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The Employment Effects of the October 2003 Increase in the National Minimum Wage

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Abstract
There is a growing body of research that measures employment effects of the minimum wage by using longitudinal data on individuals to compare job loss of workers affected by a minimum wage increase with those who are not directly affected. This sort of study requires good quality wage data in order to clearly identify these treatment and control groups. Much of the evidence on the impact of the UK minimum wage uses this technique with poor quality wage data. This paper examines the impact of the October 2003 increase in the National Minimum Wage (NMW) using a much better measure of the wage. We find insignificant negative effects on the employment retention rates of all adults and, most notably, male workers. Analysis of the probability of employment retention across different hourly wage rates also show how sensitive this methodology can be to different definitions of the treatment and control group.

Keywords: Minimum Wages, Employment Transitions, Wages
JEL Classifications: J31, J63
data: Quarterly Labour Force Survey

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1. Introduction

Initial research on the employment impact of the introduction of the National Minimum Wage has shown no evidence of any significant employment loss (Stewart, 2002, 2003, 2004). Against this background the NMW was raised substantially in October 2003 from £4.20 to £4.50 and again in October 2004 to £4.85. These are quite large increases in the NMW and they have been predicted to raise the wages for a substantial proportion of employees in the UK (see Table 1). Some concerns have been raised in the business community about the size of these increases, with some large employers claiming that for the first time the NMW will affect their pay structures.

In this report we examine the impact of the October 2003 increase in the NMW on employment. We use a methodology first proposed by Linneman (1982) and used more recently by Stewart (2003, 2004) to examine the introduction of the minimum wage. This essentially examines individual transitions out of employment, comparing a group of workers directly affected by the NMW with a similar but unaffected group. In the next section we discuss this methodology and detail how we examine both flows out of and into work. In Section 3 we outline the data used in this report. In Section 4 we present information on the impact of the October 2003 increase in the wage distribution. Section 5 reports our results of the impact on employment and Section 6 concludes.

2. Methodology

During the period that the NMW was introduced and the subsequent up-ratings employment growth in the UK has been strong. However, we cannot infer from this that the minimum wage has had no effect on employment since we do not know what would have happened to employment in the absence of the minimum wage. It may have risen even faster. The standard technique used by economists to evaluate the impact of an intervention such as the NMW is to compare changes in employment for those workers covered by the legislation with employment change for those not covered. A problem arises here since the National

Minimum Wage applies to all workers in the UK. As such, there is no comparison group of workers who are not covered by the legislation.[1]

A technique that has been developed to deal with this problem is to focus on individuals who are directly affected by the NMW to the extent that their pay has to be raised to comply with the new minimum rate. The employment patterns of this *treatment* group are examined over the period that the NMW is raised. Ideally, we would like to compare this employment change for the treatment group with that employment change that would have occurred in this group in the absence of any change in the NMW. This is clearly not possible. The approach usually taken tries to find a naturally occurring comparison group that mimics the properties of a control group in a properly designed experiment. In the minimum wage literature this comparison group is often chosen to be those workers who are not directly affected by the increase in the NMW. Workers whose pay is already above the new minimum rate do not have to have their pay increased to comply with the new NMW and so are a potential comparison group. This methodology is often termed "difference in differences" since the employment outcome before and after the reform for the treatment group is compared with the before and after contrast for the comparison group. We utilise this methodology here to examine the impact of the October 2003 increase in the NMW.

The difference in difference methodology assumes that in the absence of a NMW the differences in employment outcomes between the treatment and control groups is the same in each time period, or that the growth in employment is the same for each group. Mean employment $\overline{E}$ in each group g and time period t, in the absence of the minimum wage is then given by:

$$\overline{E}_t^g = \alpha_g + \phi_t$$

where α_g is a group specific effect and ϕ_t is a time effect common across groups. The NMW has a constant effect Ψ on the employment for the treatment group and no effect for the control group.

$$\overline{E}_t^g = \alpha_g + \phi_t + \Psi \quad \text{if } g = treatment$$
$$\overline{E}_t^g = \alpha_g + \phi_t \quad \text{if } g = control$$

The (raw) difference in difference estimator is then given by:

[1] Note that workers under the age of 18 are not covered by the NMW but their employment prospects are very different from adult workers: which means they are not a good comparison group.

$$\widetilde{\Psi} = (\overline{E}_A^T - \overline{E}_B^T) - (\overline{E}_A^C - \overline{E}_B^C)$$

where $\overline{E}$ is the mean employment outcomes for the treatment (T) and comparison (C) groups in the time period before (B) and after (A) the minimum wage increase.

The assumption of common macro time effects across the two groups is crucial for the identification of difference in difference estimates. If the evolution of employment rates is different across the groups then this assumption is violated. A second key assumption is that the introduction of the NMW has no effect on employment in the control group. This may well be violated if there are wage spillovers to those above the NMW or if there is employment substitution between the groups.

a. Job loss

In practice when estimating the employment impact of the minimum wage it is not possible to compare employment *rates* across the treatment and control groups because these groups are only defined for those individuals in employment (since they are based on the hourly wage). The convention in the literature has been to examine employment *transitions*, with a particular emphasis on job retention. So the outcome measure is the probability of employment today given employment at some previous period. $P[e_{it+1} = 1 \mid e_{it} = 1]$. The difference in difference methodology then compares job retention rates for those affected by the NMW and those not directly affected. This difference is then compared between a period when the NMW is changing and one where it is stable. The estimator is then given by:

$$\widetilde{\Psi} = (\overline{P}_A^T - \overline{P}_B^T) - (\overline{P}_A^C - \overline{P}_B^C)$$

where $\overline{P}$ is the employment retention probability in treatment (T) and control (C) groups, before (B) and after (A) the minimum wage change. Note that the underlying assumptions are now of common time effects in job retention rates and no impact of the NMW on employment retention among the control group. The above estimate could also be achieved by estimating the following linear probability model of employment:

$$P[e_{it+1} = 1 \mid e_{it} = 1] = \alpha_g + \phi_t + \Psi g_{1it} d_{t+1}$$

where α_g is the group effect from above and ϕ_t is the common macro effect, g_{1it} is a dummy variable signifying whether and individual belongs to the treatment group and d_{t+1} is a dummy that denote whether the new NMW is in force.

This model can then be generalised somewhat. We add a vector of individual (and job) characteristics, *x,* that control for any differences between the treatment and control groups not accounted for by the group and time effects. Also, we extend the number of wage groups beyond just the treatment and control groups. When the minimum wage is increased the treatment group are those workers with a wage between the old and new NMW. Our control group are those with a wage just above the new NMW. But there are also a group of workers whose wage is below the old NMW and a group whose wage is way above the new NMW. We therefore define four wage groups *g* as follows.

g=1	Below	$w_{it} < \min_t$
g=2	Treatment	$\min_t \le w_{it} < \min_{t+1}$
g=3	Control	$\min_{t+1} \le w_{it} < \min_{t+1} \times (1+c)$
g=4	High	$\min_{t+1} \times (1+c) \le w_{it}$

Here the constant *c* defines the width of the band above the new minimum for the control group. We estimate effects using $c = 0.1$ but will present results for other values of *c*.[2] We then estimate the following logit specification to obtain our (conditional) difference in difference estimates:

$$P[e_{it+1} = 1 \mid e_{it} = 1] = \Lambda[x'_{it}\beta + \alpha_1 g_{1it} + \alpha_2 g_{2it} + \alpha_4 g_{4it} + \phi_t + \gamma g_{1it} d_{t+1} + \Psi g_{2it} d_{t+1} + \varphi g_{4it} d_{t+1}]$$

The difference in difference estimate is then the marginal effect corresponding to the estimate of $\widetilde{\Psi}$.

For this type of methodology to work well we require data containing very accurate measures of the hourly wage. This is because individuals are assigned to the treatment and control groups based on their hourly wage. If this is measured with error then individuals will be incorrectly assigned to both groups and our difference in difference estimates will not capture the impact of the minimum wage on the treatment group. We shall see below that the

[2] Below we present results for different specifications of both the treatment and control groups.

measures commonly used in the UK are likely to suffer from severe measurement error and as such results are likely to be inaccurate.

b. Job entry

Most work on the impact of minimum wages on employment that uses this methodology examines transitions out of work. Employment effects can be either positive or negative; on the demand side workers may be priced out of their jobs, or on the supply side quits are reduced by a higher minimum wage. Very little work examines flows into employment. These could fall with an increase in the minimum wage if firms reduce hiring of low productivity workers or rise if labour supply is increased as wages rise.

We present results on the impact of the October 2003 up-rating on employment inflows. The choice of treatment and control groups here is somewhat different. Specifically, we examine whether new hires are more or less likely to be minimum wage workers, or workers in some comparison group. The treatment group is therefore defined as those workers who are paid at or below the NMW, and the control group as those workers in a 10% band above the NMW. In our estimation we also include a high wage group; those above the control group.

g=1	Treatment	$w_{it+1} \leq \min_{t+1}$
g=2	Control	$\min_{t+1} < w_{it+1} \leq \min_{t+1} \times (1+c)$
g=3	High	$\min_{t+1} \times (1+c) < w_{it+1}$

We then estimate the following logit specification to obtain our (conditional) difference in difference estimates of job entry probabilities:

$$P[e_{it} = 1 \mid e_{it+1} = 1] = \Lambda[x'_{it+1}\beta + \alpha_1 g_{1it+1} + \alpha_3 g_{3it+1} + \phi_t + \Psi g_{1it+1} d_{t+1} + \varphi g_{3it+1} d_{t+1}]$$

The difference in difference estimate is then the marginal effect associated with the estimate of $\widetilde{\Psi}$.

3. Data

The data used in this report comes from the Quarterly Labour Force Survey (QLFS). The QLFS is a panel of households and individuals. Individuals are interviewed each quarter for five quarters. Information on their labour market status is collected each quarter but wage information is only collected at the first and last interview (i.e. five quarters apart). The empirical methodology we employ to examine job retention rates requires that we observe an individuals wage and then their employment status in subsequent quarters. We require this for the period spanning the up-rating of the minimum wage in October 2003 but also for a period where there is no change in the NMW. In order to fit this data requirement we examine six month employment retention rates. For the period spanning the October 2003 up-rating in the NMW we examine employment transitions between April-September 2003 and October 2003-March 2004. For example, we observe someone interviewed in May 2003 and then again six months later in November 2003. For the period with no change in the NMW we examine transitions between October 2002 – March 2003 and April-September 2003.

Periods used to examine the October 2003 up-rating

	t	t+1
Pre NMW period	Oct 02 – Mar 03	Apr 03 – Sep 03
NMW period	Apr – Sep 03	Oct 03 – Mar 04

Let us turn to the wage measures. Individuals are asked about their most recent earnings and their usual number of hours worked. An estimate of weekly earnings is divided by weekly hours to compute an hourly wage, called HOURPAY. However, there is good reason to think there is a large amount of measurement error in HOURPAY (Dickens and Manning, 2004). Given this, identification of individuals into the treatment and control groups is likely to be problematic. We shall turn to this below. In March 1999 (one month before the minimum wage was introduced) changes were made to the LFS such that workers are now asked if they are paid by the hour and, if so, their hourly rate: this variable is denoted by HRRATE. We think that this provides a much more accurate picture of the distribution of hourly wages and is likely to be a better indicator of whether someone belongs to the

treatment or control group. However, there are a number of problems with this measure. Firstly, it is only recorded for those workers paid by the hour, which means employment effects can only be estimated for these workers. This is not a problem if the employment impact on the treated group is the same whether or not they are paid by the hour. Secondly, because this wage measure has only been recorded since March 1999 it cannot be used to examine the introduction of the NMW, only the subsequent up-ratings.

4. Impact on Wages of the October 2003 Up-Rating

a. Which hourly wage measure is suitable?

If we expect the NMW to have an effect on employment transitions then this must operate through the individual's wage. So first, we must check that increases in the NMW do actually increase individual's wages. As we mentioned above, we suspect that the commonly used derived hourly wage measure in the Labour Force Survey, HOURPAY, is subject to severe measurement error. This has resulted in overestimates of the proportion of workers low paid in the UK, and hence the likely beneficiaries of the minimum wage. We also suspect that it may have resulted in miss classification of workers into treatment and control groups in previous work of this kind.

In order to examine this let us look at the changing wage distribution over the period of the October 2003 up-rating of the NMW from £4.20 to £4.50 for adults. If the NMW legislation is working correctly and the wage measure is accurately recording wage changes then we would expect to see all workers paid below £4.50 just prior to October 2003 to have their wage rate increased to £4.50 after October 2003. Figure 1a presents the proportional change in the hourly wage by percentile between September 2003 and November 2003 for the derived pay measure *HOURPAY*. Also reported is the compliance change, the increase in wages required at each percentile to comply with the new NMW of £4.50. Using this measure, we estimate that the bottom 5% of the wage distribution are paid below £4.50 in September 2003 and that wages need to rise between 2% and 50% to comply with the new NMW. However, wages in this bottom 5% barely change at all over this up-rating period, and actually decline slightly at the 1st percentile. This suggests that either nobody is being

paid the NMW or that this HOURPAY measure is not sufficiently measuring hourly wages. We suspect it is the latter.

Figure 1b presents the same information for the wage distribution measured by *HRRATE*. This shows that about 7% of workers are paid below £4.50 in September 2003. Note this is 7% of *hourly paid* workers which is likely to be an over-estimate of the proportion of all workers.[3] Apart from the bottom 2% of workers, all of these experience wage rises of between 2% and 7% that take their pay up to the new NMW of £4.50. This suggests that there is a high degree of compliance with the NMW legislation as most of the workers paid below are raised to the new minimum when it comes into force. Also, it appears that the *HRRATE* measure is an accurate measure of the hourly wage distribution.

The worry here is that the derived *HOURPAY* measure is just not capturing these changes in the bottom of the wage distribution due to measurement error in this variable. Use of this variable to define our treatment and control groups is likely to mean that we do not identify these groups properly. Indeed, we have seen that our treatment group do not experience any wage increases at all at up-rating, so it is unlikely that we will find any employment effects using this measure.

Figure A1a-A1d in appendix 1 present the same changes in the wage distribution for the HOURPAY measure for the introduction and the other up-ratings in the NMW. The same picture emerges. There is a large tail in the wage distribution below the NMW but little in the way of wage increases when the NMW rises. Only the introduction in April 1999 and the October 2001 up-rating appear to influence wages at the bottom of this distribution, but by nowhere near enough to raise these wages to the NMW. Figure A2a-A2d reports the same information for the HRRATE measure. Here we see changes in the bottom of the wage distribution that better comply with the NMW which makes us more confident in this wage measure.

