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### **Is the Minimum Wage Efficient? Evidence of the Effects of the UK National Minimum Wage in the Residential Care Homes Sector**

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# Is the Minimum Wage Efficient? Evidence of the Effects of the UK National Minimum Wage in the Residential Care Homes Sector

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## **Abstract**

In this paper we exploit a natural experiment provided by the 1999 introduction and 2001 increase of the UK National Minimum Wage (NMW) to investigate the relationship between wages and supervision and to test for efficiency wages considerations in a low-wage labour market, the UK residential care homes sector. We also provide evidence of the effects of the UK National Minimum Wage introduction and increase on the main labour market outcomes in the sector. We find evidence supporting a wage-supervision trade-off for the 1999 NMW introduction but no evidence of a trade-off for the 2001 NMW increase. We also find that the 1999 NMW introduction caused significant growth in average home hourly wages but only moderate negative employment effects and no significant effect on other outcomes as prices and profits. Finally, we find that the 2001 NMW increase generated higher wage growth than the 1999 introduction but had no employment effect, which can be possibly explained by the fact that homes increased the price of care to offset the increased wage costs generated by the NMW increase.

**Keywords:** Efficiency Wages, National Minimum Wage, Difference-in-Differences, Instrumental Variables.

JEL Classification: J38, J41, J48.

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## 1 Introduction

Efficiency wage theory has been used to provide an explanation of involuntary unemployment and other phenomena as dual labour markets, that are at odds with the competitive model of the labour market (Katz 1985, Rebitzer 1989).

The essence of efficiency wages is that they are more likely to arise in situations, where there are restrictions in the implementation of efficient contracts, as bonding and several theoretical arguments have been offered to support or dismiss the validity of restrictions that open the door to efficiency wages (Manning 2003).

Moreover, several theoretical arguments can be provided to support the view that there are valid efficiency wages considerations in low-wage labour markets. For example, one can argue that a binding minimum wage, will prevent employers to tilt the wage-tenure profile in order to solve agency problems in low-wage labour markets (Krueger 1991), and also that low-wage workers are more likely to face financial constraints that prevent them from posting a bond, compared to better-paid workers (Weiss 1991).

However, efficiency wages in low-wage labour markets cannot be supported or dismissed on a priori theoretical grounds and evidence is needed (Dickens, Katz and Lang 1985).

Although, the relevance of efficiency wages in general is mainly an empirical question, there is a lack of credible empirical evidence on efficiency wages, as most empirical studies in the topic are unpersuasive and inconclusive (Autor 2003).

This lack of strong evidence on efficiency wages is mainly explained by problems that make the empirical testing of efficiency wages particularly vexing (Rebitzer 1995, Manning and Thomas 1997).

In particular, an overview of the empirical literature of studies that test efficiency wages by testing the hypothesis of a wage-supervision trade-off suggests that it is quite difficult to overcome the endogeneity problem successfully and that is why there have been very few studies that manage to address the problem (Groschen and Krueger 1990, Krueger 1991, Rebitzer 1995) by using a valid instrument for the causing variable of interest (in this case supervision intensity).

In this paper we exploit the ideal research design provided by the 1999 introduction and 2001 increase of the UK National Minimum Wage (NMW henceforth) in order to identify the relationship between wages and supervision and to test the validity of efficiency wage theory in a low wage labour market. We also offer evidence of the effects of the UK National Minimum wage on other labour market as wages, employment, prices, profits and output as well as on other efficiency wages related outcomes as staff recruitment and

quits.

The data used were drawn from two before and after surveys conducted in the UK residential care homes industry, one of the sectors with the lowest pay in the UK economy, where one expects that the minimum wage is binding.

The identification strategy used, under this research design, is based on the implementation of the difference in differences methodology (Meyer 1995, Angrist and Krueger 1999).

In general, the introduction or increase of a minimum wage provides an ideal setting for testing efficiency wages, as the essence of efficiency wages is that above market clearing wages generate gains in terms of employees' productivity.

In summary our findings have as follows: we find a sizeable effect of the 1999 NMW introduction and 2001 increase on wages of care homes from the south coast of England but we find some moderate negative effects on employment for the 1999 introduction and no employment effects for the 2001 increase. Our empirical analysis further suggests that the minimum wage hasn't affected prices in 1999 but increased the price of care homes services in 2001 and in general caused no effect on profits in either 1999 and 2001.

Furthermore, as far as any longer run effects of the minimum wage is concerned, we find no effects of the minimum wage introduction and subsequent increases up to 2001 on home closures and no effect on the skill composition of workforce and on capital/labour ratio. We also find some evidence of output reduction to be the case after the 1999 NMW introduction and effort increases for the 2001 NMW increase.

Moreover, although we find some evidence of moderate output reductions in 1999 we fail to find any evidence of significant changes in productivity and subjective effort across care homes in both the 98/99 and 01/02 samples.

Finally and most importantly, after addressing the main empirical problems that hinder the estimation of the wage-supervision relationship, we find that the 1999 increase in the minimum wage followed by decreases in home's intensity of supervision a finding which provides support to the efficiency wages predictions of the wage-supervision trade-off.

Moreover, after testing whether higher wages paid for themselves, we find some evidence that the wage increases resulted by the 1999 NMW introduction, were more or less exactly offset by the fall in supervision costs, a finding which if combined with the evidence of the wage-supervision trade-off, seems to support that the minimum wage may have operated as an efficiency wage in the care homes industry.

Although, the above findings provide some support to the 'shirking' efficiency wages model we do not find any evidence that supports

the ‘turnover’ or ‘adverse selection’ models of efficiency wages as we fail to find significant effects of the impact of the 1999 NMW introduction and 2001 increase on the average job experience of workforce and on average tenure as well as the quit and recruitment rates.

The rest of the paper is structured as follows: Section 2 summarizes the main recent studies in the topic of minimum wages in general and in the UK in particular and highlights the most important gaps in the minimum wage literature.

Section 3 and 4 present a discussion of the data and some descriptive statistics of the care homes samples before and after the 1999 NMW introduction and 2001 increase respectively and our analysis on the effects of the NMW introduction and increase on wages and employment respectively.

Section 5 presents our findings on the NMW effects on prices, profits and any long run outcomes (closures, substitution of capital and higher skilled labour for low skilled labour). Finally, section 6 investigates the effects of the NMW introduction and increase on labour productivity, effort, supervision intensity and other potential gains of higher wages in terms of changes in the average age-experience of the workforce, and changes in the average tenure as well as the quit and recruitment rates. Section 9 then concludes.

## **2 A Brief Review of the Minimum Wage Literature**

The economic effects of minimum wages is a heavily researched issue that generates much controversy, especially after the striking findings of Card and Krueger (1995) that cast doubt on the orthodox view of the effects of the policy on employment. Although, the debate on the issue remains, one of the points of agreement among economists in the topic of minimum wages is that more evidence is needed to inform the debate and improve insight (Card and Krueger 1995, Dolado et al 1996, Dolado et. al 2000).

It is true that the vast majority of empirical evidence on the economic effects of minimum wages is from the US labour market and little is known about the effects of the minimum wage policy in other labour markets (Dolado et al 1996, Dickens Machin and Manning 1999, Lemos 2002).

There are many economists among those who believe that more international empirical work could cast much light in many fields of labour economics and in particular in the topic of minimum wages (Hamermesh 2002).

As far as the UK is concerned, Machin and Manning (1994) and Dickens, Machin and Manning (1999) investigated the economic ef-

ffects of Wages Councils and their 1993 abolition on economic outcomes. Since the abolition of Wages Councils in 1993 there was no minimum wage in the UK (except of the agricultural sector), but the labour party which was reelected in the government in 1997 committed to introduce a minimum wage. A UK wide (national) minimum wage was legislated with effect from April 1999, fact which provided a unique setting for the investigation of the economic effects of the minimum wage introduction in a previously unregulated labour market.

Machin, Manning and Rahman (2002, 2003) exploited this unique setting and conducted two surveys in order to collect primary data before and after the introduction of the UK National Minimum Wage from firms in the residential care homes industry, one of the sectors with the lowest wages in the UK. The data collection enabled Machin et al. to implement what is now considered a standard technique in the empirical literature of minimum wages (Card and Krueger 1995). Their main conclusions were that the minimum wage introduction caused a substantial increase in wages and significantly compressed the wage distribution in the care homes sector but had only moderate employment effects.

Moreover, Machin and Wilson (2004) used additional data (collected by the Centre for Economic Performance) to that of the Machin et al (2003) study from residential care homes from the south coast of England, to investigate the effects of the 2001 UK NMW increase, the largest increase in minimum wage rates since the introduction of the policy. In particular, Machin and Wilson used the data from the before and after 1999 NMW introduction on care homes throughout the UK (used in the Machin, Manning and Rahman study) and the additional data from the before and after 2001 NMW increase on care homes from the south coast of England to implement the standard difference-in-differences technique and investigate the effects of the 1999 introduction and 2001 increase in UK NMW on wages and employment.

Additionally, Machin and Wilson (2004) also address the effects of minimum wages on closures of south coast care homes between the period before the 1999 introduction and before the 2001 increase. The results produced by Machin and Wilson (2004) study show sizeable wage effects and some evidence of employment reductions but their analysis suggests that the 1999 introduction of the NMW had no effect on home closures.

The main objective of this paper is to exploit the unique research design provided by the UK NMW 1999 introduction and 2001 increase by using the south coast care homes data (which were kindly provided by Steve Machin and Joanne Wilson) to identify the relationship between wages and supervision and to test the validity of efficiency wages theory.

Another objective of this paper is to use the care homes data in order to reproduce the Machin and Wilson work but also to extend it in order to investigate the effects of the NMW introduction and increase on other outcomes such as prices, profits, effort and productivity, as well as looking for other longer run adjustments such as closures and factors substitution.<sup>1</sup> This is because another gap than the lack of non-US evidence in the minimum wage literature is the lack of sufficient evidence of the effects of minimum wages on other outcomes than employment and the wage distribution and particularly on the effects of minimum wages in the long-run (Brown 1999).

The investigation of any minimum wage effects on prices, profits, and long-run outcomes as the exit and entry of affected firms could provide a more complete picture of how low-wage labour markets operate and especially on the effects of the policy on employers as most of the existing research has focused on minimum wage effects on affected workers (Card and Krueger 1995). Moreover, as suggested by the review of the theory of the minimum wage of the previous section, theory generates both short run and long run predictions and thus it is important for empirical studies to distinguish between the two.

In particular, Brown (1999) indicates that the long-run effects of minimum wages consist the larger and most important gap in the minimum wage literature. This is the case for many reasons as for example there is a stunning lack of credible studies or even attempts on the effects of minimum wages in the long run (Brown 1999). However, the main reason why this remains an important gap is that it may provide a potential explanation of the puzzle that recent estimates of minimum wage employment elasticities are small. As suggested by theory the demand for labour is more elastic in the long run, where firms may downsize or exit and potential entrants may be deterred. Moreover, most of the empirical studies that investigate the employment effects of minimum wages focus on sample of firms that continue operation and do not look for any exit that may have taken place as a result of the policy (Rebitzer and Taylor 1995).

In general exit or entry effects cause discontinuous changes in employment and thus should be seriously considered (Hamermesh

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<sup>1</sup>Machin, Manning and Rahmans paper was firstly published in 1999 as a CEP discussion paper but revised drafts have been produced until 2002. The discussion paper included also investigation of minimum wage effects on prices, closures and openings and worker's productivity, but the closures-openings analysis wasn't included in their published paper (Machin, Manning and Rahman 2003), probably because the authors didn't find any significant minimum wage effects on closure and opening probabilities and on county closure and opening rate, but also because the timing of the post-introduction survey is such that any long-run adjustments cannot be identified as it is unlikely that long-run adjustments as exit and entry took place in such a short-time after the NMW introduction.

1992). This is another reason why it is important to look for evidence on other longer run outcomes.

We do not know many studies that look also on other possible adjustments to the policy other than employment (Card and Krueger 1995). Card and Krueger (1995) and Brown (1999) devote some of their analysis in investigating or reviewing the minimum wage effects on prices, profits, fringe benefits and stock value of employers of low-wage workers among other outcomes but their main conclusion is that more evidence is needed to provide a more clear picture.

Although some evidence has been produced on any other short run adjustments the main lack of evidence as suggested above has been on the effects of the policy on exit and entry of firms in the long run. Indeed we know very few studies that address these issues and their characteristic is that all studies devote only a minor part of the analysis on these hypotheses.

Zavodny (1996) addresses this particular question using data on drinking and eating places, retail trade and clothing stores the two largest employers of minimum wage workers in the US and thus the most heavily affected sectors. The main findings of the study suggest that the minimum wage had a positive effect on the number of firms in the affected sectors.

On the other hand Waltman, Mcbride and Camhout (1998, 1999) examine the same hypothesis using data on the aggregate rate of business failures. This study finds no effect of the minimum wage on business failures, but suffers from many econometric problems as suggested by Taylor and Arnold (1999). Moreover, Alpert (1986) also examines this issue together with other issues using US quarterly data on restaurant failures. Alpert finds no evidence that restaurant failures have been adversely affected by the minimum wage

Card and Krueger (1995), also investigate a similar hypothesis by considering the effects of minimum wages on Mcdonald's fast-food restaurants openings during the years 1986 and 1991 and in contrast to conventional wisdom they find positive but not statistically significant effects of minimum wages in restaurant openings, suggesting that the minimum wage does not seem to have a strong (negative or positive) effect on openings in the fast-food industry. All, the above evidence reviewed are from the US and indeed we know only two recent studies or empirical attempt to identify exit or entry effects of the minimum wage policy in Europe.

This evidence is provided by Machin, Manning and Rahman (2003) (MMR henceforth) and Machin and Wilson (MW henceforth) who both exploit the fact that a nation wide minimum wage has been introduced in April 1999<sup>2</sup> and has been increased several times

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<sup>2</sup>The UK National Minimum Wage has been set at £3.6 for those aged 22 and over (the

since then with the bigger increase being the one that took place in October 2001.<sup>3</sup>

MMR (1999) used the data collected from a sample of residential care homes throughout the UK in order to investigate also the impact of the minimum wage on the probability of closure across homes as well as in the closure and opening rate across counties, between July 1998 and May 1999, but they fail to find any significant effects. However, they suggest that the timing of the post NMW introduction survey may not allow for the identification of long run adjustments to the minimum wage as that of exit and entry.

MW (2004) offer a more credible attempt to investigate the relationship between the impact of the minimum wage and home closures using a sample of care homes from 8 south coast UK counties between the July 1998 and just before October 2001 NMW increase, a period that possibly allows for the identification of any minimum wage effects on exit. The main finding of MW, as also discussed above, is that they fail to find any systematic relationship between home closures and the NMW introduction.

### 3 The Data and Descriptive Statistics

#### 3.1 Survey Design

Our analysis is based on information on all workers in each home and on home characteristics from a sample of care homes in the South Coast of England, collected by two surveys, the one conducted before and after the 1999 NMW introduction and the other before and after the 2001 NMW increase.

The data were collected through the Centre for Economic Performance (CEP) in LSE, by mailing a survey questionnaire to the manager of each care home. Both surveys were implemented using lists of homes from the Yellow Pages Business Database, but the

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adult rate) and at £3.00 for those between 18 and 21 years old inclusive (the development rate). Those below 18 were not covered.

<sup>3</sup>The development rate has been increased to £ 3.20 in June 2000, whereas the adult rate increased again to £ 3.70 in October 2000. Since then, both rates have been increased every October with the biggest increase to date the one that took place in October 2001 where the adult rate increased from £3.70 to £4.10 and the development rate from £3.20 to £3.50, a 10.8 % and 9.3% increase respectively (Metcalf 2004) (The increase in the development rate in October 2004 from £3.80 to £4.10 per hour is the same as that in 2001 in absolute figures but proportionately lower). The most recent increase in the NMW has been in October 2004 where the adult and development rates have been increased from £4.5 to £4.85 and from £3.80 to £4.10 respectively and a NMW introduced for those aged 16 and 17 (above compulsory school leaving age) at £3.00 per hour. Moreover, the Low Pay Commission recommended an increase in the adult rate from £4.85 to £5.05 and to the development rate from £4.10 to £4.25 effective from October 2005. For an overview of the history and a review of the evidence produced up to now on the effects of the UK NMW see Metcalf (2004). MMR provide also a good background of the NMW policy (2003).

before and after 1999 NMW introduction focused on the whole UK population of residential care homes in July 1998 and in May 1999,<sup>4</sup> whereas the before and after the 2001 NMW increase survey focused on the population of homes from the South Coast UK counties in August 2001 and February 2002.<sup>5</sup>

The questionnaire included questions about manager's information and attitude towards the NMW,<sup>6</sup> as well as questions about the home ownership type (private, local authority, etc.), whether the home is part of larger organisation, the number of registered beds, the number of residents, etc. Managers were also asked to provide data on job title, sex, age, length of service, possession of a nursing qualification, weekly hours and hourly wages for all workers in the home.

