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FOR UPDATE

Spikes and Spill-overs: The Impact of the National Minimum Wage on the Wage Distribution in a Low-Wage Sector

Richard Dickens* and Alan Manning**

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Abstract

The National Minimum Wage (NMW) that was introduced in April 1999 is sometimes paraded as evidence of the Blair government's commitment to reversing the rise in inequality that was characteristic of the last 25 years. But, because the NMW has been set at a very modest level and because aggregate evidence suggests very small spill-over effects, it has had only a minimal impact on UK wage inequality. But, the small spill-over effects might be because of the small numbers of workers affected and it is possible that there was widespread anticipation of the introduction of the NMW making the impact effect appear very small.

In this paper we have investigated these issues using data collected in a postal survey of care homes where the NMW affected 40% of workers. But, we still find no evidence of large spill-over effects and very small amounts of anticipation of the NMW.

* Department of Economics, Queen Mary Westfield College, University of London and Centre for Economic Performance, London School of Economics.

** Department of Economics and Centre for Economic Performance, London School of Economics.

Introduction

The National Minimum Wage (NMW) that was introduced in April 1999 is sometimes paraded as evidence of the Blair government's commitment to reversing the rise in inequality that was characteristic of the 1980s (mostly) and the 1990s (a little). But, how much of an effect has it really had? One would expect the impact to depend on:

- the level at which the NMW is set
- the level of compliance
- the extent of spill-overs on those initially paid more than the minimum

And, trivially, the measure of wage inequality that one is looking at. In other work (Dickens and Manning, 2002, 2003) we have used data from the Labour Force Survey to investigate some of these issues. The conclusion from that research was that the NMW had been set at a level much lower than originally envisaged (see Low Pay Commission, 1998; Metcalf, 1999), affecting no more than 6% of workers and quite possibly less. And, that there seemed to be little in the way of spill-overs further up the wage distribution. The bottom line was that while the impact of the NMW was detectable at the 5th percentile, it had no noticeable impact on the 10th percentile. The effect on the most commonly used measures of wage inequality was minimal. The subsequent up-ratings in the NMW have done little to alter this: our estimates suggest that the rise in the adult NMW to £4.10 per hour in October 2001 was broadly equivalent in its real impact to the initial adult rate of £3.60 per hour.

But, this research left a number of questions unanswered. First, suitable earnings data for the analysis of the impact of the NMW only started being available in March 1999, one month before the introduction. Although there only seems to be a small spike in the wage distribution (approximately 1%) at what was going to be the

NMW in March 1999, it is possible that the apparently small impact effect was partly the result of employers anticipating the NMW and raising wages in advance. And, perhaps the apparently small spill-over effects were the result of the low level at which the NMW was set: one might think that the pressure for the restoration of wage differentials would be very small if very few workers are having their wage raised by the minimum. And, underlying this, there are still concerns about the quality of the earnings measures in the LFS: only about 40% of workers are reporting an hourly rate and the remaining 60% have to be 'estimated' in some way as the only earnings measure available for them is weekly earnings divided by weekly hours, a measure that has been shown to have very large measurement error.

In this paper, we try to answer some of these questions by using data from another source, a postal survey of workers in residential homes for the elderly. Our sample design was to sample the population of UK care homes before and after the introduction of the minimum wage. We obtained lists of all homes from the Yellow Pages Business Database in July 1998 (for the pre-minimum sampling) and in May 1999 (for the post-minimum sampling). There were 11635 care homes in the former and 11036 homes in the latter. As one of the things one might be interested in is the extent to which employers adjusted wages before the minimum wage introduction we sampled (based on area stratification) one-ninth of the homes in each of the nine months before minimum wage introduction, and then we re-sampled the homes (including new homes), again one-ninth at a time, in the nine months following the introduction of the wage floor. We also identified home closures that occurred over this time period.

The questionnaire was mailed to the manager of the care homes and asked a range of questions about the home and about the views managers (who are often home owners) had about the minimum wage. For obvious reasons, the precise nature of the attitudinal questions was different for questionnaires sent out before and after the introduction of the minimum wage.[1] 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.