We are confident that *HRRATE* is a much better measure of the hourly wage distribution than *HOURPAY*. Given that the latter measure seems to contain so much measurement error we believe that it does not provide a good indicator of individuals in our treatment and control groups and that its use in such a methodology is likely to be problematic. Below we report employment estimates using both hourly wage measures for comparative purposes.

[3] See Dickens and Manning (2004) for evidence of this.

b. How many workers are affected by the NMW

The problems experienced with the derived hourly pay variable in the QLFS, has resulted in over-estimates of the proportion of workers whose wage is affected by the introduction and subsequent up-ratings of the NMW. The Low Pay Commission commissioned work to investigate this issue (Skinner and Beissel, 2001; Dickens and Manning, 2002). The ONS preferred methodology takes estimates of the wage distribution from both the QLFS and the New Earnings Survey (now ASHE). Dickens and Manning (2002) use information on the hourly rate from the QLFS to estimate the wage distribution but have to re-weight this to account for those individuals not paid by the hour. They use a number of different methods of which more details can be found in their report.

We report here estimates using their *propensity score approach* and *wage re-weighting approach*. Table 2 presents the proportion of adult workers paid the NMW and the proportion below the NMW for the introduction in 1999 through to the last up-rating in 2003. Results are presented for the un-weighted hourly rate distribution, for the propensity score weighted and for the wage re-weighted estimates. Note that the un-weighted distribution over-estimates the proportion of affected workers, since hourly pay workers are more likely to be low paid. The two re-weighting methodologies give similar results, reducing the raw proportion at the NMW by about a half. For example, some 7.09% of workers are estimated to be paid at the NMW of £4.50 in Nov 2003 from the un-weighted hourly rate variable. The propensity score estimates suggest this proportion is more like 3.4% and the wage re-weighting 3.65%. These estimates are far below the estimated proportions computed by the LPC in the last report; 5.2%-7.4% of adult workers. Part of the reason for the difference arises from the fact that the LPC are estimating these proportions at least a year in advance. A difficulty then arises in estimating what will happen to the wage distribution over this year. These estimates from Table 2 are based on data from November 2003, when the new NMW was in place.

Nevertheless, the initial suggestion that when the NMW was raised to £4.50 an hour it would bite much further into the pay distribution than it had done previously appears to be untrue. In fact, examination of Table 2 shows that the proportion of workers affected by the NMW in October2003 is less than at introduction and than the other large up-rating in October 2001. Given that previous research could find no dis-employment effects from the NMW set at these levels we may not expect to find them here.

Tables 3a and 3b then present the same information for men and women separately. There is a large difference in the impact of the NMW on the wages of men and women. Women are three to four times more likely to have their pay affected by the NMW than men. For example, we estimate that some 5.4-5.6% of women are at the NMW of £4.50 in November 2003, compared to about 1.5-1.6% of men. Again we see that the *bite* of the NMW in a historical context is not so large for both men and women. Indeed, introduction appeared to affect somewhere between 8.5-10% of women. As such, we may expect employment effects to be small but it seems likely that they are more likely to be evident among women than men.

Table 4 then presents results for young workers (18-21 year olds). Low pay is more prevalent among younger workers but the NMW is set lower accordingly. This means that a similar proportion of young workers as adults are affected by the NMW. We find that about 3.7-4.3% of young workers are paid the NMW of £3.80 in November 2003. Note that the re-weighting process makes less difference to the original estimates based on those reporting an hourly rate. This is because more young workers are paid by the hour.

c. Wage spillovers and anticipatory wage changes

One concern about the NMW is that it will have significant spillover effects on workers paid above the minimum. If this is the case then any employment effects may be larger than anticipated. Dickens and Manning (2002) report very little evidence of spillovers from the minimum wage. We can see from Figure 2a that wages below the NMW of £4.50 are raised up to it between Sep-Nov 2003 but there is little evidence that wages above the NMW are increasing very much, at least at this time. In addition, evidence from the care home industry (Machin, Manning and Rahman, 2003) shows very little in the way of wage spillovers, even though the spike in the wage distribution at the NMW is almost 30%!

However, one concern is that wages may be raised to the NMW prior to the actual date of up-rating, and spillover effects may be evident at this time. Many companies have their pay rounds in January or at the start of the tax year in April. Qualitative survey evidence from IDS (2004) suggests that many companies adjust to the NMW at these dates rather than in October of each year. If this is the case then our employment methodology may not capture fully the treated group since their pay has already been raised to the NMW prior to October 2003. We can examine this issue using the QLFS. Figure 2 presents the proportion of workers paid £4.50 an hour or below from January 2002 to May 2004 (the

minimum rate that was in force from October 2003). Also, presented is the proportion paid £4.20 an hour (the rate in force from October 2002-October 2003). If there are anticipatory effects then we would expect to see an increase in the percent of workers paid £4.50 prior to October 2003. This is not evident from this figure, even at the time of pay rounds in January and April 2003. Note that the rate of £4.50 was confirmed by the government in March 2003 so we would be most likely to see any anticipatory effects around this time. We do see a large increase in the spike at £4.50 in October 2003 as we would expect if firms wait until as late a possible to comply with the legislation.

We do observe a declining proportion of workers paid below £4.50 over this whole time period as there is general wage growth in the economy. This proportion does fall quite sharply between March and April 2003, but as we have seen there was no corresponding increase in the proportion at £4.50. But it could be the case that as workers are moved to £4.50, those on £4.50 are moved above. We examine this in Figure 3 which presents the change in the hourly rate at each percentile between March and May 2003. Also, presented is the change required to raise each percentile up to the newly announced NMW. We do observe some wage changes at the lower percentiles, with about two percentiles being raised up to £4.50. We also see that among the percentiles at and just above £4.50 there are some wage increases. This suggests that there may be some small anticipatory effects of the minimum and small spillovers at this time.[4] The problem is attributing these changes to the NMW. We know that hourly rates tend to be paid at round numbers such as £4.50 and so these changes at a time of pay rounds may have little to do with the NMW.

5. Impact on Employment

a. Employment effects of the October 2003 up-rating

Let us now turn to examine the impact of the recent up-rating in the NMW on employment. We begin by estimating the impact on exits from employment using the difference in difference technique outlined above. Table 5 presents results using this methodology where the *treatment* group are those workers between the old and new NMW and the *control* group are those whose wage is between the new NMW and 10% above this. (i.e. we set c=0.1). We

[4] We also examined wage changes over the January pay round but could find no pay increases around the new NMW. Figure A3 in the appendix reports evidence on this.

present results for both the raw difference in difference model and the logit model with a set of individual and job controls. We include indicators of age group, qualifications, region of residence, ethnic origin, full/part time job, temporary job, public sector job, month of interview and also a cubic in the hourly rate. Results are presented for all adult workers, female adults, male adults and all youths.[5] We present estimates where we have used hourly rate to define the treatment and control groups and where we use derived hourly pay to define these groups. Reported in the table are marginal effects.

We find that the up-rating of the NMW in October 2003 had a negative effect on employment retention for all adult workers, when we define the groups by the hourly rate. The estimated marginal effect is -0.022. This says that the up-rating in the NMW reduced employment retention among the affected group by 2.2 percentage points. To give some flavour of the size of this response, the employment retention rate for the treated group prior to the up-rating was about 93%, so the policy is estimated to reduce this to about 91%. But note that this is not significantly different from zero at standard levels of statistical significance (t-stat of 1.12). When we add control variables the marginal effect declines somewhat (-0.015) and the t-statistic falls to 0.94.

Turning to the estimates based upon the derived hourly pay measure for wages. We find that the estimated marginal effects are close to zero and that the t-statistics are very low. These results are very similar to those found by Stewart (2003) who used this wage measure and methodology to examine the introduction of the NMW. We are finding somewhat stronger effects when using the hourly rate measure, albeit not significantly different from zero.

To check that the differences in results between the derived hourly pay and hourly rate measure are not being driven by samples differences we also report estimates based on the derived measure but for the sample of those that report and hourly rate. We find little difference in the results with marginal effects close to zero and very small t-statistics.

Table 5 also reports estimates for men and women and for young workers. Some differences do emerge. For men, who remember are much less likely to be affected by the NMW, the marginal employment effects are quite large at -0.081 using our preferred hourly rate definition. But the t-statistic is quite low at 1.36. When we add controls the marginal effect fall to -0.034 and the t-stat falls to 0.88, so we cannot reject the hypothesis that these estimates are equal to zero. With the derived measure we find smaller employment effects

[5] Sample numbers mean that we cannot estimate separate results for male and female youths.

for men and again the t-stats are low at around unity. The results for women and young workers are all very close to zero and in both cases we cannot reject the null that the estimated effects are zero for any specification.

The model presented in Table 5 assumes that all workers in the *treated* group face the same change in employment probability. However, one can imagine that those workers whose pay is further below the new NMW may well face a higher probability of losing their job. One way to allow for this is to replace the treatment dummy in the estimated equation with a variable that measures how much an individuals wage needs to rise to comply with the new NMW. This should capture the differential treatment intensity. We specify this *wage gap* measure in the following way:

$$\text{wage gap}_{\text{it}} = \begin{matrix} \min_{t+1} - w_{it} & \text{if} \quad \min_t \le w_{it} < \min_{t+1} \\ 0 & \text{otherwise} \end{matrix}$$

We specify this model in the same way as the difference in differences model but the dummy for the treatment group g_{1it} is replace with the *wage gap* measure. Estimates are reported in Table 6 for the hourly rate and derived hourly pay measures once more. For all adults we find a marginal effect of -0.093. This has a t-statistic of 1.5 which suggests this specification is more significant than the standard difference in difference model. This marginal effect signifies the change in employment retention probability per unit change in the wage gap. Given that the NMW was raised from £4.20 to £4.50 then the maximum wage gap is 0.30. This would result in a fall in employment retention of about 3%, similar to the size of the impact from the diff in diff model in Table 5. But it is important to remember that this is not significantly different from zero.

The estimated effects all follow a similar pattern to those in Table 5. We find the effects reduced with the introduction of control variables. The estimates from derived hourly pay are smaller and less significant. Also, we find the effects for females and young workers insignificant and close to zero, but for males slightly larger and with higher t-values.

Robustness checks

None of the results reported so far are able to convince us that there are significant employment effects (positive or negative) from this up-rating in the minimum wage. The results for men are the strongest but even these we are not able to conclude are statistically different from zero. But one problem with this whole methodology is that the choice of the

control group, and to some extent the treatment group, is somewhat arbitrary. We would like our control group to be as similar as possible to the treatment group but to be unaffected by the policy change. But the choice of a group whose wage is 10% above the minimum is an arbitrary one, which may well have a large impact on the estimated results especially when working with limited sample numbers. Furthermore, we saw above that there may well be some small spillovers of the NMW onto wages further up the pay distribution. If this is the case then taking the group just above the NMW is likely to bias the results downwards.

In order to investigate the impact of the choice of control group on these estimates we examine the employment loss probability for workers at different hourly wages, both before and after the up-rating in the NMW. Figure 4 plots the employment loss probability for the hourly rate distribution between £4.20 and £7.00 an hour. We plot vertical lines at the old NMW of £4.20, the new NMW of £4.50 and the upper threshold of the control group (10% above the NMW which is approximately £4.90). The striking thing about this figure is that employment loss varies quite widely at different wage levels. While there is a general pattern that those on lower hourly wages tend to exit more frequently these lines are not smooth, suggesting a large amount of variation between wages quite close to each other. Secondly, this variation is different for the pre and post NMW periods.

The difference in difference estimates presented above are essentially derived from taking the change in average employment loss in the treated group from that change in the control group. But this figure shows that there is a large variation in that change both within the treatment and control groups, and that the choice of the control group will affect the results. For example, we see that employment loss rises for those in the treated group who initial wage is £4.20 but declines for those with wages at £4.30. Similarly, employment loss declines for those at the lower end of our control group (£4.50) but rises for those at the top end. And if we were to take as our control group those in a band just above £4.90 then since their employment loss has clearly risen we will more likely estimate a positive effect of the NMW.

Figures 5a and 5b present the same information for men and women respectively. For men we see a large increase in the employment loss probability post NMW in the treatment group, apart from those men paid close to the new NMW already. The change in employment loss for the control group is close to zero, which explains the estimated negative effects above (but remember these were not significantly different from zero). Note also that since fewer men are affected by the NMW that the sample numbers are small, leading to a high degree of variation from one pay band to the next.

For women, there is less of a clear change in employment loss for the treatment group. But we can see that if we were to pick a control group further up the wage distribution then we may well find positive employment effects, since employment loss rises for those with wages between about £4.80 and £5.20. The difficulty with this approach to estimating employment effects is that it is very hard to know which wage range provides a good control group to provide estimates of what would have happened in the treatment group in the absence of the minimum wage. Given the evidence of some spillover effects above it may well be that a better control group are those with wages a little way above the new NMW. But we see from these figures that the choice of group is likely to have a large impact on the estimated results.

To demonstrate this Tables 7 and 8 present marginal employment retention estimates using two alternative control groups; those workers whose wage is initially between 5 % and 15% above the new NMW and those whose wage is initially between 10% and 20% above the new minimum. This changes the results somewhat. For all adults and young workers the results are essentially the same; close to zero and insignificant. But for male and female adults stronger differences begin to emerge. Using the 5%-15% above control group, in the raw difference in difference specification with the hourly rate we find a large negative effect for men, which is significant at the 5% level. This effect is reduced by the inclusion of control variables and the t-statistic falls to 1.85. For women we find a positive impact of the NMW on employment retention with a t-stat of about 1.5, and this holds up to the inclusion of control variables.

If we use the 10%-20% above control group then we find a significant positive effect for women and a significant negative effect for men. The latter is largely knocked out by the inclusion of controls but not among women where the marginal effect of 0.021 remains significant at the 5% level. The conclusion here would be that the NMW has reduced employment loss for women, with no effects for men. But one needs to be a bit wary of drawing too strong a conclusion here since we have already seen that the choice of control group can have large effects on our estimates.

b. The Effect of the October 2003 increase on employment entry

As we discussed above most of the work using this technique focuses on job exits arising form changes in the minimum wage. However, it may also be the case that job entry is affected by increases in the NMW. In this section we examine job entry using a similar

methodology to that for job loss. We analyse where in the wage distribution new hires are made and compare changes in hires among a treated group with a comparison group. The treatment group are now defined to be those at or below the NMW, while the control group are (initially) those in the 10% band above the NMW.

Table 9 presents the results for this control group on job entry. Again we present marginal effects where the groups are defined by the hourly rate and the derived hourly pay variables. We report estimates for all adults, male and female adults and all young workers. In none of the specifications to any significant results emerge. Most of the estimated marginal effects are small and the t-statistics are suggestive of insignificant effects. The strongest results are for women, where the marginal effect is estimated to be -0.018, which suggests that job hires are 1.8% points lower in the treated group due to the NMW, but the t-stat is only 1.12.