All (three) surveys achieved a reasonable response rate for a mail survey (around 20%).<sup>7</sup> Note that as in MMR (2004) in the cases where there was missing information on hourly wages and

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<sup>4</sup>However, as also mentioned above, our analysis on the effects of the 1999 NMW introduction is based on a subset of this data set including only homes from the South Coast of England, see MW (2004) for a detailed discussion.

<sup>5</sup>The former population was around 11, 000 and the latter around 2,500 care homes. In fact there was another survey implemented in the population of South Coast care homes around the window of the 1992 elections, since the Labour Party had committed to introduce a minimum wage if they were elected. The fact that they were not elected meant that data could not be used for the purpose of evaluating minimum wage effects. However, the after 1992 elections survey implemented anyway, as the data considered useful for the study of the wage structure of the Care homes sector (Machin and Manning, 2002), and in order to look at wage and employment changes during a period when the minimum wage was not introduced (MMR 2003). See also MMR (2003) for a more detailed discussion of the 1999 survey design and data collection and MW (2004) for a further discussion of all three surveys.

<sup>6</sup>These kind of questions differed between the before and after the NMW introduction and increase for apparent reasons.

<sup>7</sup>The care homes samples of the before and after 1999 surveys included 1866 and 2142 homes respectively, whereas the 2001 before and after survey samples included 411 and 333 homes respectively. MMR apply their analysis also in two subsets of the whole 1999 care homes sample: the balanced panel of homes which includes 641 homes that were surveyed both before and after the 1999 NMW introduction and a subset of the balanced sample which excludes homes with lots of missing information, i.e. homes in which more than half of information on hourly wages and weekly hours is missing, and includes in total 615 homes. The South Coast subset of homes in the MMR sample, on which our analysis focuses includes 548 homes prior to the NMW introduction and 579 after the introduction, and the balanced sample includes 195 homes but no other subset of the balanced sample is produced, as there are no South Coast homes with more than half workers information missing. Note that there is a difference in the size of the 1999 balanced sample of South Coast carehomes included in our analysis and that of MW, who report that their wage and employment change regressions were based on a sample 181 homes, where no controls were included. This is why we also try to check the matching of homes included in the before 2001 NMW increase sample with homes included in the after 2001 sample. We find that the balanced sample of homes seems to be consisted of 152 homes in contrast to MW who report 173 homes in their wage change regressions and 193 homes in their employment change regressions when no controls were included. Finally, as the 2001 balanced sample included also 22 homes with more than half workers information missing we also present survey statistics for a subset of 130 homes from balanced sample which excludes homes with lots of missing information on workers' hourly wages and weekly hours.

weekly hours we impute them using the home average, but both statistics with imputed and non-imputed are reported. MMR and MW provide evidence and discussions that seems to support the fact that all (three) samples of care homes were representative of the corresponding population in terms of workers' wage, age, hours and tenure<sup>8</sup> (see Woodland, 1993, MMR 2003 and MW 2004 for a detailed discussion).

### 3.2 Descriptive Statistics

The two survey statistics that are presented in table 1 and table 2 (see tables section at the end) summarise the main features of the whole sample of care homes before and after the 1999 NMW introduction and the 2001 NMW increase, as well as the main characteristics of the balanced sample of homes, which includes only homes that were surveyed both before and after the Minimum wage introduction/increase. The vast majority of care homes in all samples is consisted of private and independent establishments with very few homes being voluntary or owned by local authority and a part of a large organisation.<sup>9</sup> As presented in table 1 and 2, the main sample statistics are fairly similar between the whole sample and the balanced samples of homes in both the 1999 and 2001 before-after surveys, suggesting that the balanced sample is representative of the whole sample and the population of care homes as a whole.

The two tables clearly reveal some of the main characteristics of the care homes sector. Firstly, the average hourly wage is quite low and in particular slightly above the adult rate both in the period before the NMW 1999 introduction and 2001 increase,<sup>10</sup> a fact which suggests that the sector is expected to be heavily affected by the minimum wage introduction and increase.

The fact that the vast majority of employees at home are female, that the average number of hours is below thirty and that care

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<sup>8</sup>The same holds for the 1999 South Coast care homes sample used in our analysis, which is a subset of the whole 1999 care homes sample used in MMR's analysis, as sample statistics of wages, hours, age and tenure between the two samples are very similar.

<sup>9</sup>In particular 481 out of 548 homes in the before 1999 NMW introduction sample are private and only 65 are part of a large organisation. Moreover, 489 out of 579 homes in the after 1999 NMW are single independent establishments and 499 are privately owned. The 1999 balanced sample includes only 25 homes that are not privately owned and 19 that are part of large organisation, out of 195 care homes included in total. Similarly, 368 out of 411 homes in the before 2001 sample are privately owned and only 60 are part of a large organisation. The after 2001 NMW increase sample includes 45 non-privately owned homes and 281 independent establishments out of 333 care homes in total. Finally, the 2001 balanced sample includes only 14 non-private homes and only 18 homes that are part of a larger organisation, out of 152 observations in total.

<sup>10</sup>The average hourly wage is around £4 before the 1999 NMW introduction with the introductory level of the adult national minimum rate being £3.6, whereas the same statistic is around £4.7 before the 2001 increase with the 2001 adult rate being £4.1.

assistants is the principal occupation, as well as the fact that a very small fraction of employees has a nursing qualification<sup>11</sup> seems to provide further support to the argument that minimum wage effects on care homes outcomes are expected to be substantial. This is actually the case because low-skilled, female and part-time workers form the majority of low-wage workers in the economy as a whole (Low Pay Commission 1998, Metcalf 1999). Moreover, as it is also suggested by the New Earnings Survey (MMR 2002) the occupation of care assistants is probably the lowest paid occupation in the UK (MMR 2003).

Other dominant characteristics of the 1999 and 2001 care homes samples is that the representative home is small in size both in terms of the number of employees and the number of residents (the number of employees ranges between 14 to 16 on average per home in both surveys), the majority of workers are older on average (the average age at home is around 40 years old in both surveys) and that a significant proportion of residents have their fees paid by local authorities (between 45% and 50% in both surveys), with the latter observation suggesting that it is even more likely to find minimum wage effects in the industry as the price of services for those residents are capped by local authorities and thus the ability of home owners/managers to pass some of the NMW costs on prices is expected to be quite limited (MMR 2003).<sup>12</sup>

In their earlier paper MMR (2003) suggested that the fact that the average home is small in size not only makes feasible the collection of good data on all employees but also implies that monitoring problems are not expected to be severe. This may further suggest that any efficiency wages considerations arising from imperfect monitoring are unlikely to be valid in the sector.

Although home size may be an indicator of monitoring problems, it is a good proxy for monitoring intensity only if there is only one employee (probably the home owner) engaged in monitoring activities, which is unlikely to be the case, as the nature of the business is such that home operates twenty four hours a day and seven days per week which makes it impossible for a single owner to be physically present and monitor employees all the time. Therefore, a better measure of monitoring intensity (or observability of workers' effort),

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<sup>11</sup>Nursing qualification is the only relevant qualification in the care homes sector. Note that the proportion of workers at home with a nursing qualification is the only statistic that differs significantly between the 1999 and 2001 surveys.

<sup>12</sup>MMR (2002, 2003) and MW (2004) provide information that the level of the local authorities security funding was not increased to meet the National Minimum Wage increase in 1999 which further implies that home owners-managers couldn't pass on the costs to residents (at least those that have their fees paid by local authorities) by setting higher prices. This conjecture is further supported by a comparison of the average weekly price of bed in the before and after 1999 NMW introduction in the whole and balanced samples which indicates no significant change in care homes prices.

than the inverse of employment at home, could be the number of managerial workers per non-managerial employee, the most popular proxy of monitoring intensity in the literature (Odiorne 1963, Gordon 1990, 1994). The fact that supervision intensity may be better approximated by the ratio of managerial to non-managerial employees than the inverse of employment is also supported by the scatter plots of the number of non-managerial and managerial employees of the whole care homes samples before the 1999 NMW introduction and before the 2001 NMW increase presented in figure 1 and 2 respectively. As indicated in the figures, although there are homes with no or one manager only, in a significant share of the sample there are more than only 1 manager at home.<sup>13</sup>

Moreover, as presented in tables 2 and 3, there is approximately one supervisor<sup>14</sup> for every nine employees in the 1999 sample of homes on average and around 1 managerial employee for every eleven non-managerial employees on average in homes in the 2001 sample.

These averages, combined also with the “twenty four-seven” nature of the business as well as the fact that the quality and not only the quantity of monitoring is important in order to tackle agency problems (Kruse 1992, Brunello 1995) may suggest that imperfect monitoring considerations may still be valid, despite the small size of the average home. Other considerations for the relevance of efficiency wages in care homes could be suggested by the quite high turnover rate.<sup>15</sup>

Furthermore, differences in average hourly and weekly wages between before and after the NMW introduction and increase seem to further indicate that indeed as expected homes in the sample have experienced significant increases in wage costs.<sup>16</sup> In particular information from the balanced sample of homes suggests that hourly wage was increased on average by 12 and 15 pence after the 1999 NMW introduction and the 2001 increase respectively (with or without imputed information).<sup>17</sup>

<sup>13</sup>This holds more strongly for the 2001 than the 1999 sample of care homes.

<sup>14</sup>As managerial employees are classified employees with job titles: manager, home owner, matron, deputy matron, assistant matron, deputy manager and assistant manager.

<sup>15</sup>Sample averages of the quit and recruitment rate for both the before the 1999 and before 2001 surveys, on average 13% of staff has left and the same proportion was recruited the last three months from the time of each survey. These turnover figures may be considered as quite significant considering the short period for which the relevant information is reported.

<sup>16</sup>This issue is analysed further in the next section where more measures of the ‘bite’ of the minimum wage introduction and increase on the distribution of wages are presented.

<sup>17</sup>This means a 2.8% and a 3.4% increase in average hourly wages for the 1999 introduction and the 2001 increase respectively. Note that our figures for the 1999 introduction differ significantly from those presented in MMR (2003), where the authors report an increase in hourly wages of 24 pence or 6% for the sample of care homes throughout the UK. Another interesting observation is that the absolute or proportionate change in the average hourly wage is not found to be higher for the period of the introduction compared to that of the NMW increase, as it would be expected.

Moreover, as presented in table 2 the survey statistics (for the balanced panel) do not seem to differ significantly between the pre and post 1999 introduction period, except of hourly and weekly wage (with or without imputed information), and of the ratio of managerial to non-managerial employees at home. The noticeable increase in supervision intensity in the after 1999 NMW introduction (in the balanced sample)<sup>18</sup> can be better explained by looking at the changes in the cross section distribution of the number of managerial and non-managerial staff between the before and after the NMW introduction. Figures 3 and 4 indicate that the distribution of the number managerial staff across homes remained fairly the same after the NMW introduction, whereas in figures 5 and 6 we observe that the distribution of the number of non-managerial staff becomes more skewed to the right after the introduction of the NMW.

Comparisons of pre and post survey statistics for the 2001 NMW increase (from the balanced panel of care homes) suggest that again the most heavily affected outcomes in the sector are again hourly and weekly wages as well as the proportion of care assistants, average weekly price of bed<sup>19</sup> as well as the proportion of residents whose fees are paid by local authorities and the ratio of managerial to non-managerial employees.<sup>20</sup> In contrast to the 1999 introduction, supervision intensity in this case decreases after the NMW increase, a result which can be explained by the fact that the distribution of the number of non-managerial staff seems less compressed after the increase compared to before the increase, whereas the distribution of the number of managers becomes more skewed to the right and more compressed after the 2001 NMW increase (see figure 7, 8, 9, and 10).

The increase in hourly wages combined with the fall in the number of managerial relative to non-managerial employees and the proportion of care assistants at home (who are low-skilled, non-managerial employees) and may further reconcile with the prediction of the shirking models of efficiency wages (Shapiro and Stiglitz 1984, Georgiadis 2001) that wage increases lead to relaxation in monitoring intensity.<sup>21</sup>

In general indications of significant wage effects of the minimum wage is a necessary condition, as also suggested by MMR (2003) in

<sup>18</sup>For the whole sample also the number of beds and number of residents seem to differ significantly between the pre and post NMW introduction.

<sup>19</sup>As discussed in one of the following sections although prices remained capped by local authorities in the period before and after 1999, they were not capped for the period around the 2001 NMW increase.

<sup>20</sup>The same seems to be the case for the whole sample of care homes, except of the fact that the supervisor/supervisee ratio remains unchanged, whereas the number of beds differs noticeably between the pre and post increase survey.

<sup>21</sup>However this prediction is generated for given everything else, which is not the case for the descriptive analysis.

order to proceed and investigate the effects of the policy on other outcomes. Descriptive statistics from both surveys provide this kind of indication but we need to address this issue properly by especially looking whether wage effects of the minimum wage were higher in homes that seemed to be affected the most by the policy.

## 4 The Effects of the UK National Minimum Wage on Wages and Employment

### 4.1 Wage Effects

In the previous sections as well as in Machin, Manning and Rahman (2003) it is suggested that the wage effects of the introduction and increase in the NMW should be looked first of all, as a significant NMW effect on wages is a prerequisite in order to look for further effects on employment and other outcomes.

In tables 4 and 5 are presented measures of the ‘bite’ of the minimum wage 1999 introduction and 2001 increase respectively. Both tables indicate that a large proportion of workers were affected by both the introduction and the increase in the minimum wage and also that these minimum wage changes generated a sizeable increase in the wage bill, as suggested by the wage gap variable.

Additionally, both tables indicate that the adult rate is binding for most care homes in 1999 and 2001, as around 30% of all workers in the full and balanced sample of homes are paid below the adult rate before the 1999 NMW introduction, whereas around 30% and 26% of all workers in the full and balanced sample respectively are paid below the adult rate before the 2001 NMW increase. Moreover, the wage gap measure of the impact of the minimum wage suggests a sizeable impact as tables 4 and 5 indicate a 2.4% and 2% increase in the weekly wage bill for the full and balanced sample respectively for the 1999 introduction as well as a 1.5% and 1.3% increase for the full and balanced sample of homes respectively for 2001.