This survey might be expected to remedy some of the weaknesses of the LFS data. First, because we have data for the 9 months prior to the introduction of the NMW, we can address the question of anticipation by employers. Secondly, in this sector, a very large number of workers were affected by the NMW so we can address the question of whether the small spill-over effects observed in the aggregate data were the results of the small numbers being paid the NMW. And, for most of the workers, wages are reported as an hourly rate so the problem of measurement error is also reduced. However, while this survey does have its strengths relative to the LFS, there are also some weaknesses. First, it is a postal survey with a response rate of only 20% raising concerns about representativeness. Appendix 1 provides some re-assurance on this point: the distribution of the characteristics of workers in our sample and equivalent workers (i.e. in the same occupation and industry) in the LFS are very similar: it appears that our sample is fairly representative.

The plan of the paper is as follows. In the next section, we present basic information on the numbers paid below, at and just above the NMW in the nine months before and after the introduction of the NMW. The second section then considers the change in the whole distribution of wages.

[1] The actual questionnaires are available on request from the authors.

1. Spikes

We start be presenting information on the proportion of adult workers paid the NMW. Figure 1 presents a time series for the months before and after the introduction of the NMW on the proportion of adult workers (i.e. those aged 22 or above) in our sample paid below the NMW (£3.60 per hour), exactly the NMW, and in the region £3.60-£3.80 and £3.80-£4.00. Several points stand out. First, prior to the introduction of the NMW, a large proportion (about 40%) of workers in this sector were paid below what is going to become the NMW. This proportion falls only slowly until the month of introduction when it falls to very low levels.

Several conclusions are prompted by this finding. First, there is not much in the way of anticipation by employers of the introduction of the NMW: virtually all of the fall in the proportion of workers paid below what is going to be the minimum wage occurs in the month of introduction: to a first approximation, there is a `big bang' effect. Secondly, this data does not suggest that there is a serious problem with non-compliance.

But, this might be a complacent conclusion that could be misleading because employers who do not comply with the NMW are unlikely to respond to our survey or, if they do, to lie and claim they are paying the NMW when they are not. There are several pieces of evidence that make us think that non-compliance is relatively rare e.g. sample response rates do not fall much at introduction and the distribution of wages in our sample is similar to that reported by workers in the Labour Force Survey. But there are other ways in which we can look for evidence of non-compliance.

In the first wave of our survey conducted before the introduction in the NMW when there was nothing illegal about paying any hourly wage we did not mention that there would be a follow-up survey conducted after the introduction. So, there is no particular reason to believe that employers who were subsequently failing to comply would be less likely to respond to the first wave of the questionnaire. But, it does seem plausible to imagine that employers who were subsequently breaking the law would be less likely to respond to the second wave. If this is the case we would expect to see evidence that those who were initially paying below the NMW are less likely to respond in the second wave assuming (as is plausible) that employers who were initially paying above £3.60 per hour continue to do so. Table 1 investigates this hypothesis estimating a probit model for whether there is a response in the second wave as a function of the reported level of wages in the first wave. As a measure of the likely impact of the NMW we use the wage gap the percentage increase in the wage bill required to raise all workers to the minimum wage.

Whatever the controls included, there is no evidence in Table 1 that employers paying initially below the NMW are less likely to respond to the second wave which we might expect to see if there was widespread non-compliance.

There remains the possibility that employers simply lie in their responses. But we think that employers are much more likely to throw our survey in the bin than to go to the effort of lying in responding to it. And, as mentioned earlier, the distribution of hourly wages in our sample is very close to that reported by workers in the Labour Force Survey. Evidence from this and other surveys suggesting relatively high levels of compliance need to be put against evidence like that contained in Boyle (2000) that there are egregious cases of non-compliance: this report also singles out the care home sector as being particularly problematic.

Another feature of Figure 1 is that the proportions paid £3.60 in the subsequent months is somewhat below those paid below £3.60 in the prior months. This is suggestive of spill-over effects: this is the subject of the next section.

