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Quantile estimates of counterfactual distribution shifts and the impact of minimum wage increases on the wage distribution*

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Abstract

This paper presents a method for estimating the effects of a policy change on an outcome distribution that uses a comparator quantile rather than a control group and provides methods for estimating the variances of the estimators. The empirical analysis presents estimates of "spillover" effects of increases in the UK minimum wage, i.e. effects on the wages of those already above the minimum, under different counterfactual distribution shift assumptions. Evidence is presented against a simple scaled counterfactual. On the basis of the proposed counterfactual estimated spillover effects are small and in most cases do not reach above the 5th. percentile.

Keywords: Policy change effects; Distributional effects; Counterfactual distributions; Quantile variances; Minimum wages; spillover effects.

1 Introduction

This paper estimates the effect of a policy change on the distribution of an outcome variable. There is an extensive literature in statistics, econometrics and other disciplines on the estimation of the average effect of a policy change, or treatment, and such methods have been employed in a vast range of empirical contexts. In many situations researchers are interested in estimating the distributional effects of a policy change or treatment, rather than just the average effect. Although less studied, there is now a growing literature on the estimation of such distributional effects.

The estimation of distributional effects has been approached in a number of different, but interrelated, ways. However, all the methods in some way involve estimating a counterfactual distribution. In distributional extensions of the difference-in-differences approach, for example, this involves a comparison of treatment and control groups and a comparison of before and after time periods. The post-change counterfactual distribution for the treatment group is then estimated under certain assumptions using the treatment group pre-change distribution and the pre- and post-change distributions for the control group (see Athey and Imbens, 2006).

The estimators used in this paper are similar to those proposed in these related literatures in terms of general approach and framework, but differ in an important regard. Rather than comparing treatment and control groups, as the difference-in-differences estimator does, the estimators in this paper use a comparator that is another quantile of the same distribution. The fact that these quantiles are correlated has important implications for the estimation of the variance of the difference and hence of the variances of the estimators used here.

An important identifying assumption for the difference-in-differences estimators is that those in the control group are unaffected by the policy change or treatment. In some empirical situations a useful alternative approach involves the comparison of two parts of the same distribution. The equivalent identifying assumption used in this case is that the policy change or treatment only affects part of the distribution, i.e. that there is another part of the distribution not affected. In this sense the estimator is like a distributional effects difference-in-differences estimator, but with the comparator based on the same distribution.

The empirical context studied in this paper is the impact of minimum wage increases on the wage distribution. The paper estimates effects at different points in the distribution. In particular it examines the effects of such increases on the wages of those already above the minimum, known as "spillover" or "ripple" effects of the increase in the minimum.

The approach taken compares quantiles of the observed wage distribution after an increase in the minimum wage with those of an estimated counterfactual wage distribution if there had not been an increase in the minimum wage. This counterfactual is constructed by making appropriate adjustments to the observed wage distribution before the increase. The approach measures what are known as quantile treatment effects under different potential counterfactuals.

The approach can also be viewed as an extension of the informal method used by Dickens and Manning (2004a) and others since. Their method is based on percentile plots and informal visual inspection. To evaluate the effect of the introduction of the new UK national minimum wage in April 1999 they compare the observed change in log wage percentiles with the "compliance change" assuming no spillovers above the minimum and adjusting by the change in the median.

The main contributions of this paper are threefold. First, it presents a method for estimating the effects of a policy change on an outcome distribution that uses a comparator quantile rather than a control group and provides methods for estimating the variances of the estimators. Second, it presents a formalisation and extension of the commonly used "scale by the median" method for examining distribution shifts, together with estimation methods for the variances of these estimators. Third, it estimates the "spillover" effects of increases in the UK national minimum wage, i.e. the effects on the wages of those already above the minimum, under different counterfactual distribution shift assumptions. In particular this includes estimates based on a more credible counterfactual distribution than those used previously in the literature.

The next section describes the increases in the UK minimum wage since its introduction. It then considers why we might expect spillover effects of a minimum wage increase and why such effects are potentially important. It also provides a brief review of the existing literature on minimum wage spillover effects. Section 3 describes the data used in this paper. Section 4 lays out the empirical framework employed to conduct the tests for spillover effects including derivation of estimators of the variances of quantile treatment effects estimators. Sections 5 and 6 present the results under two different counterfactual assumptions and Section 7 gives the conclusions.

2 Minimum wage increases and spillovers

A national minimum wage was introduced in the UK in April 1999 following a period in which there was no wage floor. The tests in this paper are applied to the initial introduction in 1999 and subsequent upratings from October 2000 onwards. The data used cover the period up to April 2008. In some cases these upratings have been larger than the prevailing underlying wage growth and in others smaller. Table 1 shows how the adult minimum wage has changed over the period under consideration and how these changes compare with changes in the general level of wages and with price inflation.

The 2001 minimum wage rise was the largest in percentage terms during this period – about 6% above general wage growth and about 9% in real terms. The 2003 and 2004 rises were also above the general rate of increase in wages (by 3 to 4%) and prices (by 4 to 5%). The other rises over the period have been smaller relative to general wage or price growth than these.

Much research has been conducted on the effects of the minimum wage introduction and subsequent upratings, with a particular focus on the effects on employment. The evidence suggests little or no impact on employment; see Metcalf (2008) for a review. The evidence assembled by the Low Pay Commission (LPC) suggests that firms have used a variety of strategies to adjust to minimum wage increases, differing across sectors of the economy. Much of the evidence is mixed. There is some evidence of increased costs being passed on via higher relative prices for minimum wage-produced consumer services; of a reduction in the relative profits of firms employing low-wage workers in some sectors; of reductions in hours rather than workers; of increases in training; and of labour market frictions and company wage setting power facilitating such adjustments (Metcalf, 2008).

Minimum wage spillover effects on the wage distribution might be expected for a number of reasons. First, the increase in the minimum raises the relative price of low-skilled labour. This may lead to a rise in the demand for certain types of more skilled labour, depending on substitutability, and hence to increased wage rates for certain types of worker already above the minimum. Second, it may lead firms to reorganise how they use their workforce to realign the marginal products of their minimum wage workers with the new minimum, and this may have effects on the marginal products of other workers. Third, it may lead to increases in wages for some workers above the minimum in order to preserve wage differentials that are potentially important for worker morale and motivation and hence may affect productivity.

Fourth, the rise may increase the reservation wages of those looking for jobs in certain sectors and hence push up the wages that employers must pay in those sectors to recruit. Falk et al. (2006) find in a laboratory experiment that minimum wages have a significant effect on subjects' reservation wages. They suggest that the minimum wage affects subjects' fairness perceptions and speculate that this response may lie behind any observed spillover effects. Flinn (2006) shows that minimum wages can also affect workers' reservation wages in search and matching models with wage bargaining.

Whether or not these potential spillover effects above the minimum occur when the minimum wage is raised, and if so how extensive they are, are important for several reasons.

First, they are important in the evaluation of the impact on the wage distribution as a whole and through this on measures for which wages are an important component, such as household incomes and welfare. Second, they are important in the investigation of how minimum wages affect wage inequality and its evolution over time.

Third, ignoring any spillover effects leads to a potential underestimation of the effect of any increase in the minimum wage rate on the wage bill. This may in turn lead one to underestimate the effect on prices, profits, etc. Fourth, the potential presence of spillovers is important for the key underlying assumption in much of the difference-in-differences methodology that has been used to evaluate the effect of the minimum wage on various outcomes. In this approach the group initially just above the new minimum is used as the "control" group under the assumption that they are not affected by the rise in the minimum. The approach has been used extensively to evaluate the UK minimum wage, to look at effects on employment (e.g. Stewart, 2004 and Dickens et al., 2009), hours (Stewart and Swaffield, 2008, Dickens et al., 2009) and second job holding (Robinson and Wadsworth, 2007) among other outcomes.

Minimum wage spillover effects have been investigated by a number of authors from Gramlich (1976) onwards. Most of these studies have been for the United States. The much quoted study by Lee (1999) examines the cross-state variation in the relative level of the US federal minimum wage and finds evidence of substantial spillover effects on certain specified percentiles of the wage distribution.

The influential book by Card and Krueger (1995) also finds evidence of spillovers, although rather more limited in scope. They find significant positive effects of increases in the US federal minimum on the 5th and 10th percentiles of the wage distribution using data across states, but not on the 25th. However, Neumark and Wascher (2008) point out that the Card and Krueger analysis does not necessarily identify spillover effects, because "workers at the 5th percentile (and perhaps even at the 10th percentile in low-wages states) can be minimum wage workers" (2008, p. 117). The Card and Krueger estimates measure a combination of effects on the spike in the distribution at the minimum and any spillover effects above it. This is an inherent difficulty with percentile-based methods. It is addressed in the empirical framework for this paper outlined in section 4 below.

The results in DiNardo et al. (1996) are consistent with spillovers above the minimum and Neumark et al. (2004), who examine effects on individual wage changes directly, also find evidence of substantial spillover effects. In contrast Autor et al. (2010) find much less evidence of spillovers and stress the important impact of measurement error on estimated effects.

There has been much less work testing for minimum wage spillover effects for the UK.

Dickens and Manning (2004a, 2004b) provide the main evidence available on such effects for the introduction of the minimum wage in 1999 and do not find evidence of spillover effects. As a result the UK is often pointed to as the exception to the finding of spillover effects of minimum wages in other countries, for example by Falk et al. (2006). Dickens and Manning (2004a) provide evidence in the form of percentile plots, but do not provide a formal test or estimates. This paper builds on their approach.

Subsequent studies have extended their analysis of changes in wage percentiles. Butcher (2005), Butcher et al. (2008) and LPC (2009) examine percentage changes in hourly pay percentiles relative to corresponding changes in the median for longer time spans. Although no standard errors or confidence intervals are presented, LPC (2009) provide evidence suggesting spillovers for the period 1998–2004, but a far smaller impact for the minimum wage rises during 2004-2008.

3 Data used

The analysis presented in this paper is based on data from the Annual Survey of Hours and Earnings (ASHE). The ASHE, developed from the earlier New Earnings Survey (NES), is conducted in April of each year. It surveys all employees with a particular final two digits to their National Insurance numbers who are in employment and hence aims to provide a random sample of employees in employment in the UK. The ASHE is based on a sample of employees taken from HM Revenue and Customs "Pay-as-you-earn" (PAYE) records. Information on earnings and paid hours is obtained in confidence from employers, usually directly from their payroll records. It therefore provides very accurate information on earnings and paid hours. Providing accurate information for this survey is a statutory requirement under the Statistics of Trade Act.