Therefore, in contrast to the changes in the average hourly wage between the pre and post introduction and increase surveys discussed in the previous section, the measures of the ‘bite’ of the NMW suggest a stronger impact of the minimum wage on the wage structure for the 1999 introduction compared to the 2001 increase, something not surprising if one accounts that in 1999 the NMW was introduced in a previously unregulated labour market.<sup>22</sup>

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<sup>22</sup>Note that ‘bite’ statistics presented in table 4 and 5 are different (especially for the proportion of affected workers) than those presented by MW (2004). The difference can be mainly explained by two points: The one is that difference in the way the two measures of the impact of the NMW were calculated. As suggested by MMR (1999), the data were punched in the computer by various students, and that is why we had a closer look for any errors in the variables of interest. In particular, MMR and MW use two different measures for the impact

The summary statistics presented in table 3 and 4 also suggest a noticeable spike of the wage distribution on the adult rate as around 24% of all workers in both the full and balanced sample for the 1999 and around 20% and 18% of all workers in the full and balanced sample respectively for 2001 are paid the adult rate after the NMW introduction/increase. The spike of the wage distribution on the minimum wage is one of the main stylized facts of the minimum wage literature (Brown 1999).<sup>23</sup>

The “bite statistics” combined with the spike at the adult rate of the post-1999 and 2001 youth hourly wage distributions presented in figures 11 and 12 seem to support a low utilisation of youth subminimum wages, another well-documented fact of the minimum wage literature.<sup>24</sup>

The spike of the wage distribution at the adult rate is clearly observed in figures 14 and 16 (see figures section at the end) of the hourly wage distribution for all workers in the post 1999 NMW introduction and post 2001 NMW period respectively. More comparisons the hourly wage distribution before and after the 1999 NMW introduction (figure 13 and 14) and 2001 NMW increase (figure 15 and 16) suggests clear compression of the distribution of hourly

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of the minimum wage: the proportion of workers whose wage is affected by the minimum wage (covered workers that are paid an hourly wage that is below the age specific minimum rate before the NMW introduction and increase) and the wage gap (the proportional increase in the weekly wage bill at home if the wages of workers affected is increased to meet the age specific minimum wage). We calculate the proportion of affected by the minimum wage workers as the ratio of the number of workers with information on whether they are covered and affected (i.e. whether they are paid below their age-specific minimum rate) to all workers with information on whether they are affected or not by the NMW. The same approach is used in the calculation of the sample values of the wage gap variable. A comparison of the minimum wage impact variables calculated as described above with those calculated by MW (which were included in the data sent to us by the authors), seems to suggest that the former take into account more information and that is why their values are noticeably different than the latter (for example we account those that are not covered, i.e. those below 18 years old and those paid above the adult rate as not affected even in the case where there is not information on their hourly wage and age respectively). Moreover, the difference between our “bite statistics” and that presented in MW could be also attributed to the differences in the size of the sample of all care homes from the south coast of England. In particular, MW report statistics based on a sample of 530 and 581 carehomes in the whole sample in the pre and post 1999 survey respectively, whereas our figures are derived from a sample of 548 and 579 homes respectively. Moreover, for 2001 MW report a pre and post NMW increase sample of 482 and 404 respectively whereas our analysis was based on 411 and 333 homes respectively. Moreover, our analysis of the balanced sample of homes is based on 195 homes in 1999 and 152 homes in 2001 respectively, whereas MW report statistics and estimates from a sample of 181 and 173 homes for 1999 and 2001 respectively.

<sup>23</sup>Once more and for the reasons explained above, the our measures of the spike at the adult rate slightly differ from those presented by MW (2004).

<sup>24</sup>Note that out of 827 employees from the full sample and of 275 from the balanced sample that are covered by the development rate only 41 and 14 employees respectively are paid exactly the youth rate after the 1999 NMW introduction. Similarly 11 out of 390 employees in the full sample and 1 out of 178 covered by the development rate are paid this rate after the 2001 NMW increase.

wages which is also consistent with findings presented by Dickens and Manning (2004), supporting the argument that minimum wages decreased wage inequality in the care home sector.

Following the above discussion, the next question to be addressed is whether the highest wage changes occurred at homes that were affected the most by the minimum wage.

The basic empirical specification used throughout is

$$\Delta O_{ht} = \alpha_0 + \alpha_1 MIN_{h,t-1} + \alpha_2 \mathbf{X}_{h,t-1} + \varepsilon_{ht} \quad (1)$$

where  $\Delta O_{ht}$  measures the change in the outcome of interest,  $O$ , for home  $h$  in period  $t$  before and after the minimum wage introduction/increase.  $MIN_{h,t-1}$  is the pre-minimum wage variable,  $X$  is a set of pre-minimum wage home characteristics and  $\varepsilon_{ht}$  is a random error.

As long as the minimum wage randomly assigns the population of carehomes into more and less affected the parameter  $\alpha_1$  is a true measure of the effect of the minimum wage on the outcome of interest. The main concern about the validity of the latter identification assumption is that, given that the NMW is set at the same level nationally, variation in the minimum wage impact measures comes from variation in initial wages. MMR test this identifying condition by looking whether there is any (or a different) relationship between initial wages or minimum wage impact measures and the change in the outcome of interest (in particular the change in wages and the change in employment) in a period where no NMW was introduced.<sup>25</sup> The main findings indicate that there is no or little relationship between initial starting wages and the minimum wage impact measures and the change in the wages and employment, and that these relationships have significantly shifted at the time of the 1999 NMW introduction.<sup>26</sup>

In table 5 we present estimation results from wage specifications with dependent variable the change in the log average hourly and change in the log weekly wage respectively. We find a positive and significant effect of both the 1999 introduction and 2001 increase in the minimum wage on the change in average hourly wage, that persists throughout all specifications and measures of the minimum wage impact.

A striking finding is that our estimates suggest that the 2001 NMW increase had a significantly larger impact on care homes av-

<sup>25</sup>The counterfactual was the South Coast care home sample surveyed the responded in the two surveys for data collection in the window of the 92/92 UK elections.

<sup>26</sup>This finding limits any concerns of endogenous variation in the minimum wage impact measures due to variation in initial wages across homes. The introduction of controls for home and employers characteristics further limits any variation in the minimum wage impact measures that is generated from variation in initial wages.

erage hourly wages compared to the 1999 NMW introduction.<sup>27</sup> This finding may not seem surprising if we consider the changes in the average hourly and weekly wages before and after the 1999 introduction and 2001 increase discussed in the previous section but it may seem if one considers the measures of the ‘bite’ of the NMW for the period of the introduction and the increase which suggest that the NMW was more binding in 1999 compared to 2001. However, the descriptive analysis of the ‘bite’ of the NMW does not necessarily contradict the regression analysis of wages as the two are complements for each other.

In particular, the interpretation of estimates of specification (1) from both the 98/99 and 01/02 samples suggests that a home that has 1% higher proportion of affected employees experienced on average a 0.12% higher growth on hourly wages after the 1999 NMW introduction compared to a 0.26 % higher hourly wage growth after the 2001 NMW increase. The same is the case if one compares estimates from specifications that include the wage gap as the minimum wage impact measure. Particularly estimates from specifications (3) of both samples indicate that a home with a 1% higher increase in its weekly wage bill after the 1999 NMW introduction will experience on average 1.2% higher hourly wage growth compared to 3.7% hourly wage growth after the 2001 NMW increase.

The latter result seems to be against conventional wisdom as one wouldn’t expect that the NMW increase would have had greater impact on wages compared to the NMW introduction to a previously unregulated labour market. Moreover, this result is also different than the findings of MW, who although they also find positive and significant wage effects of the NMW introduction and increase throughout specifications and minimum wage measures, they report higher wage estimates for the 1999 NMW introduction compared to the 2001 NMW increase from the sample of care homes from the south coast of England.

A potential explanation of this finding (except of the differences between our analysis and that conducted by MW in terms of sample sizes and minimum wage measures) may be that although at the period of the introduction the NMW floor was set in a previously unregulated labour market, its level is quite prudent as also suggested by the Low Pay Commission reports (LPC) (June 1998, February 2000), probably because of fears of employment losses. Moreover, the 2001 increase is the most generous to date, because the levels were set after detailed research and consultation as well as after

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<sup>27</sup>For the 98/99 balanced sample our estimates are slightly different than MW, which could be explained by the difference in the measures of the minimum wage impact and the sample size discussed above as well as the fact that we use imputed information for missing values of the controls included in regressions as well as missing value dummies so that to avoid reductions in the sample.

taking into account previous experience of the NMW increases that may have limited concerns of employment losses.

Furthermore, as presented by table 5 we find that the effect of minimum wage on the growth of average weekly wage is positive but insignificant throughout all specifications and minimum wage measures for the 98/99 sample and positive but usually in the fringe of statistical significance for the 01/02 sample.

Once more we find that the NMW 2001 increase had a more sizeable impact on the change in the log weekly wage across homes. Moreover, our findings in this case differ from those of MMR(2003) who found that the NMW not only had a significant effect on both hourly and weekly wages but also that the effects were very similar in magnitude across care homes from all areas of the UK.<sup>28</sup>

An explanation of our findings of a weaker minimum wage effect on weekly compared to hourly wages could be that although the hourly wage may have increased as a result of the minimum wage, number of hours may have changed as well but in the opposite direction (this hypothesis is investigated in a following section) leading to lower weekly wage increases than those expected if hours were not affected by the policy.

In conclusion in this section we present evidence of significant positive effects of minimum wage on wages, and thus we can now look for effects of minimum wages on other outcomes starting from the employment effects, the most heavily researched hypothesis in the minimum wage literature.

## 4.2 Employment Effects

In this section we look at the effects of the NMW on employment. In particular, we investigate the effects of the 1999 NMW introduction and increase on both the number of employees and on total hours in the care homes sector.<sup>29</sup>

Results on the change in log number of employees and change in log total weekly hours for both the 98/99 and 01/02 periods are presented in table 6.

First of all we find that the NMW effects on the change in the number of employees are negative and significant for the period of the 99 introduction only when the proportion of low-paid is used

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<sup>28</sup>In our case, even when the NMW effect on weekly wages are significant (as it is the case for the 01/02) their magnitude is lower than the NMW effects on hourly wages.

<sup>29</sup>MW report estimates of the effects of both the NMW introduction and increase for only one employment measure. It is not clear whether this measure is the number of employees or total hours but considering the similarity of their estimates for the 98/99 from the sample of care homes from all UK areas used also by MMR, with MMRs estimates using the number employed as the employment measure, we infer that MW results involve the number of employees. The investigation of the effects on both bodies and hours is important as employment adjustments may involve both bodies and hours.

as the measure of the impact of the minimum wage, whereas it is negative but insignificant when the wage gap is used as the minimum wage measure.<sup>30</sup> In particular, our estimate from specification (1) in the upper panel of table 6 which reports estimates for the effect of the 1999 NMW introduction, suggests that a home with 10% higher proportion of affected workers before the NMW 1999 introduction will experience on average a 2.8% lower growth in the number of employees.

As far as the effects of the 2001 NMW increase in the change in the log number of employees is concerned our estimates reported in table 6 indicate that employment effects are insignificant throughout all specifications and all measures of the minimum wage impact used. Interestingly, the estimated employment effects are positive in most of the cases.<sup>31</sup> Again as in the case of wage effects our results are different than those produced by MW as they find negative and most of the times insignificant employment effects of the 2001 NMW increase. Furthermore, MW's estimates from the 98/99 sample of homes from south coast suggests that their estimates are negative and significant but slightly different than those presented in table 6. Once more, differences in the estimated results could be attributed in differences in the sample size, the minimum wages measures and the use of imputed information for missing values and missing value dummies for the controls included in regressions.

Furthermore, when the change in log total weekly hours is used as the employment measure in regressions we find a negative but either insignificant or marginally significant effect of the NMW 1999 introduction on employment.<sup>32</sup> Once again we find a positive and insignificant effect of the 2001 NMW increase in total weekly hours to be the case in all specifications and under both measures of the impact of the minimum wage.<sup>33</sup>

The fact that we find some negative effects of the 1999 NMW introduction on the change in weekly hours is consistent with the above result of a weaker positive effect of the NMW introduction on weekly wages. Moreover, in the case of the 2001 NMW increase

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<sup>30</sup>As suggested by MMR (2003) and MW (2004) both measures were used in regressions, as we cannot be sure which one is the best measure to pick up the effect of the minimum wage.

<sup>31</sup>However, note that the NMW effect on the number employed is negative and insignificant when one accounts only for the employment of care assistants.

<sup>32</sup>Estimates are more strongly significant when the dependent variable is the change in total hours of care assistants.

<sup>33</sup>However, we find some negative and sometimes on the fringe of statistical significance estimates when the dependent variable is the change in log total weekly hours of careassistants. Because the hours of care assistants fell slightly as a result of the 2001 NMW increase one could expect that total hours at home should also have fallen, if the hours of the rest of workers were unaffected. We found that the NMW had a positive but insignificant effect on the hours of non care assistants, and thus the positive and insignificant effect of the 2001 increase on total hours of all employees may be probably explained by the fact that hours of care assistants, as suggested by the overall results fell slightly.

we do not find any significant effects on total weekly hours under all specifications and minimum wage measures used, a finding which further explains why the minimum wage increase had a positive and significant effect on weekly wages, considering also the positive and significant effect it had on hourly wages.<sup>34</sup>

All in all the evidence suggests that although the minimum wage generated a significant boost in the wages of care home workers, there are some employment losses only in the period of its introduction, a finding that is consistent with both previous studies conducted by MMR and MW. On the other hand we fail to find any significant (negative) employment effects of the 2001 NMW increase, a finding that seems quite surprising considering also the more sizeable impact that the 2001 NMW increase had on wages in care homes.

This latter finding is surprising, as provided the evidence that the 2001 NMW increase generated on average a more significant boost in hourly and weekly wages than the 1999 NMW introduction, we would expect more significant disemployment effects of the 2001 NMW increase compared to the 1999 NMW introduction. However, a better explanation of the observed employment effects of the NMW introduction and increase could be provided once we also get some evidence of the effects on other outcomes as at the same time other adjustments may have taken place.

The negative minimum wage employment effects of 1999 seem to be quite moderate, considering the impact that the minimum wage had on the wage sector of the care homes sector. An explanation of the moderate or no employment effect of the NMW introduction and increase, could be provided by the claim of the low pay commission (Low Pay Commission 1998) that “a detailed investigation-consultation across the country takes place before any wage increase so that the rates will be calculated and set at a prudent level that will make sure that there is a boost in low-wage workers incomes that doesn’t destroy their jobs”.

In general, our findings are in line with MMR and MW that suggest that a minimum wage introduction and increase boosts low-paid workers earnings and cause little or no harm in their employment

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<sup>34</sup>The fact that we found some evidence (not presented in the regressions of table 7) that the hours for care assistants fell slightly whereas the number of all employees and the number of all employees and the number of careassistants in particular didn’t change significantly after the NMW 2001 increase may explain why the NMW effects on hourly wages are larger in magnitude and more significant than that of weekly wages.

## 5 The Effects of the UK National Minimum Wage on Other Outcomes

In the following sections we present our findings on the effects on the NMW 2001 increase on other outcomes such as prices, labour productivity and effort as well as the effects of both the 1999 NMW introduction and increase on profits and long-run outcomes as the skill composition of workforce and capital/labour relative utilisation.<sup>35</sup> Moreover, we also look at any NMW effects on supervision intensity and other offsets that are predicted by efficiency wages as changes in employees' quality, tenure and turnover.

Our main objectives are: to extend MW's analysis that focused on wages, employment and closures and in this way to look for other possible adjustments that may have been the result of minimum wage increases; to investigate the effects of the minimum wage policy in the long run and thus provide a credible attempt to fill an important gap of the literature; and finally to test one the main premise of our thesis that efficiency wages considerations may be particularly relevant in low wage labour markets by looking into some of the implications of the theoretical model we developed in chapter 2, as well as the implications of other seminal efficiency wages models (Salop 1979, Weiss 1980, 1990).

### 5.1 Prices

The effects of the minimum wage on product prices of low wage firms consist another important gap of the minimum wage literature (Card and Krueger 1995, Brown 1999). MMR investigated the effects of the 1999 NMW introduction on average weekly price of bed in their sample of care homes across the UK and find no evidence of price effects. This finding was explained by the fact that there is a significant proportion of care homes residents for whom the price of services is regulated by local authorities and thus cannot be freely adjusted by the owners of care homes. This is actually one of the characteristics that rendered the care homes sector an ideal one to study the effects of the NMW, as discussed by MMR and MW. In fact, further information provided by local authorities indicates that local authorities/social security funding didn't increase after the 1999 NMW introduction and that is why care homes couldn't

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<sup>35</sup>The effects of the 1999 NMW introduction on prices, labour productivity and effort have been investigated by MMR (2003). MW investigated the effect of the NMW 1999 on home closures, as discussed in section 3. When we reproduced the same investigation using the data from the south coast subsample of care homes for 1999 we find very similar results as MMR and MW and thus we exclude the presentation of these results from our analysis. In the following sections we present our findings if they have not been investigated by existing studies or if they have been investigated by existing studies, but they are significantly different.

pass on the increased wage costs generated by the NMW introduction to prices (see MMR 2002).

However, MW present no evidence of price effects of the 2001 NMW increase on care homes from the south coast of England. This evidence is important as they could cast further light on the overall impact of the 2001 NMW increase in the care homes sector and could also provide a better picture of any possible offsets generated by minimum wage increases.

Estimates of minimum wage effects on the change in log average weekly price of bed of south coast care homes are presented in table 7. Estimates of the price effects of the NMW 2001 increase are not in line with those from the 1999 introduction. In particular, we find that prices grew faster in homes that were most affected by the minimum wage 2001 increase. For example as suggested by the estimated results summarised in the right panel of table 7 the average weekly price of bed growth was on average higher by 2.2% in a home that had 10% more affected employees or at a home that experienced a 1% higher increase in the weekly wage bill as a result of the minimum wage compared to other homes.