2. Spill-Over Effects

It is relatively easy to provide theoretical models for why the minimum wage should have an effect on the wages of those paid above the minimum i.e. those workers who are not directly affected by it. For example, Teulings (2000) presents a competitive model of the labour market in which those workers initially paid just above the minimum wage are close substitutes for those paid just below it so that one cannot alter the wages of one group without having a big influence on the wages of other groups. And non-competitive models of the labour market can also be used to explain the existence of spill-over effects.

How large are spill-over effects in practice? The evidence on this is surprisingly small but there is a recent literature on the US federal minimum wage that suggests they are quite large. A number of papers (diNardo, Fortin and Lemieux, 1998; Lee, 1999; Teulings, 2000) have argued that the minimum wage has a more substantial impact on the wage distribution than previously thought and that much of the rise in US wage inequality in the bottom half of the distribution is the result of changes (or lack of them) in the federal minimum wage. Because the direct effect of the minimum wage is small, this can only be explained if there are sizeable spill-over effects.

Both Lee (1999) and Teulings (2000) estimate the spill-over effect though it is not the main focus of their papers. The approach in Teulings (2000) is based on a competitive model of the labour market in which technology is assumed to have the realistic but messy feature of a 'decreasing in distance elasticity of substitution' between workers with different skill levels. Lee (1999) takes a less structural approach and this is the one we will use here. He assumes that, in the absence of the minimum wage, the wage at position F in the wage distribution is given by w*(F): call this the latent wage distribution. With the introduction of a minimum wage, w_m, the actual wage distribution, w(F), will differ from the latent wage distribution. For example, if there are no spill-over effects and the minimum wage is fully-enforced then the wage distribution will be given by:

$$w(F) = w^*(F) + \max(w_m - w^*(F), 0) \tag{1}$$

Lee (1999) generalizes this and proposes the following model that allows for spill-over effects:

$$w(F) = w^*(F) + \frac{w_m - w^*(F)}{1 - e^{(1/\beta)(w_m - w^*(F))}} \tag{2}$$

where $\beta > 0$ is a parameter which measures the size of the spill-over effect. If $\beta = 0$ the model of (2) reduces to that of (1) so that an increase in β is an increase in the spill-over effect. The spill-over effect, s(F), can be written as:

$$s(F) = \frac{w_m - w^*(F)}{1 - e^{(1/\beta)(w_m - w^*(F))}} - \max\left(w_m - w^*(F), 0\right) \tag{3}$$

i.e. it is the difference between the total effect [w(F)-w*(F)] and the direct effect as measured by the final term in (1). Inspection of (3) shows that the spill-over effects in the Lee model depend only on the gap between the minimum wage and the latent wage and the single parameter β. There are several implications of the particular model of spill-overs in (3). The spill-over effects are largest for those just affected by the minimum wage (i.e. those for whom w_m=w*(F)) and, for these workers, the increase in log wages is equal to β. Secondly, the spill-over effects decline as one moves away from these wages, the rate of decline also being determined by β.

In a more general model of spill-overs, one might think of estimating:

- the wage at which the spill-over effect is greatest
- the maximum spill-over effect
- how wide are the spill-over effects.

i.e. a three parameter model instead of the single parameter model estimated by Lee (1999). However, the Lee model does surprisingly well in estimating the spill-over effects for the US aggregate wage distribution (see Manning, 2003, chapter 12) and, as we shall see in modelling the impact of the NMW in the UK.

To estimate the model, one has to make some assumption about the latent wage distribution w*(F). We make the simple assumption that the latent log wage distribution is that given in a period prior to the introduction of the minimum wage. In the empirical application below we use the first four months of our sample, September to October 1998. As we saw in Figure 1 there is little indication of sizeable anticipation effects so this wage distribution is probably not contaminated by the impact of the NMW.

