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Effects of Increasing Minimum Wages on Employment and Hours: Evidence from Sweden's Retail Sector

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Abstract

This paper examines the effects of collectively agreed increases in real minimum wages on employment transitions and hours among manual workers in the Swedish retail sector over the period 2001–05. The findings indicate that increases in real minimum wages are associated with more separations, whereas hours are less affected because separated workers put in relatively fewer hours before being separated. Among the young, however, both employment and hours are negatively affected. Labour-labour substitution seems to be important, since increases in minimum wages promote employment among workers with higher wages than those directly affected by the increases. The assumptions of the econometric model were tested by imposing fictitious minimum wages on lower-level non-manuals in the same industry, with turnover characteristics similar to those of manuals but covered by a different collective agreement with non-binding actual minimum wages.

Keywords: Minimum wages; Labour-labour substitution; Employment
JEL codes: J23; J21; J31

1. Introduction

Minimum wages in Sweden and the other Nordic countries are not regulated by law, but are subject to industry-wide bargaining between employers and trade unions. Since the coverage of collective agreements is high, the negotiated minimum wages effectively serve as wage floors, much in the way as a statutory minimum would. The collective agreements tend to also cover workers with atypical employment, such as those in fixed-term and part-time jobs. Minimum wage structures are highly complex, with different rates depending on, for example, age, occupation and experience.

Negotiated minimum rates in the Nordic countries are among the highest in the world, both in absolute terms and in relation to median wages. In 2006, the Swedish minimum wage bite for unskilled workers in selected industries ranged between 61 and 71 per cent of median wages in the private sector, while the bite reached 63–73 per cent in Norway and 56–57 per cent in Finland (Skedinger, 2008). The corresponding figures for other countries, with a statutory minimum, are considerably lower in most cases. Minimum wage policies in the Nordic countries have contributed to the fact that few workers earn low wages and the virtual absence of the "working poor" phenomenon (Skedinger, 2010).

The purpose of this study is to examine the effects of minimum wages on employment and working hours in the Swedish retail sector, an industry with binding minimum wages and employing relatively many young workers, part-timers and females. The study exploits payroll data for all unskilled manual workers covered by the industry collective agreement during 2001–08. Real minimum wages increased considerably over this period.

The econometric approach in this paper relies on the identification of workers affected by minimum wage changes, depending on their position in the wage distribution, and contrasts outcomes for these workers to those for unaffected workers, with slightly higher wages. The hypothesis in the standard competitive model of the labour market is that increasing minimum wages contribute to more separations from employment in the affected group and that decreasing minimum wages are associated with more job accessions, while total hours worked should decrease (increase) with increasing (decreasing) minimum wages. Relaxing the assumption that efficiency units are the product of hours and workers, Strobl and Walsh (2010) show that effects of minimum wages on employment and total hours are ambiguous in a competitive model framework. The predictions regarding changes in hours among remaining

workers (in case of an increasing minimum) and already employed workers (in case of a decrease) are ambiguous, even in the standard competitive model.

The econometric approach is similar in spirit to the ones used by, for example, Currie and Fallick (1996), Zavodny (2000) and Neumark et al. (2004) for the US, Abowd et al. (2000) for the US and France, Kramarz and Philippon (2001) for France, Stewart (2004) for the UK and Skedinger (2006) for Sweden. With the exception of Zavodny (2000) and Abowd et al. (2000) for the US (but not France), and Stewart (2004), these studiers find that minimum wages create disemployment effects. Few studies have attempted to estimate the effects of minimum wages on hours and the results are mixed. The results in Couch and Wittenburg (2001) and Stewart and Swaffield (2008) suggest an hours-reducing effect of increasing minimum wages in the US and the UK, respectively, while Böckerman and Uusitalo (2009), Zavodny (2000) and, for the UK, Connolly and Gregory (2002) and Dickens et al. (2009) find small or no significant changes in hours worked.

Despite the obvious potential for strong disemployment effects of minimum wages that cut large into the wage distribution, very few studies on the subject exist in the Nordic countries. Skedinger (2006) finds that increasing minimum wages decrease employment in Swedish hotels and restaurants during the period 1979–99. The introduction of special and lower rates for teenagers in 1993 eliminated negative employment effects of subsequent increases for this particular group of workers. For Finland, Böckerman and Uusitalo (2009) are unable to find any employment-increasing effects of the introduction of sub-minimum wages for young workers in the retail sector in 1993–95. The different results for Sweden and Finland may be due to the fact that the Finnish minimum wage reform was of a temporary nature, while the lower rates for teenagers in Sweden were permanent.

A crucial assumption of the chosen econometric approach is that transitions in and out of employment and changes in hours do not differ between affected (the treatment group) and unaffected workers (the control group) beyond what is captured by available controls, such as worker and firm characteristics. If unobserved variables, correlated with minimum wage changes, contribute to, for example, fewer transitions among high wage workers than among workers with lower pay, this assumption is violated. For example, one could imagine that turnover is higher among low-paid workers for structural reasons. Unlike previous studies, I am able to check the robustness of the estimates in this respect by considering, as an alternative control

group, workers in the same industry with the same wages as the treatment group, and with similar turnover characteristics, but who are *not* covered by the minimum wages. Lower-level non-manual workers are covered by a different agreement, with lower and non-binding minimum wages, and thus seem suitable for examining differences in structural turnover across groups in the wage distribution.