The ASHE survey and follow-up design provides better coverage than the old NES of employees who changed, or started new, jobs after sample identification. Technical details of the ASHE are given in Bird (2004); for a review of the issues involved in, and the investigations conducted for, the redevelopment of the NES into the ASHE see Pont (2007). Subsequently the Office for National Statistics (ONS) have constructed consistent back series by applying ASHE-consistent methodology to NES data back to 1997. Some summary ASHE results for the period 1997 to 2008, which is the same as the period for the data used in this paper, are provided in Dobbs (2009).

There are some limitations of the data that should be born in mind when interpreting the results of the analysis. There is some under representation of low paid employees. This is for two reasons. First, the survey under samples those with weekly earnings below the PAYE deduction threshold. This affects predominantly part-time employees with particularly short weekly hours. It also predominantly affects employees in businesses that return paper questionnaires rather than those who provide data electronically on all relevant employees in the return.

The second reason stems from the fact that there is a short gap between the drawing of the sample from HMRC records and the survey reference week. Those who enter employment from non-employment during this period may get excluded. Those who change employer may also get missed if they cannot be traced. Again businesses that provide data electronically reduce this. This time gap may also result in under representation of low paid employees to the extent that they have higher turnover.

Since 2004 supplementary data has been collected to address the latter reason. This has improved the coverage of employees who either changed employer or entered employment. This therefore represents a discontinuity in the data. Another discontinuity occurred in 2007. In the 2007 and 2008 surveys the sample size was reduced by 20%. There were also more minor changes in ASHE methodology in 2005 and 2006, but these are not expected to impact on the analysis in this paper. See Dobbs (2009) for more details of the changes.

The wage variable used for the analysis in this paper is defined as average hourly earnings for the reference period, excluding overtime. It is constructed by dividing average gross weekly earnings excluding overtime for the reference period by basic weekly paid hours worked. The original returned data is for the most recent pay period and is converted to a per week basis if the pay period is other than a week. Both overtime earnings and overtime hours are excluded in the construction of the wage variable used.

The focus of attention in this paper is on the wages of adults and the adult minimum wage rate. As Neumark et al. (2004) point out, "policymakers typically are most concerned with adult workers near the minimum wage", because young workers are still in the early part of their wage-experience profile. The data used here are restricted to those aged 22 or over (the age cut-off for the minimum wage adult rate), who are on full adult rates, and whose pay in the reference period was not affected by absence. This produces a sample for the 12 years used, 1997 to 2008, of about 1.65 million observations, an average of 137,500 observations per year.

4 Empirical framework

The estimation of the distributional effects of the policy change involves the comparison of the observed post-change distribution of the outcome variable with an estimate of the counterfactual distribution. The possible estimators can be viewed as extending the techniques developed for the estimation of average treatment effects; see Imbens and Wooldridge (2009) for a review.

The analysis is based on differences in (adjusted) percentiles of log wages. Since quantiles are commutative with any monotonic transformation, these differences can also be viewed as the logs of ratios of (adjusted) percentiles of wages. To explain the framework used, consider a comparison of two dates either side of a single policy change, e.g. an uprating of the minimum wage. Denote by $t = 1$ and $t = 2$ observation periods before and after this uprating respectively. For example these might be the ASHE survey dates in April 2001 and April 2002, respectively 6 months before and after the October 2001 uprating of the minimum wage. Denote the cumulative distribution functions of log wages at these two dates by $F_1(.)$ and $F_2(.)$ respectively. Evaluation of the impact of the uprating on the wage distribution then requires a comparison of F_2 with a counterfactual estimate of what the distribution would have been if there had not been an uprating, with this latter being based on adjusting F_1.

4.1 Quantile treatment effects

Consider first the simplest case where it is hypothesised that in the absence of the minimum wage increase there would have been no changes in wages. In this case we simply need to compare F_2 with F_1. This simple case is just an application of the two-sample treatment response model of Doksum (1974) and Lehmann (1974). Suppose that the increase in the minimum wage adds $\delta(w)$ to the log-wage of someone whose log-wage in the absence of the increase in the minimum wage would have been w. Then the distribution F_2 of post-increase log wages is that of $w + \delta(w)$, where w has distribution F_1. Following Doksum and Lehmann, define $\delta(w)$ as the horizontal distance between F_1 and F_2 at w so that

$$F_1(w) = F_2(w + \delta(w)) \tag{1}$$

and therefore $\delta(w)$ is given by

$$\delta(w) = F_2^{-1}(F_1(w)) - w \tag{2}$$

Evaluating at a specified quantile, $\theta = F_1(w)$, for $\theta \in (0, 1)$, gives what is known as the "quantile treatment effect" (QTE) as

$$\Delta(\theta) = \delta(F_1^{-1}(\theta)) = F_2^{-1}(\theta) - F_1^{-1}(\theta) \tag{3}$$

This is simply the difference in θ-quantiles between time periods $t = 1$ and $t = 2$. In this simple two-sample case with the counterfactual being no change in wages, the QTE can be estimated by the difference in the estimated log wage quantiles

$$\hat{\Delta}(\theta) = \hat{F}_2^{-1}(\theta) - \hat{F}_1^{-1}(\theta) \tag{4}$$

where $\hat{F}_j$ denotes the empirical distribution function of log wages in the sample for time $t = j$. Construction of the variance of this estimator is addressed in section 4.3 below, together with that for the estimator considered in section 4.2.

This estimator can be formulated as a quantile regression model (Koenker, 2005), estimated on the pooled data (for $t = 1$ and $t = 2$ combined) with a binary sample indicator:

$$Q_\theta(w_{it}|D_{it}) = \alpha(\theta) + \beta(\theta)D_{it} \tag{5}$$

for $\theta \in (0, 1)$, where $Q_\theta(w|D)$ denotes the conditional θ-quantile of the distribution of w given D and where $D_{it} = 1$ if $t = 2$ and $D_{it} = 0$ if $t = 1$. Quantile regression estimation of this equation gives estimates $\hat{\alpha}(\theta) = \hat{F}_1^{-1}(\theta)$ and $\hat{\beta}(\theta) = \hat{F}_2^{-1}(\theta) - \hat{F}_1^{-1}(\theta) = \hat{\Delta}(\theta)$.

The above framework provides a formalization of the informal method used by Dickens and Manning (2004a) when they compare the observed change in the log wage percentiles from March 1999 to May 1999 with the "compliance change" required for the minimum wage introduction, since the difference between them is equal to the difference between the observed May 1999 percentiles and the counterfactual ones using the March 1999 distribution and assuming compliance with the minimum but otherwise no wage changes including no spillovers.

The "compliance change" is zero above the new (increased) minimum, i.e. for log-wages such that $F_1^{-1}(\theta) > m_2$, where m_2 is the log of the minimum wage in year $t = 2$. Hence tests for spillovers can be conducted by testing $\Delta(\theta) > 0$ for $\theta > F_1(m_2)$, i.e. for $\theta \in (F_1(m_2), 1)$. In practice we are interested in testing for "spillover" effects only at the bottom end of the wage distribution. Thus tests are conducted for $\theta \in (F_1(m_2), \theta^U)$, where θ^U is a pre-specified upper limit for the range of tests. $\theta^U = 0.25$ is used in the empirical part of this paper.

4.2 Simple scaled counterfactual

The simple model outlined in section 4.1 assumes that in the absence of the minimum wage increase there would have been no changes in wages. This is almost certainly not an appropriate assumption. There would likely have been movements in the wage distribution between, say, April 2001 and April 2002. Therefore one needs to specify a counterfactual distribution and estimate the effect relative to that. This requires a hypothesis of how individual wages would have moved in the absence of the increase in the minimum.

When Dickens and Manning (2004a) look at longer time gaps than the March–May 1999 one that was their initial comparison, they examine quantile changes relative to the change in the median. This is to account for "nominal wage growth in the whole economy". Although they do not view it in these terms, the implicit counterfactual is therefore that in the absence of the minimum wage change all wages would have risen in line with the median. This is also used as the implicit counterfactual in the subsequent papers referred to in section 2 that examine adjusted percentile plots. It is used as the initial simple counterfactual examined in this paper.

In general the adjusted effect therefore takes the form

$$\Delta(\theta) = [F_2^{-1}(\theta) - F_1^{-1}(\theta)] - [F_2^{-1}(\eta) - F_1^{-1}(\eta)] \tag{6}$$

for some $\eta > \theta^U$. The Dickens-Manning case described above is for $\eta = 0.5$. The estimator is given by replacing each $F^{-1}(.)$ in this expression by the appropriate empirical quantile for the appropriate year. It is a difference between two estimated QTEs as defined in section 4.1.

This estimate of the adjusted QTE in equation (6) is equivalent to the difference between actual and counterfactual θ-quantiles for log wages in year 2:

$$\hat{\Delta}(\theta) = \hat{F}_2^{-1}(\theta) - \hat{F}_2^{*-1}(\theta) \tag{7}$$

where $\hat{F}_2^{*-1}(\theta)$ is the estimated counterfactual year 2 quantile under the scenario where all wages go up in line with the rise in the median (or other quantile), and in general

$$\hat{F}_2^{*-1}(\theta) = \hat{F}_1^{-1}(\theta) + [\hat{F}_2^{-1}(\eta) - \hat{F}_1^{-1}(\eta)] \tag{8}$$

which is the log of the scaled year 1 wage θ-quantile. The adjustment used by Dickens and Manning (2004a) and others since takes the form of equation (6), but is equivalent to a comparison with a counterfactual distribution of the form of (8).

It is worth pointing out similarities with, and differences from, other estimators that have been proposed in the literature. This estimator can, for example, be viewed in a difference-in-differences framework. In the simplest version of the difference-in-differences setup individuals are observed in two groups and two time periods. Individuals in one group (the treatment group) are affected by the policy change and those in the other group (the control group) are not. The two time periods are before and after the policy change.

In this setup the estimator of the average effect of the policy change is the difference between the time change in average outcome for the treatment group and that for the control group. Under certain assumptions this double differencing removes time-invariant group differences and common time effects. The estimator can also be viewed as the difference between the actual average post-change outcome for the treatment group and the counterfactual average for this group if they had not been treated. This counterfactual average is estimated by adjusting the pre-change average for this group by the observed change experienced by the control group.