Of course, these estimates are subject to the same criticism as above that the choice of treatment and control group may impact upon the results. Figure 6 presents the job entry probability at different wages both before and after the NMW (this can be compared with figure 4 on job loss). We do see that both job entry and the change in job entry at different wages varies substantially. Even within the treated group we see that those on lower wages experience a fall in entry while those on higher wages a rise. The results for males and females (Figures 7a and 7b) are similar. The figure for males seems to contain a large amount of noise in it, perhaps because sample numbers are lower here, whereas the figures for women look more like the aggregate numbers in Figure 6. Tables 10 and 11 again present employment entry estimates with two different control groups; those 5-15% above the NMW and those 10-20% above. But in none to the estimated specifications do we find significant effects of the impact of the NMW on job entry.

6. Conclusions

Since introducing the national minimum wage in 1999 the government has progressively increased the real value of both the adult and youth "development" rates. This policy therefore provides a good quasi-experiment for examining the employment impact of minimum wages. That is, given large increases in the minimum wage one would expect to see a more marked response in terms of employment change.

In common with previous studies (Stewart 2003, 2004) the results presented in this report for the 2003 up-rating reveal no clear, statistically significant evidence of employment losses, measured either in terms of employment outflows or inflows. In part, this may be because the coverage of the minimum wage is still limited, with adjusted estimates of the incidence of affected workers for the 2003 up-rating suggesting some 3.5% - 4.0% of adults affected. However, it must be noted that while a smaller *number* of workers may be affected by the NMW the rate at which they may suffer employment losses is not automatically lower. Instead what this means is that *economy-wide* estimates of potential employment effects are necessarily lower (i.e. changes at the microeconomic level must be extrapolated to a smaller group at the macroeconomic level).

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Table 1: Estimated Beneficiaries of Recommended Increases in the National Minimum Wage

	October 2001	*October 2002*	*October 2003*	*October 2004*
Based on Prices				
Adults 22+				
Number	1.4 million	900,000	1.5 million	2.3 million
Proportion Effected (%)	6.1	3.9	6.5	9.7
Young people				
Number	90,000	90,000	100,000	150,000
Proportion Effected (%)	5.3	4.9	7.4	11.3
Based on Earnings				
Adults 22+				
Number	1 million	900,000	1.2 million	1.6 million
Proportion Effected (%)	4.4	3.9	5.2	6.9
Young people				
Number	90,000	90,000	80,000	100,000
Proportion Effected (%)	5.3	4	5.2	6.9
Level of National Minimum Wage				
Adult Rate	£4.10	£4.20	£4.50	£4.85
Youth Rate	£3.50	£3.60	£3.80	£4.10

Source: The Fourth Report of the Low Pay Commission, Table 2.1 (p12) and Table 6.2 (p218). Information on wage rates taken from "History of the National Minimum Wage", DTI website.

Notes
1. Estimates of Adult beneficiaries rounded to the nearest 100,000 and youth beneficiaries to the nearest 10,000.

Table 2: Percent Affected by NMW - Adult Workers

		Actual Hourly Rate		Propensity Score		Wage Weighted	
	NMW Rate	% below	% at NMW	% below	% at NMW	% below	% at NMW
Mar-99		11.50	1.88	8.34	0.86	6.42	0.92
May-99	£3.60	2.21	***9.94***	1.37	***5.67***	1.16	***5.24***
Sep-00		4.58	0.84	2.92	0.57	2.72	0.51
Nov-00	£3.70	2.07	***3.97***	1.61	***2.27***	1.29	***1.95***
Sep-01		9.48	1.96	4.93	1.10	5.20	1.12
Nov-01	£4.10	2.70	***8.42***	1.85	***4.72***	1.59	**4.87**
Sep-02		6.08	2.44	3.24	1.37	3.40	1.48
Nov-02	£4.20	1.74	***5.52***	1.19	***2.91***	1.06	***2.82***
Sep-03		7.62	2.37	3.89	1.27	3.97	1.34
Nov-03	£4.50	2.24	***7.09***	1.11	***3.40***	1.25	***3.65***
May-04	£4.85	12.61	0.92	5.79	0.44	6.69	0.47

% at NMW: % of employees within 1 pence of NMW rate

Table 3a: Percent Affected by NMW - Females

		Actual Hourly Rate		Propensity Score		Wage Weighted	
	NMW Rate	% below	% at NMW	% below	% at NMW	% below	% at NMW
Mar-99		16.13	3.20	9.71	1.94	11.19	2.00
May-99	£3.60	3.02	***14.84***	1.85	***8.47***	2.11	***10.14***
Sep-00		6.03	1.15	4.09	0.84	3.99	0.76
Nov-00	£3.70	2.49	***5.35***	1.6	***3.41***	1.43	***2.87***
Sep-01		12.39	2.90	7.37	1.89	7.38	1.86
Nov-01	£4.10	2.71	***11.85***	2.05	***7.65***	1.67	***7.76***
Sep-02		8.12	3.41	4.64	2.2	4.9	2.31
Nov-02	£4.20	2.33	***6.5***	1.72	***3.61***	1.65	***3.89***
Sep-03		9.71	3.42	5.55	2.04	6.02	2.3
Nov-03	£4.50	2.4	***9.31***	1.51	***5.58***	1.52	***5.39***
May-04	£4.85	16	0.89	8.69	0.53	9.43	0.51

% at NMW: % of employees within 1 pence of NMW rate

Table 3b: Percent Affected by NMW - Males

		Actual Hourly Rate		Propensity Score		Wage Weighted	
	NMW Rate	**% below**	**% at NMW**	**% below**	**% at NMW**	**% below**	**% at NMW**
Mar-99		6.60	0.48	7.46	0.17	3.21	0.19
May-99	£3.60	1.27	***4.22***	0.95	***3.19***	0.50	***1.87***
Sep-00		2.56	0.4	1.59	0.25	1.36	0.23
Nov-00	£3.70	1.45	***1.99***	1.62	***1.1***	1.13	***0.95***
Sep-01		5.22	0.59	2.51	0.3	2.76	0.28
Nov-01	£4.10	2.70	***3.56***	1.64	***1.72***	1.49	**1.66**
Sep-02		3.03	0.97	1.73	0.47	1.75	0.56
Nov-02	£4.20	0.84	***4.1***	0.62	***2.14***	0.44	***1.72***
Sep-03		4.74	0.92	2.23	0.52	1.95	0.39
Nov-03	£4.50	1.96	***3.8***	0.75	***1.47***	0.96	***1.64***
May-04	£4.85	7.95	0.96	3.1	0.35	3.71	0.43

% at NMW: % of employees within 1 pence of NMW rate

Table 4: Percent of Affected by NMW - Young Workers

		Actual Hourly Rate		Propensity Score		Wage Weighted	
	NMW Rate	% below	% at NMW	% below	% at NMW	% below	% at NMW
Mar-99		4.24	4.24	4.45	2.97	4.54	2.17
May-99	£3.00	1.29	***4.52***	1.41	***4.00***	1.29	***3.76***
Sep-00		4.32	2.47	3.88	2.47	3.7	2.25
Nov-00	£3.20	3.57	***4.37***	4	***4.46***	3.76	***4.12***
Sep-01		2.95	0.99	2.85	0.33	2.74	0.63
Nov-01	£3.50	4.25	***3.03***	5.25	***3.58***	3.63	**2.61**
Sep-02		6.54	0.47	7.17	0.35	6.65	0.33
Nov-02	£3.60	2.56	***2.56***	3.18	***1.37***	2.52	***1.06***
Sep-03		7	0	5.82	0	6.74	0
Nov-03	£3.80	1.84	***4.17***	1.9	***4.32***	1.92	***3.66***
May-04	£4.10	8.33	0.98	7.05	1.83	6.65	1.14

% at NMW: % of employees within 1 pence of NMW rate

Table 5: Difference in difference employment loss estimates of October 2003 uprating

	All Adults	Adult Females	Adult Males	All Youths
Raw linear difference in difference estimates				
Wage based on hourly rate	-0.022	-0.010	-0.081	0.044
	(1.12)	(0.50)	(1.36)	(0.41)
Wage based on derived hourly pay	0.004	0.014	-0.046	-0.104
	(0.35)	(1.01)	(1.11)	(0.78)
Wage based on derived hourly pay	-0.005	0.006	-0.065	0.016
Hourly Rate Sample only	(0.25)	(0.25)	(0.96)	(0.14)
Logit difference in difference estimates with controls				
Wage based on hourly rate	-0.015	-0.007	-0.034	0.036
	(0.94)	(0.41)	(0.88)	(0.36)
Wage based on derived hourly pay	0.003	0.009	-0.047	-0.112
	(0.24)	(0.77)	(1.21)	(0.88)
Wage based on derived hourly pay	-0.010	-0.004	-0.064	-0.032
Hourly Rate Sample only	(0.55)	(0.21)	(1.07)	(0.23)

Labour Force Survery Matched panels
T-stats in brackets

Table 6: Wage Gap Estimates of employment effect of October 2003 uprating

	All Adults	Adult Females	Adult Males	All Youths
Raw linear wage gap model				
Wage based on hourly rate	-0.093	-0.062	-0.197	-0.110
	(1.51)	(0.85)	(1.52)	(0.15)
Wage based on derived hourly pay	-0.002	0.043	-0.127	-0.053
	(0.03)	(0.56)	(1.05)	(0.08)
Logit Model with controls				
Wage based on hourly rate	-0.064	-0.041	-0.107	-0.308
	(1.27)	(0.72)	(1.05)	(0.40)
Wage based on derived hourly pay	-0.004	0.027	-0.111	-0.016
	(0.08)	(0.44)	(1.04)	(0.03)

Labour Force Survery Matched panels
T-stats in brackets

Table 7: Difference in difference employment loss estimates of October 2003 uprating - Control group 5-15% above NMW

	All Adults	Adult Females	Adult Males	All Youths
Raw linear difference in difference estimates				
Wage based on hourly rate	0.002	0.021	-0.203	0.044
	(0.14)	(1.41)	(2.39)	(0.48)
Wage based on derived hourly pay	0.003	0.013	-0.065	-0.009
	(0.21)	(1.07)	(1.44)	(0.10)
Logit difference in difference estimates with controls				
Wage based on hourly rate	0.005	0.017	-0.110	0.034
	(0.37)	(1.48)	(1.85)	(0.39)
Wage based on derived hourly pay	0.002	0.011	-0.058	-0.011
	(0.24)	(1.00)	(1.43)	(0.13)

Labour Force Survery Matched panels
T-stats in brackets

Table 8: Difference in difference employment loss estimates of October 2003 uprating - Control group 10-20% above NMW

	All Adults	Adult Females	Adult Males	All Youths
Raw linear difference in difference estimates				
Wage based on hourly rate	0.009	0.027	-0.115	0.041
	(0.66)	(1.90)	(1.90)	(0.45)
Wage based on derived hourly pay	-0.003	0.010	-0.108	0.029
	(0.27)	(0.76)	(1.85)	(0.35)
Logit difference in difference estimates with controls				
Wage based on hourly rate	0.009	0.021	-0.059	0.035
	(0.80)	(1.97)	(1.43)	(0.43)
Wage based on derived hourly pay	-0.002	0.009	-0.089	0.025
	(0.15)	(0.78)	(1.76)	(0.37)

Labour Force Survery Matched panels
T-stats in brackets

Table 9: Difference in difference employment entry estimates of October 2003 uprating

	All Adults	Adult Females	Adult Males	All Youths
Raw linear difference in difference estimates				
Wage based on hourly rate	-0.014	-0.018	0.004	0.470
	(1.01)	(1.12)	(0.11)	(1.63)
Wage based on derived hourly	-0.009	-0.011	-0.007	-0.006
	(0.98)	(0.90)	(0.53)	(0.07)
Logit difference in difference estimates with controls				
Wage based on hourly rate	-0.012	-0.016	0.012	0.475
	(0.94)	(1.16)	(0.38)	(1.62)
Wage based on derived hourly	-0.007	-0.009	-0.004	-0.018
	(0.84)	(0.89)	(0.35)	(0.21)

Labour Force Survery Matched panels
T-stats in brackets

Table 10: Difference in difference employment entry estimates of October 2003 uprating Control Group 5-15% above NMW

	All Adults	Adult Females	Adult Males	All Youths
Raw linear difference in difference estimates				
Wage based on hourly rate	-0.008	-0.010	0.000	0.119
	(0.65)	(0.71)	(0.00)	(0.58)
Wage based on derived hourly	-0.006	-0.007	-0.006	-0.072
	(0.73)	(0.67)	(0.48)	(0.45)
Logit difference in difference estimates with controls				
Wage based on hourly rate	-0.008	-0.010	0.000	0.150
	(0.71)	(0.79)	(0.01)	(0.69)
Wage based on derived hourly	-0.005	-0.007	-0.004	-0.085
	(0.71)	(0.70)	(0.34)	(1.18)

Labour Force Survery Matched panels
T-stats in brackets

Table 11: Difference in difference employment entry estimates of October 2003 uprating Control Group 10-20% above NMW

	All Adults	Adult Females	Adult Males	All Youths
Raw linear difference in difference estimates				
Wage based on hourly rate	-0.006	-0.009	0.004	0.100
	(0.45)	(0.61)	(0.14)	(0.57)
Wage based on derived hourly	-0.008	-0.010	-0.007	-0.069
	(1.16)	(1.05)	(0.68)	(0.89)
Logit difference in difference estimates with controls				
Wage based on hourly rate	-0.006	-0.008	0.000	0.101
	(0.54)	(0.61)	(0.02)	(0.55)
Wage based on derived hourly	-0.007	-0.010	-0.002	-0.082
	(1.08)	(1.15)	(0.25)	(1.15)

