SLSb is using the wage gap as an instrument for the change in the wage. Controls include proportion female, average age, proportion with nursing qualification, proportion of la/dss residents, county dummies, response-month dummies, whether part of larger organisation, ownership type, recruitment rate, quit rate, average tenure and the ratio of residents per employee.

Estimates of the wage supervision trade-off allow us to provide a test of whether higher wages "paid for themselves" (Levine, 1992) using information from the sample and comparing the marginal cost of higher wages to their

the disutility of the extra effort exerted by employees.

[26] Results of NMW effects on profitability are not reported here but they are available on request. Machin and Wilson (2004) report evidence that the NMW had no effect on the exit of firms in the sector, which seems to be consistent with an insignificant profit effect of the NMW care homes sector, given that care homes operate in a competitive industry.

marginal benefit[27]. Our simple calculations suggest that the marginal benefit more than offset the marginal cost of the higher wage[28].

However, one should be very cautious in interpreting these results as supporting a motivation/efficiency-enhancing effect of the higher wage, as it may be the case that a decrease in the ratio of senior to junior carers may reflect a fall in the quality of care, as there are less senior (qualified) employees to support care homes residents after the NMW introduction. In order to test this hypothesis we estimate reduced form equations in which the dependent variable is the change in hours of senior carers per resident and hours of employees with nursing qualification per resident between the period before and after the NMW introduction[29].

The results presented in table 6 suggest no significant effect of the NMW on the number or hours of senior carers and employees with nursing qualification per resident, and thus no indication of a fall in the quality provided by care homes. The latter finding combined with the results indicating a fall in the number of senior carers implies a fall in the actual number of residents in the average home in the sector. This is evidence suggesting a contraction in the volume of services (output) which seems no surprising given the moderate reduction in employment in the sector.

Overall, our findings presented in this section seem to support the wage-supervision trade-off prediction of the shirking model which can be interpreted as providing indirect evidence of productivity-enhancing effects of higher wages. Combining the latter evidence with other findings from the care homes sector that employers didn't dissipate rents generated by the NMW introduction by increasing effort on the job and that there was no significant profit effects of higher wages may suggest that the NMW operated as an efficiency wage in the care homes sector.

Thus, this evidence may provide a potential explanation of recent findings that although the wage structure in the sector was heavily affected by the NMW there were only moderate employment effects, which further provides empirical support to efficiency wages models of the minimum wage literature (Calvo and

[27] Etimated results from table 5 suggest that a 1% increase in the average wages at home resulted in 1.2 less senior carer per junior care or 1.7 less hours of senior carers per hour of junior carer. The marginal cost of higher wages is calculated as 1% increase in the average wage of supervised employees (junior carers) and the benefit of the wage is the fall in supervision costs generated by the fall in the number (hours) of senior carers per (hour of) junior carer.

[28] The marginal cost of the wage in this particular case is $1\% * £3.8 = £0.038$, which is the average wage of junior carers in the sample, i.e. 3.8 pence per hour per junior carer. The marginal benefit based on both estimates of the trade-off (in bodies and in hours) is the same, i.e. $\frac{1.2}{0.28} * \frac{£4.28}{100} = £0.18$, and $\frac{1.7}{0.4} * \frac{£4.28}{100} = £0.18$, 18 pence per hour of junior carer where 0.28 is the average ratio of the number of senior to junior carers in the sample, 0.4 is the average ratio of hours of senior to junior carers and £4.25 is the hourly wage of senior carers. Note however that these calculations may be somewhat imprecise due to imprecision in the estimation results.

[29] The number of hours of carer/nurse per resident seems a better proxy for the quality of care than the actual number of carers/nurses per resident. Nevertheless, we found no significant effect of the NMW on the quality of care when the bodies-based measure was used as the dependent variable.

Wellisz, 1979; Manning, 1995; Rebitzer, 1995).

Table 6: Reduced form estimates of quality of care adjustments

	Change in hours of senior carers per resident		Change in hours of qualified per resident	
	(1)	(2)	(3)	(4)
%Low-paid	-0.67		-1.03	-0.75
	(0.51)		(0.52)	(0.74)
Wage- gap		-3.39		
		(2.38)		
Controls	Yes	Yes	Yes	Yes
R-squared	0.26	0.28	0.27	0.27
Number of homes	629	629	622	622

* significant at 5%, robust s.e. in parentheses. Controls include proportion female, average age, proportion with nursing qualification, proportion of la/dss residents, and county dummies.

6 Conclusions

Efficiency wages cannot be dismissed on a priory theoretical grounds and evidence is needed. The large number of empirical studies in the field, except of some few credible attempts, hasn't produced persuasive or conclusive evidence mainly due to empirical problems that render the empirical investigation of efficiency wages particularly vexing. Thus, more credible empirical studies are needed in order to decide the fate of efficiency wages theory.

In this paper, we exploit the ideal research design provided by the UK NMW introduction in a very low-pay sector, the residential care homes in order to overcome any identification problems associated with testing the wage-supervision trade-off prediction of the shirking model. The NMW introduction except of generating exogenous variation in care homes wages, provides also rents to employees in minimum wage jobs, as it increases the opportunity cost of job search and forms a wedge between the wage received at the current job and expected alternative job opportunities, which is the defining property of efficiency wages.

We find some weak evidence supporting a wage-supervision trade-off for non-managerial employees and for employees with no nursing qualification and strong evidence in favour of a trade-off for care assistants which is the principal occupation in the sector. This evidence should be interpreted as supporting the tenet of the shirking model that higher wages and supervision are substitutes in eliciting effort by employees, and thus as indirect evidence of productivity enhancing effects of higher wages.

The latter findings combined also with evidence that employers didn't dissipate wage rents by increasing job intensity and that higher wages didn't seem to have a negative effects on profits, may suggest that the NMW may have operated as an efficiency wage in the care homes sector, which also explains the recent findings of moderate (negative) employment effects in the sector as a result of the NMW introduction. Our analysis also provides a direct test that supports efficiency wages models developed to explain empirical findings of a non-negative employment effect of the minimum wage.

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