Some estimates are reported in Table 2. The model estimated is a non-linear least squares model where the dependent variable is the change in the log wage at different percentiles of the distribution. If the initial wage distribution is $w_0(F)$ and the later wage distribution is $w_1(F)$ then we estimate the following specific version of the Lee model of (2):

$$w_1(F) - w_0(F) = \frac{w_m - w_0(F)}{1 - e^{(1/\beta)(w_m - w_0(F))}} \qquad (4)$$

For the later period we initially use the five months after the introduction of the NMW i.e. April-August 1999. We do not include in the estimation the few percentiles where the wage after the introduction of the NMW is below the minimum was the model is not capable of explaining these observations. We also start by estimating the model excluding the top 10 percentiles.

The results are reported in Table 2. The first row reports the spill-over parameter β. The estimate of 0.075 can be interpreted to mean that those workers initially paid £3.60 have their pay raised by 7.5%, an estimate that is not enormous but is not small either. To put it in some sort of context, this implies that that direct effect of the NMW is to raise the average log wage by 4.7% but the total effect is for it to rise by 8.2% implying that the spill-over effects increase the total effect by 3.5% i.e. slightly over two-thirds.

However there is good reason to think that this very simple model over-states the size of the spill-over effects as it ascribes any wage growth seen at the higher percentiles of the wage distribution to spill-over effects. But, because an average period of 9 months elapses between our initial and final wage distributions part of this can probably be explained by general wage growth. The second row of Table 2 estimates a simple model that allows for this assume that, in the absence of the minimum wage, log wages would grow by the same amount at all points of the wage distribution. If wage growth is g, then we estimate the following augemented version of (4):

$$w_1(F) - w_0(F) = g + \frac{w_m - \left(w_0(F) + g\right)}{1 - e^{(1/\beta)(w_m - w_0(F) - g)}} \tag{5}$$

The estimates in the second row of Table 2 show that, as one would expect, the estimated spill-over effects are reduced and the estimated general wage growth is positive though at 6.7% for 9 months it is on the high side. This implies that both the direct and spill-over effects of the minimum wage are smaller than the previous estimates.

The rest of Table 1 then investigates the robust of the results to the percentiles that are modelled (we consider only those below the median) and the sample period used by considering the final period to be September to December 1999 to allow more time for spill-over effects to work their way through the wage distribution. But, in all the models that allow for general wage growth the estimated spill-over effects are very small: indeed, they are essentially zero for estimates that only use the bottom half of the wage distribution.

These results are consistent with those using aggregate LFS data where spill-over effects were found to be very small. This suggests that the small spill-over effects are not the result of the fact that the NMW had only a minimal impact on the aggregate wage distribution: we get very similar results here. This is explored a bit further by disaggregating our care homes data into 12 regions. An individual observation is now a percentile of the wage distribution in a particular region. The results are reported in Table 3. There is some evidence that the spill-over effects are larger but not much.

An alternative way to investigate the plausibility of the claim that spill-over effects are minimal is to look at firm level data and exploit the panel nature of our data set. Define the variable GAP to be the percentage increase in the average hourly wage bill required to bring the initial (i.e. before the NMW is introduced) level of wages up to the minimum. If this is all that firms do and there are no spill-over effects we will have that in firm i:

$$w_{1i} = w_{0i} + \ln(1 + GAP_i) \approx w_{0i} + GAP_i \qquad (6)$$

If on the other hand there are spill-over effects that are largest where GAP is largest then we would not expect to be able to accept the hypothesis that the coefficient on GAP is equal to one. Some estimates for the change in log average hourly wages at firm-level are presented in Table 4.

In the first column we estimate a model where only GAP is included. This has a coefficient of 0.84 suggesting that there is incomplete compliance. But, once other controls are included (column 3) the coefficient rises to 1.022 suggesting close to full compliance with little in the way of spill-over effects. To test the hypothesis that employers with larger gaps are more likely to have to raise the wages of other workers we include a quadratic in the wage gap (column 2 without other controls and column 4 with). This term is not significantly different from zero though it is imprecisely estimated.

The GAP measure in the first 4 columns is based on assuming that all workers have their wages raised to the minimum appropriate to their age. But, as we have seen, many young workers receive the adult minimum so the second four columns repeat the regressions computing the GAP measure assuming that the adult minimum applies to everyone. The results are very similar.