Figure 1a: Change in the distribution of derived hourly pay, Sep-Nov 2003

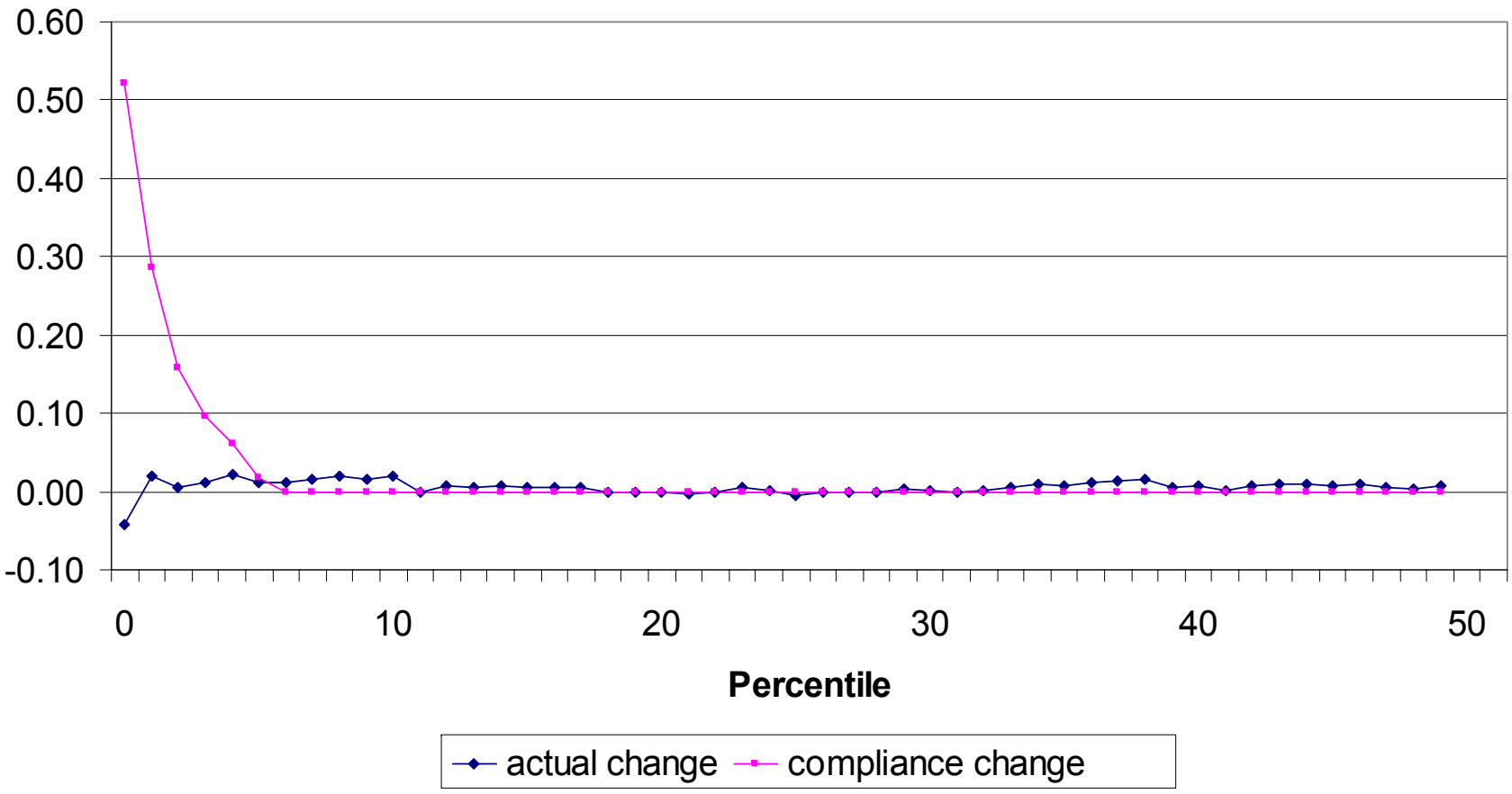


Figure 1b: Change in the distribution of hourly rate, Sep-Nov 2003

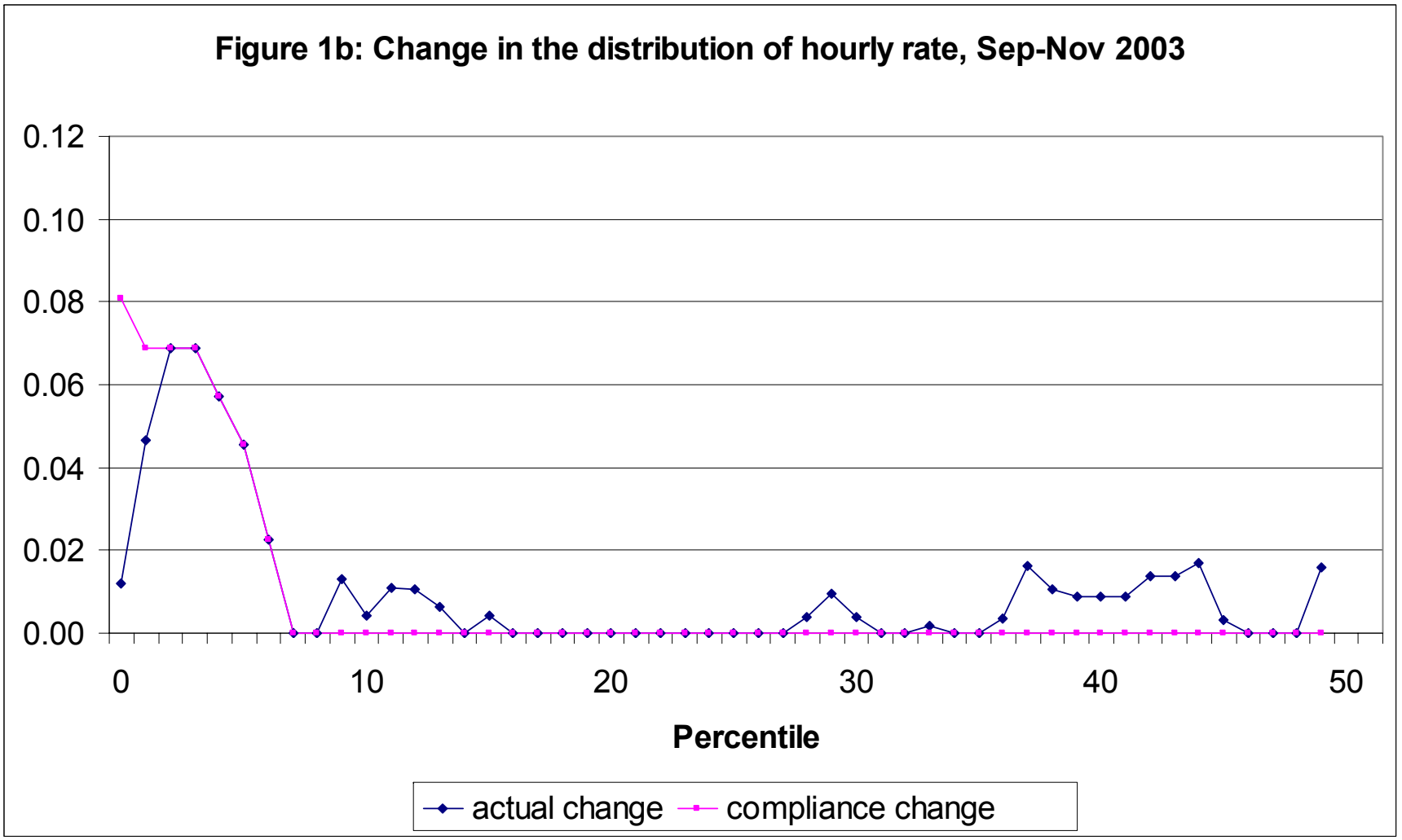


Figure 2: Adjustment to the NMW
Percent of workers at and below the NMW rate

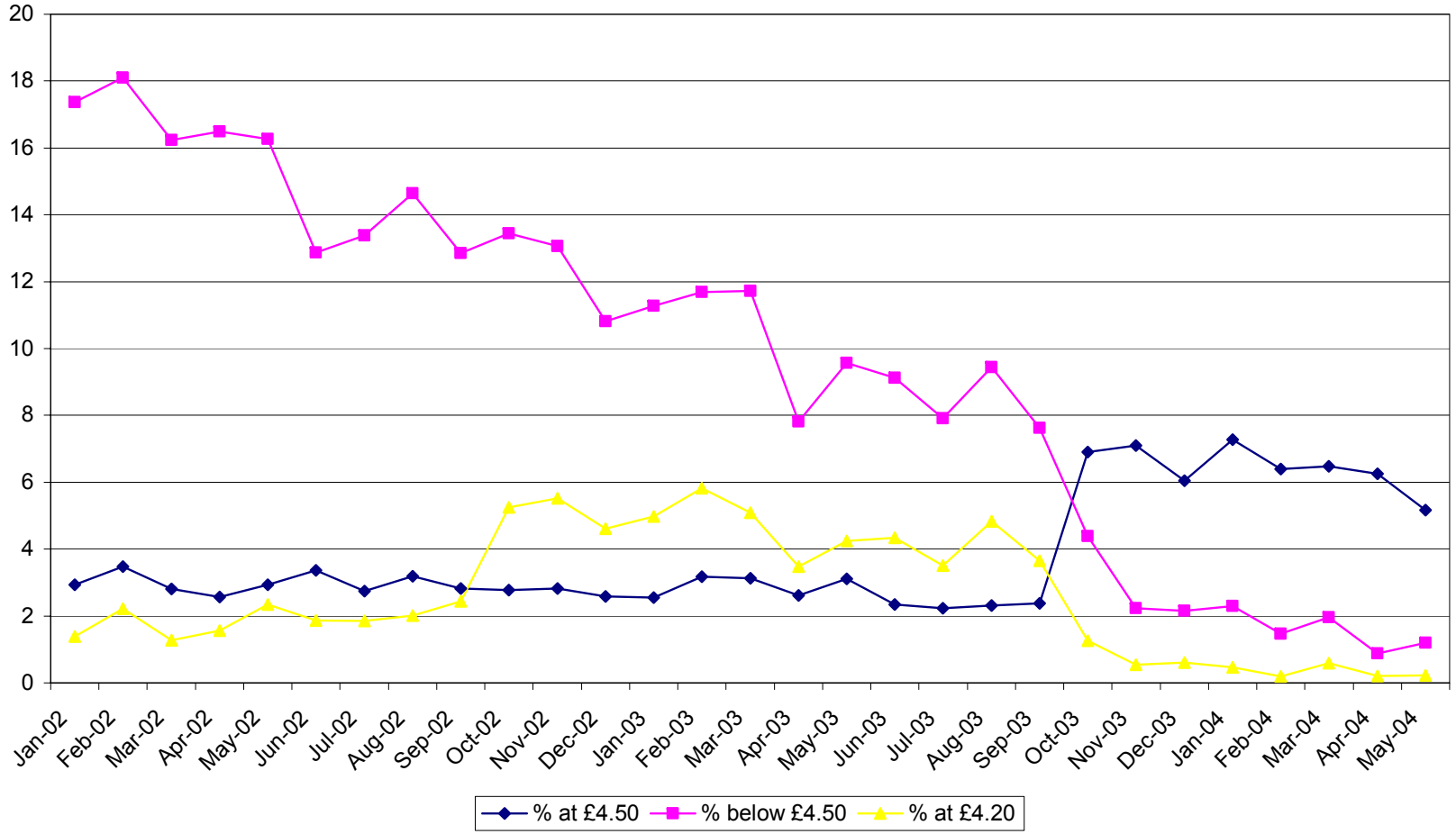


Figure 3: Change in the distribution of hourly rate, Mar-May 2003

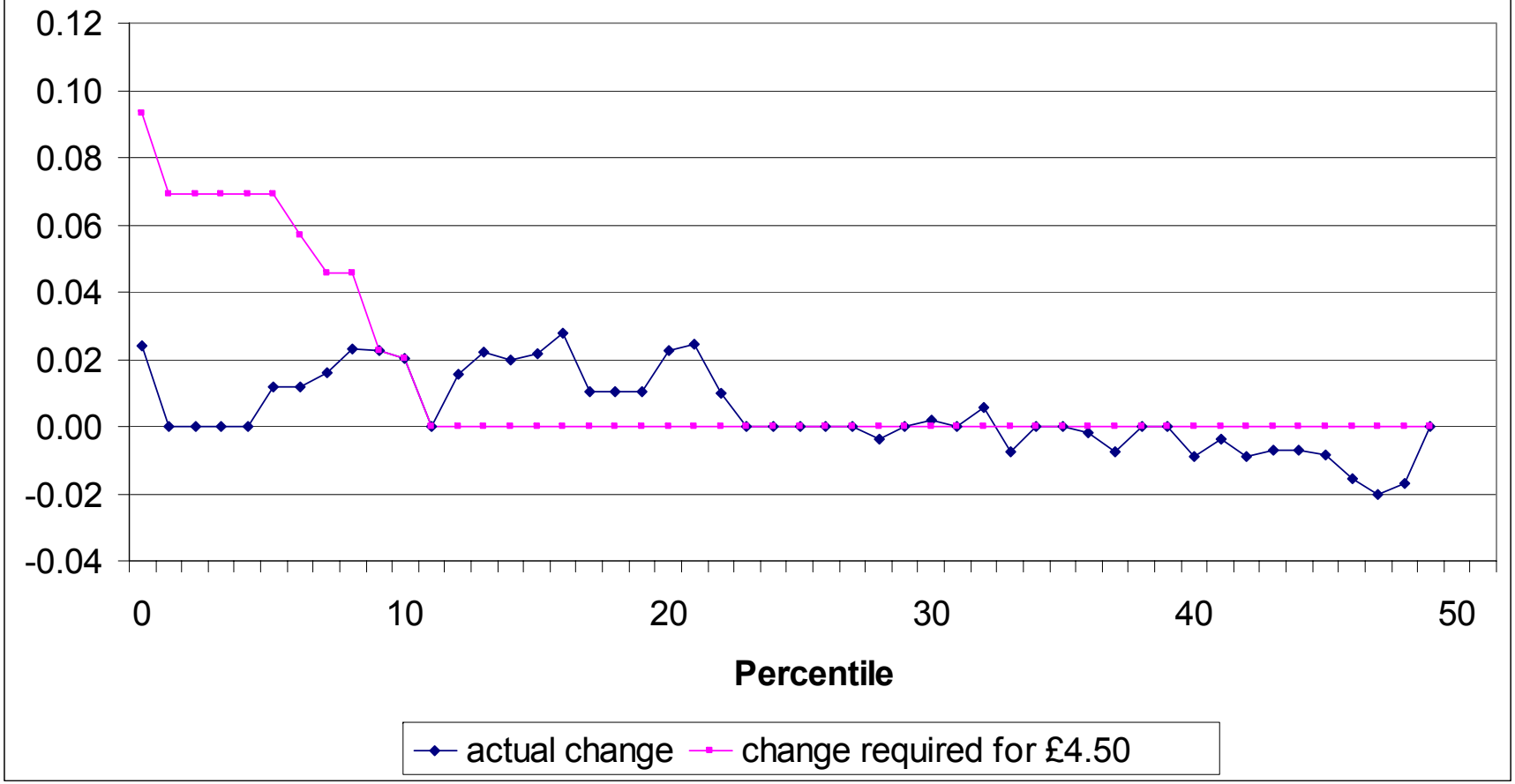
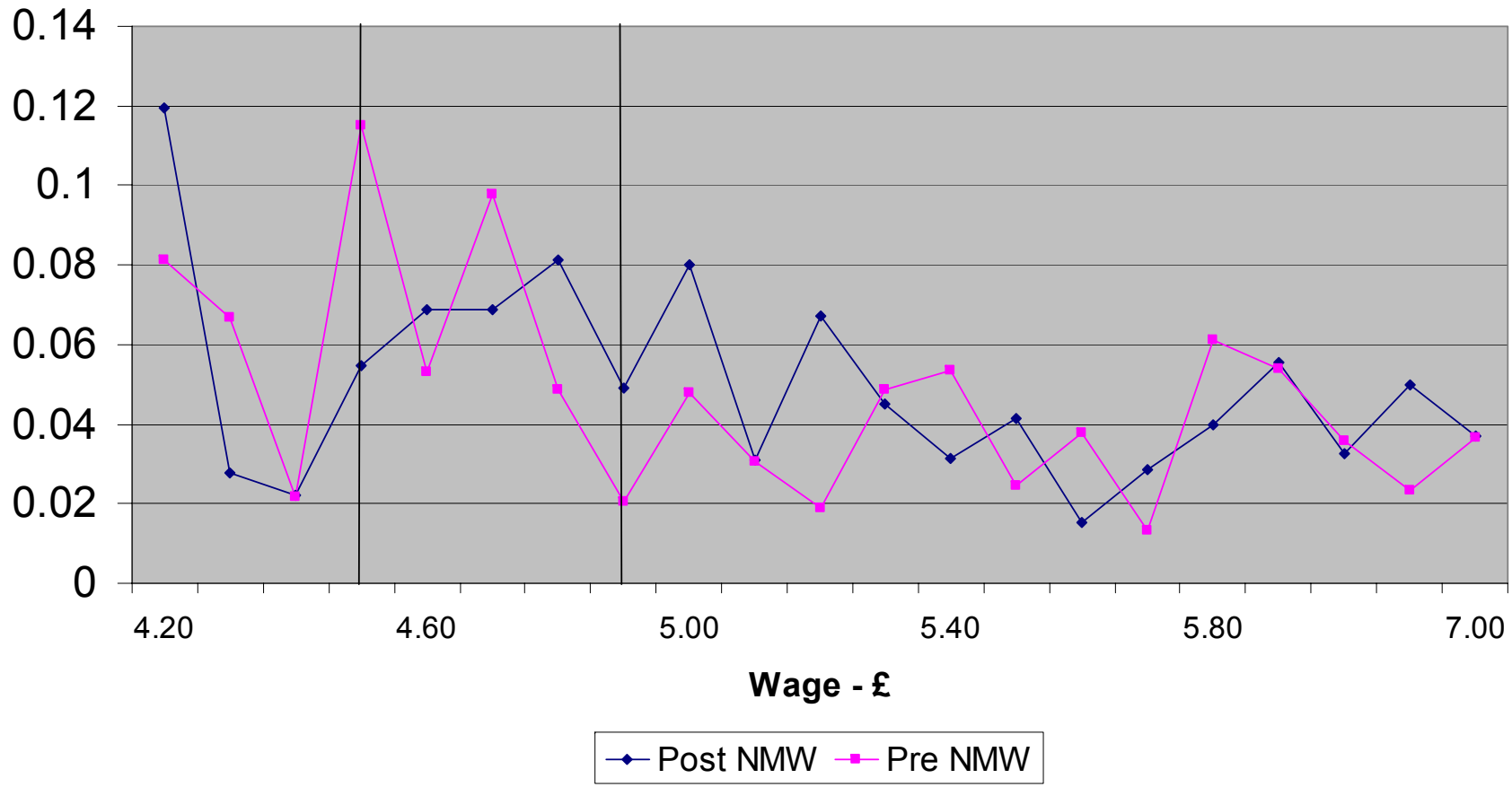