Information from local councils suggests that although prices of care services provided by care homes are regulated by local authorities, in contrast to the period before and after the 1999 introduction, there wasn't any price capping in the period before and after the 2001 NMW increase.<sup>36</sup>

Therefore, based on evidence on price effects of the NMW 2001 increase we may infer that although prices were capped for the period around the 1999 NMW introduction, the same wasn't probably the case for the 2001 NMW and thus the NMW increase generated substantial increases in the average price of services of residential care homes.

## 5.2 Profits

Most theoretical models developed to analyse the effects of the minimum wage policy in low-wage labour markets or low-wage industries generate predictions on the effects of minimum wages on profits. However, few empirical studies investigate the effects of minimum wages on profits (Card and Krueger 1995, Brown 1999). Fur-

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<sup>36</sup>This information is further supported by the fact that we fail to find a significant effect of the proportion of dss residents on price growth. The same is the case for an interaction variables of minimum wage impact measures and the proportion of residents at home whose fees are not paid by local authorities. These results suggests that we find no evidence that price increases in 2001 were the result of increases only in the price of services for residents for whom fees are not covered by local authorities, because of local authority funding capping that restricted care homes managers/owners to pass on wage costs to residents with fees covered by local authorities.

thermore, the two previous care homes studies produced by MMR and MW do not address this issue.

In this section we are investigating the effects of the NMW 1999 introduction and 2001 increase on trading surplus, which is our proxy for profits.<sup>37</sup> As suggested by results reported in table 8, we find no significant changes in the trading surplus rate of care homes between the period of pre minimum and post minimum wage introduction and increase, a result which again persists across specifications and minimum wage measures used.

The main difference between the estimated effects of the minimum wage introduction and increase on the trading surplus of care homes was that estimates from the 98/99 sample are positive and insignificant, whereas those from 01/02 sample are negative and insignificant. However, estimates from 98/99 should be more reliable, compared to those from 01/02 because in the latter case there wasn't sufficient information in order to use an unbiased measure of trading surplus.

In general, as far as the 1999 NMW introduction is concerned we find that there is a substantial increase in wage costs across homes followed by some small reduction in employment (in hours and bodies) and no change in the price of service, findings which may suggest that we should expect that significant effect of the NMW 1999 introduction on profits should have been observed.

Nevertheless, in order to provide a better explanation on the effects on profits it is better to look also for other possible adjustments in care homes that may have taken place, as substitution of production inputs and changes in effort or productivity across homes.

### 5.3 Long Run Effects

As suggested above more evidence on the long-run effects of minimum wages is needed. As far as the NMW effects in the care homes sector MMR and MW investigated the relationship between homes exit and entry and the NMW introduction but failed to find any systematic relationship between the minimum wage and homes closures or openings.

In this section we are looking for evidence on long-run adjustments as a response to the NMW, such as substitution of capital and higher skilled for lower skilled labour. These issues haven't been investigated by the main previous studies of the effects of the NMW.

Theoretical models often used to predict the impact of the minimum wage on labour market outcomes suggest that in the long-run a minimum wage increase may cause substitution of capital (or higher

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<sup>37</sup>See notes of table 9 for details of how trading surplus was calculated.

skilled labour) for lower skilled affected labour or vice versa. In order to test these predictions we estimate specifications where the dependent variable is the log number of beds to number of employees and log number of beds to number of total weekly hours of work at home ratio,<sup>38</sup> and the ratio of skilled (proportion of employees with nursing qualification) to unskilled employees (those with no qualification, as nursing qualification is the only relevant qualification in the industry).

As indicated in the upper panel of table 9 we find a positive and significant effect of the NMW 1999 introduction on the number of beds per employee and on the number of beds per weekly hour of work which are our proxies for the capital/labour ratio, only for specifications that include the proportion of affected workers as the minimum wage measure. Moreover, as suggested by the upper and lower panel of table 9 we find no effect of the 2001 NMW increase in the change in the number of beds per employee or per weekly hour of work.

Given our results on the effects of the NMW 1999 introduction and 2001 increase on the employment in bodies and in hours presented in table 6, it is very likely that the results of the effects of the NMW on the ratio of beds per employee or per weekly hour are driven by the changes on the two measures of employment generated by the changes in the NMW. This explanation is supported further by estimation results of the effect of the impact of the NMW introduction and increase on the log number of beds presented in the lower panel of table 11.

As indicated in the lower panel of table 11, we do not find evidence of any minimum wage effect on the change in the number of beds in care homes for both the 98/99 and 01/02 periods. Therefore, the evidence suggests that overall there was no change in the capital/labour ratio as measured by the number of beds per employee or per weekly hour of work that can be attributed to substitution of capital for labour for both the period of the NMW introduction and increase.

Furthermore, table 10 presents estimation results for the effects of the NMW on the ratio of employees with nursing qualification to employees with no nursing qualification, which is a proxy of the ratio of high to low skilled labour at home. Again we fail to find any evidence of changes in the relative utilisation of skilled labour, as suggested by the estimates presented in table 10.<sup>39</sup>

<sup>38</sup>The main justification of the use of the number of beds is that this is the only capital information included in the 98/99 and 01/02 care homes data.

<sup>39</sup>We also looked for any changes in the employment of employees with nursing qualification relative to those with no qualification in terms of hours. We find an increase in the hours of qualified employees only for the 98/99 sample and only in the same specifications for which a significant employment effect in terms of hours of all workers or all care assistants was

All in all, the evidence in this section suggests that home owners/managers didn't increase the number of beds relative to employment measured in bodies or in hours in order to offset some of the costs generated by the minimum wage.

Finally, we find no evidence of any minimum wage effect on the change in the skill-composition of workforce in care homes in the 98/99 and in the 01/02 sample. Again a potential explanation of the lack of any evidence that suggests substitution between factors of production may be that the post-introduction and post-increase surveys conducted relative close to the time of the intervention and thus it is not likely that long-run adjustments as that of substitution between factors of production may have occurred.

## 5.4 Output, Workers Productivity and Effort

In the upper panel of table 11 are presented estimation results from specifications with dependent variable the change in the natural logarithm of the number of residents (a measure of care homes output) at home in 98/99 and 01/02 periods.

Results suggest that the change in the NMW had no effect on output as measured by the number of residents at home, except of the case of the 1999 NMW introduction and only when the proportion of affected workers at home is the measure of the impact of the minimum wage, where we find that output fell as a result of the introduction. The pattern of the NMW output effects resembles closely that of the NMW employment effects as measured by the number of employees and total weekly hours of work.<sup>40</sup>

Table 12 summarises the results from the estimation of various specifications with dependent variable the change in employees productivity as measured by the number of residents in home per employee and the number of residents per weekly hour of work and employer's perception of changes in effort as a result of the NMW 2001 increase.<sup>41</sup>

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produced. This suggests that any evidence of relative increase in the hours of work of more qualified employees is not due to substitution of these workers for workers with no nursing qualification but mainly because of the reduction in the hours of work of low-skilled workers. However, results from regressions of the change in the total hours of employees with nursing qualification to those with no qualification are likely not to be so robust as information was quite patchy.

<sup>40</sup>Taking into account both the results on the NMW effects on the number of residents and the number of beds presented in table 13, we can infer that the NMW introduction and increase had no significant effect on the occupancy rate in the care home sector.

<sup>41</sup>The data collected before and after the 2001 NMW increase do not include information on the number of residents but only the number of residents whose costs are paid by local authorities and the department of social security. Information on the change in effort is based on homes managers/owners subjective answers to questions on whether or not work effort changed as a result of the NMW introduction and increase and if effort has changed, whether it decreased or increased (see MMR 2002 for the questions included in the questionnaire used

Estimates presented in the upper and middle panels of table 12 suggest that the 2001 NMW increase had no significant effect on either the change in the log number of residents per employee or the change in log number of residents per weekly hour of work in care homes. This finding is consistent with the results of MMR that find no effects of the NMW 1999 introduction on the number of residents per weekly hour of work. This can be also inferred by the similar patterns in the number of employees and weekly hours and the number of residents for 1999 presented in tables 6 and 11 respectively.

In general evidence of wage effects on employees' productivity is of essence for the validity of efficiency wages. However, in this case the fact that we find no evidence of any NMW effects on labour productivity across care homes should be interpreted with caution as this evidence can be mostly viewed as minimum wage effects on the relative change of output and employment rather than as minimum wage effects on the change of employees' productive behaviour which is a necessary condition for the validity of efficiency wages.

Therefore, this evidence does not particularly mean that because output per worker hasn't changed implies that employees' productive behaviour (for example the intensity by which employees work) hasn't also changed, as it may be the case that when employment fell, the fall was such that more than offset an increase in employees effort and in this way may have led to output reductions per employee or per hour of work.

We now turn to the effects of the NMW on effort across care homes. The lower panel of table 12 presents ordered probit estimates of the effects of the NMW introduction and increase on subjective responses of homes owners/managers about changes in work effort.<sup>42</sup> Results suggest that the NMW had no impact on the probability that effort will be increased relative to that of being decreased for the 04/02 balanced sample of care homes. MMR found similar results for the NMW 1999 introduction.

Again this evidence shouldn't be interpreted as being against of the validity of efficiency wages as first of all the information on effort is subjective and second there may be other realised gains arising from the NMW introduction and increase. We looked into this question in the next section.

In summary, the investigation of the effects of the NMW intro-

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for the data collection).

<sup>42</sup>The effort specification we used in the ordered probit estimation is the empirical counterpart of the non-shirking condition for continuous effort derived in our theoretical model of chapter 2. Note that according to the theoretical specification of NSC supervision intensity should be also included as one of the explanatory variables in the effort empirical specification. However, as we also show below, supervision intensity is endogenous and is correlated with the minimum wage measures and that is why we exclude it here.

duction and increase on output, labour productivity and subjective effort in care homes produces some evidence of a fall in output after the 1999 NMW introduction and no evidence of effects of the 2004 NMW increase on output, labour productivity and effort. However, as discussed above this evidence does not imply anything about any effects the minimum wage may have had on employees productive behaviour in particular, and that is why in the next section we are investigating this issue by looking whether the NMW generated other gains in terms of reduction in costs associated with personnel practices in homes such as supervision or in terms of employees' quality.

## 5.5 Supervision

The main objective of this paper is to test empirically the validity of the hypothesis that efficiency wages considerations may be particularly valid in low wage labour markets, by testing the prediction of the shirking models of efficiency wages of a wage-supervision trade-off.

In this analysis the design of both surveys enables us to address probably the most important problem that hinders the identification of the relationship between wages and supervision, namely the endogeneity bias. Moreover, the choice of the care homes sector which is non unionised, with relatively homogeneous occupations and skills but also homogeneous services (MMR 2003), significantly abates other concerns and problems generated by unobserved heterogeneity.

Additionally, the fact that we have observations on the variables of interest at two different points in time allows us also to control for time invariant unobserved factors that are correlated with supervision intensity and wages, including also measurement error that is another problem that makes the identification of the relationship between wages and supervision particularly vexing.<sup>43</sup>

Supervision intensity and wages are both choices of the firm and in a regression of wages on supervision it is expected that unobserved factors that affect wages will be also correlated with supervision intensity.<sup>44</sup> Another important issue is concerned with the direction of the endogeneity bias. The sign of the bias depends on the rela-

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<sup>43</sup>The empirical literature of the wage-supervision relationship suggests that the problem of measurement is particularly related to supervision intensity, as the measure of most often in the literature is the ratio of supervisors to staff that may overestimate the extent of monitoring as supervisors may not all the time monitor employees but just simply coordinate and guide them on the job (Rebitzer 1995). Moreover, the supervisor to staff ratio does not pick up any differences in the quality of monitoring across care homes (Brunello 1995).

<sup>44</sup>The same holds if one regresses supervision on wages as the determination of both wages and supervision by the employer can be modeled using a simultaneous equations model.

tionship of omitted variables with wages and supervision. Moreover, the identification of the main confounding factors and the nature of their relationship with wages and supervision is usually specified by the rational or theory underlying the determination of wages and supervision.

For example under the rationale provided by efficiency wages models, effort is increasing in the wage and supervision and thus in equilibrium, provided that everything else is constant there is a trade-off between wages and supervision. If the relationship between wages and supervision is as predicted by efficiency wages, then we would expect that the omission of factors that affect effort intensity across establishments will cause a positive bias because firms that require their workers to work harder will set supervision and wages at a higher level. Similarly, again under the efficiency wages assumption that wages and supervision are effort regulating devices, we expect that firms which have better employment practices or use other motivation devices as well will pay lower wages and supervise workers less stringently, to achieve a given effort target (Rebitzer 1995).

Another factor that may also cause an upward bias in the relationship of interest is unobserved differences in effort technology across firms, for example because of the systems that govern labour-management relations that may make effort more responsive in wage and supervision in some firms (Gordon 1990, 1994).

On the other hand other theories of the wage-supervision determination specify different omitted factors of interest and predict a negative bias. This is the case if it is true that better quality workers, which are paid higher wages, are supervised less stringently because they need less guidance and coordination on the job. According to this “sorting by ability” model (Groschen and Krueger 1990), unobserved workers’ quality will cause a negative bias in the relationship of interest.

Another explanation of a wage-supervision trade-off which again makes an assumption about workers’ (not union’s) preferences on supervision can be provided by the “equalizing differences” theory that predicts that if workers like supervision, because for example supervision helps them to achieve career goals (Groschen and Krueger 1990), then they will be willing to receive lower wages if they will be supervised tighter (i.e. they will receive a negative wage-differential as supervision is regarded as a good working condition).

Our analysis in this section uses a different identification strategy than the previous sections, as we use the minimum wage as an instrument to obtain IV-2SLS estimates of the structural parameter of the change in wages on the change in supervision intensity and in this way to test the prediction of the shirking model that in equilibrium there is a trade-off between wages and supervision.

In particular, a simple structural model of the change in supervision is derived from Georgiadis (2004) that extends the standard Shapiro-Stiglitz (1984) shirking model to allow for endogenously determined supervision intensity. The structural model is given by equation (2):

$$\Delta S_{it} = \beta_0 + \beta_4 \Delta \ln W_{it} + \beta_2 \Psi_{i,t-4} + u_{it} \quad (2)$$

where  $\Delta S_{it}$  is the change in supervision intensity,  $\Delta W_{it}$  is the change in the natural logarithm of the hourly wage at home and measured as the ratio of managerial to non-managerial employees between the pre and post NMW introduction/increase period,  $\Psi_{i,t-4}$  is a vector of firms and workers' characteristics that also determine the intensity of supervision<sup>45</sup> and  $u_{it}$  is a random error. In order to derive the reduced form of model (2), we substitute the following reduced form equation that expresses the change in the wage as a linear function of the instrument  $MIN_{i,t-4}$  and the control variables of model (2). The latter reduced form model is given by equation (3):

$$\Delta \ln W_{it} = \gamma_0 + \gamma_4 MIN_{i,t-4} + \gamma_2 \Psi_{i,t-4} + v_{it} \quad (3)$$

where  $MIN_{i,t-4}$  is a measure of the impact of the minimum wage in the pre-NMW introduction/increase period (either the proportion of workers affected or the wage gap) and  $v_{it}$  is an error term. Substituting  $\Delta \ln W_{it}$  from (3) into (2) we derive the estimable reduced form supervision change equation of the form:

$$\Delta S_{it} = \delta_0 + \delta_4 MIN_{i,t-4} + \delta_2 \Psi_{i,t-4} + \xi_{it} \quad (4)$$

where  $\xi_{it}$  is a random error.