This difference-in-differences framework can be extended to distributions. This allows the effects to differ systematically across individuals and provides an estimator of the entire counterfactual distribution. The estimator with the scaled counterfactual given in equation (8) is similar in some ways to the "quantile difference-in-differences" (QDID) estimator used for specific quantiles by Meyer et al. (1995) and generalized to the full distribution by Athey and Imbens (2006). Meyer et al. (1995) examine separately the difference-in-differences for two quantiles (the median and the 75th percentile) of the outcome variable.

Differencing in that estimator is across time and between two mutually exclusive groups. Labelling the treatment group A and the control group B, the QDID estimator is given by

$$\Delta^{QDID}(\theta) = [F_{A2}^{-1}(\theta) - F_{A1}^{-1}(\theta)] - [F_{B2}^{-1}(\theta) - F_{B1}^{-1}(\theta)]$$

The counterfactual distribution for the treatment group in time period 2 is such that its inverse is given by

$$F_{A2}^{*-1}(\theta) = F_{A1}^{-1}(\theta) + [F_{B2}^{-1}(\theta) - F_{B1}^{-1}(\theta)]$$

The adjustment is by the change in the same θ-quantile for the alternative (i.e. control) group, whereas for the estimator used in this paper the adjustment is by the change in a different quantile for the same population. This difference complicates the estimation of the variance of the estimator. There is an important covariance between two quantiles of the same distribution to take into account for the estimator used in this paper which is not present for the QDID estimator.

Athey and Imbens (2006) propose a different generalization of difference-in-differences to distributions which they call the "changes-in-changes" (CIC) estimator and describe the disadvantages of the QDID estimator relative to this CIC estimator. For the CIC estimator the counterfactual distribution for the treatment group in time period 2 is given by

$$F_{A2}^{*}(y) = F_{A1}(F_{B1}^{-1}(F_{B2}(y)))$$

Thus the CIC estimator of the effect of the treatment on quantile θ is given by

$$\begin{aligned}\Delta^{CIC}(\theta) &= F_{A2}^{-1}(\theta) - F_{A2}^{*-1}(\theta) \\ &= F_{A2}^{-1}(\theta) - F_{B2}^{-1}(F_{B1}(F_{A1}^{-1}(\theta)))\end{aligned}$$

However, unlike the QDID estimator, the CIC estimator does not have a natural analogue for the current context.

Another related approach to the estimation of distributional effects of policy changes on which there is a growing literature uses assumptions about the relationship between the outcome variable and a set of covariates and about the change in the distribution of the covariates resulting from the policy changes to estimate the counterfactual distribution. The estimator proposed by Rothe (2010), for example, has the same general form as equation (7) above. His approach estimates the conditional distribution of the outcome variable given the covariates by nonparametric kernel methods and then uses that together with the, assumed known, counterfactual distribution of the covariates to estimate the counterfactual unconditional distribution of the outcome variable, F^*. The same estimators as in this literature are also used in the decomposition methods for changes or differences in distribution (Fortin et al., 2010). The components in such decompositions are equal to differences between counterfactual and actual distributions. The estimators in this approach use the assumption that the conditional distribution of the outcome variable given the covariates is unaltered by the policy change or treatment being analysed.

As was the case for the simple QTE specification in section 4.1, formulating the estimation and testing in terms of quantile regression equations provides a useful framework for conducting the analysis of these quantile shifts. The estimates of the θ-quantiles for each of the two periods described above and the difference between them can be derived by estimation, using pooled data for the two periods, of equation (5) as before and this can be combined with the equivalent median regression estimate (i.e. that for $\theta = 0.5$) to give $\hat{\Delta}(\theta) = \hat{\beta}(\theta) - \hat{\beta}(0.5)$. This can also be extended to a "regression adjusted" estimator that controls for changes in other factors by including them in both quantile regressions.

Asymptotic standard errors for the QTE estimator using the scaled counterfactual described in this subsection can be derived by analytic methods and are described in the next two sub-sections. Bootstrap standard errors can also be constructed, e.g. using simultaneous quantile regression where the quantile regression equations for quantile θ and the median are estimated simultaneously. Both are used in section 5 below to test for $\Delta(\theta) > 0$ and construct confidence intervals for $\Delta(\theta)$ for minimum wage changes.

4.3 Variances of quantile differences

Start with a single point in time and suppose that the log wage random variable, w, has probability density function $f(.)$ and cumulative distribution function $F(.)$. Consider two quantile points, $\theta < \eta$, and suppose that the quantiles at these two points have been estimated using a sample of size n. Then the asymptotic variances and covariance of the quantiles $F^{-1}(\theta)$, $F^{-1}(\eta)$ are given by

$$\mathrm{var}(F^{-1}(\alpha)) = \frac{\alpha(1-\alpha)}{nf_\alpha^2} \qquad \alpha = \theta, \eta \tag{9}$$

$$\mathrm{cov}(F^{-1}(\theta), F^{-1}(\eta)) = \frac{\theta(1-\eta)}{nf_\theta f_\eta} \tag{10}$$

where $f_\alpha = f(F^{-1}(\alpha))$ for $\alpha = \theta, \eta$. (See Kendall, 1940, and Kendall and Stuart, 1977, pp. 251-4.) Thus the variance of the difference between the two quantiles is given by

$$\operatorname{var}(F^{-1}(\theta) - F^{-1}(\eta)) = \frac{1}{n}\left\{\frac{\theta(1-\theta)}{f_\theta^2} + \frac{\eta(1-\eta)}{f_\eta^2} - \frac{2\theta(1-\eta)}{f_\theta f_\eta}\right\} \tag{11}$$

In particular the asymptotic variance of the difference between a specified quantile (< 0.5) and the median is given by this expression with $\eta = 0.5$.

The asymptotic distribution of $(F^{-1}(\theta), F^{-1}(\eta))$ is bivariate normal. (See David, 1970, pp. 201-3, and papers cited therein.) Hence the quantile difference, $[F^{-1}(\theta) - F^{-1}(\eta)]$, is asymptotically normal with variance given by equation (11).

Estimation of this variance requires estimation of the density function at two quantile points, $f(F^{-1}(\alpha))$ for $\alpha = \theta, \eta$. This estimation is returned to in section 4.4 below.

Now consider two time periods, $t = 1$ and $t = 2$. Let the initial (pre-uprating) distribution of w have probability density function $f_1(.)$ and cumulative distribution function $F_1(.)$ and the post-uprating distribution have probability density function $f_2(.)$ and cumulative distribution function $F_2(.)$, and suppose that the quantiles are estimated with samples of size n_1 and n_2 respectively. The adjusted quantile change in equation (6) can be rewritten as

$$\Delta(\theta) = \left[F_2^{-1}(\theta) - F_2^{-1}(\eta)\right] - \left[F_1^{-1}(\theta) - F_1^{-1}(\eta)\right] \tag{12}$$

The estimator of section 4.2 uses $\eta = 0.5$ and replaces each quantile by its empirical counterpart. Its variance is given by

$$\operatorname{var}(\Delta(\theta)) = \frac{1}{n_2}\left\{\frac{\theta(1-\theta)}{f_{2\theta}^2} + \frac{1}{4f_{2\eta}^2} - \frac{\theta}{f_{2\theta}f_{2\eta}}\right\} + \frac{1}{n_1}\left\{\frac{\theta(1-\theta)}{f_{1\theta}^2} + \frac{1}{4f_{1\eta}^2} - \frac{\theta}{f_{1\theta}f_{1\eta}}\right\} \tag{13}$$

where $f_{t\alpha} = f_t(F_t^{-1}(\alpha))$ for $\alpha = \theta, \eta$ and $t = 1, 2$. Of course a choice of η other than 0.5 can also be used. This expression assumes a zero covariance between the quantiles in periods 1 and 2. For the more general case where there is a partial overlap between the individuals in the two time periods, it provides only an approximation.

4.4 Estimation of the sparsity function

The estimation of the asymptotic variance of Δ requires the estimation of the density function at particular quantiles, or the reciprocal of this function, known as the "sparsity function":

$$s(\theta) = \left[f(F^{-1}(\theta))\right]^{-1} \tag{14}$$

The sparsity function, also called the "quantile-density function", reflects the density of observations near the θ-quantile. The more sparse the data around the θ-quantile (i.e. the higher is $s(\theta)$), the less precisely estimated will be the quantile (i.e. the higher its variance).

Estimation of the sparsity function raises the issue of smoothing. The sparsity function is the derivative of the quantile function, $s(\theta) = dF^{-1}(\theta)/d\theta$. A natural choice of estimator is therefore that suggested by Siddiqui (1960) and Bloch and Gastwirth (1968):

$$\hat{s}(\theta) = \frac{\hat{F}^{-1}(\theta + h) - \hat{F}^{-1}(\theta - h)}{2h} \tag{15}$$

where $\hat{F}^{-1}$ is an estimate of F^{-1} and $h > 0$ is a bandwidth that tends to zero as $n \to \infty$. This then raises the question of the choice of bandwidth. Bofinger (1975) showed that to minimize mean squared error the optimal choice was

$$h = n^{-1/5} \left\{ 4.5[s(\theta)/s''(\theta)]^2 \right\}^{1/5} \tag{16}$$

An estimate of $[s(\theta)/s''(\theta)]$ is required to evaluate this parameter. Fortunately this ratio is not very sensitive to F (Koenker, 2005, p.139), and so we can approximate it using a Gaussian distribution.