The results in Table 4 are consistent with the earlier results from the cross-sectional distribution of wages that spill-over effects are very small.

The discussion so far has assumed that changes in the wage distribution can only be the result of spill-over effects. But, an alternative hypothesis is employment losses. While Stewart (2001) suggests that, in aggregate, the introduction of the NMW did not cause any job losses, Machin, Manning and Rahman (2003) find, using this data set, that there were employment losses. The next section discusses whether one can hope to distinguish between these two hypotheses?

3. Spill-overs or Disemployment Effects?

There are different reasons why one may see a change in the distribution of wages across a period in which a minimum wage is introduced or raised:

- general wage growth that would have occurred in the absence of the minimum wage
- change in wage inequality that would have occurred in the absence of the minimum wage
- the direct effect of the minimum wage and associated spill-over effects
- the direct effect of the minimum wage and associated employment effects

Can we distinguish between these hypotheses? One would expect there to be different effects. Figure 2 may help us to understand what we might expect to see. Assume the initial log wage distribution is normal with mean zero and a standard deviation equal to 0.3. Now, imagine that there is general wage growth of 2%: this obviously shifts the wage distribution up uniformly at all points as the line marked 'general wage growth' shows. Now, imagine a minimum wage equal to –0.4, which affects about 9% of workers is introduced (this is represented by the horizontal line on Figure 2). If there are dis-employment effects that we assume to be that 25% of workers initially paid the minimum lose their jobs then the spike at the minimum wage will be lower than the proportion initially paid below the minimum and the wage distribution will rise at other points. The change in the wage distribution is largest at the bottom but does not go to zero as we move up the pay distribution, essentially because the truncation of the wage distribution raise the density of wages at all points where there is no truncation. In contrast a model of spill-over effects plausibly has the effect on the wage distribution going to zero.

This discussion suggests that one might hope to be able to distinguish between spill-over and disemployment effects of the minimum wage using the different effects we would expect on different parts of the wage distribution. Unfortunately, things may not be so straightforward in practice as a combination of general wage growth and spill-over effects will lead to a change that looks rather like the dis-employment model. In this case we would want to be able to exploit the fact that the introduction of the minimum wage is a sudden change.

Changes in underlying wage inequality are going to make things even more tricky: in terms of Figure 2 a rise in wage inequality would make the line steeper.

In a couple of papers Meyer and Wise (1983a,b) suggested that one could estimate the dis-employment effects of the minimum wage by looking att he shape of the wage distribution and seeing whether there was any effect like that suggested in Figure 2. But, in the absence of any observation of a period in which the minimum wage was not in place to estimate the counter-factual wage distribution, the estimates are very sensitive to the assumptions made about the functional form of the wage distribution and spill-over effects: Dickens, Machin and Manning (1998) conclude that it is completely unreliable as a way to estimate the employment effects of the minimum wage.

Here, one might hope to be able to use it with more success as we do have some information about the wage distribution in the absence of the minimum wage. But, it is still hard to separately identify the effect of general wage growth and spill-over effects. And, given that this model does so well in explaining the data it is likely that one cannot distinguish these two hypotheses. But, to the extent that part of the spill-over effects we observe could be the result of dis-employment effects, it seems plausible to conclude that they can only be smaller than the estimates we have provided.

4. Conclusion

Because the NMW has been set at a very modest level and because aggregate evidence suggests very small spill-over effects, it has had only a minimal impact on UK wage inequality. But, the small spill-over effects might be because of the small impact and it is possible that there was widespread anticipation of the introduction of the NMW making the impact effect appear very small.

In this paper we have investigated these issues using data collected in a postal survey of care homes where the NMW affected 40% of workers. But, we still find no evidence of large spill-over effects and very small amounts of anticipation of the NMW. Quite why these effects should be so small is not clear but an interesting question for future research.