Therefore we are able to estimate parameter  $\beta_4$  by using  $MIN_{i,t-4}$  as an instrument for  $\Delta \ln W_{it}$  in model (2). In this particular case (when there is only one instrument) it could be shown that  $\beta_4 = \frac{\delta_4}{\gamma_4}$ , and thus an estimate of  $\beta_4$  can be derived by estimating models (3) and (4) and calculate the ratio of the OLS estimates of  $\delta_4$  and  $\gamma_4$ .<sup>46</sup>

<sup>45</sup>These characteristics aim to pick up heterogeneity in the quit rate, the probability of finding a job once a worker is dismissed and the time preference rate across homes, which are determinants of the intensity of supervision according to the theoretical model

<sup>46</sup>The main identifying assumptions of the IV is that the instrument is correlated with the endogenous variable of interest and uncorrelated with the error term. Results from the estimation of wage change equations in one of the previous sections indicate a strong correlation of minimum wage impact measures and the change in the wage and thus seem to support that the minimum wage is a strong instrument for the change in the wage. Moreover, MMR's test discussed previously seems also to support the fact that variation in the minimum wage measures is exogenous which further supports the instrument exogeneity condition. Finally, we expect that there is no concern that the instrument would affect the change in supervision through another channel than that of the change in hourly wages, as the minimum wage is a pure exogenous wage increase.

Firstly, we are interested in investigating the direction of the endogeneity bias. This is why we compare estimation results from OLS regressions of the change in supervision intensity on the change in log average wage across homes surveyed before and after the 1999 NMW introduction and 2004 NMW increase, with two-stage least squares (2SLS) estimates of the relationship between supervision and wages.

In table 13 we present comparisons of two-stage least squares (2SLS) estimation results with OLS estimates using the same specifications for the 98/99 and 04/02 samples when the instrument for the change in the average wage is the proportion of affected workers (upper panel) and the wage gap (lower panel) respectively. In both the upper and the lower panels OLS estimates of the effect of change in the wage on the change in supervision intensity are in their vast majority positive and significant. This result is consistent with the prediction of our theoretical model that the relationship between wages and supervision is positive, when everything else is not constant.<sup>47</sup>

As indicated in table 13, two-stage least squares estimates are systematically smaller (not in absolute value) than the corresponding OLS estimates and in some specifications are turning to negative and significant. The former result supports the prediction of a positive endogeneity bias. The wage-supervision trade-off is significant for the 98/99 sample when the proportion of affected workers is used as an instrument for the change in average wage<sup>48</sup> and marginally significant when the wage gap is the selected instrument<sup>49</sup> and when also controls for employees and home characteristics, including controls for turnover, effort and monitoring costs.

Furthermore, for the 04/02 sample although we do find evidence of a negative OLS bias we fail to find any negative and significant estimates of the relationship of the change in supervision intensity and the change in average hourly wage across homes.<sup>50</sup>

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<sup>47</sup>In particular the prediction of our theoretical model (Geogiadis 2001) suggests that the wage-supervision relationship is expected to be positive when effort or/and employees' quality are not held constant. In other words, this latter prediction indicates the expected sign of the endogeneity bias of OLS estimates of the wage-supervision relationship, when one fails to control for effort or/and employees' quality differences across firms. Another effort related positive bias may be produced by differences in effort technology across homes, because in homes that effort is more responsive in effort and supervision are expected to set both higher wages and supervision levels. However, in general the use of first differences of the supervisor to staff ratio may abate the OLS bias.

<sup>48</sup>A two-tailed test of the null hypothesis of no systematic relationship between the change in supervision intensity and the change in average wage is rejected at 4.7% level of significance, whereas a one-tail test of the null where the alternative is that the relationship of interest is negative rejects the null at 2.35% level of significance.

<sup>49</sup>A one-tail test rejects the null at around 8% level of significance.

<sup>50</sup>It may be the case that the lack of significant results for the 01/02 sample is due to the fact that less information is available compared to the 98/99 sample and thus less relevant

As long as the 2SLS is a consistent estimator of the true relationship between wages and supervision, we find evidence of a wage-supervision trade-off for the 98/99 sample, presented in table 13. A negative relationship between wages and supervision is consistent with three theories/rationales: a) efficiency wages, b) the ‘sorting by ability’ model and c) “equalizing differences” under the assumption that supervision is a ‘good’ working condition.<sup>51</sup>

If the ‘sorting by ability’ model consists the main explanation of our results then we would expect that the OLS bias caused by unobserved differences in workers’ quality across homes, would be negative,<sup>52</sup> which is not the case here. Furthermore, unless unobserved quality is uncorrelated with observed, we find no evidence that unobserved workers’ quality across homes causes a negative bias in the estimate of interest, as suggested by the estimation results summarised in table 13, where the inclusion of controls for workforce quality characteristics (proportion female, proportion with nursing qualification, etc.) indicates that there is any unobserved quality bias is positive rather than negative.<sup>53</sup> Therefore, based on the latter arguments, one could probably dismiss the ‘sorting by ability’ interpretation of the negative wage-supervision relationship.

Finally, if “equalizing differences” consist the only or dominating explanation of our findings, then we would expect that the direction of the OLS bias will be negative and not positive, as if workers ‘like’ supervision then we would expect that lower wages are paid in homes with higher supervision intensity.<sup>54</sup> However, as discussed above a positive OLS bias is the case here that further suggests that if “equalizing differences” are true then this finding implies that workers’ consider supervision as a ‘bad’ and not as a ‘good’ working condition.

Therefore, again as suggested by the latter argument which is based on our results on the direction of the endogeneity bias of OLS

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controls are included in the regressions of the change in supervision.

<sup>51</sup>The wage-supervision trade-off can also be explained by differences in union bargaining power, under union-firm bargaining over wages and supervision when unions bargain over working conditions and effort and when unions “like” wages but “dislike” supervision. However, in this case this theory does not apply as the care homes sector is non-unionised.

<sup>52</sup>This holds either because the ‘sorting by ability’ is the only underlying theory of the wage-supervision relationship or because, even in the case the other theories are involved that lead to positive biases, the sorting by ability is the dominating explanation and thus the negative bias dominates.

<sup>53</sup>If the fact that the dependent variable includes first differences implies no concern for unobserved workers quality, as care homes is a very homogenous sector, then the combination of first differences and of inclusion of worker’s quality controls should result in a positive and significant relationship between wages and supervision, provided that the ‘sorting by ability’ model is true (see chapter 3 for detailed explanation).

<sup>54</sup>In other words, according to the ‘equalising differences’ explanation where supervision is a ‘good’ working condition, unobserved workers’ preferences over supervision suggest a negative OLS bias.

estimates, any “equalizing differences” explanations could be also dismissed, as our findings imply that if workers’ preferences over supervision are important for wage determination then it is more likely for workers to “dislike” than to “like” supervision, which cannot explain the negative relationship between wages and supervision implied by our results.<sup>55</sup>

Thus, the above discussion may suggest that the evidence of a wage-supervision trade-off should be interpreted as supporting the hypothesis that wages and supervision intensity are effort regulating devices. The latter hypothesis is a prediction of efficiency wages or agency theories in general, of which the efficiency wage theory is a special case. An important feature of our analysis is that it addresses two of the most important empirical problems of the estimation of the wage-supervision relationship namely endogeneity bias and observational equivalence of empirical results.

Additionally, Rebitzer (1995) suggested that except of endogeneity another problem that may hinder the identification of the relationship between wages and supervision is measurement error as the ratio of supervisors to supervised may consistently overestimate the intensity of supervision as supervisors may not have a solely monitoring role in production.

According to this argument it may be the case that supervisors are more skilled production workers that coordinate the activities of lower-skilled workers, fact which further implies, as also emphasized by Groshen and Krueger (1990) and Rebitzer (1995) that under any production function that allows for a non-zero marginal rate of substitution between inputs, an exogenous increase in the wage (as it is the case here) will generate substitution of higher skilled workers i.e. supervisors/managers for lower skilled-non-managerial workers.

In our case the fact that we do not have accurate and detailed supervision data may suggest that measurement error is a potential problem. However, the fact that in our case measurement error concerns are related to the dependent and not the independent variable of interest and the use of first differences and of the two-stage least squares estimation are expected to abate any measurement error problem. Moreover, in this case measurement error concerns the dependent and not the independent variable, and thus if measurement error in monitoring intensity consists a valid problem in this case, then by standard econometric theory (Dougherty 2004) we should expect that this will lead to consistent coefficient estimates but with larger standard errors. Therefore, in our case the presence of measurement error on supervision makes our findings of

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<sup>55</sup>Note also that again the use of first differences may net out the effect of unobserved workers’ preferences on supervision which are expected to be relatively fixed.

a wage-supervision trade-off even more compelling.

As far as the potential substitution of supervisors for workers is concerned, this may be a problem as the minimum wage generates an exogenous increase in the wage of staff relative to those of supervisors. However, Groshen and Krueger (1990) suggest that as long as the wage of supervisors varies independently of that of workers, then substitution is not expected to be a problem.

In our case the situation described by Groshen and Krueger may hold as for example for the 98/99 sample we find that the average hourly wage of managerial workers is £5.5 compared to non-managerial workers for which the average hourly wage is £3.87, which combined with the recent evidence that further suggests no spill-over effects of the NMW introduction (Dickens and Manning 2004), implies that the minimum wage may generate an independent variation in the wages of non-managerial employees compared to wages of managerial employees. This latter proposition is further supported by the results (not reported here) from regressions of the change in log average hourly wage of managerial workers on the two measures of the NMW impact with or without controls for both the 1999 introduction and 2004 increase which show no significant impact of the national minimum wage on the change in average managerial wages, whereas a strongly and positive relationship is the case for non-managerial wages and measures of the minimum wage impact.

Moreover, the fact that in our previous analysis we failed to find any evidence of substitution of high for low-skilled employees in bodies or even in hours, which may be explained by the argument that no long-run effects could have taken place during the time between the NMW introduction/increase and the post-minimum data-collection, may further suggest that substitution may not be a problem in our case. Finally, if any substitution of supervisors for staff concerns remain valid even when the above conditions hold, the fact that the substitution bias is expected to be positive, makes our finding of a negative wage-supervision relationship even more compelling and suggests that our estimates of the wage-supervision trade-off are moderate estimates of the true relationship of interest.

Therefore, the evidence produced in this section provide support to a negative relationship between wages and supervision intensity across homes for the period surrounding the 1999 NMW introduction but no support of a wage-supervision trade-off for the period of the 2004 NMW increase.

The evidence from previous sections also imply that the NMW both in 1999 and 2004 was binding, as suggested both by the survey statistics presented in the previous section, where it is found that a very large proportion of employees across care homes will be affected by the NMW introduction and increase and also that in the case

that wages of affected workers will be increased for employers to comply with the law this will generate considerable increases in care homes wage bill. In addition to the above evidence, the fact that in 1999 the NMW was introduced in a previously unregulated labour market and that the 2004 increase was the most generous to date (around 13.1% and 9 % increases in the adult and development rate respectively) may provide further support to the fact that the NMW had a significant ‘bite’ in the care homes sector and thus increased wages in the sector above the ‘market-clearing’ wage in this period.

Given also the estimation results presented earlier in this section and the arguments used in order to sort-out alternative theoretical explanations of the wage-supervision trade-off, the evidence of the trade-off could be interpreted as that wages and supervision are substitutes in eliciting productive behaviour by employees. Combining the latter two points the evidence could be interpreted as supporting a fundamental assumption of efficiency wages (and in particular of the “shirking” and “gift-exchange” models) that above market clearing wages generate gains in terms of employees’ productivity (in this particular case savings in supervision costs).

However, the latter evidence is necessary but not sufficient to support efficiency wages as one needs also to show that the above market clearing wages were actually efficient, i.e. to show that wages ‘paid for themselves’ (Levine 1992).

A simple test of the latter hypothesis can be provided by comparing the associated benefits and costs of the higher wage, based on estimates from the 98/99 care homes sample. The marginal cost of the higher wage can be calculated as a 4% increase in the average wage of non-managerial, affected workers, which for the pre-introduction survey is £3.87 per hour. Thus an estimate of the marginal cost of a 4% increase in the wage of non-managerial employees above the initial (market clearing) wage is £0.0387 per hour, per employee.

The marginal benefit of the wage is the fall in supervision costs as we estimated that wage increases are associated with reduction in the number of managerial employees per non-managerial employee. In particular a 4% increase in the average wage at home is associated with a 5.2% fall in the number of managerial employees per worker.<sup>56</sup> Therefore, provided that the average number of managerial employees per non-managerial employee is 0.13.1 in the before 1999 introduction sample and that the average wage of managerial employees is £5.5, the fall in supervision costs generated by the 4%

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<sup>56</sup>The 2SLS estimate from specification (2) of the upper panel of table 15 is not an estimate of the elasticity of supervision intensity w.r.t the wage. In particular our estimate provides that  $\frac{d(N/L)}{d \ln w} = -0.57$ , where  $N/L$  is the ratio of supervisors to workers and  $w$  is the wage. Therefore to derive the associated elasticity we need to divide the estimate with the sample average of the ratio of managerial to non-managerial employees, which is 0.11.

increase in the wage will be equal to £0.0313<sup>57</sup> per employee, per hour, which is lower than the increase in the wage costs.

A wage-supervision trade-off estimate for which the increase in wage costs generated by the minimum wage is exactly offset by a fall in supervision costs should be around 0.7. However, in the case that the increase in the wage generates substitution of managerial for non-managerial employees, the 2SLS estimate of the wage-supervision trade-off is a moderate<sup>58</sup> estimate of the true wage-supervision trade-off, which further implies that it may be possible that the NMW introduction may be such that the increase in wage costs per employee is exactly or more than offset the increase in supervision costs.

All in all the evidence in this section provide support to the tenet of efficiency wages that above market-clearing wages generate gains in terms of employees productivity and thus because of this reason a binding minimum wage may not hurt profits very much, fact which could further explain why although the minimum wage generated significant increases in wage costs across care homes, employment effects were quite moderate. This latter finding provides support to the main hypothesis of interest which suggests that because of important efficiency wages considerations in low-wage labour markets, the imposition of a binding minimum wage is not expected to generate significant increases in costs and thus undesired employment adjustments for workers or policy makers.

## 5.6 Employees Quality and Turnover

Different theoretical models of efficiency wages focus on different gains in terms of employees' productivity generated by higher wages. In the shirking model (Stiglitz 1984) higher wages increase effort or reduce shirking by employees, whereas in the turnover model (Salop 1979) and the adverse selection model (Weiss 1980, 1990) higher wages reduce turnover and improve the average 'quality' of firm's applicants respectively.

In the previous sections we found some evidence that higher wages generated by the NMW 1999 introduction reduce supervision intensity and therefore supervision costs but no evidence that the NMW 1999 introduction and 2004 increase, raised effort or the intensity of work for employees in care homes. In this section we are looking for any evidence on any other employees' productivity related gains that may have been the result of the above market clearing pay generated by the NMW introduction and increase.

<sup>57</sup>Calculated as follows:  $0.052 \times 0.11 \times \text{£}5.5$ .

<sup>58</sup>Rebitzer (1995) also notes that in general IV estimates are moderate estimates of the true relationship of interest.

Tables 14 and 15 summarise estimation results of the impact of the NMW on the ‘quality’ and the turnover rate of the employees across care homes. According to the adverse selection model of efficiency wages, higher wages will improve the average quality of firm’s applicants. Moreover, based on the turnover model, as higher wages will decrease quits and increase the recruitment rate of the firm average tenure in the firm will rise and in turn this will lead in an increase in the average stock of human capital of firm’s employees.

In the upper panel of table 14 are summarised the results of the NMW impact on the change in average age of employees at home, where average age is viewed as a proxy of the employees’ experience on the job and thus the average human capital in the care home. We find no evidence of an NMW effect on the change in the average age of employees across care homes.

Similarly the lower panel of table 14 presents estimates of the effect of the NMW on the average tenure of employees in care homes. Again, no evidence is produced of any significant effect of the NMW on the average tenure of employees in care homes for both the 98/99 and 01/02 samples.

Finally, table 15 summarises estimation results of the effects on the NMW on change in care homes quit and recruitment rate respectively. Once more, the results are uniformly insignificant across all specifications and minimum wage measures used.

Therefore, the main conclusion of this section is that we find no evidence of any other gains, as reductions in quits or increase in recruitment and in average tenure or average age of employees in care homes, resulted from the NMW introduction and increase.

## 6 Conclusions

The main objective of this paper is to test empirically the validity of efficiency wages theory exploiting a quasi-natural experiment from a low-wage labour market, the UK residential care homes industry, provided by the 1999 introduction and 2001 increase in the UK National Minimum Wage. The imposition of a binding minimum wage in general offers an ideal research design to test the validity of efficiency wages as the essence of efficiency wages is that above market clearing wages are generating gains in terms of employees’ productivity. This ideal research design offers the opportunity to test one of the main predictions of the ‘shirking’, that in equilibrium an increase in the wage will cause a fall in supervision intensity, *ceteris paribus*.