Using the Gaussian approximation to $[s(\theta)/s''(\theta)]$ in this expression for h gives

$$h = n^{-1/5} \left\{ \frac{4.5\phi^4(\Phi^{-1}(\theta))}{[2\Phi^{-1}(\theta)^2 + 1]^2} \right\}^{1/5} \tag{17}$$

This choice of h is optimal for the estimation of $s(\theta)$. But Hall and Sheather (1988) argue that a smaller value of h is needed for the optimal bandwidth for the use of $\hat{s}(\theta)$ for constructing tests and confidence intervals. They show that nh needs to be of order $n^{2/3}$ rather than $n^{4/5}$ in this case. They suggest the bandwidth

$$h = n^{-1/3} z_\alpha^{2/3} [1.5 s(\theta)/s''(\theta)]^{1/3} \tag{18}$$

where $\Phi(z_\alpha) = 1 - \alpha/2$ and α is the desired size of the test. Using the same Gaussian approximation to $[s(\theta)/s''(\theta)]$ as above gives

$$h = n^{-1/3} z_\alpha^{2/3} \left\{ \frac{1.5\phi^2(\Phi^{-1}(\theta))}{2\Phi^{-1}(\theta)^2 + 1} \right\}^{1/3} \tag{19}$$

As an alternative to the Siddiqui-Bloch-Gastwirth histogram estimator, a kernel estimator of the quantile density function can be used. The issue of the choice of bandwidth of course arises with this estimator too. As long as a suitable bandwidth is chosen, the choice of kernel function is far less important.

The Silverman plug-in estimate of the bandwidth (Silverman, 1986) is used here. It minimizes the mean integrated squared error, taking a Gaussian approximation to the integral of the square of the second derivative of the density. Using this, the bandwidth estimate is given by

$$b = \left(\frac{8\sqrt{\pi}}{3} \right)^{1/5} \delta n^{-1/5} \sigma^* \tag{20}$$

where $\sigma^* = \min(\sigma, r/\{\Phi^{-1}(0.75) - \Phi^{-1}(0.25)\})$, σ is the sample standard deviation, r is the sample inter-quartile range, and δ is a constant that depends on the kernel function used. The Epanechnikov kernel is used in this paper. For this kernel, $\delta = 15^{1/5}$ and hence $b/(n^{-1/5}\sigma^*) = (40\sqrt{\pi})^{1/5} = 2.3449$ (Wand and Jones, 1995). The use of the scaled inter-quartile range as an alternative estimate in σ^* is to protect against outliers, which can otherwise increase s and lead to too large a choice of b.

4.5 Double scaled counterfactual

The counterfactual in section 4.2 assumes that in the absence of the minimum wage increase all wages would have gone up in line with the median. The evidence in section 6 below indicates that this is probably not an appropriate assumption to make. One advantage of studying the UK minimum wage is that for a period of about 5 years prior to its introduction in 1999 there was no wage floor in the UK, enabling one to look at wage distributions in the absence of the minimum wage directly. Applying the estimation procedure of section 4.2 to changes in quantiles between 1997 and 1998, prior to the introduction of the minimum wage, produces evidence of strongly significant estimates of $\Delta(\theta)$ for some quantiles at the bottom of the wage distribution in the absence of any minimum wage increases (or indeed of a minimum wage).

The availability of data for this period without a minimum wage in either year suggests using this period to improve the estimation of the counterfactual distribution. If we assume instead that wages at each quantile would have risen relative to the median as they did in the period 1997-98 when there was no minimum wage, then we have what might be labelled a "double scaled" counterfactual. The quantile effect estimator using this can be viewed as a quantile difference-in-differences estimator for the quantile change.

Write the simple adjusted effect in equation (6) based on comparing years s and $s+1$ as

$$\Delta_s(\theta) = [F_{s+1}^{-1}(\theta) - F_s^{-1}(\theta)] - [F_{s+1}^{-1}(\eta) - F_s^{-1}(\eta)] \tag{21}$$

The further adjusted effect being proposed here using the "double scaled" counterfactual is then given by

$$\Delta_s^*(\theta) = \Delta_s(\theta) - \Delta_{97}(\theta) \tag{22}$$

Thus the counterfactual distribution is given by

$$\begin{aligned} F_{s+1}^{*-1}(\theta) \quad = \quad & F_s^{-1}(\theta) + [F_{s+1}^{-1}(\eta) - F_s^{-1}(\eta)] + \\ & [F_{98}^{-1}(\theta) - F_{97}^{-1}(\theta)] - [F_{98}^{-1}(\eta) - F_{97}^{-1}(\eta)] \end{aligned} \tag{23}$$

and the proposed quantile effect is given by

$$\Delta_s^*(\theta) = F_{s+1}^{-1}(\theta) - F_{s+1}^{*-1}(\theta) \tag{24}$$

The estimator is given by replacing each of the quantiles in (23) and (24) by their empirical counterparts.

As before this can be formulated in terms of quantile regressions. If we write the quantile regression in equation (5) for the θ-quantile estimated on pooled data for years s and $s+1$ as

$$Q_{\theta s}(w_{it}|D_{it}^{s+1}) = \alpha_s(\theta) + \beta_s(\theta)D_{it}^{s+1} \qquad t = s, s+1 \tag{25}$$

where $D_{it}^{\tau} = 1$ if $t = \tau$ and $D_{it}^{\tau} = 0$ otherwise, the estimate of $\beta_s(\theta)$ gives the quantile difference between years s and $s+1$ for the θ-quantile.

Then the proposed quantile effect estimator can be written as

$$\begin{aligned} \hat{\Delta}_s^*(\theta) &= [(\hat{\beta}_s(\theta) - \hat{\beta}_s(0.5)] - [(\hat{\beta}_{97}(\theta) - \hat{\beta}_{97}(0.5)] \\ &= [(\hat{\beta}_s(\theta) - (\hat{\beta}_{97}(\theta)] - [\hat{\beta}_s(0.5) - \hat{\beta}_{97}(0.5)] \end{aligned} \tag{26}$$

The quantile regressions for the separate pairs of years can be combined as

$$\begin{aligned} Q_{\theta s}^*(w_{it}|x_{it}) &= (\alpha_j^s + \beta_j^s D_{it}^{s+1})(1 - D_{it}^{97} - D_{it}^{98}) + (\alpha_j^{97} + \beta_j^{97} D_{it}^{98})(D_{it}^{97} + D_{it}^{98}) \\ &= \alpha_j^s + (\alpha_j^{97} - \alpha_j^s)(D_{it}^{97} + D_{it}^{98}) + (\beta_j^s - \beta_j^{97})D_{it}^{s+1} \\ &\qquad + \beta_j^{97}(D_{it}^{98} + D_{it}^{s+1}) \end{aligned} \tag{27}$$

This can be estimated as a quantile regression using data for years 1997, 1998, s and $s+1$ with $x_{it} = [D_{it}^{s+1}, (D_{it}^{97} + D_{it}^{98}), (D_{it}^{98} + D_{it}^{s+1})]'$. The coefficient on the first of these variables gives $[\hat{\beta}_s(\theta) - \hat{\beta}_{97}(\theta)]$. $\hat{\Delta}_s^*$ is then given by the difference between this and the equivalent estimate for the median (i.e. $\theta = 0.5$).

As for the simple counterfactual estimator in section 4.2, analytic or bootstrap standard errors can be constructed for this estimator. The derived expression for the asymptotic variance in section 4.3 can be extended to this estimator. It is important to distinguish two cases. For the upratings, the years involved in the estimation of $\Delta_s^*(\theta)$ do not involve 1997 or 1998 and we can estimate the required variances as

$$\text{var}(\Delta_s^*(\theta)) = \text{var}(\Delta_s(\theta)) + \text{var}(\Delta_{97}(\theta)) \tag{28}$$

However for the minimum wage introduction, we are comparing the 1998-99 and 1997-98 changes and so must take account of the covariance. Thus for $s = 1998$, there is an extra term to be included in the variance expression. Denote the variance of the difference for the single year s by

$$V_s(\theta) = \text{var}(F_s^{-1}(\theta) - F_s^{-1}(\eta)) \tag{29}$$

with this given by the expression in equation (11). Then $\text{var}(\Delta_{98}(\theta)) = V_{99}(\theta) + V_{98}(\theta)$, $\text{var}(\Delta_{97}(\theta)) = V_{98}(\theta) + V_{97}(\theta)$ and $\text{cov}(\Delta_{98}(\theta), \Delta_{97}(\theta)) = -V_{98}(\theta)$. Thus

$$\text{var}(\Delta_{98}^*(\theta)) = V_{99}(\theta) + V_{97}(\theta) + 4V_{98}(\theta) \tag{30}$$

Each component of this can be estimated using equation (11) and the methods described in section 4.4.

As before bootstrap standard errors can also be calculated with the quantile regressions for quantiles θ and 0.5 estimated simultaneously. Both are used and compared in section 6 below.

5 Results for the simple scaled counterfactual

In this section the estimator based on the simple scaled counterfactual of section 4.2 is used to investigate the impact of minimum wage increases on the wage distribution using the ASHE data described in section 3. The estimated effects are given in Table 2 for log wage percentiles for the minimum wage introduction. (Equivalent estimates for each of the upratings from 2000 to 2007 are given in the online appendix.) Table 2 therefore gives estimates of the effects defined in equation (6) for $\theta = j/100$ for integers j such that θ is in the range defined at the end of section 4.1. To simplify notation $\Delta_j = \Delta(\theta)$, $\theta = j/100$, is used to denote the effect at the j-th percentile.

Table 2 gives four different estimates of the standard errors and corresponding implied (one-sided) p-values for the tests of $\Delta_j > 0$. The literature has been concerned with the issue of positive spillovers. Hence the envisaged tests have a null hypothesis of no or negative spillovers. To this end one-sided p-values are given in the table. Three analytic asymptotic standard errors are presented based on the variance given in equation (13). The first two of these use the Siddiqui-Bloch-Gastwirth rectangular estimator of the sparsity function (equation (15)), one using the Bofinger bandwidth rule (equation (17)) and one using the Hall-Sheather rule (equation (19)). The third uses a kernel estimate of the quantile density function, with the Epanechnikov kernel and the Silverman bandwidth rule (equation (20)). The final standard error presented is the bootstrap estimate based on 1000 bootstrap replications.

For the case of a single quantile the bootstrap quantile variance estimator converges more slowly than the Siddiqui-Bloch-Gastwirth estimator and the coverage error of confidence intervals and the level error of hypothesis tests for population quantiles constructed using the bootstrap variance estimator are inferior to those based on the Siddiqui-Bloch-Gastwirth variance estimator with bandwidth chosen to minimize coverage / level error (Hall and Martin, 1991).

Figure 1 plots the changes in log percentiles for each year together with 95% confidence intervals based on the standard errors constructed using the Siddiqui-Bloch-Gastwirth estimator of the sparsity function and the Hall-Sheather bandwidth rule (SBG–HS). As explained at the end of section 4.1, testing for spillovers involves testing for $\Delta_j > 0$ for $j/100 > F_1(m_2)$. Tests are conducted here at percentile points that satisfy this condition up to the lower quartile. The lowest percentile satisfying this condition is shown by the vertical line on each of the graphs in Figure 1. Percentiles that do not satisfy this condition are excluded from Table 2.