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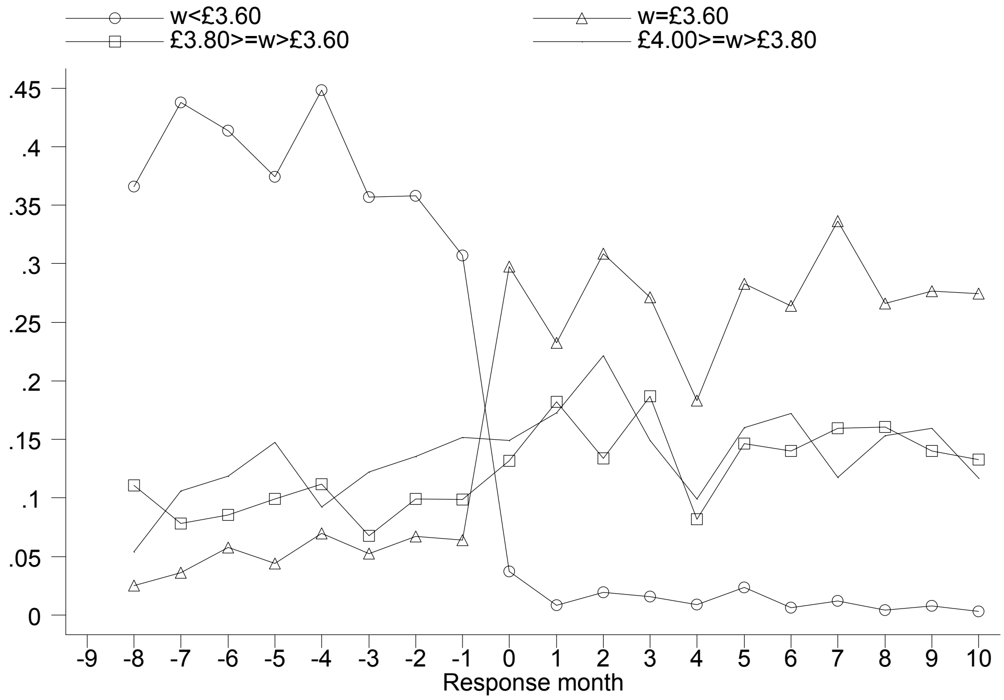
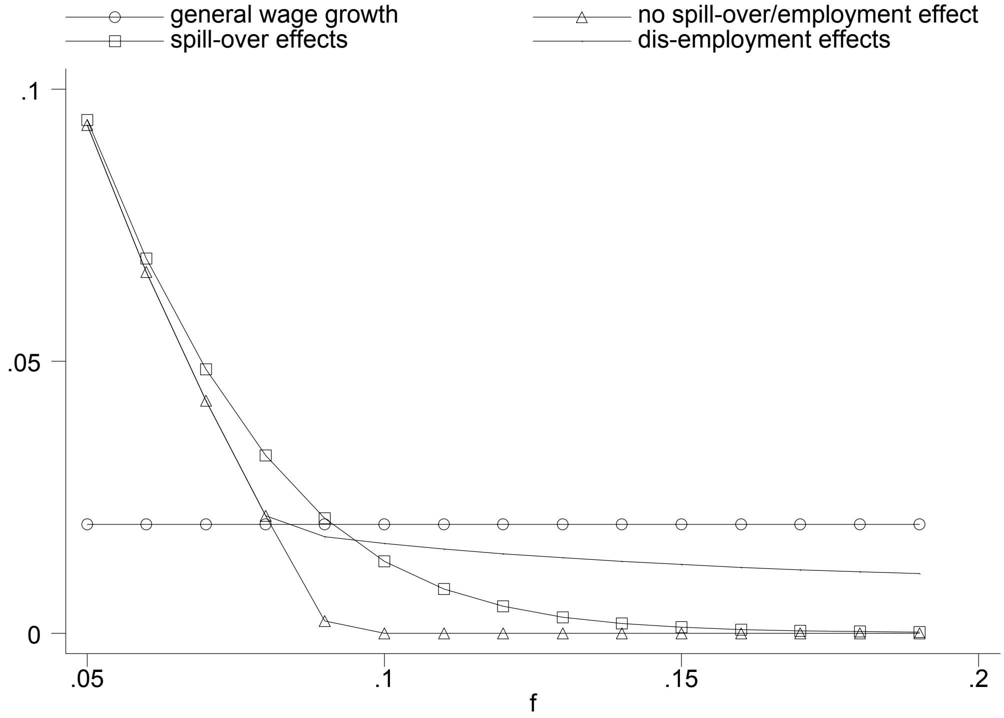
Figure 1
Proportions Paid Below, At and Just Above the NMW



Notes.