The data set is rich and contains detailed information on various wage components, age, gender, occupation, region, various measures of hours worked and firm size. Data on minimum wages have been added from the collective agreements. Many manual workers in Swedish retail are on part-time schedules, so there should be a potential for adjustment through changing hours in this industry.

The following section of this paper discusses the fixing mechanisms of minimum wages in the retail industry and the evolution of minimum wages over time. The data set is described in Section 3, while the econometric model and the results are presented in Sections 4 and 5, respectively. Section 6 concludes the paper.

2. Minimum wages in the retail trade

In the Swedish retail sector, minimum wages for manual workers are determined in collective agreements between the Commercial Employees' Union (*Handelsanställdas förbund*) and the Swedish Trade Federation (*Svensk Handel*). The agreements specify contractual wage increases as well as minimum wage levels at the industry level for various categories of workers. Since minimum wages are never bargained over at the firm level, it seems reasonable to assume that these rates are exogenous to the firm in its decisions on hirings and firings.

The agreement period varies and has been 1 year during the period 1 April 1998 – 31 March 2001 and 3 years for agreements concluded since 1 April 2001. Regardless of contract length, contractual wage increases and minimum wage levels are determined for each year during the period.

Minimum rates are specified on hourly as well as monthly bases and are conditional on age and experience. Different rates apply for workers aged 16, 17, 18 and 19 or older. For workers aged 18 or older who have acquired industry-specific experience, rates increase with 1, 2 or 3 or more years of such experience. Thus seven different minimum rates apply for manual workers in the retail sector. During unsocial

hours, all basic wages, including the minimum rates, are subject to a premium between 50 and 100 per cent, depending on when the work is carried out.[1]

Figure 1 displays the evolution of various hourly minimum wages, for regular working hours, deflated by the output price in retail, for manual workers aged 18 or older over the period 1998–2008.[2] Real minimum rates have increased considerably: The minimum, in 2008 prices, for an 18-year-old stood at SEK 70.84 in 1998 and rose to 94.90 in 2008, which represents an increase of 34 per cent in real terms.[3] For a worker aged 19 or older with at least 3 years' experience, the increase was 25 per cent, from SEK 88.33 in 1998 to 110.14 in 2008. Only in one year, 2008, real minimum rates did not rise and this is due to a fairly large price hike (6.6 per cent) in this particular year. Over the period taken as a whole, output prices increased at the relatively modest rate of 14.8 per cent.

The minimum wage bite is presented in Figure 2. The bites are large and have continued to increase over time for most worker categories to reach 85 per cent of the industry median wage for 18-year-olds and even as high as 99 per cent for workers on the highest minimum**.** The bite has increased to varying degrees and much less than real minimum wages, from 1 percentage point for the highest minimum wage group to 6 percentage points for the lowest, so that the minimum wage structure has become more compressed over time. The rising median wages that lie behind this development reflect the positive growth in the industry over the period, fuelled by rising disposable incomes and increasing private consumption. Figure 3 shows the development of employment and hours per worker over the period 2000–09, in retail and in the economy as a whole. In retail, employment has increased at a rather steady rate. After having decreased in the first half the decade, hours per worker returned to or surpassed its initial level by the end of the period

In order to further illustrate how the minimum wage system in retail works over time, Figure 4 depicts the steep "minimum wage path" facing an imaginary 18-year-old worker, hired at three different points in time, 1998, 2001 and 2004. An 18-year-old hired in, say 1998, is subject to the minimum wage of a 19-year-old with 1 year's experience in 1999, and so on. Assuming the minimum wage employee is hired

[1] The premium is 50 per cent for work on Mondays to Fridays between 6.15 PM and 8 PM, 70 per cent on Mondays to Fridays after 8 PM and 100 per cent on Saturdays after 12 AM, on Sundays and on public holidays.
[2] The rates applying to minors (16- and 17-year-olds) are not shown. These workers are not included in the payroll data.
[3] 94.90 SEK was equivalent to 14.42 USD and 9.88 EUR in 2008.

in 1998 with a pay equivalent to his or her marginal product, worker productivity needs to increase by at least 26 per cent by 2001, in relation to the productivity of the median worker, for the employee to keep the job. The path for a similar worker hired in 2004 was less steep, though, requiring a productivity increase of at least 19 per cent by 2007. On the other hand, the entry point in 2004 implied a 6 per cent higher marginal productivity than in 1998.

The minimum wage numbers for non-manual workers in the same sector tell a rather different story. Here, the minimum rates are differentiated only by age and they are considerably lower, both in absolute terms and in relation to median wages (among non-manuals). In 2008, minimum wages for 20- to 23-year-olds amounted to SEK 83.73 and 98.19 for 24-year-olds or over.[4] Moreover, Figure 5 reveals that minimum wage bites for non-manual workers have *declined* over time, from 76 to 71 per cent for those aged 24 or more and from 64 to 60 per cent for 20–23-year-olds since 1999, when the special rate for the latter group was introduced.