Our main contribution is that we address the main empirical problems, namely endogeneity, measurement error and observational equivalence, that hinder the estimation of the wage-supervision re-

lationship. Probably the most important problem, that makes the empirical testing of efficiency wages particularly vexing and seems to be neglected in the literature, is that overcoming the main empirical problems and producing evidence that supports a wage-supervision trade-off is a necessary but not sufficient condition for the validity of efficiency wage theory.

Although, the central tenet of efficiency wages that suggests that above market-clearing wages affect workers' productive behaviour, the second fundamental feature of efficiency wages is that, the above-market clearing setting of wages is done in an efficient, profit-maximising way. Therefore we also exploit the nature of the minimum wage introduction into a previously unregulated labour market and the evidence of a wage-supervision trade-off produced for the 1999 NMW increase to test the latter condition of efficiency wages.

We find that wage increases generated by the 1999 NMW introduction reduced the intensity of supervision in the care homes industry, and thus this finding provides support to the efficiency wages theoretical rationale that wages and supervision are substitutes in regulating employees effort. This finding can be interpreted as indirect evidence of wage effects on workers' productive behaviour. Moreover, we also find that the increase in wage above the market clearing level produced by the introduction of a binding minimum wage in a previous unregulated low-wage industry, was more or less exactly offset by the fall in supervision costs. This latter finding combined with the first finding that supports the productivity augmenting property of higher wages provide support to efficiency wages and thus also to the central premise of the thesis that efficiency wages may be particularly valid in low-wage labour markets, where the minimum wage is binding.

The latter result has important policy implications and suggests that efficiency wages may provide valuable insight into how low-wage labour markets operate. In order to further test the latter conjecture we also estimate the effects of the 1999 and 2001 NMW introduction on wages, employment, prices, profits, output, effort and productivity, as empirical evidence can be further used to check the usefulness of our efficiency wages theoretical model in explaining the evidence on the economics effects of the minimum wage in a low-wage labour market, the UK residential care homes industry.

In sum, we find that the NMW increased wages in both 1999 and 2001 but the 2001 wage effects are larger in contrast to the findings of Machin and Wilson (2004) who found higher effects for the 1999 introduction. Moreover, although wages increased significantly as a result of the NMW introduction and increase, we find some evidence of relative moderate employment reductions for the 1999 and no employment effects for the 2001 period.

Our analysis finds that prices haven't changed as a result of the

policy in 1999, fact that can be explained by local authority regulations that capped prices, whereas prices have increased after the 2001 and profits weren't affected in both periods. We also find no evidence of any NMW effects on long-run outcomes such as exit for the 1999 and factors substitution for both periods, with the latter being explained mainly by the fact that it is unlikely that substitution have taken place within a period of two or three months after the policy, when the 'post-treatment' surveys have been conducted. Finally, except of the evidence in support of a wage-supervision trade-off produced for the 1999 NMW introduction we also find some evidence that output fell in 1999 and that effort based on subjective employers' responses has remain unchanged in 1999 and 2001.

All in all our findings in this paper seem to provide support to efficiency wage theory and thus may further suggest that efficiency wages could shed much light into the working of low-wage labour markets.

## 7 Tables

**Table 1: July 1998/May 1999 Survey Descriptive Statistics**

	All Firms		Balanced Panel	
	Pre-Minimum	Post-Minimum	Pre-Minimum	Post-Minimum
Number of Homes	548	579	195	195
Number of Workers	14.93 (10.61)	16.67 (39.12)	14.36 (9.16)	14.56 (9.41)
Hourly Wage (None imputed)	4.065 (1.098)	4.364 (1.229)	4.055 (0.71)	4.17 (0.51)
Hourly Wage (With imputed)	4.069 (1.091)	4.362 (1.219)	4.003 (1.031)	4.18 (0.52)
Weekly Hours (None imputed)	24.64 (11.33)	24.74 (11.02)	23.751 (11.437)	24.03 (11.24)
Weekly Hours (With imputed)	24.69 (11.22)	24.71 (10.74)	23.753 (11.323)	23.05 (7.24)
Weekly Earnings (None imputed)	102.06 (61.86)	109.67 (68.32)	96.31 (39.42)	98.01 (31.08)
Weekly Earnings (With Imputed)	102.59 (61.09)	110.44 (66.82)	96.61 (58.07)	99.03 (31.03)
Proportion of Workers with missing information	0.11	0.098	0.045	0.086
Proportion Female	0.89	0.9	0.91	0.90
Average Age	40.44 (7.12)	40.84 (7.45)	40.46 (7.23)	40.58 (7.57)
Proportion Care Assistants	0.63	0.62	0.63	0.64
Proportion With Nursing Qualification	0.1	0.11	0.12	0.12
Number of Beds	17.74 (10.06)	19.94 (29.87)	16.93 (9.25)	17.3 (9.54)
Number of Residents	15.33 (9.25)	18.00 (28.42)	14.67 (8.23)	15.21 (8.54)
Occupancy Rate	0.85	0.88	0.86	0.87
Average Weekly Price per Bed	252.45 (79.23)	253.8 (79.27)	252.83 (84.97)	254.42 (79.00)
Proportion DSS/Local Authority	0.49	0.53	0.47	0.5
Supervision Intensity	0.11	0.13	0.11	0.126

**Notes:** Standard errors in parentheses. Pre-minimum and post-minimum statistics are calculated using observations from the pre-NMW introduction and post-NMW introduction sample. All averages are calculated across homes. Supervision intensity is calculated as the ratio of managerial to non-managerial employees at home. The occupation of care assistants includes senior, day and junior carers but exclude night carers and sleep-ins.

**Table 2: August 2001/February 2002 Survey Descriptive Statistics**

	All Firms		Balanced Panel		Balanced Panel (Excluding firms with a lot of missing worker information)	
	Pre- Minimum	Post- Minimum	Pre- Minimum	Post- Minimum	Pre- Minimum	Post- Minimum
Number of Homes	411	333	152	152	130	130
Number of Workers	15.93 (10.36)	15.70 (9.64)	15.15 (8.75)	15.36 (9.28)	15.45 (8.91)	15.74 (9.40)
Hourly Wage (None imputed)	4.691 (1.244)	4.816 (1.285)	4.686 (1.237)	4.84 (1.21)	4.702 (1.259)	4.85 (1.25)
Hourly Wage (With imputed)	4.695 (1.236)	4.840 (1.307)	4.684 (1.228)	4.85 (1.23)	4.700 (1.254)	4.870 (1.27)
Weekly Hours (None imputed)	26.232 (11.451)	25.85 (11.62)	25.640 (11.445)	25.58 (11.57)	25.72 (11.42)	25.83 (11.62)
Weekly Hours (With imputed)	26.271 (11.302)	25.88 (11.38)	25.658 (11.191)	25.68 (11.33)	25.75 (11.28)	25.92 (11.36)
Weekly Earnings (None imputed)	125.99 (73.54)	126.37 (72.05)	123.69 (75.32)	125.65 (73.78)	124.00 (75.79)	127.05 (74.95)
Weekly Earnings (With Imputed)	126.57 (72.55)	127.5 (72.31)	123.85 (73.62)	128.07 (74.38)	124.30 (74.94)	129.57 (75.63)
Proportion of Workers with missing information	0.13	0.099	0.11	0.10	0.025	0.081
Proportion Female	0.86	0.88	0.9	0.88	0.90	0.87
Average Age	40.84 (7.00)	42.16 (6.71)	41.638 (6.32)	41.94 (6.17)	41.21 (6.26)	41.66 (5.95)
Proportion Care Assistants	0.55	0.61	0.605	0.63	0.61	0.63
Proportion With Nursing Qualification	0.023	0.02	0.023	0.022	0.025	0.022
Number of Beds	20.44 (35.28)	18.61 (29.87)	18.56 (18.14)	17.70 (8.60)	18.93 (20.19)	18.01 (8.95)
Average Weekly Price per Bed	285.76 (97.52)	281.33 (99.69)	279.013 (76.53)	287.00 (90.47)	282.42 (75.84)	291.66 (93.90)
Proportion DSS/Local Authority	0.48	0.46	0.48	0.45	0.497	0.467
Supervision Intensity	0.076	0.076	0.09	0.074	0.089	0.077

**Notes:** Standard errors in parentheses. Pre-minimum and post-minimum statistics are calculated using observations from the pre-NMW introduction and post-NMW introduction sample. All averages are calculated across homes. The last two columns include homes in the balanced sample with less than half workers information missing. Supervision intensity is calculated as the ratio of managerial to non-managerial employees at home. The occupation of care assistants includes senior, day and junior carers but exclude night carers and sleep-ins.

**Table 3: The “Bite” of the 1999 National Minimum Wage Introduction**

98/99				
	All Firms		Balanced Panel	
	Pre-Minimum	Post-Minimum	Pre-Minimum	Post-Minimum
% Paid Less Than Their Age-Specific Minimum Wage	23	0.66	21	0.75
% Paid Less Than The Adult Minimum Wage (only covered workers are considered)	29	1.8	28.8	2.2
% Paid Less Than The Adult Minimum Wage (all workers)	30	3.3	30.5	4
Wage Gap	0.024	0.0012	0.019	0.0007
% Paid Exactly Their Age Specific Minimum Wage	7.9	19.8	8.6	20
% Paid Exactly The Adult Minimum Wage (only covered workers)	8.1	22.6	8.7	22.6
% Paid Exactly the Adult Minimum Wage (all workers)	8.8	24	9.2	23.8
Number of Homes	548	579	195	195

**Notes:** The age specific National Minimum Wage is £3.6 per hour for those above 21 years old (the adult rate) and £3 per hour for those between 18 and 21 inclusive and also including those that are above 21 who are undertaking formal training and who are in the first six months of employment (the development rate). The wage gap variable indicates the proportional increase in the weekly wage bill if the wages of those affected were increased to their age specific minimum rate.

**Table 4: The “Bite” of the National Minimum Wage 2001 increase**

	All Firms		Balanced Panel		Balanced Panel (Excluding firms with a lot of missing worker information)	
	Pre-Minimum	Post-Minimum	Pre-Minimum	Post-Minimum	Pre-Minimum	Post-Minimum
% Paid Less Than Their Age-Specific Minimum Wage	22.3	1.7	19	1.4	20.1	1.4
% Paid Less Than The Adult Minimum Wage (only covered workers are considered)	25.2	2.7	21.5	2.1	23	2.2
% Paid Less Than The Adult Minimum Wage (only covered workers)	29.1	4.6	25.7	4.06	26.7	4.06
Wage Gap	0.0153	0.0052	0.0135	0.0038	0.0134	0.0042
% Paid Exactly Their Age Specific Minimum Wage	4.1	18.2	4.1	16.1	4.5	15.7
% Paid Exactly The Adult Minimum Wage (only covered workers)	4.2	18.1	4.4	16.8	4.9	16.7
% Paid Exactly the Adult Minimum Wage (all workers)	4.7	19.5	4.6	17.7	5.09	17.4
Number of Homes	411	333	152	152	130	130

**Notes:** The age specific National Minimum Wage is £4.1 per hour for those above 21 years old (the adult rate) and £3.5 per hour for those between 18 and 21 including those that are above 21 who are undertaking formal training and who are in the first six months of employment (the development rate). The wage gap variable indicates the proportional increase in the weekly wage bill if the wages of those affected were increased to their age specific minimum rate.

**Table 5: The Effects of the UK National Minimum on Home Level Average Hourly and Average Weekly Wages**

Change in log Hourly Wage								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
% Low-paid	0.12 (0.024)	0.13 (0.027)			0.26 (0.048)	0.26 (0.05)		
Wage gap			1.16 (0.18)	1.16 (0.19)			3.69 (0.61)	3.7 (0.60)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.12	0.18	0.18	0.24	0.18	0.31	0.21	0.36
Number of Homes	182	182	182	182	134	134	134	134
Change in log Weekly Wage								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
% Low-paid	0.08 (0.069)	0.098 (0.076)			0.17 (0.1)	0.14 (0.1)		
Wage gap			0.49 (0.53)	0.42 (0.56)			2.44 (1.29)	2.28 (1.26)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.008	0.1	0.0048	0.097	0.02	0.22	0.026	0.23
Number of Homes	182	182	182	182	134	134	134	134

**Notes:** Standard errors in parentheses. Controls include: proportion female, average age, proportion with nursing qualification, proportion of la/dss residents and county dummies, as well as missing values dummies for proportion of la/dss residents and for average age for both the 98/99 and the 01/02 samples (missing values were imputed with sample means).

**Table 6: The Effects of the UK National Minimum Wage on Home Level Number of Employees and on Total Weekly Hours**

Change in log Number of Employees								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
% Low-paid	-0.28 (0.1)	-0.33 (0.11)			0.17 (0.22)	0.15 (0.23)		
Wage gap			-1.01 (0.8)	-1.05 (0.86)			0.55 (2.64)	-0.031 (2.8)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.038	0.089	0.0085	0.077	0.004	0.093	0.0003	0.088
Number of Homes	194	194	188	188	148	148	141	141
Change in log Total Weekly Hours								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
% Low-paid	-0.23 (0.15)	-0.19 (0.17)			0.18 (0.25)	0.17 (0.27)		
Wage gap			-1.74 (1.23)	-1.44 (1.33)			1.48 (3.23)	0.82 (3.4)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.012	0.063	0.01	0.063	0.004	0.07	0.0016	0.07
Number of Homes	185	185	185	185	135	135	135	135

**Notes:** Standard errors in parentheses. Controls include: proportion female, average age, proportion with nursing qualification, proportion of la/dss residents and county dummies, as well as missing values dummies for proportion of la/dss residents and for average age for both the 98/99 and the 01/02 samples (missing values were imputed with sample means).

**Table 7: The Effects of the National Minimum Wage on Average Weekly Price of Bed**

Change in the log average weekly price of bed				
	01/02			
	(1)	(2)	(3)	(4)
%Low-paid	0.22 (0.077)	0.2 (0.08)		
Wage gap			2.19 (0.91)	2.16 (0.96)
Controls	No	Yes	No	Yes
R-squared	0.056	0.13	0.04	0.14
Number of Homes	142	142	135	135

**Notes:** Standard errors in parentheses. Controls include: proportion female, average age, proportion with nursing qualification, proportion of la/dss residents and county dummies, as well as missing values dummies for proportion of la/dss residents and for average age for both the 98/99 and the 01/02 samples (missing values were imputed with sample means).

**Table 8: The Effects of the UK National Minimum Wage on Weekly trading surplus rate of Care Homes**

Change in Weekly Trading Surplus Rate								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
%Low-paid	0.02 (0.22)	0.31 (0.24)			-0.37 (0.76)	-0.39 (0.55)		
Wage gap			0.012 (1.71)	0.84 (1.89)			-7.02 (9.09)	-5.15 (7.4)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.0001	0.093	0.0001	0.096	0.0021	0.55	0.004	0.55
Number of Homes	150	150	144	144	117	117	112	112

**Notes:** Standard errors in parentheses. Controls include: proportion female, average age, proportion with nursing qualification, proportion of la/dss residents and county dummies, as well as missing values dummies for proportion of la/dss residents and for average age for both the 98/99 and the 01/02 samples (missing values were imputed with sample means). Weekly trading surplus rate is calculated as weekly revenue (average weekly price of bed times number of residents minus total costs (total labour costs calculated as the total weekly wage bill divided by the proportion of labour costs in total costs) and all this divided by weekly revenue. Because of lack of information on the number of residents in 01/02 sample, trading surplus was calculated as described above but using the number of beds instead of the number of residents.

**Table 9: The Effects of the UK National Minimum Wage on Measures of Capital/Labour Ratio**

Change in log Number of Beds per Employee								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
% Low-paid	0.16 (0.089)	0.19 (0.1)			-0.16 (0.18)	-0.2 (0.18)		
Wage gap			0.57 (0.69)	0.55 (0.75)			-1.09 (2.15)	-0.7 (2.26)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.018	0.065	0.0037	0.076	0.0056	0.14	0.0019	0.14
Number of Homes	192	192	186	186	148	148	141	141
Change in log Number of Beds per Weekly Hour								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
% Low-paid	0.21 (0.11)	0.19 (0.12)			-0.19 (0.2)	-0.22 (0.21)		
Wage gap			1.2 (0.87)	1.06 (0.92)			-1.39 (2.66)	-0.89 (2.71)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.019	0.096	0.01	0.09	0.0063	0.13	0.002	0.13
No of Homes	183	183	183	183	135	135	135	135

**Notes:** Standard errors in parentheses. Controls include: proportion female, average age, proportion with nursing qualification, proportion of la/dss residents and county dummies, as well as missing values dummies for proportion of la/dss residents and for average age for both the 98/99 and the 01/02 samples (missing values were imputed with sample means).