Figure 4: Employment Loss Probability, Oct 2003 Uprating



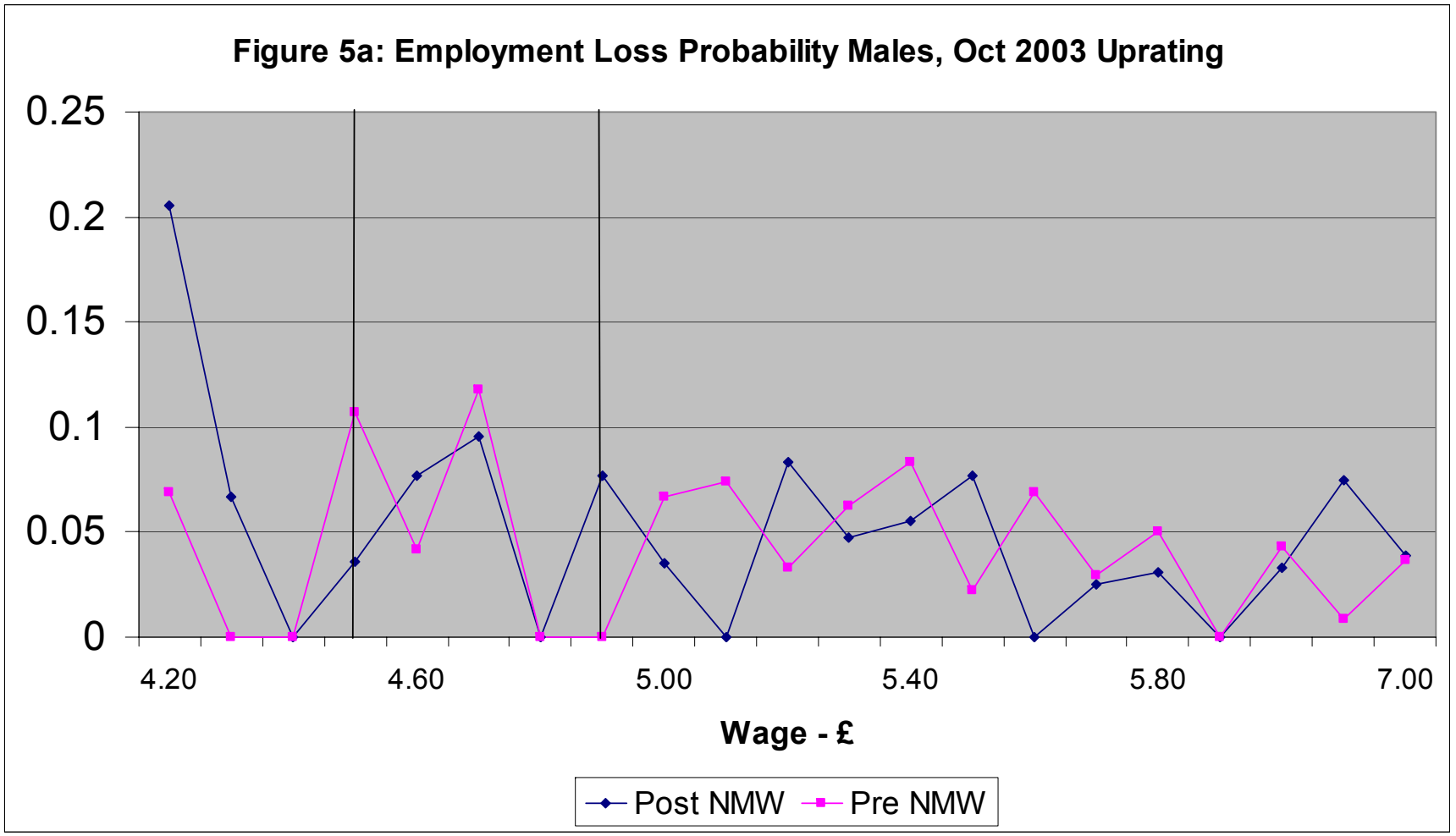


Figure 5a: Employment Loss Probability Males, Oct 2003 Uprating
0.25
0.2
0.15
0.1
0.05
0
4.20
4.60
5.00
5.40
5.80
7.00
Wage - £
Post NMW
Pre NMW

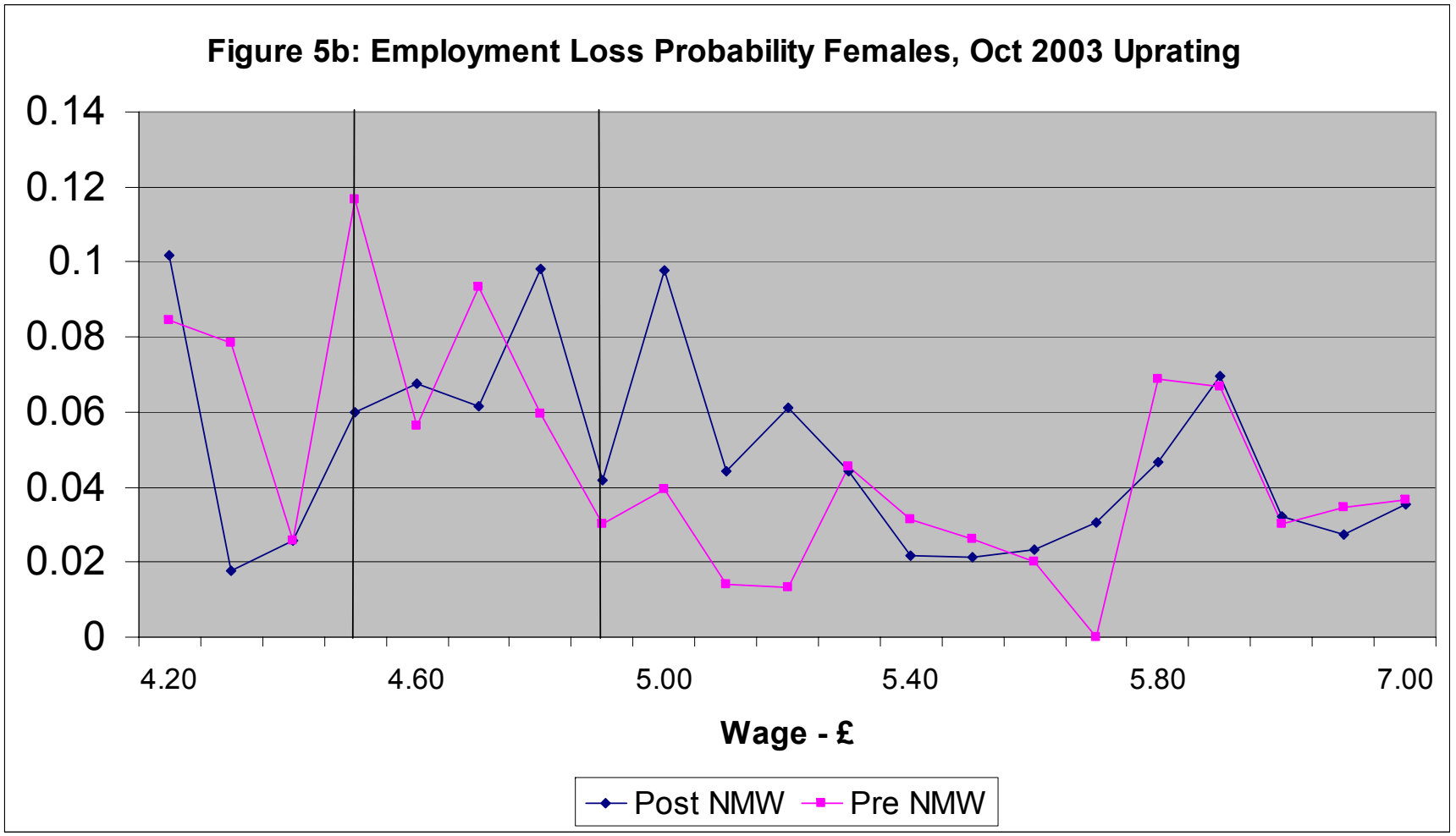


Figure 5b: Employment Loss Probability Females, Oct 2003 Uprating
0.14
0.12
0.1
0.08
0.06
0.04
0.02
0
4.20
4.60
5.00
5.40
5.80
7.00
Wage - £
Post NMW
Pre NMW

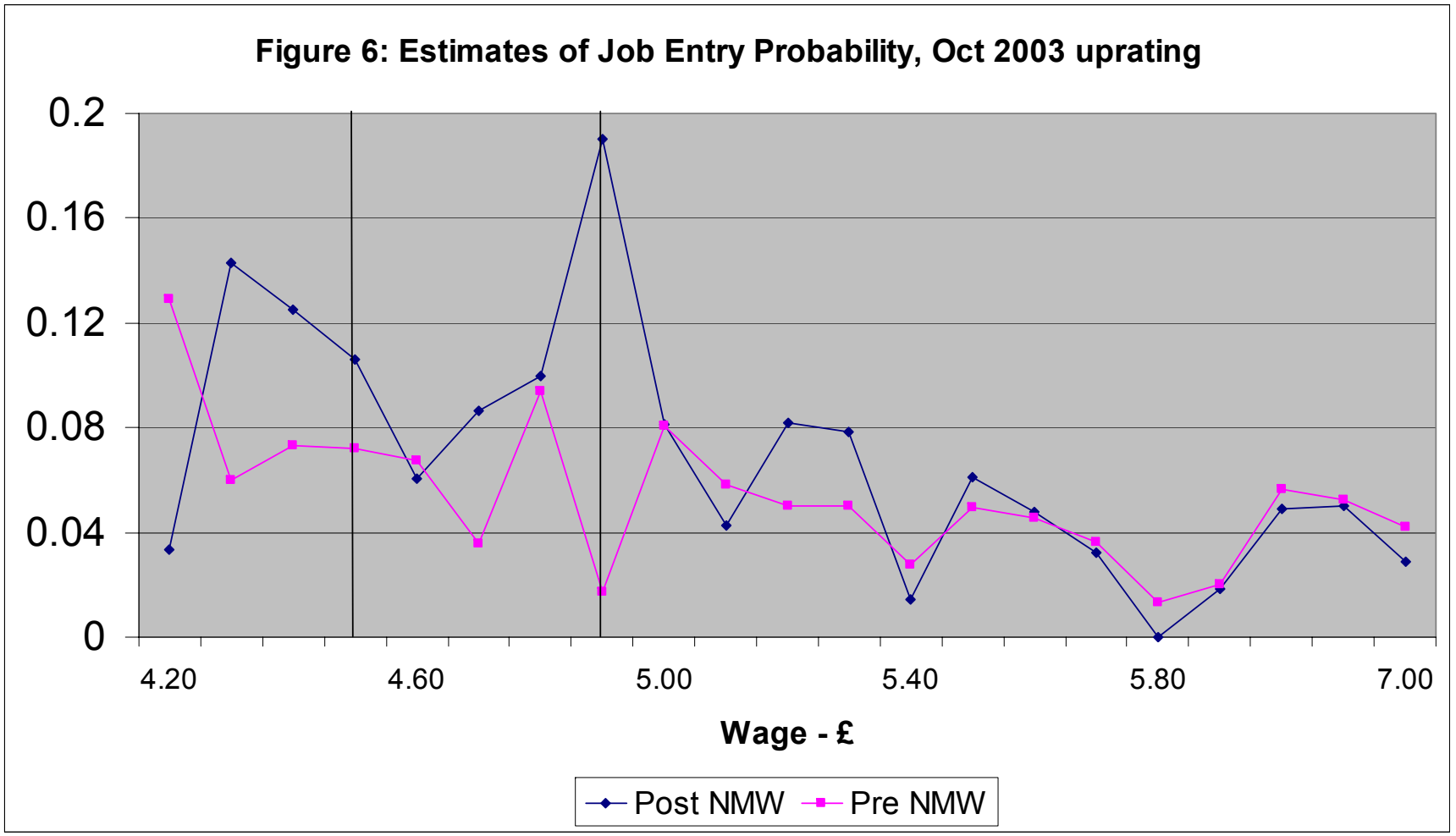


Figure 6: Estimates of Job Entry Probability, Oct 2003 uprating
0.2
0.16
0.12
0.08
0.04
0
4.20
4.60
5.00
5.40
5.80
7.00
Wage - £
Post NMW
Pre NMW