To be clear about what these estimates show it is useful to explain Table 2 and the first graph in Figure 1, which relate to the introduction of the minimum wage, in some detail. The 4th and 5th percentiles of the 1998 wage distribution are respectively £3.50 and £3.61. Thus the 5th percentile is the first above the level at which the minimum wage was introduced in 1999. Thus the tests for spillovers start at the 5th percentile. The "compliance change" is zero for all the percentiles tested.

The median wage increased from £7.34 in 1998 to £7.68 in 1999, i.e. by 4.6%. The counterfactual being considered here is therefore that, in the absence of spillovers, wages at the 5th percentile and above would all have increased by 4.6%. The 5th percentile actually increased by 6.6% between 1998 and 1999, which is 2.0% above the increase in the median. Hence the estimate of Δ_5 given in Table 2 and shown in Figure 1 is 0.02.

To view this another way, the 5th percentile increased from £3.61 in 1998 to £3.85 in 1999. If it had increased in line with the percentage increase in the median (4.6%) it would only have increased to £3.78. Hence these results suggest a 7p spillover at the 5th percentile from the introduction of the minimum wage.

The four estimates of the standard error of $\hat{\Delta}_5$ in Table 2 are between 0.003 and 0.004. On the basis of any of these therefore $\hat{\Delta}_5$ is significantly greater than zero at standard levels. (The p-value is 0.000 for all four standard error estimates.)

Turning to the 6th percentile, there was an increase of 6.1% between 1998 and 1999, which is 1.5% above the increase in the median. (The estimate of Δ_6 given in Table 2 and shown in Figure 1 is 0.014.) The 6th percentile increased from £3.74 in 1998 to £3.97 in 1999. If it had increased in line with the percentage increase in the median it would only have increased to £3.91, a difference of 6p. Again all four estimates of the standard error of $\hat{\Delta}_6$ are between 0.003 and 0.004. On the basis of any of them $\hat{\Delta}_6$ is significantly greater than zero, with a p-value of 0.000 for all four standard error estimates.

Looking at the 7th percentile, it increased by 4.9%, i.e. only 0.3% above the increase in the median. (The estimate of Δ_7 given in Table 2 and shown in Figure 1 is 0.003.) The 7th percentile increased from £3.84 to £4.03. If it had increased in line with the percentage increase in the median it would have increased to £4.02, a difference of just 1p. The estimate of Δ_7 is not significantly greater than zero. (The p-value is 0.158 or higher for all four standard error estimates.) Thus there is evidence of significant spillovers at the

5th and 6th percentiles, but not at the 7th. Above that there is some evidence of significant estimates of Δ_j for $j = 9$, 10 and 12 at the 5% level, but not at the 1% level. For these three percentiles the estimate of Δ_j is about 0.005 and hence considerably smaller than for $j = 5$ and 6. The estimated effects in Table 2 use the estimator defined by equation (6). Adding controls for individual characteristics such as age, gender and industry changes the estimates very little.

For most percentiles there is a fairly good agreement between the four standard error estimates. The bootstrap standard error estimate is typically slightly larger than the other three. The average across the 21 percentiles in Table 2 is 0.0027 for each of the other three estimates and 0.0030 for the bootstrap estimates. Taking the SBG–HS standard error as the baseline for comparison, the average absolute percentage differences from it are 1.9% for the SBG–B standard error and 2.4% for the kernel standard error, but somewhat larger at 10.8% for the bootstrap standard error. The bootstrap standard error is in fact larger than the SBG–HS standard error for all 21 percentiles. Taking the SBG–HS standard error as the baseline for comparison again, the SBG–B standard error ranges from 7% below it to 3% above it and the kernel standard error ranges from 10% below to 7% above, but the bootstrap standard error ranges from 1% above to 21% above.

The estimates of Δ_j for the October 2000 uprating (the second graph in Figure 1) are all negative. There is no evidence in these estimates of positive spillover effects. The third graph in Figure 1 gives the estimates for the October 2001 uprating, which was the largest in percentage terms that there has been. The estimates here indicate a spillover effect of about 2% at the 5th percentile and of about 1% between the 6th and 11th percentiles. However, the strongest evidence of spillovers is probably in the estimates for the October 2002 uprating, which is surprising since it was the smallest increase in percentage terms in the period covered by the data (see also Swaffield, 2008). In this case the estimates indicate spillovers of between 1% and 2% up to about the 20th percentile. All are significantly greater than zero up to this percentile, with a p-value of 0.000 for all four standard error estimates.

The estimates for the October 2003 uprating imply spillovers of about 2% for the 4th, 5th and 6th percentiles. The evidence for spillover effects from the next four upratings is less clear and the estimated effects smaller. For the first three of these, there is some evidence of effects of about 1% for some percentiles, but it is not strong. For the 2007 uprating the effects are all negligible and insignificantly different from zero.

It should also be remembered that it is inherent in the identification strategy that the estimated effects may conflate any spillover effects with those of other policy changes if these other policies also have differential effects at low percentiles and the median. For example, if the introduction of the Working Time Regulations that limited hours of work had differential effects of this form on the wage distribution, then the 1998-99 estimates would incorporate that. If the introduction of paid paternity leave had differential effects of this form on the wage distribution, then the 2002-03 estimates would incorporate that. This is inherent in all estimators of this type.

As for the introduction of the minimum, there is a fairly good agreement between the four standard error estimates for most percentiles for each of the upratings. Taking the SBG–HS standard error as the baseline for comparison again, the average absolute percentage differences from it over the 8 upratings are 2.6% for the SBG–B standard error and 2.4% for the kernel standard error, similar to those above for the introduction. For the bootstrap standard error, average absolute percentage difference is still larger than for the other two standard errors, but at 5.7% is only about half what it was for the introduction.

The assumption made for the estimates presented in this section is that, in the absence of spillovers, wages above the new minimum would all have risen in line with the proportional rise in the median. The remainder of this section and the next section examine whether this is an appropriate counterfactual.

Consideration is given first to the sensitivity of the results to the choice of comparator quantile, η. The estimates above use $\eta = 0.5$. However it may be that $\eta = 0.5$ is rather too high up the distribution for an appropriate comparison when investigating minimum wage spillovers. The characteristics of those at the median may be rather different to those in receipt of the minimum wage or subject to its spillover effects.

One can think of the double scaling of section 4.5 as one way of accounting for this. Results are given for this in the next section. Alternatively one can consider alternative choices of η. However the choice of the most suitable comparator is hard to judge. The optimal choice would be a quantile just slightly above that where any spillovers run out. There is a trade-off here. If one chooses η too high (as one might argue $\eta = 0.5$ to be), then the similarity of characteristics of individuals at the points of comparison is weakened. On the other hand if one chooses η too low, then the comparator quantile (η) might itself be affected by spillovers. Much of the spillovers literature asks how far up the distribution spillover effects reach. The comparison here seeks to choose a counterfactual adjusting quantile that is beyond this point so that we can be confident that it is uncontaminated by spillovers, but not so far beyond that comparability is lost.

The same estimator based on the simple scaled counterfactual that was used for the estimates so far in this section is used next with alternative choices of η. Table 3 gives estimates for the minimum wage introduction using different choices of η ranging from 0.25 to 0.5. (Equivalent estimates for each of the upratings from 2000 to 2007 are given in the online appendix.) The counterfactual assumption is that, in the absence of spillovers, wages above the new minimum would have risen in line with the proportional rise in the chosen percentile, with this ranging from the 25th to the 50th. Figure 2 plots the changes in log percentiles for each year and their confidence intervals corresponding to Figure 1, but with the 30th percentile used as the counterfactual comparator. (Corresponding figures for the other comparators are given in the online appendix.)

The results for the 45th percentile comparator in Table 3 are similar to those for the median. The estimated effects increase slightly for percentiles 5 to 12. Those for the 40th and 35th percentile comparators are then similar to these. Those for the 30th percentile comparator are again slightly increased for percentiles 5 to 12. When the 25th percentile is used for the comparator the estimated effects for percentiles 5 to 12 fall back again, to below those for the median.

Thus in Figure 2 there is slightly more evidence of spillover effects from the introduction of the minimum wage (the first graph in the figure) than in Figure 1, but the differences are slight. Turning to the other years, in contrast to this, the evidence of spillovers is slightly reduced for the 2001, 2002 and 2004 upratings.

6 Results for the double scaled counterfactual

To consider the question of the appropriate counterfactual, it is useful to look at the proportional changes in wage percentiles relative to the median for 1997-98, when there was no minimum in place in either year, and for 1999-2000, when there was no uprating. The former are given in Table 4, together with their standard errors and implied p-values. (The equivalent for the latter is given in the online appendix.)

In both these years there is evidence of significant positive relative changes relative to the changes in the median of around 1% or slightly more: up to about the 9th percentile in the case of 1997-98 and about the 11th in the case of 1999-2000. Thus in these years these percentiles went up slightly more than the median rather than in line with it. One possible explanation for this is as a manifestation of Galtonian regression towards the mean. These findings call into question the standard simple counterfactual used above and in the previous literature that the analysis here builds on.

Figure 3 therefore presents the equivalent adjusted proportional changes in wage percentiles to those given in Figure 1, but taken relative to the corresponding changes in 1997-98, a period completely prior to the introduction of the minimum wage. That is estimates of the effects $\Delta_s^*(\theta)$ defined by equations (23) and (24) in section 4.5. The estimates for the first graph in Figure 3, the introduction of the minimum wage, are tabulated in Table 5 together with their standard errors and corresponding implied test statistic p-values. (Equivalent estimates for the upratings from 2000 to 2007 are given in the online appendix.) The counterfactual in this case, rather than being that all wages would have gone up in line with the median, is that a given percentile would have risen relative to the median as it did in 1997-98, when there was no minimum wage. This is the "double scaled" counterfactual described in section 4.5.