1. The vertical axis is the proportion of workers paid wages in the categories reported.
2. The figure relates only to those workers aged 22+ who are eligible for the adult NMW of £3.60
3. The response month '0' is April 1999 the month of introduction of the NMW.

Figure 2
The Likely Effects of General Wage Growth, Spill-over effects and job losses on changes in the wage distribution



Notes.

Table 1
The Determinants of Second Wave Responses

	(1)	(2)	(3)	(4)	(5)	(6)
Initial wage-gap	-0.314		-0.814	0.589		0.097
	[0.432]		[0.465]	[1.473]		[1.551]
Initial log average		-0.252	-0.313		-0.292	-0.285
hourly wage		[0.095]	[0.101]		[0.281]	[0.297]
Constant	-0.305	0.834	1.149	-0.993	0.077	0.044
	[0.036]	[0.434]	[0.470]	[1.753]	[2.413]	[2.471]
Other Controls	No	No	No	Yes	Yes	Yes
Observations	1682	1682	1682	501	501	501

Notes.

1. Standard errors in brackets
2. Sample is restricted to those homes where less than 50% of observations are imputed.
3. Where other controls are included these are the initial proportion female, with a nursing qualification, of the residents paid for by the DSS, the average age, the occupational structure, the months of the survey and the county in which the home is located.

Table 2
Estimates of the Lee Spill-over model: Aggregate Estimates

Sample Period	Sample Percentiles	Spill-over Parameter	General Wage Growth
July/Oct 98- Apr/Aug 99	<90	0.075 (0.006)	
July/Oct 98- Apr/Aug 99	<90	0.011 (0.006)	0.067 (0.002)
July/Oct 98- Apr/Aug 99	<50	0.053 (0.003)	
July/Oct 98- Apr/Aug 99	<50	0.008 (0.002)	0.070 (0.001)
July/Oct 98- Sept/Dec 99	<90	0.061 (0.004)	
July/Oct 98- Sept/Dec 99	<90	0.023 (0.003)	0.045 (0.002)
July/Oct 98- Sept/Dec 99	<50	0.047 (0.002)	
July/Oct 98- Sept/Dec 99	<50	0.007 (0.002)	0.062 (0.001)

Notes.

1. These are the results of estimating the Lee model of (4) (without general wage growth) or (5) (with general wage growth).
2. Percentiles below the minimum wage after April 1999 are excluded.

Table 3
Estimates of the Lee Spill-over model: Disaggregate Estimates

Sample Period	Sample Percentiles	Spill-over Parameter	General Wage Growth
July/Oct 98-Apr/Aug 99	<90	0.062 (0.002)	
July/Oct 98-Apr/Aug 99	<90	0.045 (0.003)	0.025 (0.002)
July/Oct 98-Apr/Aug 99	<50	0.043 (0.001)	
July/Oct 98-Apr/Aug 99	<50	0.046 (0.002)	-0.005 (0.001)
July/Oct 98-Sept/Dec 99	<90	0.065 (0.002)	
July/Oct 98-Sept/Dec 99	<90	0.028 (0.003)	0.045 (0.002)
July/Oct 98-Sept/Dec 99	<50	0.046 (0.001)	
July/Oct 98-Sept/Dec 99	<50	0.019 (0.002)	0.049 (0.004)

Notes.