Figure 7a: Estimates of Job Entry Probability Males, Oct 2003 uprating

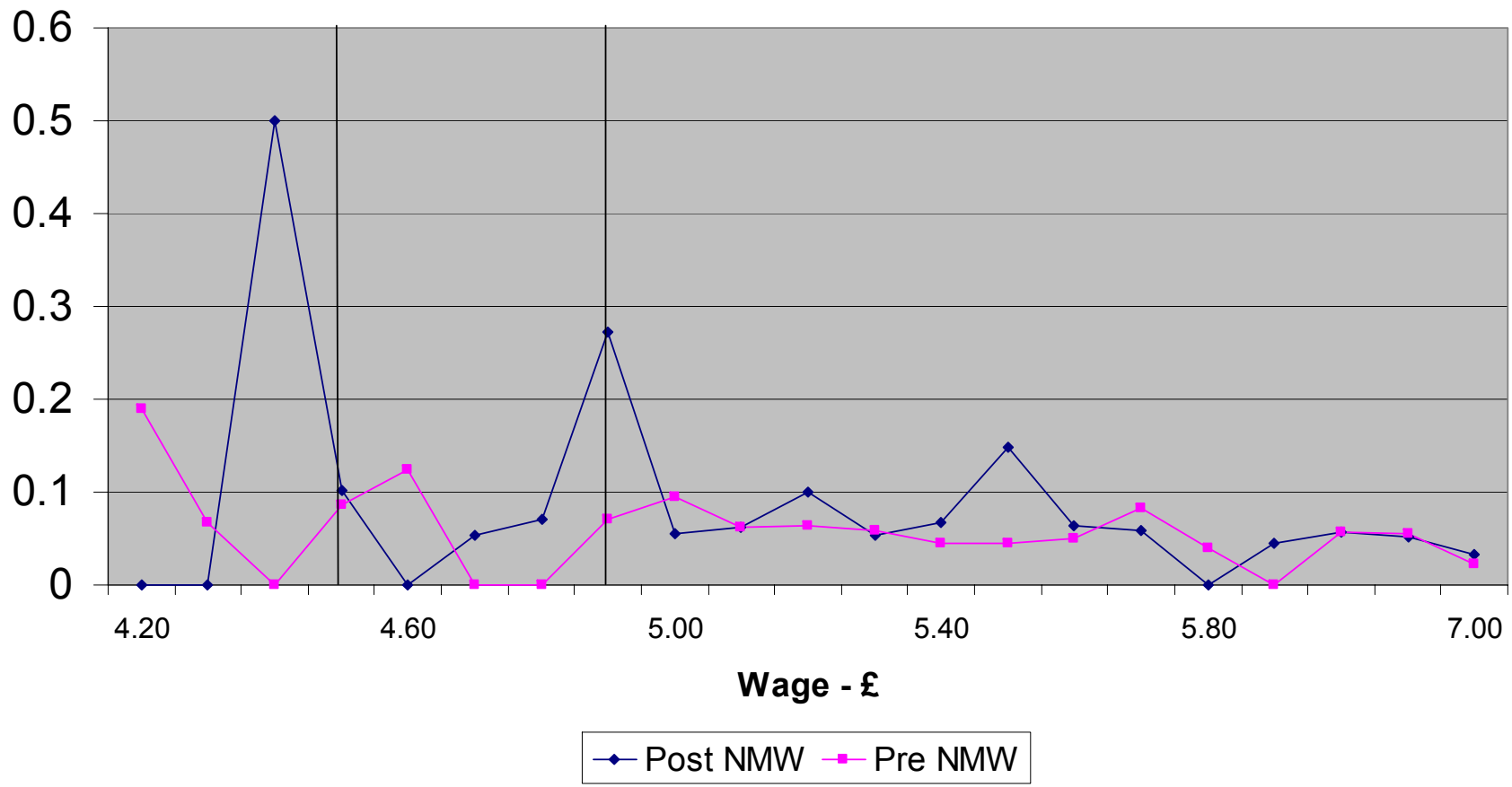
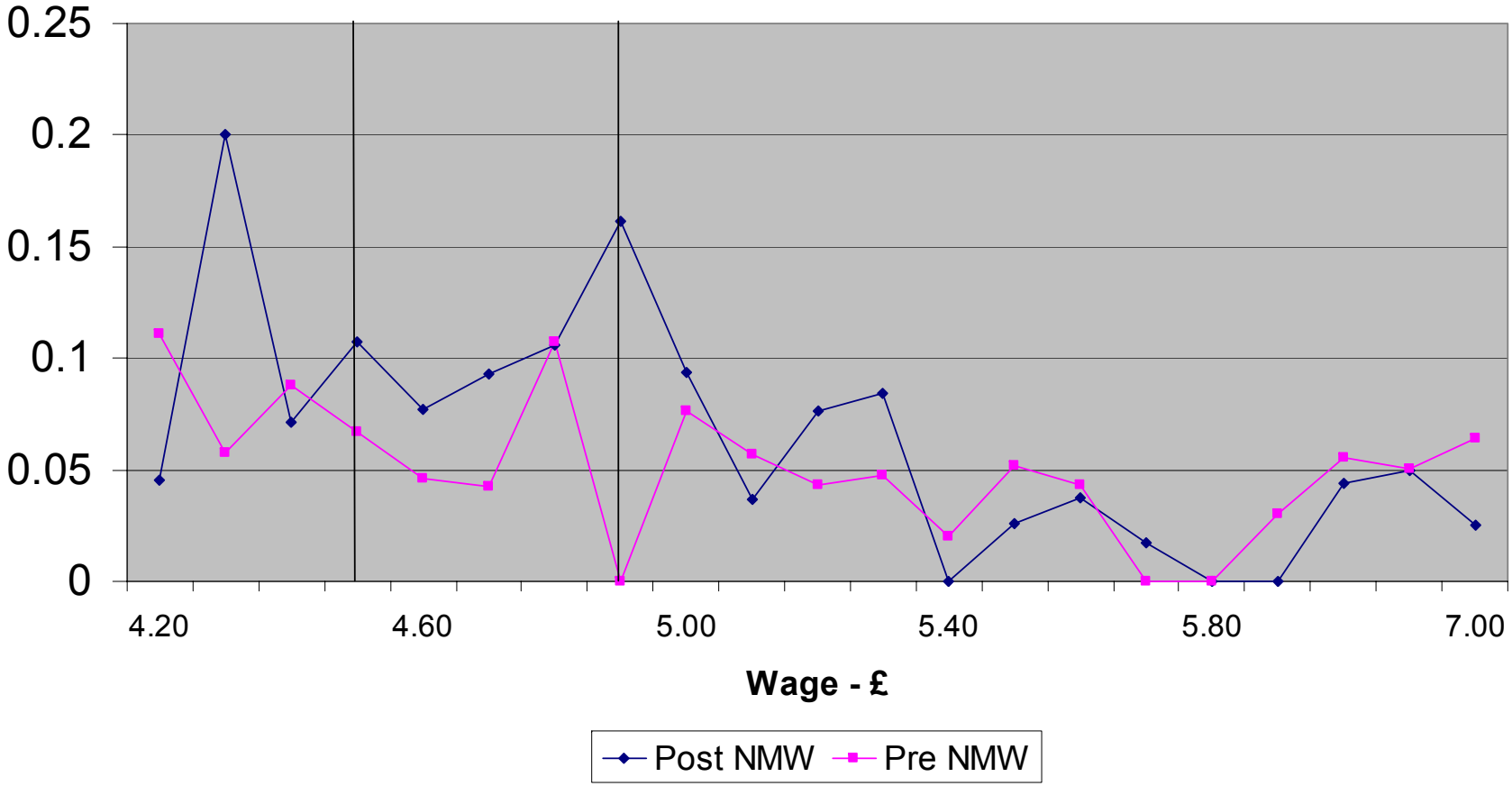


Figure 7b: Estimates of Job Entry Probability Females, Oct 2003 uprating



Appendix A1

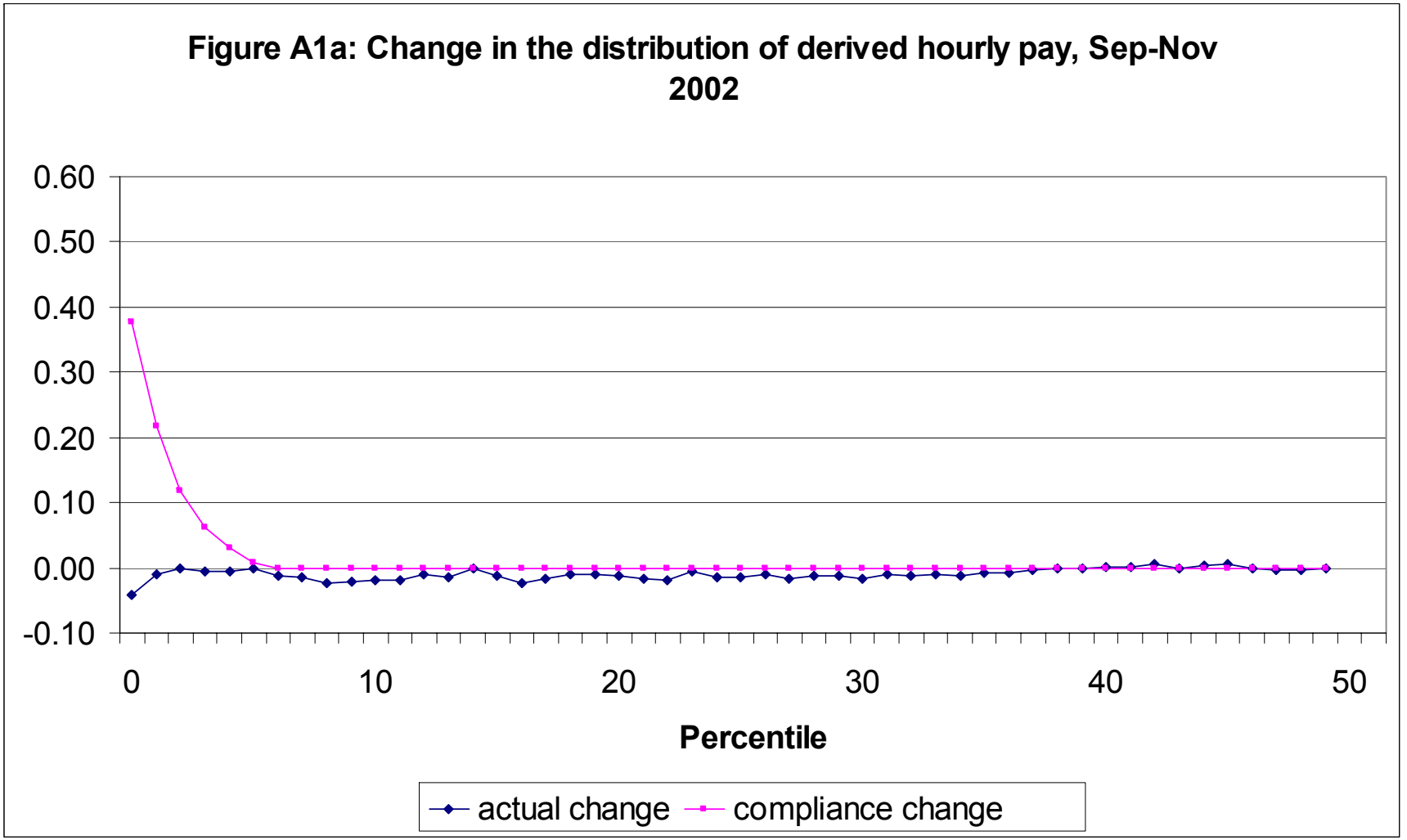


Figure A1a: Change in the distribution of derived hourly pay, Sep-Nov 2002
0.60
0.50
0.40
0.30
0.20
0.10
0.00
-0.10
0
10
20
30
40
50
Percentile
actual change
compliance change