The evidence for spillover effects is much less under this counterfactual assumption. For the introduction of the minimum wage the estimated spillover effect at the 5th percentile is 1.6% with a p-value of between 0.003 and 0.013 depending on which standard error estimate is used. For the 6th percentile upwards the estimates are negligible and insignificantly different from zero, for all four standard error estimates, or negative. As for the simple scaled counterfactual, adding controls for age, gender and industry changes the estimates very little. The estimate for the 5th percentile falls slightly from 0.0156 to 0.0142, but remains significantly greater than zero. Those for the 6th percentile onwards remain insignificant.

For the October 2001 uprating the estimated spillover effect at the 5th percentile is 1.6% with a p-value of 0.001 or below. Above that the effects are mostly (but not all) insignificantly different from zero. Compared to those under the simple counterfactual assumption in the previous section, these show both a reduction in the magnitude of the estimated effect at the 5th percentile and a loss of statistical significance at the 6th percentile in 1999 and for the 6th to 11th percentiles for 2001.

As in the previous section, the estimated effects for the October 2000 uprating are all negative. The effects of the 2002 uprating are still significantly greater than zero at the 5% significance level for most of the percentiles considered, but typically reduced in magnitude from those in Figure 1. Typically these spillovers are now of around 1% and stretching quite a way up the distribution. Just under half of the estimated effects are significantly greater than zero at the 1% level for at least one of the standard error estimates, and less than one third (7 out of 23) are for all four of the standard error estimates. Never-the-less it is again the 2002 uprating that shows the most evidence of spillover effects.

The estimates for the October 2003 uprating imply a significant spillover at the 5th percentile, but not at the 4th or 6th percentiles, for which there were highly significant effects with the counterfactual used in the previous section, or any of the higher percentiles. There is little evidence of systematic spillover effects relative to this counterfactual for the upratings that took place in 2004 to 2007 inclusive. Overall, for all years, there is much less evidence of spillovers under this counterfactual assumption than under the simpler counterfactual assumption in the previous section.

7 Conclusions

This paper presents an examination of a method for estimating the effects of a policy change on an outcome distribution that uses a comparator quantile rather than a control group. In particular, it provides methods for estimating the variances of these estimators.

The empirical analysis conducted in the paper estimates the "spillover" effects of increases in the UK national minimum wage. Such spillover effects are important for a number of reasons, discussed in Section 2. Much research on the effects of the minimum wage on various outcomes has been conducted using a difference-in-differences approach with a group initially just above the increased minimum used as the "control" group under the assumption that they are not affected by the rise in the minimum, and in particular that their wages are not affected by spillover effects.

Spillover effects are estimated in this paper by comparing percentiles of the observed wage distribution after an increase in the minimum wage with those of an estimated counterfactual distribution of wages at the same date if the minimum wage had not been increased. This counterfactual wage distribution is constructed by making hypothesised adjustments to the observed wage distribution before the increase.

The results presented indicate that the conclusions about minimum wage spillovers are sensitive to the assumptions made to construct the counterfactual distribution. The first simple counterfactual used assumes that in the absence of an increase in the minimum wage all wages would have risen in line with the observed growth in the median. Under this assumption, the introduction of the minimum wage in 1999 produced significant spillovers at the 5th and 6th percentiles, while the 2001 uprating produced spillovers between the 5th and 11th percentiles that were significant. Somewhat surprisingly the strongest evidence of spillovers is found in the estimates for the October 2002 uprating, up to about the 20th percentiles. However significant positive proportional changes relative to that in the median are also found in some lower percentiles for the two "no change" years examined when either the minimum wage had not yet been introduced or when it did not change. This casts doubt on the assumption underlying the simple scaled counterfactual.

An alternative "double scaled" counterfactual in which each percentile would have risen relative to the median as it did in 1997-98, when there was no minimum wage, is proposed. Based on this counterfactual distribution the evidence of spillover effects is much reduced. Once again the most evidence of spillovers is found for the October 2002 uprating, but the significant estimated effects are now rather smaller at only about 1%. A significant spillover effect is found at the 5th percentile for the 1999 introduction and the 2001 uprating based on this counterfactual distribution, but not above that.

The overall conclusion of this paper is that the evidence on minimum wage spillover effects depends on the counterfactual distribution assumed. Evidence presented calls into question the assumption underlying the simple scaled counterfactual. On the basis of the "double scaled" counterfactual proposed in this paper any spillovers are small – about 1% at most – and, apart from the October 2002 uprating, typically do not reach above the 5th. percentile.

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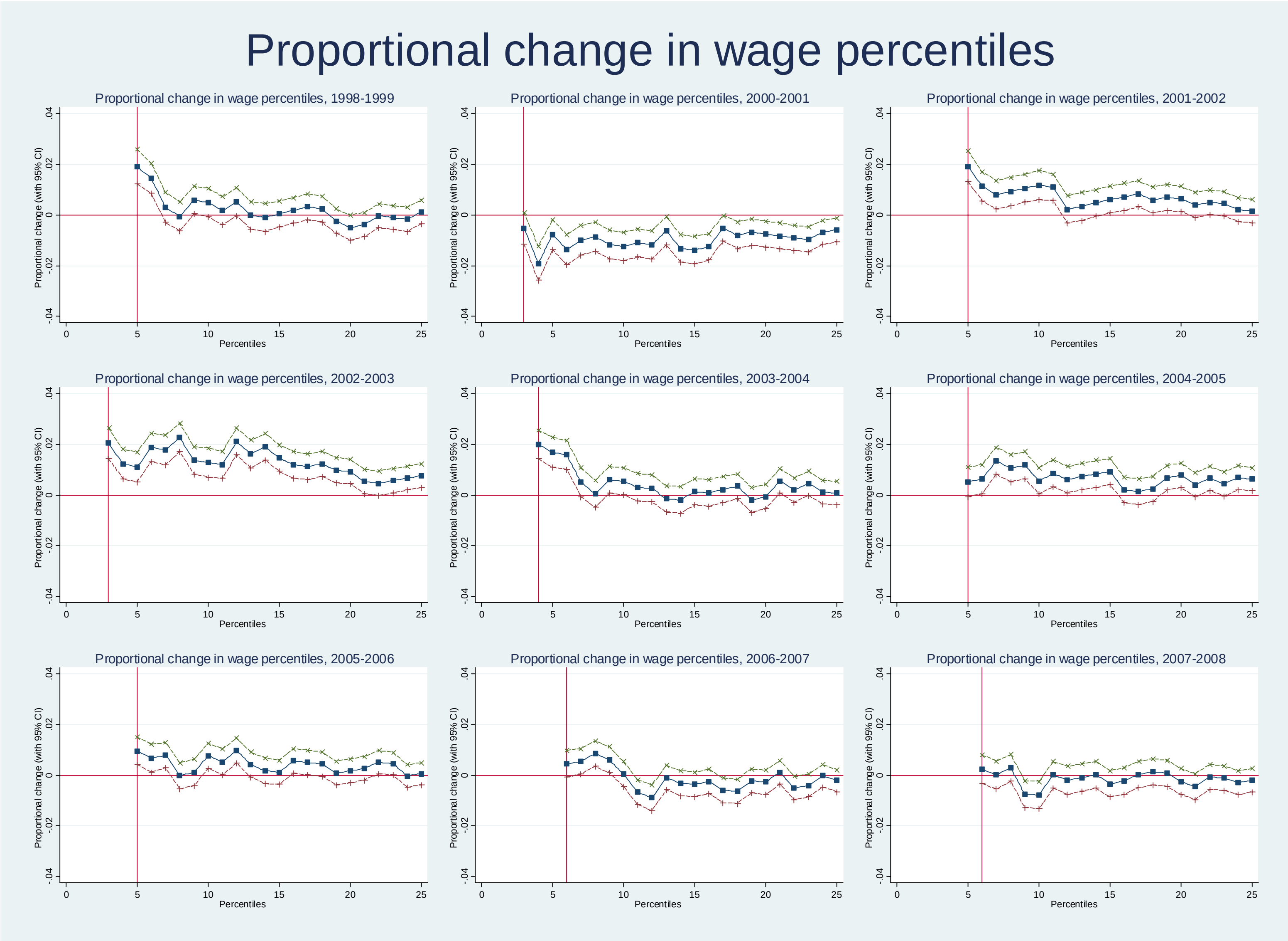

Figure 1
Estimated spill-over effects and 95% confidence intervals – simple scaled counterfactual