**Table 10: The Effects of the UK National Minimum Wage on High to Low Skilled Employees Ratio**

Change in ratio of Employees with Nursing Qualification to those with no Qualification								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
%Low-paid	0.026 (0.24)	-0.006 (0.22)			0.014 (0.023)	-0.001 (0.018)		
Wage gap			-0.42 (1.9)	-0.52 (1.9)			0.147 (0.26)	0.09 (0.19)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.0001	0.35	0.0003	0.35	0.002	0.51	0.002	0.54
Number of homes	182	182	178	178	148	148	141	141

**Notes:** Standard errors in parentheses. Controls include: proportion female, average age, proportion with nursing qualification, proportion of la/dss residents and county dummies, as well as missing values dummies for proportion of la/dss residents and for average age for both the 98/99 and the 01/02 samples (missing values were imputed with sample means).

**Table 11: The Effects of the UK National Minimum Wage on High to Low Skilled Employees Ratio**

Change in ratio of Employees with Nursing Qualification to those with no Qualification								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
%Low-paid	0.026 (0.24)	-0.006 (0.22)			0.014 (0.023)	-0.001 (0.018)		
Wage gap			-0.42 (1.9)	-0.52 (1.9)			0.147 (0.26)	0.09 (0.19)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.0001	0.35	0.0003	0.35	0.002	0.51	0.002	0.54
Number of homes	182	182	178	178	148	148	141	141

**Notes:** Standard errors in parentheses. Controls include: proportion female, average age, proportion with nursing qualification, proportion of la/dss residents and county dummies, as well as missing values dummies for proportion of la/dss residents and for average age for both the 98/99 and the 01/02 samples (missing values were imputed with sample means).

**Table 12: The Effects of the UK National Minimum Wage on Labour productivity and Subjective Effort**

Change in the log number of residents per employee				
	01/02			
	(1)	(2)	(3)	(4)
%Low-paid	-0.15 (0.28)	0.059 (0.28)		
Wage gap			-1.41 (3.63)	0.0049 (3.64)
Controls	No	Yes	No	Yes
R-squared	0.0022	0.18	0.0012	0.17
Number of Homes	131	131	124	124
Change in the log number of residents per weekly hour				
	(1)	(2)	(3)	(4)
%Low-Paid	-0.24 (0.27)	-0.09 (0.28)		
Wage gap			-2.59 (3.71)	-1.06 (3.77)
Controls	No	Yes	No	Yes
R-squared	0.006	0.14	0.004	0.14
Number of Homes	119	119	119	119
Change in subjective effort				
	(1)	(2)	(3)	(4)
% Low-Paid	-0.13 (0.41)	-0.41 (0.48)		
Wage gap			-5.1 (5.14)	-8.00 (5.98)
Controls	No	Yes	No	Yes
Prob>Chi-squared (LR)	0.75	0.45	0.32	0.39
No of Homes	131	131	125	125

**Notes:** Standard errors in parentheses. In the upper and middle panel we use the standard controls we use throughout all regressions, whereas in the lower panel we include also separation and recruitment rate and average tenure as well as missing value dummies for these variables. Information on dss/la residents is used in the calculation of productivity ratios because of lack of information on the actual number of residents. The lower panel presents ordered probit estimates, where the effort variable is coded as an ordered response with values 0 if effort is reported to fall, 1 if effort doesn't change and 2 if effort reported to increase.

**Table 13: OLS versus 2SLS Estimates of the Wage Elasticity of Supervision Intensity**

Change in the ratio of managerial to non-managerial employees								
	98/99				01/02			
	(1)		(2)		(1)		(2)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Change in log average wage	0.13 (0.08)	0.0035 (0.24)	-0.14 (0.1)	-0.57 (0.28)	0.24 (0.07)	-0.004 (0.17)	0.23 (0.086)	0.016 (0.2)
Controls	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.012	0.0001	0.37	0.37	0.07	0.0001	0.11	0.07
Number of Homes	178	190	142	147	133	145	131	138
Change in the ratio of managerial to non-managerial employees								
	98/99				01/02			
	(1)		(2)		(1)		(2)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Change in log average wage	0.13 (0.08)	-0.001 (0.2)	-0.14 (0.1)	-0.31 (0.22)	0.24 (0.07)	0.081 (0.14)	0.23 (0.086)	0.1 (0.16)
Controls	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.012	0.0001	0.37	0.36	0.07	0.0022	0.11	0.07
Number of Homes	178	184	142	147	133	139	131	139

Notes: The upper and lower panels present 2SLS estimates when the proportion low-paid and the wage gap are used respectively as instruments for the change in log average wage at home. Standard errors in parentheses. Controls for 98/99 include the standard controls used throughout as well as month response and ownership dummies, whether home is part of larger organisation dummy, quit rate, recruitment rate, residents per weekly hour, beds per weekly hour and all other costs to labour costs ratio as well as missing values dummies for proportion of la/dss residents and average age for which missing values were imputed with sample means. Controls for 01/02 include the standard controls used throughout as well as ownership dummies, whether home is part of larger organisation dummy, quit rate, recruitment rate, beds per weekly hour as well as missing values dummies for proportion of la/dss residents and average age for which missing values were imputed with sample means.

**Table 14: The Effects of the UK National Minimum Wage on Average Age and Average Tenure of Employees**

Change in log Average Age								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
% Low-paid	0.035 (0.039)	0.019 (0.042)			0.04 (0.052)	0.016 (0.05)		
Wage gap			0.043 (0.3)	0.002 (0.31)			0.056 (0.64.)	-0.41 (0.63)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.0045	0.083	0.0001	0.083	0.012	0.14	0.0001	0.1
Number of Homes	183	183	180	180	138	138	133	133
Change in log Average Tenure								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
% Low-paid	0.17 (0.1)	0.16 (0.11)			0.035 (0.18)	0.14 (0.19)		
Wage gap			1.24 (0.79)	1.14 (0.85)			1.49 (2.19)	2.62 (2.28)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.015	0.079	0.013	0.074	0.0003	0.11	0.0034	0.12
Number of Homes	187	187	184	184	143	143	138	138

**Notes:** Standard errors in parentheses. Controls include: proportion female, average age (included only in the average tenure regressions), proportion with nursing qualification, proportion of la/dss residents and county dummies, as well as missing values dummies for proportion of la/dss residents and for average age for both the 98/99 and the 01/02 samples (missing values were imputed with sample means).

**Table 15: The Effects of the UK National Minimum Wage on Quit and Recruitment Rate**

Change in Quit Rate								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
% Low-paid	0.081 (0.05)	0.043 (0.055)			-0.012 (0.058)	-0.024 (0.061)		
Wage gap			0.27 (0.4)	0.065 (0.4)			-0.076 (0.62)	0.27 (0.64)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.013	0.13	0.0026	0.19	0.0003	0.1	0.0009	0.12
Number of Homes	184	184	178	178	148	148	141	141
Change in Recruitment Rate								
	98/99				01/02			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
% Low-paid	0.03 (0.08)	0.03 (0.055)			0.034 (0.06)	0.035 (0.07)		
Wage gap			0.39 (0.63)	0.2 (0.6)			0.37 (0.78)	0.69 (0.83)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.0061	0.14	0.0022	0.23	0.0018	0.082	0.0016	0.092
Number of Homes	187	187	181	181	148	148	141	141

**Notes:** Standard errors in parentheses. Controls include: proportion female, average age, proportion with nursing qualification, proportion of la/dss residents and county dummies, as well as missing values dummies for proportion of la/dss residents and for average age for both the 98/99 and the 01/02 samples (missing values were imputed with sample means). The quit and recruitment rates are calculated using information on the number of employees left and the number of employees recruited in the last three months from the time of each survey.

## 8 Figures

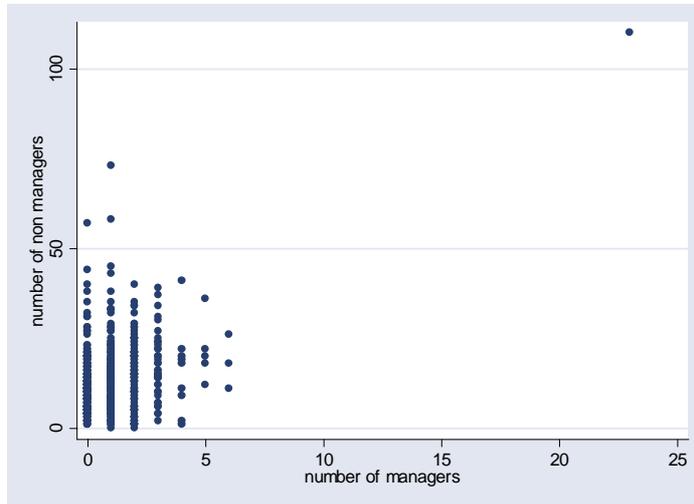


Figure 1: The scatter plot of the number of managers of the whole before 1999 NMW introduction care homes sample.

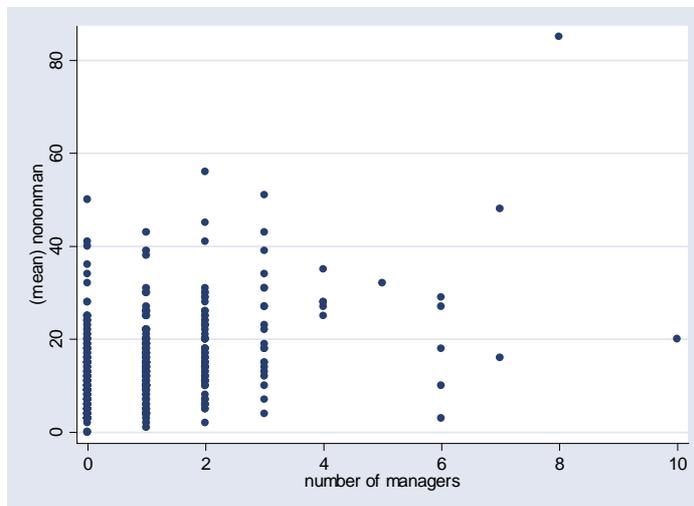


Figure 2: The scatter plot of the number of managers of the whole before 2001 NMW increase care homes sample.

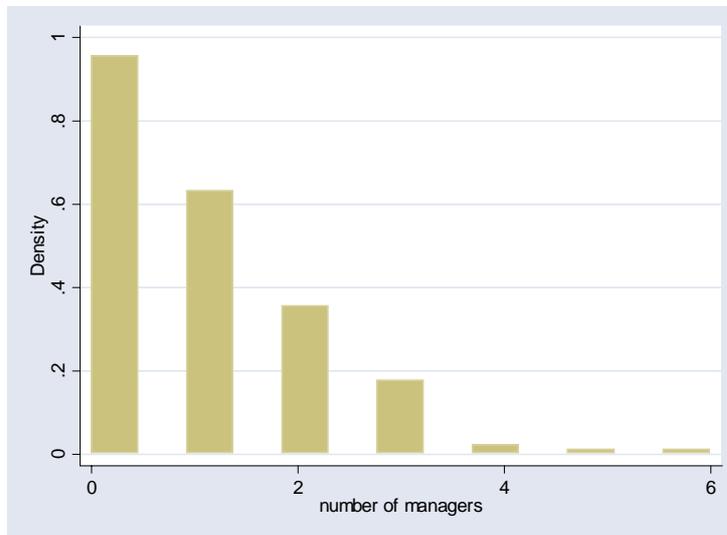


Figure 3: The cross-section distribution of the number of managers in the before 1999 NMW introduction balanced sample.

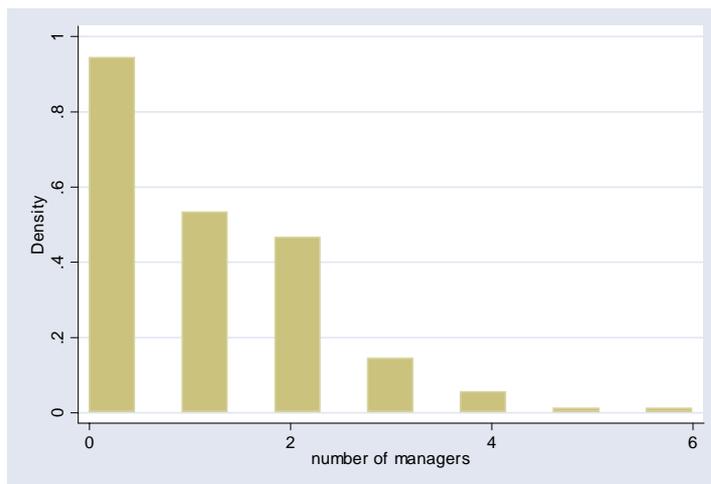


Figure 4: The cross-section distribution of the number of managers in the after 1999 NMW introduction balanced sample.

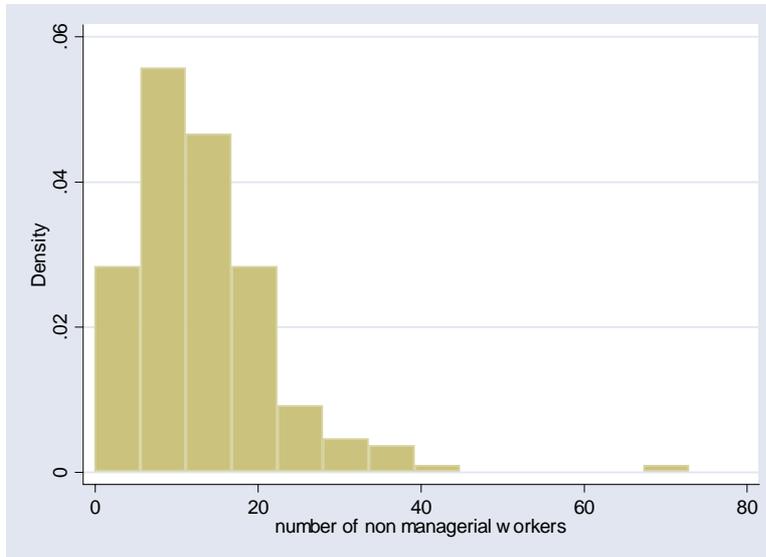


Figure 5: The cross-section distribution of the number of non managerial employees in the before 1999 NMW introduction balanced sample.

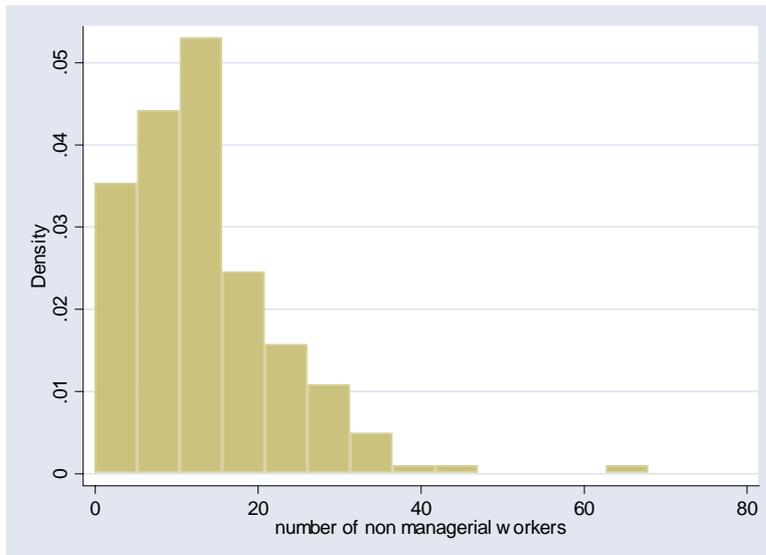


Figure 6: The cross-section distribution of the number of non managerial employees in the after 1999 NMW introduction balanced sample.