Figure A1b: Change in the distribution of derived hourly pay, Sep-Nov 2001

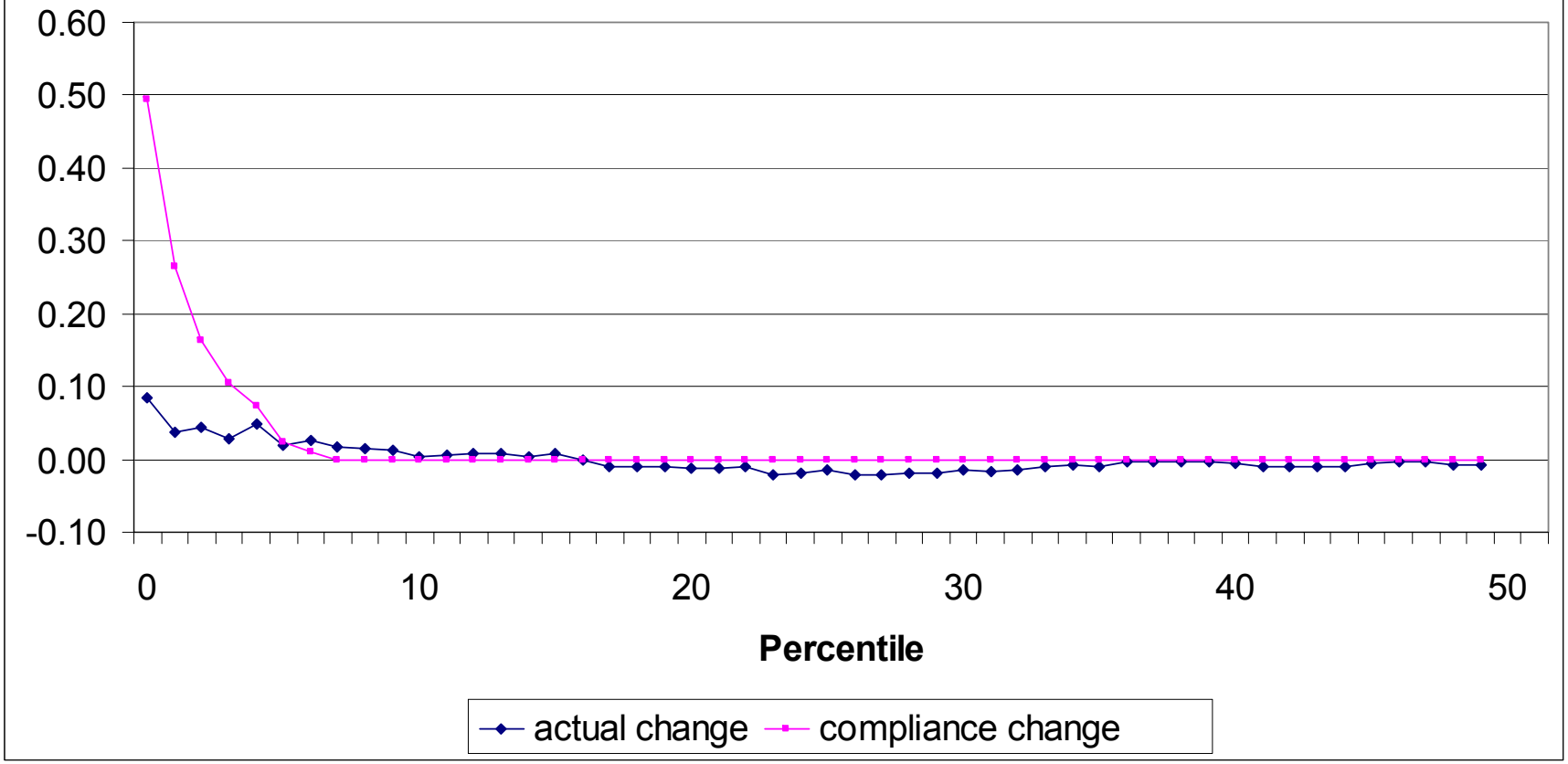


Figure A1c: Change in the distribution of derived hourly pay, Sep-Nov 2000

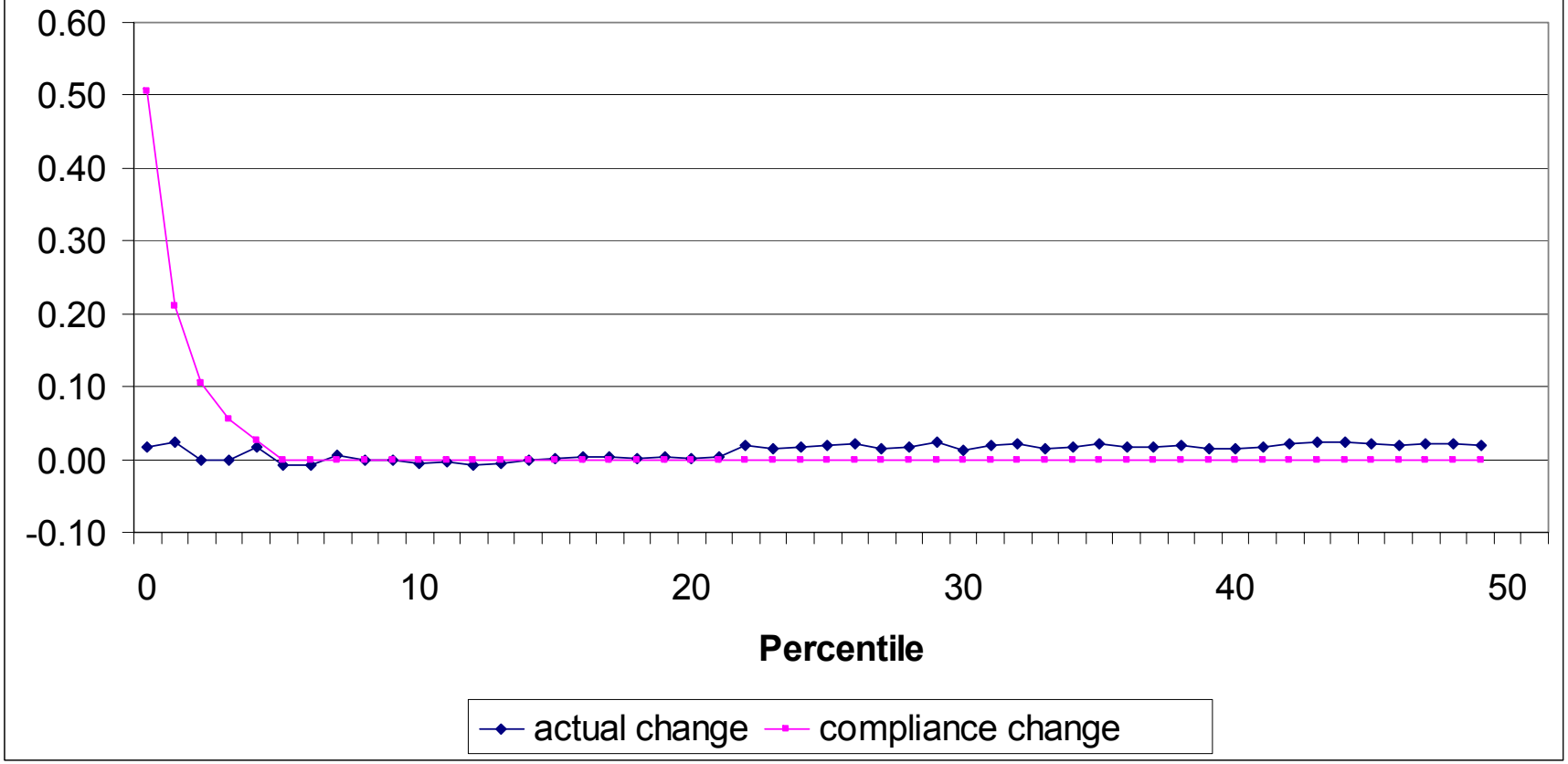


Figure A1d: Change in the distribution of derived hourly pay, Mar-May 1999

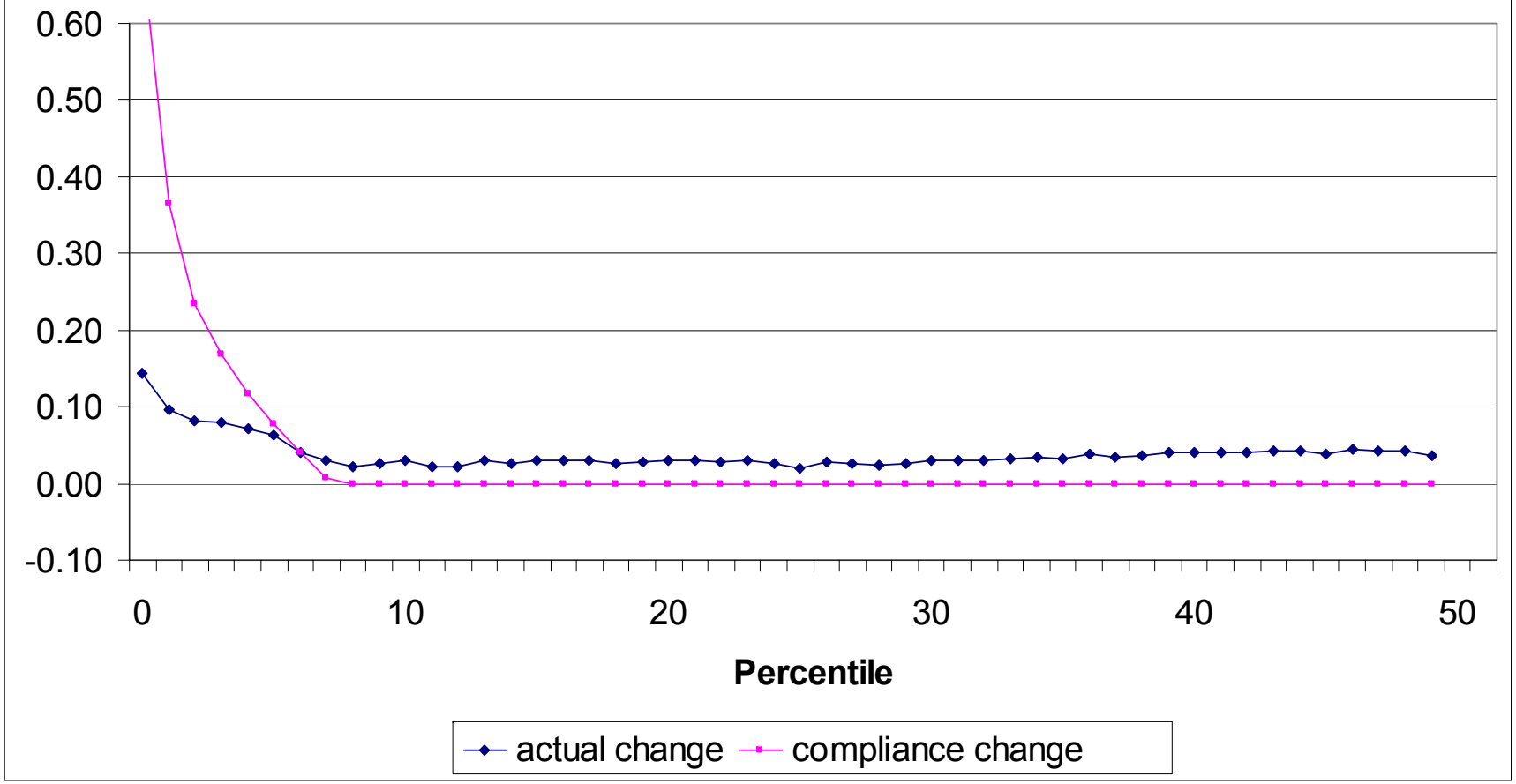
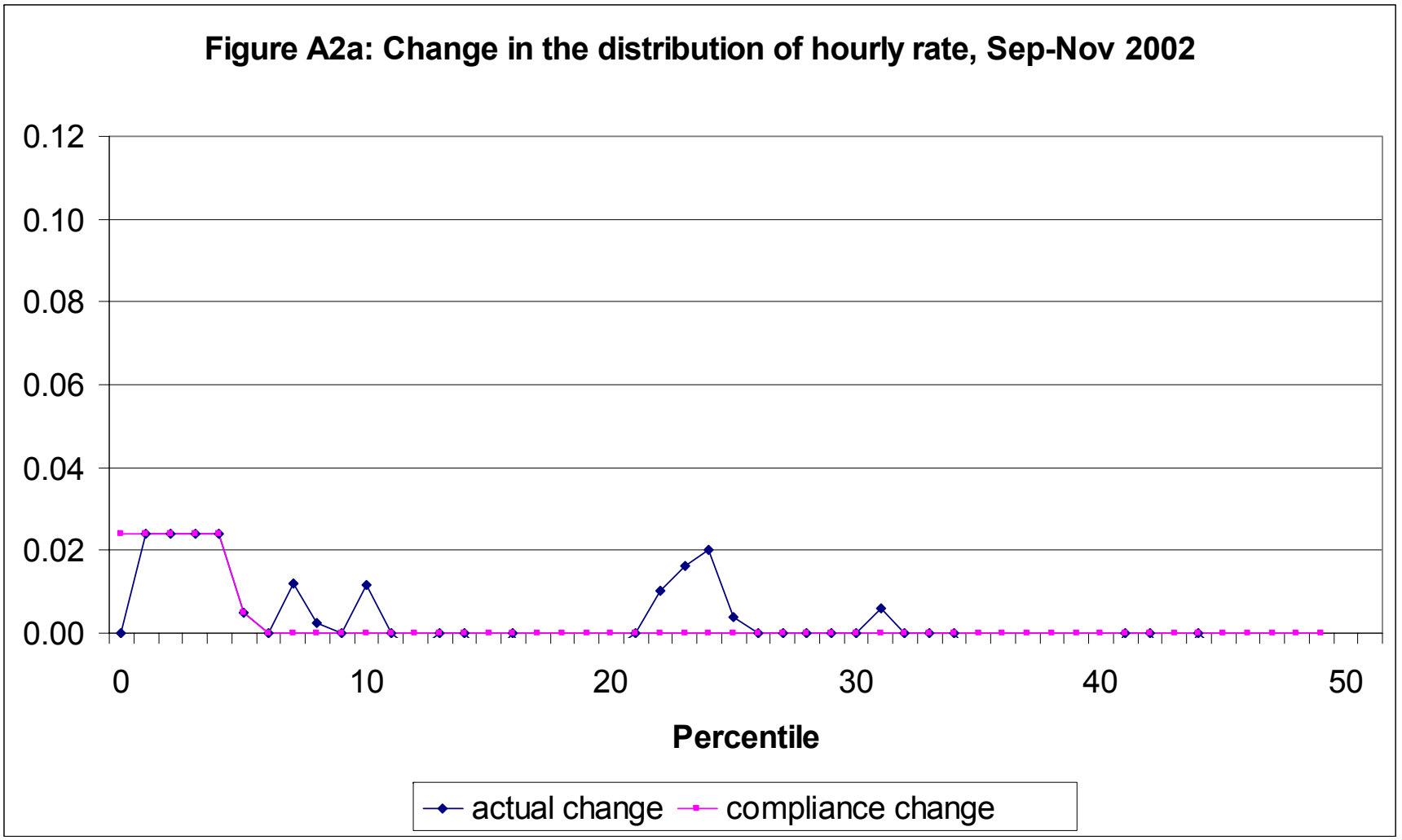