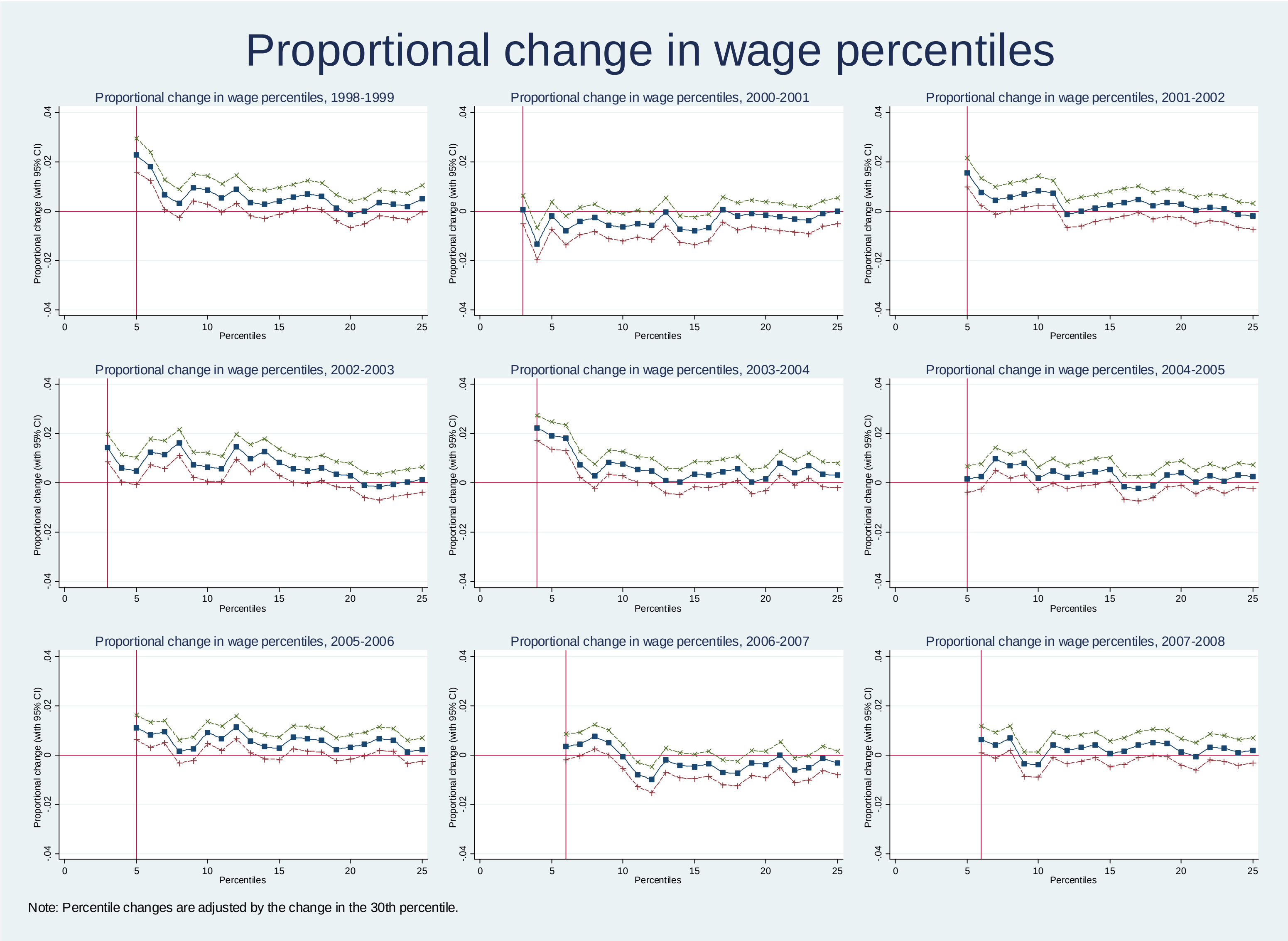

Figure 2

Estimated spill-over effects and 95% confidence intervals – using the 30th percentile as comparator

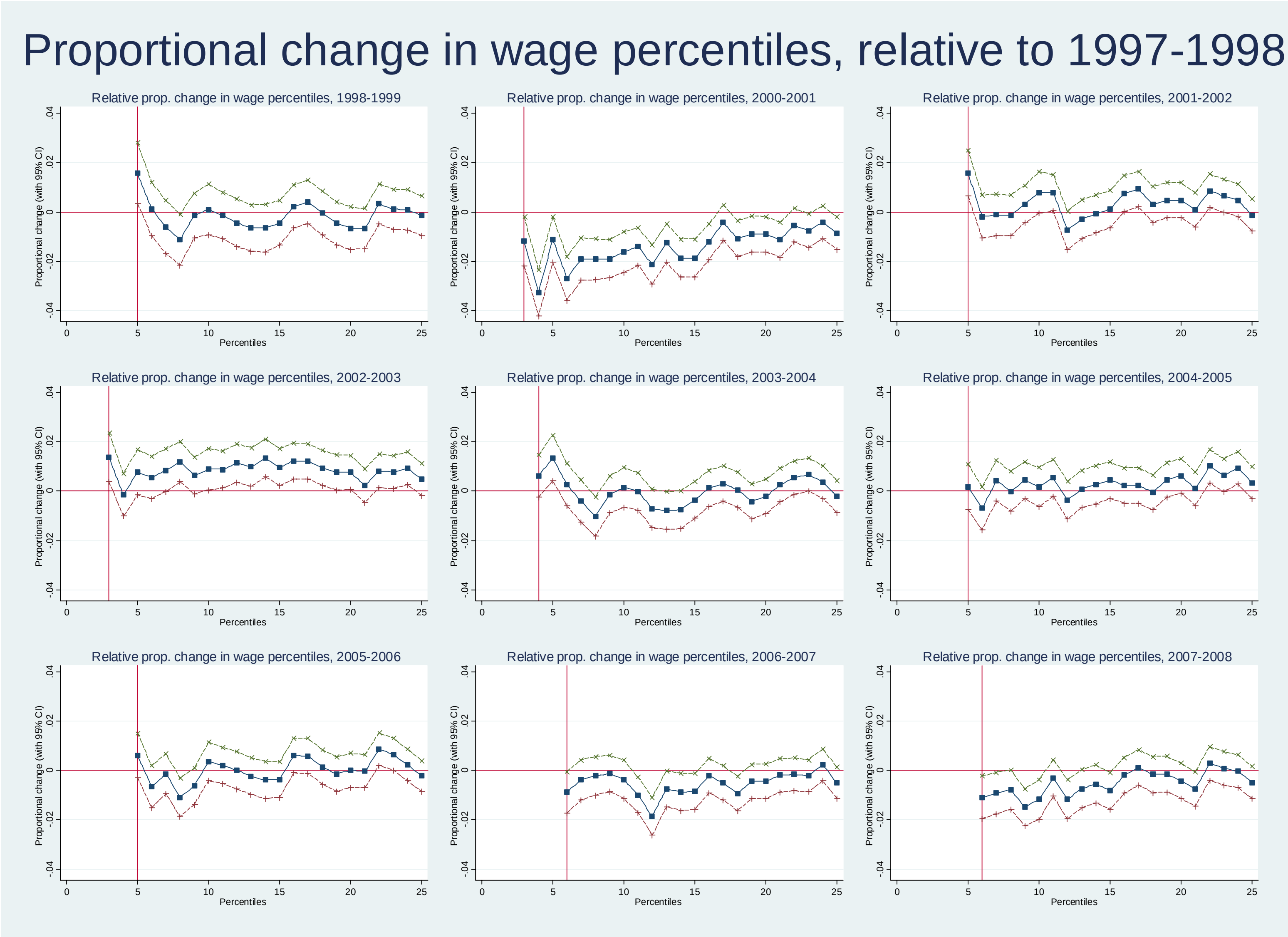

Figure 3
Estimated spill-over effects and 95% confidence intervals – “double scaled” counterfactual

Table 1

The UK National Minimum Wage - rates of increase and comparisons

	Adult NMW (£) [1]	% increase in NMW [2]	% increase in median (ASHE, April) [3]	% increase in AEI [4]	% increase in RPI [5]
April 1999	3.60				
Oct 2000	3.70	2.8%	3.0%	4.1%	2.6%
Oct 2001	4.10	10.8%	5.0%	4.1%	1.6%
Oct 2002	4.20	2.4%	4.0%	3.6%	2.1%
Oct 2003	4.50	7.1%	3.9%	3.8%	2.6%
Oct 2004	4.85	7.8%	3.7%	4.8%	3.3%
Oct 2005	5.05	4.1%	3.4%	3.2%	2.5%
Oct 2006	5.35	5.9%	3.7%	4.3%	3.7%
Oct 2007	5.52	3.2%	3.3%	3.6%	4.2%

Notes:
Column [3]: Median hourly pay excluding overtime, April of each year: ASHE (Table 1.6a). Employees on adult rates whose pay for survey pay-period was not affected by absence.
Column [4]: Average Earnings Index, whole economy, SA, including bonuses.
Column [5]: Retail Prices Index, all items index.

Table 2

Relative change in log wage percentiles, 1998-1999

ptile j	$\hat{\Delta}_j$	SBG–B SE [p-val]	SBG–HS SE [p-val]	Kernel SE [p-val]	Bootstrap SE [p-val]
5	0.0191	0.00326 [0.000]	0.00352 [0.000]	0.00317 [0.000]	0.00357 [0.000]
6	0.0144	0.00306 [0.000]	0.00303 [0.000]	0.00308 [0.000]	0.00336 [0.000]
7	0.0030	0.00301 [0.158]	0.00303 [0.160]	0.00304 [0.160]	0.00336 [0.185]
8	-0.0006	0.00287 [0.584]	0.00292 [0.582]	0.00298 [0.581]	0.00295 [0.582]
9	0.0059	0.00282 [0.018]	0.00274 [0.016]	0.00295 [0.023]	0.00297 [0.024]
10	0.0049	0.00279 [0.039]	0.00291 [0.046]	0.00291 [0.046]	0.00322 [0.063]
11	0.0017	0.00281 [0.275]	0.00284 [0.277]	0.00287 [0.280]	0.00308 [0.293]
12	0.0052	0.00279 [0.031]	0.00280 [0.032]	0.00282 [0.033]	0.00305 [0.044]
13	-0.0001	0.00277 [0.518]	0.00272 [0.519]	0.00279 [0.518]	0.00312 [0.516]
14	-0.0009	0.00269 [0.635]	0.00280 [0.630]	0.00276 [0.632]	0.00294 [0.624]
15	0.0004	0.00267 [0.436]	0.00263 [0.435]	0.00272 [0.437]	0.00283 [0.440]
16	0.0018	0.00265 [0.244]	0.00257 [0.237]	0.00269 [0.247]	0.00305 [0.273]
17	0.0032	0.00258 [0.107]	0.00262 [0.111]	0.00265 [0.114]	0.00292 [0.136]
18	0.0024	0.00256 [0.174]	0.00259 [0.178]	0.00261 [0.179]	0.00299 [0.211]
19	-0.0024	0.00250 [0.829]	0.00250 [0.829]	0.00258 [0.821]	0.00256 [0.823]
20	-0.0050	0.00249 [0.978]	0.00251 [0.978]	0.00254 [0.976]	0.00295 [0.956]
21	-0.0037	0.00246 [0.935]	0.00242 [0.938]	0.00250 [0.932]	0.00284 [0.905]
22	-0.0003	0.00242 [0.551]	0.00241 [0.551]	0.00247 [0.550]	0.00291 [0.542]
23	-0.0010	0.00244 [0.659]	0.00242 [0.660]	0.00243 [0.660]	0.00279 [0.640]
24	-0.0017	0.00237 [0.760]	0.00243 [0.754]	0.00239 [0.758]	0.00281 [0.724]
25	0.0013	0.00233 [0.285]	0.00237 [0.288]	0.00235 [0.286]	0.00249 [0.297]

Notes:
Standard errors:
SBG–B: Siddiqui-Bloch-Gastwirth estimates of sparsity function, with Bofinger bandwidth rule.
SBG–HS: Siddiqui-Bloch-Gastwirth estimates of sparsity function, with Hall-Sheather bandwidth rule.
Kernel: Kernel estimates of quantile density function, Epanechnikov kernel, Silverman bandwidth rule.
Bootstrap: Bootstrap estimates of variance-covariance matrix, using 1000 bootstrap replications.

