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

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Abstract

The UK National Minimum Wage (NMW) has had a minimal impact on UK wage inequality because it has been set at a modest level and because aggregate evidence suggests very small spill-over effects. But, the small spill-over effects might be because of the small numbers of workers affected and widespread anticipation of the introduction of the NMW might make the impact effect appear very small. This paper investigates these issues using data collected from 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.

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The National Minimum Wage (NMW) 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). In Dickens and Manning (2004) we used data from the Labour Force Survey (LFS) to investigate the impact of the NMW on wage inequality concluding 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, as there seem to be little in the way of spill-overs further up the wage distribution, while the impact of the NMW was detectable at the 5th percentile, it had no noticeable impact on the 10th percentile. Subsequent up-ratings in the NMW have done little to alter this.

This research left a number of questions unanswered. First, suitable earnings data (a direct measure of the hourly wage) 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 is 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 as any pressure for the restoration of wage differentials might be very small if few workers have their wage raised by the NMW. And, underlying this, there are still concerns about the quality of the earnings measures in the LFS: only about 40% of workers report 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 (although it is available since 1993).

In this paper, we investigate some of these issues using data from a postal survey of workers in residential homes for the elderly. In each of the nine months before the introduction of the NMW we sampled one-ninth of the population of UK care homes (of which there are approximately 11000) and then we re-sampled one-ninth in each of the nine months after the introduction. Details of the questionnaire and the sample can be found in Machin, Manning and Rahman (2003) who also document that, although the response rate is 20% (fairly typical for a postal questionnaire), the data seems very representative when compared to similar workers in the LFS. In this paper we use the data managers were asked to provide on job title, age and hourly wages on each of the workers in their firm.

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.

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. Spikes

Fig. 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.

Two conclusions are prompted by this finding. First, there is little 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, these data do not suggest that there is a serious problem with non-compliance.

This conclusion could be misleading if employers who do not comply with the NMW are less likely to respond to our survey or, if they do, to lie and claim they are paying the NMW when they are not. But there are ways in which we can look for evidence of non-compliance. The first wave of our survey was conducted before the introduction of the NMW when there was nothing illegal about paying any hourly wage and 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 LFS.

Another feature of Fig. 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 perhaps suggestive of spill-over effects, the subject of the next section.

2. Spill-Over Effects

Both competitive (see Teulings, 2000) and non-competitive models (see Manning, 2003, ch12) of the labour market can explain why the minimum wage should have a spill-over effect i.e. an effect on the wages of those not directly affected[1]. Also, if the minimum wage causes job losses (and the estimates in Machin,

[1] For competitive models the spill-overs come from the substitutability of workers paid just below and just above the minimum wage. For non-competitive models they come from the fact that employers paying just above the minimum wage are competing for workers with those firms paying the minimum wage.

Manning and Rahman, 2003, suggest modest losses in this sector) there will appear to be increases in wages at positions in the wage distribution not directly affected by the minimum wage because of the truncation effect (see Meyer and Wise, 1983). One might hope to distinguish between spill-over and dis-employment effects because spill-over effects do not plausibly reach all the way up the pay distribution whereas the truncation effect will. But, in practice, once one allows for some general wage growth, it becomes hard to distinguish between the hypothesis of spill-over effect plus general wage growth from the hypothesis of dis-employment effects. Our estimates of spill-over effects presented below will be upper bounds as they implicitly assume zero job losses.

Two recent papers (Lee, 1999; Teulings, 2000) have argued (somewhat implicitly) that there are sizeable spill-over effects from the US minimum wage. Here we follow the Lee (1999) approach that is more descriptive and less structural than that taken by Teulings (2000) and does surprisingly well in estimating the spill-over effects for the US aggregate wage distribution (see Manning, 2003, ch12).

Lee assumes that, in the absence of the minimum wage, the log wage at percentile 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 log wage distribution, w(F), differs from the latent wage distribution in the following way:

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

where $\beta \geq 0$ is a parameter that measures the size of the spill-over effect. If $\beta = 0$ the model of (1) reduces to one where those with $w^*(F) < w_m$ have their wage raised to the minimum but there are no spill-overs. 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. 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.

To estimate the model, we assume that the latent log wage distribution is that given in a period prior to the introduction of the minimum wage plus some allowance for general wage growth. In the empirical application use the first four months of our sample, September to October 1998 to estimate the initial wage distribution as Fig. 1 shows there is little indication of anticipation effects. 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 as the model is not capable of explaining these observations. We also exclude the top 10 percentiles though the results are not very sensitive to this.

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. Because the wages at different percentiles are correlated we use GLS to correct the standard errors. The first row reports the spill-over parameter β if we do not allow for general wage growth. The estimate of 0.060 can be interpreted to mean that those workers initially paid £3.60 have their pay raised by 6.0%, 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 5.1% but the total effect is for it to rise by 7.2% implying that the spill-over effects increase the total effect by 2.1% i.e. about 30% of the total.