Figure A2a: Change in the distribution of hourly rate, Sep-Nov 2002



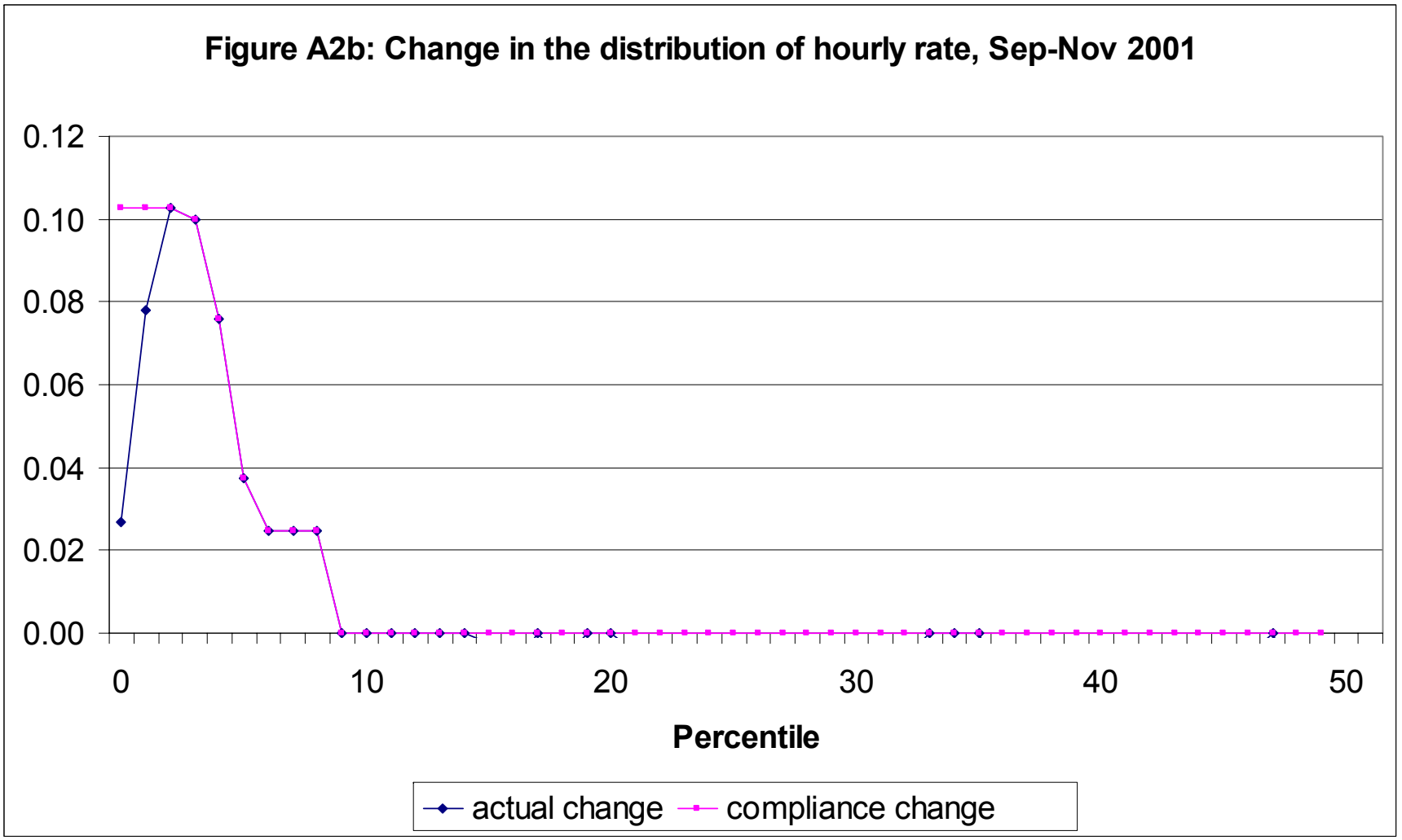


Figure A2b: Change in the distribution of hourly rate, Sep-Nov 2001
0.12
0.10
0.08
0.06
0.04
0.02
0.00
0
10
20
30
40
50
Percentile
actual change
compliance change

Figure A2c: Change in the distribution of hourly rate, Sep-Nov 2000

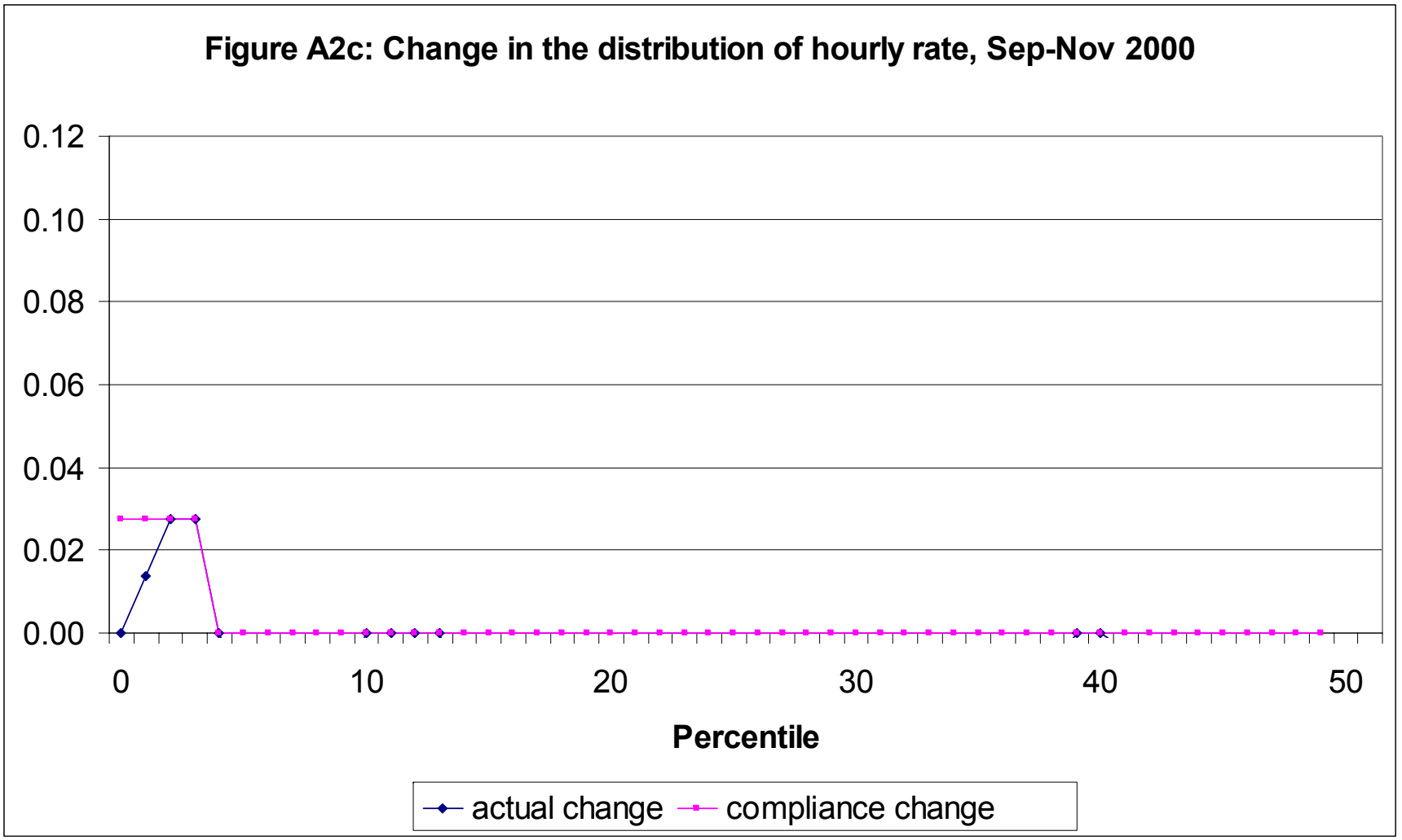


Figure A2d: Change in the distribution of hourly rate, Mar-May 1999

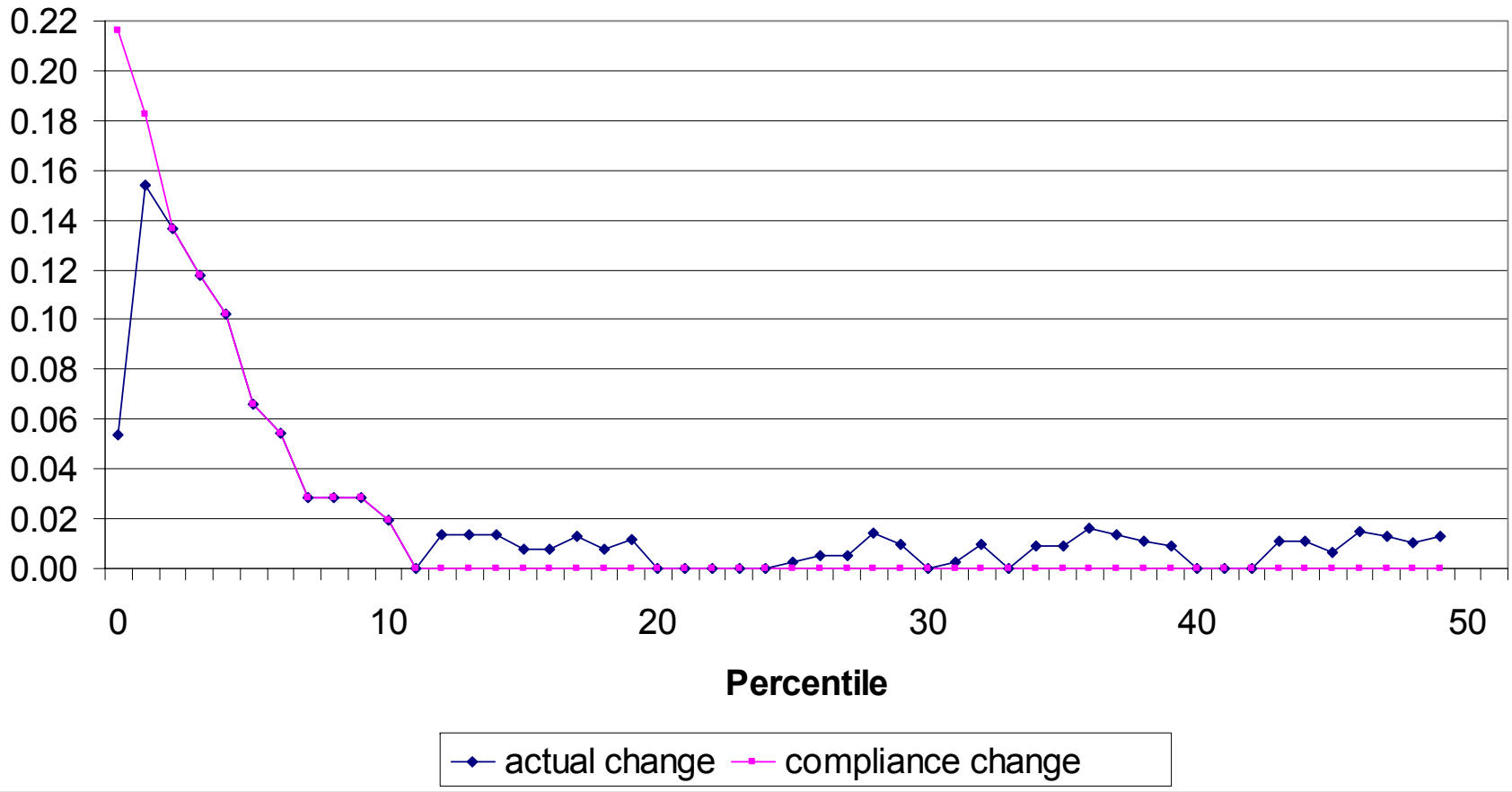
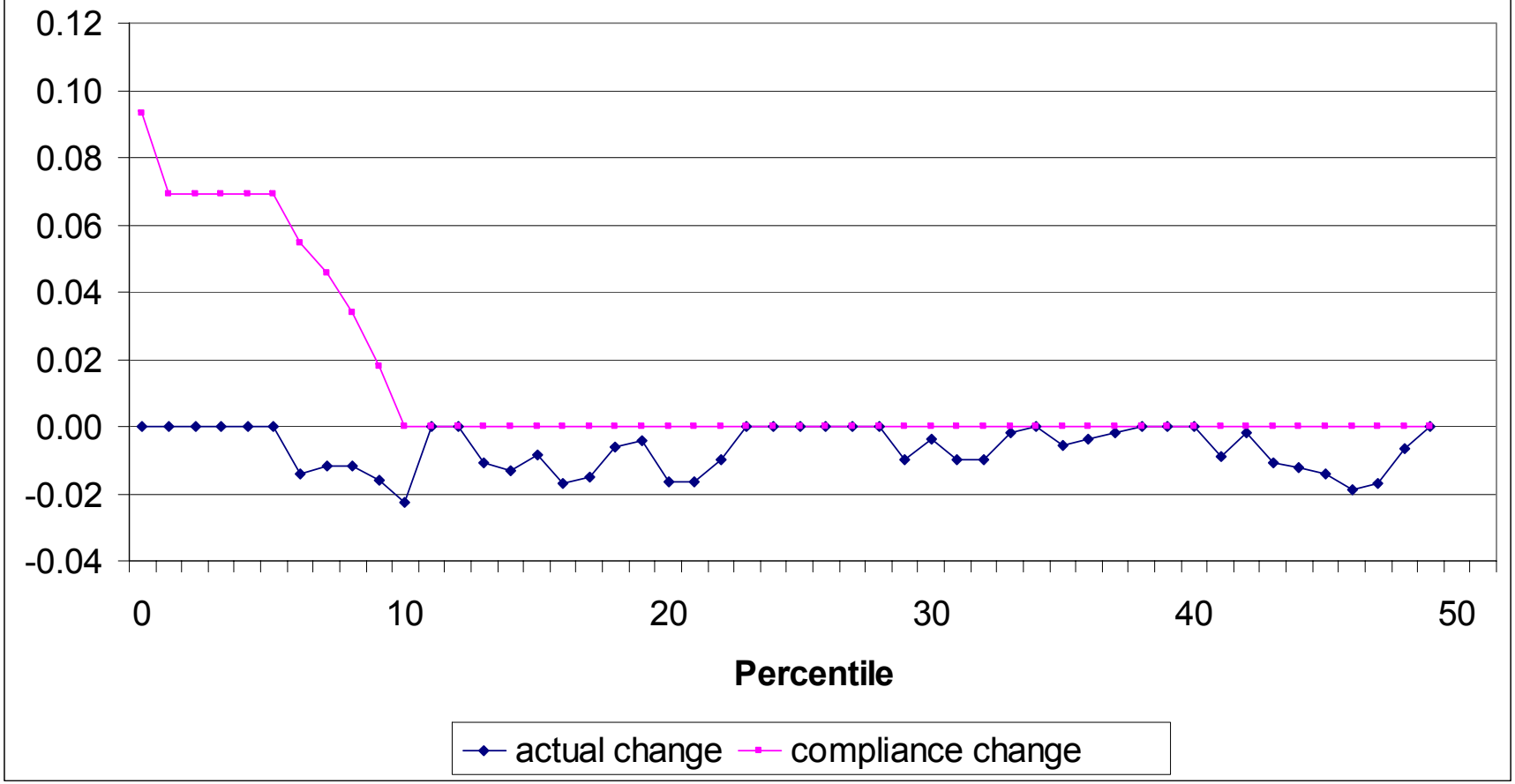


Figure A3: Change in the distribution of hourly rate, Dec 2002-Feb 2003



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