1. As for Table 2 but the data is further disaggregated into 12 regions that are.

Table 4
Estimates of Spill-Overs from Firm-Level Panel

	1	*2*	*3*	*4*	*5*	*6*	*7*	*8*
initial wage gap	0.836	1.032	1.022	0.541	0.749	0.848	0.907	0.393
	[0.121]	[0.199]	[0.170]	[0.363]	[0.112]	[0.189]	[0.155]	[0.347]
initial wage gap squared		-0.549		1.977		-0.286		2.029
		[0.444]		[1.320]		[0.439]		[1.225]
Constant	0.038	0.034	0.083	0.104	0.035	0.032	0.001	0.023
	[0.010]	[0.010]	[0.235]	[0.235]	[0.010]	[0.011]	[0.237]	[0.237]
Other Controls	no	no	Yes	yes	no	no	yes	yes
Observations	617	617	571	571	617	617	571	571
R-squared	0.07	0.07	0.24	0.25	0.07	0.07	0.24	0.25

Notes.

1. In some homes managers did not complete all the information on worker characteristics. In these cases where there was missing information on hourly wages and/or hours we imputed them using the average for that job within that firm. We then restricted the sample to where less than 50% of hours were imputed: experimentation showed that the results are very similar as long as those where the majority of hours are imputed are excluded.
2. The dependent variable is the change in the log average hourly wage. Where other controls are included these are the initial proportion female, with a nursing qualification, of the residents paid for by the DSS, the average age, the occupational structure, the months of the survey and the county in which the home is located.

Appendix: Representativeness of the Sample

In this Appendix we compare the distributions of the responses to our survey with those from the UK Labour Force Survey (LFS).

From our survey we report results for care assistants. For the LFS, we report results for private-sector workers in the ‘industry’ ‘social work with accommodation’ (class 85.31), whose occupation is ‘care assistants and attendants’. The LFS sample comes from March 1998 to February 2000 so approximately coincides with the period of our survey.

In the Table below we report selected percentiles of the distribution of the characteristics of care assistants in our sample and in the LFS. We have information on age, hours, job tenure and hourly wages. For age, hours and job tenure there is no problem in comparing the variables in our sample and the LFS and the sample sizes are both large. As can be seen from the Table, the distributions are remarkably similar.

The comparison of the distribution of hourly wages is made more difficult by deficiencies in the LFS data that lead to small sample sizes. First, wage information is only collected in waves 1 and 5 (out of 5) so is automatically missing for 60% of observations. Secondly, the main LFS pay variable (which is derived by dividing weekly wages by weekly hours) is now recognized to have very large amount of measurement error and its use led to a wild over-estimate of the numbers of workers who were affected by the minimum wage (see Dickens and Manning, 2002). In March 1999 it was supplemented by a direct measure of the hourly wage for hourly-paid workers: this measure has less measurement error but is not observed for all workers (for our sample here it is 50%).

As a result of this we only have 167 observations in the LFS on the good measure of the hourly wage. The distribution of this variable is reported in the Table below in the row labelled LFS(1). Its distribution is similar to that in our sample (we restrict ourselves here to the post-NMW period as this is the only period for which we have the LFS (1) measure). One other concern is that those who report an hourly rate in the LFS are not randomly selected. A number of methods for dealing with this have been proposed (see Dickens and Manning, 2002, for a discussion). Here we report the results using an inverse propensity score re-weighting in the row labelled LFS(2). For our sample, probably because they are so homogeneous, the re-weighted distribution is very similar to the unweighted distribution.

All of this evidence suggests that we do not have a problem with the representativeness of our sample. In fact the survey and LFS summary statistics square up exceptionally well.

	Sample	5th	10th	25th	50th	75th	90th	95th	Observations
Age	Ours	18	20	27	37	48	56	60	39316
	LFS	18	20	26	36	49	55	59	3218
Weekly	Ours	9	12	18.5	26	35	39	40	39624

Hours	LFS	10	14	20	30	36	40	42	3188
Tenure (months)	Ours	2	4	12	24	60	108	132	39905
	LFS	2	3	9	25	60	108	132	3205
Hourly Wage	Ours	3.6	3.6	3.6	3.80	4.25	5.04	5.57	21313
	LFS(1)	3.3	3.6	3.6	3.77	4.20	4.75	5.00	167
	LFS(2)	3.3	3.6	3.6	3.77	4.15	4.70	5.00	166