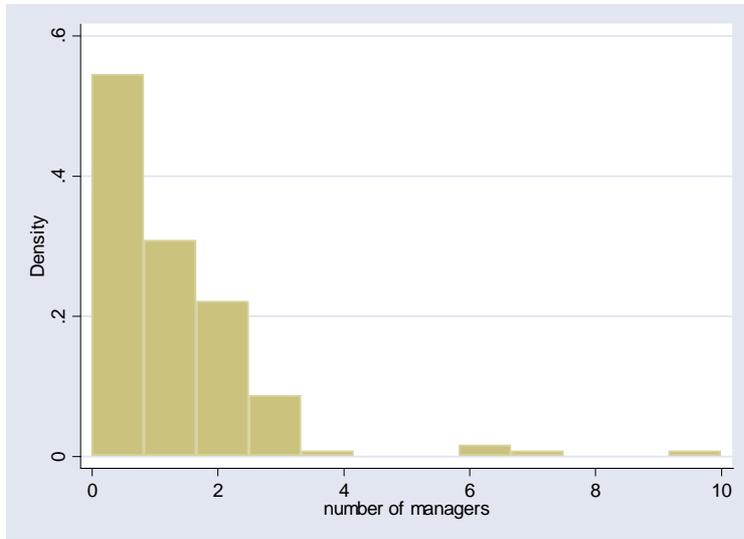


Figure 7: The cross-section distribution of the number of managers in the before 2001 NMW increase balanced sample.

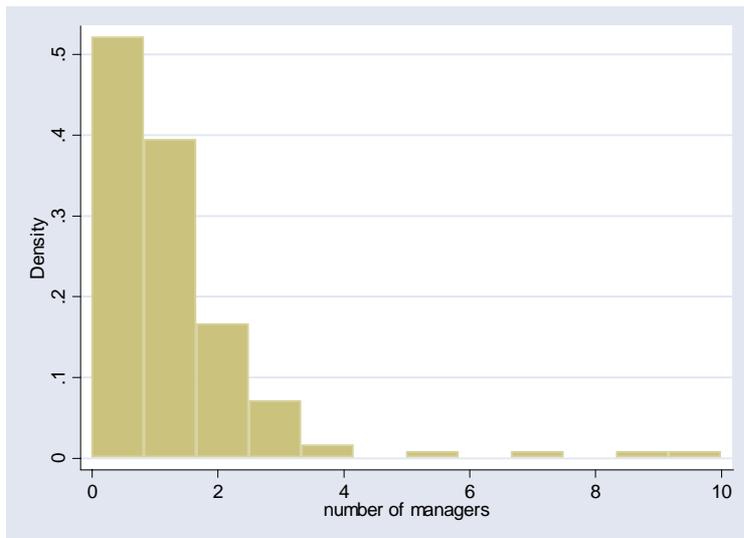


Figure 8: The cross-section distribution of the number of managers in the after 2001 NMW increase balanced sample.

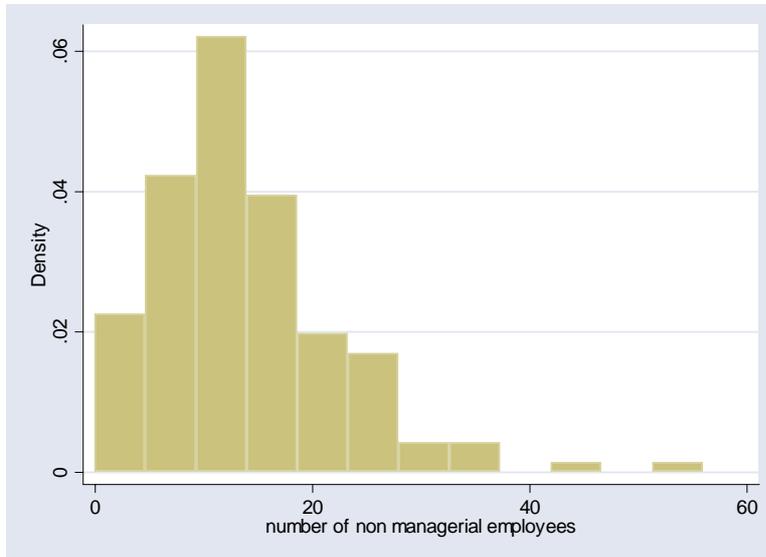


Figure 9: The cross-section distribution of the number of non managerial employees in the before 2001 NMW increase balanced sample.

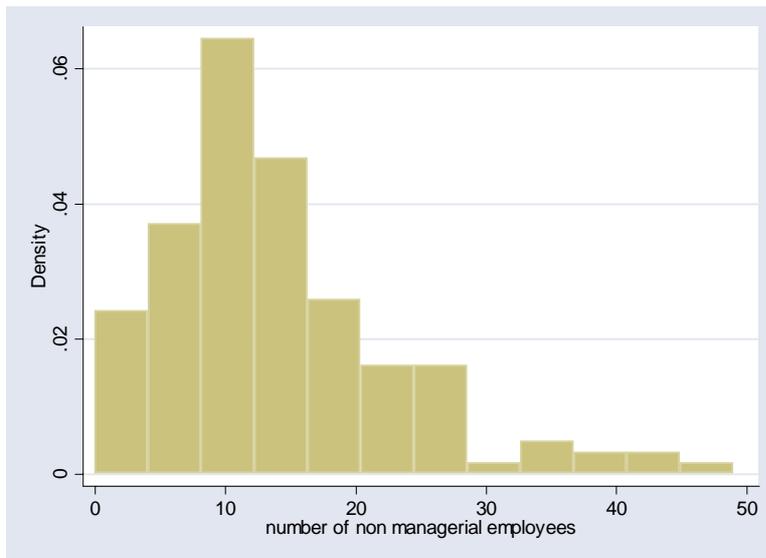


Figure 10: The cross-section distribution of the number of non managerial employees in the after 2001 NMW increase balanced sample.

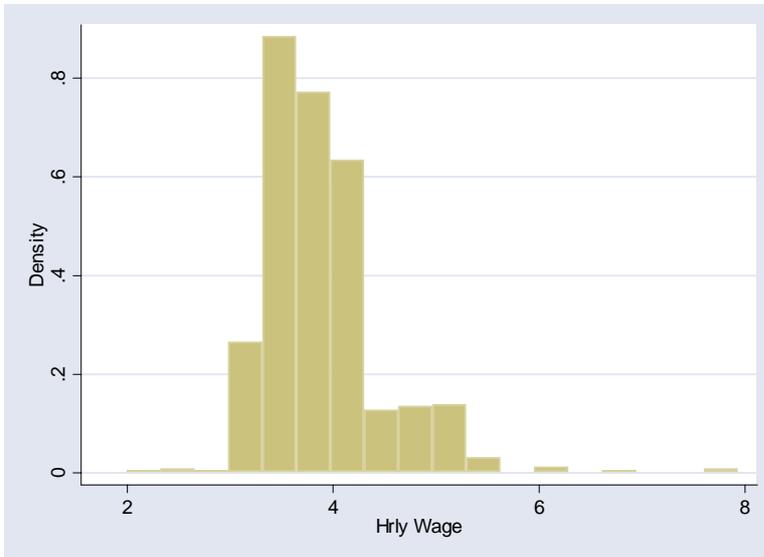


Figure.11: The distribution of hourly wages across workers covered by the development rate in the post-1999 NMW introduction..

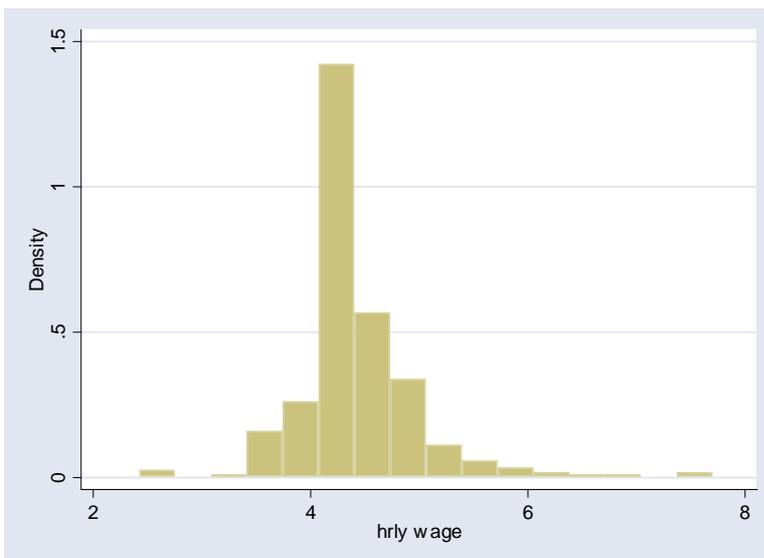


Figure 12: The distribution of hourly wages across workers covered by the development rate in the post-2001 NMW increase.

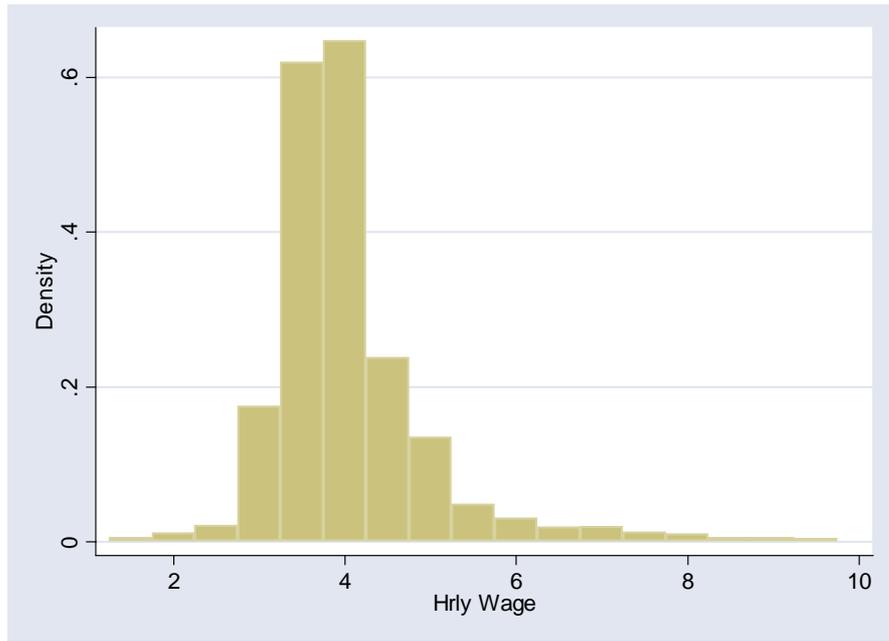


Figure 13: The distribution of hourly wages across all workers in pre-1999 NMW introduction survey.

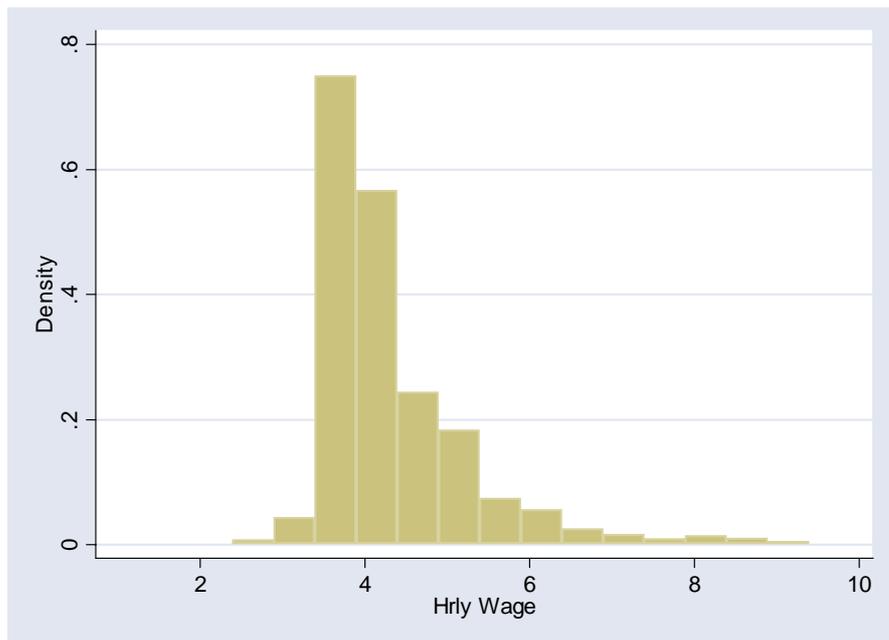


Figure 14: The distribution of hourly wages across all workers in post-1999 NMW introduction survey.

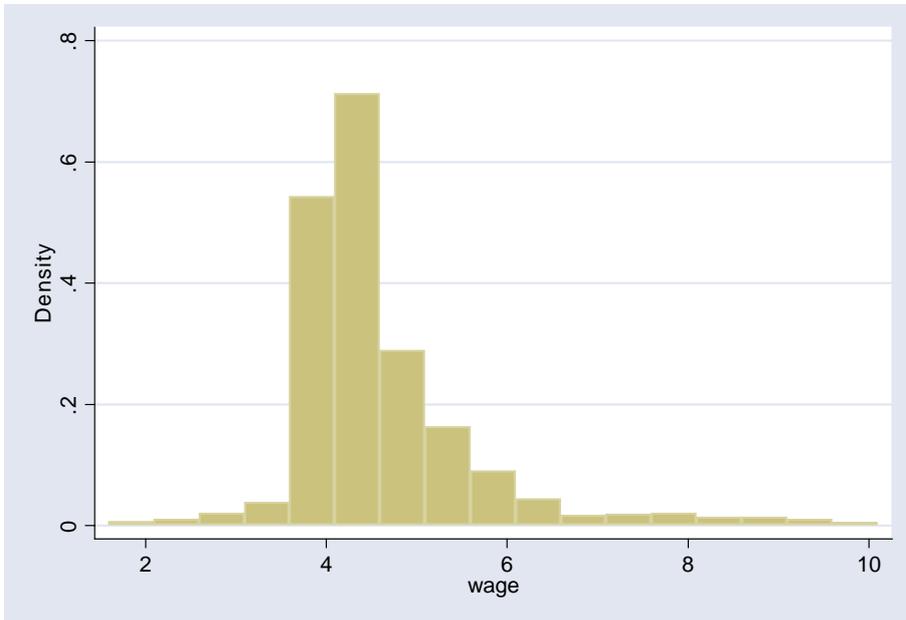


Figure 15: The distribution of hourly wages across all workers in pre-2001 NMW increase survey.

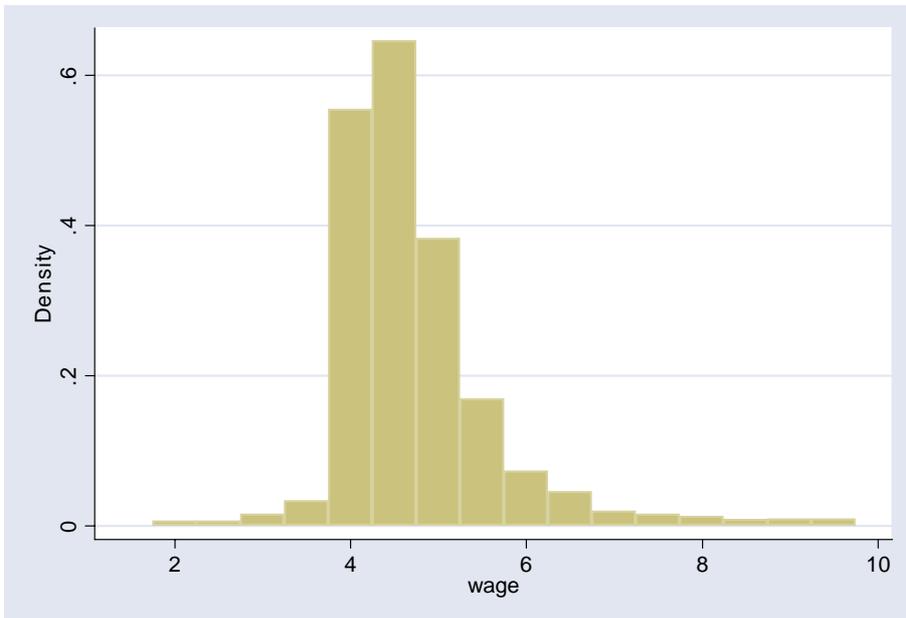


Figure 16: The distribution of hourly wages across all workers in post-2001 NMW increase survey.

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