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 allows for this assuming that, in the absence of the minimum wage, log wages grow by the same amount at all points of the wage distribution.

These estimates show that the estimated spill-over effects are reduced and the estimated general wage growth is positive though at 5.5% 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 2 then investigates the robustness of the results to the sample period used by considering the final period to be September-December 1999 to allow more time for spill-over effects to work their way through the wage distribution. After allowing for general wage growth the estimated spill-over effects are very small and insignificantly different from zero.

These results are consistent with those using aggregate data suggesting 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.

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 comply with the NMW. If this is all that firms do and there are no spill-over effects we will have that in firm i:

$$w_{1i} = w_{0i} + \ln(1 + GAP_i) \approx w_{0i} + GAP_i \qquad (2)$$

If 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 3.

The first column estimates 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. These results also support the conclusion that spill-over effects are very small.

3. Conclusion

This paper has investigated the impact of the NMW on the distribution of wages in care homes where it affected 40% of workers. The conclusions are that compliance is widespread, that there was little anticipation and virtually no spill-over effects.

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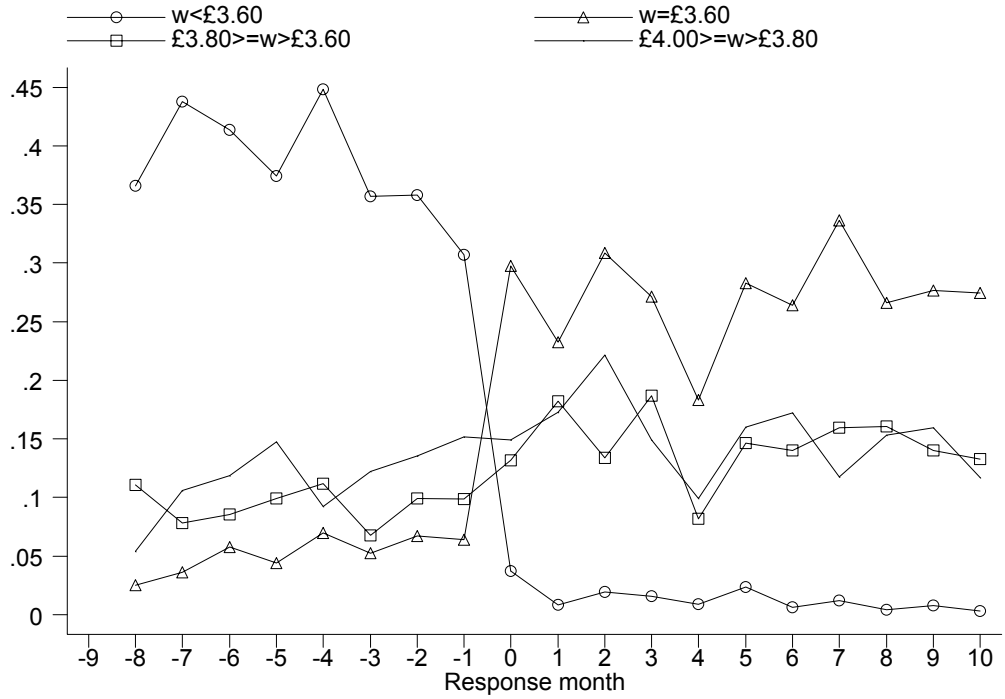
Fig. 1
Proportions Paid Below, At and Just Above the NMW



Notes.

1. The vertical axis is the proportion of workers paid wages in the categories reported.
2. The Fig. 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.

Table 1
The Determinants of Second Wave Responses

	(1)	(2)
Initial wage-gap	-0.314	0.589
	[0.432]	[1.473]
Constant	-0.305	-0.993
	[0.036]	[1.753]
Other Controls	No	Yes
Observations	1682	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

Sample Period	Spill-over Parameter	General Wage Growth
July/Oct 98-	0.060	
Apr/Aug 99	(0.007)	
July/Oct 98-	0.024	0.055
Apr/Aug 99	(0.007)	(0.007)
July/Oct 98-	0.077	
Sept/Dec 99	(0.007)	
July/Oct 98-	0.010	0.075
Sept/Dec 99	(0.010)	(0.007)

Notes.

1. These are the results of estimating the Lee model.
2. Percentiles below the minimum wage after April 1999 are excluded.
3. Standard errors in parentheses: these are computed taking account of heteroscedasitcity.

Table 3
Estimates of Spill-Overs from Firm-Level Panel

	1	*2*	*3*	*4*
initial wage gap	0.836	1.032	1.022	0.541
	[0.121]	[0.199]	[0.170]	[0.363]
initial wage gap squared		-0.549		1.977
		[0.444]		[1.320]
Constant	0.038	0.034	0.083	0.104
	[0.010]	[0.010]	[0.235]	[0.235]
Other Controls	No	No	Yes	yes
Observations	617	617	571	571
R-squared	0.07	0.07	0.24	0.25

Notes.

1. The dependent variable is the change in the log average hourly wage. Other controls are the same as for Table 1.