Table 3

Relative change in log wage percentiles for different comparators, 1998-1999

ptile	comparator: Q50	comparator: Q45	comparator: Q40	comparator: Q35	comparator: Q30	comparator: Q25
5	0.0191 [0.000]	0.0203 [0.000]	0.0203 [0.000]	0.0206 [0.000]	0.0228 [0.000]	0.0178 [0.000]
6	0.0144 [0.000]	0.0156 [0.000]	0.0156 [0.000]	0.0159 [0.000]	0.0181 [0.000]	0.0131 [0.000]
7	0.0030 [0.160]	0.0042 [0.081]	0.0042 [0.083]	0.0045 [0.069]	0.0067 [0.014]	0.0017 [0.299]
8	-0.0006 [0.582]	0.0006 [0.418]	0.0006 [0.416]	0.0009 [0.383]	0.0031 [0.147]	-0.0019 [0.734]
9	0.0059 [0.016]	0.0071 [0.004]	0.0071 [0.005]	0.0074 [0.003]	0.0096 [0.000]	0.0046 [0.059]
10	0.0049 [0.046]	0.0061 [0.017]	0.0061 [0.019]	0.0064 [0.015]	0.0086 [0.002]	0.0036 [0.126]
11	0.0017 [0.277]	0.0029 [0.154]	0.0029 [0.158]	0.0032 [0.136]	0.0054 [0.032]	0.0004 [0.455]
12	0.0052 [0.032]	0.0064 [0.011]	0.0064 [0.013]	0.0067 [0.010]	0.0089 [0.001]	0.0039 [0.104]
13	-0.0001 [0.519]	0.0011 [0.345]	0.0011 [0.346]	0.0014 [0.313]	0.0036 [0.102]	-0.0015 [0.687]
14	-0.0009 [0.630]	0.0003 [0.460]	0.0003 [0.458]	0.0006 [0.425]	0.0028 [0.174]	-0.0023 [0.765]
15	0.0004 [0.435]	0.0016 [0.268]	0.0017 [0.271]	0.0019 [0.241]	0.0041 [0.068]	-0.0009 [0.620]
16	0.0018 [0.237]	0.0030 [0.119]	0.0031 [0.125]	0.0033 [0.106]	0.0055 [0.020]	0.0005 [0.430]
17	0.0032 [0.111]	0.0044 [0.048]	0.0044 [0.054]	0.0047 [0.045]	0.0069 [0.007]	0.0019 [0.265]
18	0.0024 [0.178]	0.0036 [0.085]	0.0036 [0.092]	0.0039 [0.078]	0.0061 [0.014]	0.0011 [0.359]
19	-0.0024 [0.829]	-0.0012 [0.677]	-0.0011 [0.667]	-0.0009 [0.633]	0.0013 [0.313]	-0.0037 [0.901]
20	-0.0050 [0.978]	-0.0038 [0.933]	-0.0038 [0.923]	-0.0036 [0.908]	-0.0014 [0.690]	-0.0064 [0.985]
21	-0.0037 [0.938]	-0.0025 [0.847]	-0.0025 [0.834]	-0.0023 [0.809]	-0.0001 [0.508]	-0.0051 [0.963]
22	-0.0003 [0.551]	0.0009 [0.358]	0.0009 [0.360]	0.0012 [0.327]	0.0034 [0.103]	-0.0016 [0.715]
23	-0.0010 [0.660]	0.0002 [0.467]	0.0002 [0.464]	0.0005 [0.428]	0.0027 [0.162]	-0.0023 [0.787]
24	-0.0017 [0.754]	-0.0005 [0.573]	-0.0004 [0.566]	-0.0002 [0.529]	0.0020 [0.233]	-0.0030 [0.844]
25	0.0013 [0.288]	0.0025 [0.152]	0.0026 [0.162]	0.0028 [0.145]	0.0050 [0.033]	0.0000 [0.500]

Notes:
Standard errors use Siddiqui-Bloch-Gastwirth estimates of sparsity function, with Hall-Sheather bandwidth rule.

Table 4

Relative change in log wage percentiles in "no-change" year, 1997-1998

ptile j	$\hat{\Delta}_j$	SBG–B SE [p-val]	SBG–HS SE [p-val]	Kernel SE [p-val]	Bootstrap SE [p-val]
1	0.0618	0.01236 [0.000]	0.01178 [0.000]	0.00711 [0.000]	0.01263 [0.000]
2	0.0127	0.00488 [0.005]	0.00401 [0.001]	0.00403 [0.001]	0.00440 [0.002]
3	0.0068	0.00390 [0.042]	0.00396 [0.044]	0.00361 [0.031]	0.00431 [0.058]
4	0.0137	0.00347 [0.000]	0.00327 [0.000]	0.00344 [0.000]	0.00356 [0.000]
5	0.0035	0.00337 [0.150]	0.00362 [0.168]	0.00331 [0.146]	0.00449 [0.219]
6	0.0133	0.00326 [0.000]	0.00334 [0.000]	0.00322 [0.000]	0.00392 [0.000]
7	0.0092	0.00323 [0.002]	0.00325 [0.002]	0.00314 [0.002]	0.00381 [0.008]
8	0.0107	0.00295 [0.000]	0.00304 [0.000]	0.00308 [0.000]	0.00373 [0.002]
9	0.0074	0.00290 [0.006]	0.00270 [0.003]	0.00305 [0.008]	0.00333 [0.013]
10	0.0039	0.00279 [0.079]	0.00307 [0.099]	0.00299 [0.094]	0.00380 [0.150]
11	0.0032	0.00288 [0.136]	0.00268 [0.120]	0.00295 [0.142]	0.00348 [0.182]
12	0.0097	0.00284 [0.000]	0.00291 [0.000]	0.00291 [0.000]	0.00349 [0.003]
13	0.0064	0.00283 [0.012]	0.00282 [0.012]	0.00286 [0.013]	0.00346 [0.032]
14	0.0056	0.00284 [0.024]	0.00286 [0.025]	0.00282 [0.023]	0.00361 [0.060]
15	0.0049	0.00275 [0.039]	0.00274 [0.038]	0.00277 [0.040]	0.00335 [0.074]
16	-0.0003	0.00271 [0.551]	0.00259 [0.553]	0.00273 [0.550]	0.00322 [0.543]
17	-0.0009	0.00264 [0.633]	0.00258 [0.636]	0.00269 [0.630]	0.00317 [0.611]
18	0.0029	0.00260 [0.132]	0.00261 [0.133]	0.00266 [0.137]	0.00337 [0.195]
19	0.0022	0.00254 [0.189]	0.00260 [0.194]	0.00262 [0.196]	0.00307 [0.233]
20	0.0016	0.00254 [0.265]	0.00255 [0.266]	0.00257 [0.268]	0.00306 [0.301]
21	0.0031	0.00251 [0.110]	0.00245 [0.105]	0.00254 [0.113]	0.00313 [0.163]
22	-0.0035	0.00246 [0.925]	0.00244 [0.927]	0.00250 [0.922]	0.00304 [0.878]
23	-0.0020	0.00243 [0.798]	0.00239 [0.802]	0.00246 [0.795]	0.00309 [0.744]
24	-0.0026	0.00239 [0.859]	0.00238 [0.859]	0.00242 [0.856]	0.00285 [0.816]
25	0.0029	0.00234 [0.112]	0.00233 [0.111]	0.00238 [0.115]	0.00300 [0.171]

Notes: See Table 2

Table 5

Relative change in log wage percentiles, 1998-1999 (relative to 1997-98)

ptile		SBG–B	SBG–HS	Kernel	Bootstrap
j	$\hat{\Delta}_j^*$	SE [p-val]	SE [p-val]	SE [p-val]	SE [p-val]
5	0.0156	0.00577 [0.003]	0.00629 [0.007]	0.00563 [0.003]	0.00699 [0.013]
6	0.0011	0.00551 [0.418]	0.00552 [0.418]	0.00546 [0.417]	0.00613 [0.426]
7	-0.0062	0.00543 [0.873]	0.00547 [0.872]	0.00535 [0.877]	0.00620 [0.841]
8	-0.0113	0.00501 [0.988]	0.00521 [0.985]	0.00524 [0.984]	0.00594 [0.972]
9	-0.0015	0.00491 [0.618]	0.00463 [0.625]	0.00519 [0.612]	0.00562 [0.604]
10	0.0010	0.00480 [0.420]	0.00523 [0.427]	0.00511 [0.425]	0.00620 [0.438]
11	-0.0015	0.00493 [0.618]	0.00478 [0.622]	0.00504 [0.616]	0.00553 [0.605]
12	-0.0045	0.00488 [0.821]	0.00495 [0.817]	0.00496 [0.817]	0.00559 [0.789]
13	-0.0065	0.00484 [0.911]	0.00480 [0.913]	0.00489 [0.909]	0.00582 [0.869]
14	-0.0065	0.00475 [0.916]	0.00492 [0.908]	0.00482 [0.912]	0.00579 [0.871]
15	-0.0044	0.00466 [0.828]	0.00461 [0.831]	0.00475 [0.824]	0.00545 [0.791]
16	0.0022	0.00461 [0.318]	0.00442 [0.311]	0.00469 [0.321]	0.00533 [0.341]
17	0.0041	0.00451 [0.182]	0.00451 [0.182]	0.00462 [0.188]	0.00555 [0.230]
18	-0.0005	0.00445 [0.545]	0.00455 [0.544]	0.00456 [0.544]	0.00572 [0.535]
19	-0.0046	0.00434 [0.856]	0.00444 [0.850]	0.00449 [0.848]	0.00512 [0.816]
20	-0.0066	0.00436 [0.936]	0.00441 [0.934]	0.00442 [0.933]	0.00532 [0.894]
21	-0.0068	0.00429 [0.944]	0.00415 [0.950]	0.00436 [0.941]	0.00537 [0.897]
22	0.0032	0.00421 [0.221]	0.00418 [0.219]	0.00429 [0.226]	0.00497 [0.258]
23	0.0010	0.00421 [0.404]	0.00414 [0.402]	0.00423 [0.404]	0.00529 [0.423]
24	0.0009	0.00411 [0.414]	0.00419 [0.416]	0.00416 [0.415]	0.00495 [0.428]
25	-0.0015	0.00403 [0.647]	0.00408 [0.646]	0.00409 [0.645]	0.00471 [0.627]

Notes: See Table 2