Non-manuals in retail may be covered by different collective agreements. The minimum rates in the figure are derived from the employers' agreement with *Tjänstemannaförbundet HTF* (merged into *Unionen* in 2008), which was the major agreement in the sector during the period of study, covering lower-level occupations requiring only secondary education (equivalent to senior high school in the US or sixth form grammar school in the UK), such as clerks and salespersons. Similarly, the median wage relates to such lower-level jobs.

3. The data and the sample partition

The data set has been obtained from the Confederation of Swedish Enterprise (*Svenskt Näringsliv*) and covers all member firms of the employer organization the Swedish Trade Federation over the period 1998–2008. There are 13,000 member firms with a total of 300,000 employees in the Federation, implying a coverage of about two thirds of all employees in Swedish retail (Svensk Handel, 2011). The observations are annual and refer to September each year. The firms are bound by the collective agreements signed by the Federation and these cover all employees, regardless of union membership.

[4] Hourly rates have been converted from monthly, assuming a 40-hour working week.

The data are based on payroll records and include information on employee category (manual or non-manual), various components of pay, actual and usual hours worked, gender, age, occupation, region and number of employees in the firm.

Since real minimum wages decline in one year only (2008), the analysis is limited to examining the employment consequences of increasing minimum wages. The sample period is further restricted to exclude the year 2007, due to tax reforms introduced in that year which are likely to confound the results.[5]

The data set contains unique identifiers for firms and workers. The definition of separations follows standard procedures in the kind of data used here. A separation in year *t+1* is defined as the worker being present in the data in year *t*, but not in *t+1*, while the firm was present in both *t* and *t+1* (but not necessarily during other periods). It is not possible to distinguish between voluntary and involuntary separations in the data. Since some, mostly small, firms for various reasons may not report data in a given year, even though they are still members of the Federation, this procedure ensures that the employees of non-reporting firms are not erroneously classified as separated. It should be noted that separations thus are defined in relation to employment in the *industry*, not in the firm. Given that involuntary separations cannot be identified, separations defined in this way capture relatively more exits into unemployment than a firm-based measure.

Since not all retail firms are members of the Federation, a transition to a non-member firm may erroneously be classified as a separation in the data. However, there is little to suggest that transitions to non-member firms serve as escape routes from high minimum wages to any large extent.[6] The collective agreements influence norms for wage-setting also in non-member firms and Swedish labour law grants far-reaching rights to unions taking action against firms without collective agreements, regardless of whether the firms pay sub-minimum wages or not.

The computation of changes in hours (between *t* and *t+1*) is also conditioned on the presence of the firm in the data in two subsequent years. The measure of hours is based on *usual* hours, not actual, in order to filter out disturbances specific to the reporting month. A worker not present in the data is assigned a value of zero hours.

[5] Payroll tax rates were cut for young workers and the reduction continued into 2008. In addition, earned income tax credits were introduced and became more generous in 2008.
[6] Escape routes to employment in other low-wage industries, like hotels and restaurants, should be restricted, too, since minimum rates are similar there (Skedinger, 2010).

The data contain a direct measure of the regular hourly wage (*fast timlön*), which is likely to be measured with less error than the more commonly available alternative in the form of an hourly wage calculated as wages divided by hours (over a given period). A minority of manual workers in the retail industry are salaried. For these workers, regular full-time monthly wages (*fast heltidsmånadslön*) have been transformed into regular hourly wages under the assumption of a 40-hour working week. The wage concept used thus excludes premiums for unsocial hours, overtime pay, bonuses and fringe benefits, in order to correspond to the minimum wage in the collective agreements.

Minimum wages for manual workers have been collected from the Retail Agreement (*Detaljhandelsavtalet*) and from circulars, distributed by the Federation to employers, for non-manuals. Each worker in the data set has been assigned a minimum wage, depending on the relevant personal characteristics, such as worker category, age and professional experience within the industry. There is no explicit information in the data set on experience, which is one of the determinants of minimum wages for manual workers. Instead, this information had to be imputed from the data.[7] Since minimum wages take into account experience up to 3 years, this procedure eliminated the same number of years from the data set (1998–2000). Due to the nature of the data set, with some firms opting not to report, imputed experience is understating actual experience for some workers (which implies too low a minimum wage). Tenure with the firm, which is an explanatory variable in the regressions, has also been computed by me and along similar lines.

The analysis of manual workers is limited to cashiers and sales staff, the two major occupations that are covered by the Retail Agreement. Observations on cashiers and salespersons constitute 95 per cent of the data for manual workers. Only lower-level occupations among non-manuals were included, such as clerks and sales staff, amounting to 22 per cent of the observations for non-manuals. There is no information on education in the data, but given the narrowly defined occupations I regard this to be of little concern. Workers with very low or high wages have been excluded (below 75 per cent of the minimum wage or more than three times the median wage). Finally, workers with multiple jobs were excluded since a unique

[7] According to the collective agreements, experience counts only if the worker has worked at least 10 hours per week. This rule is taken into account in the computation of workers' experience in the empirical analysis.

minimum wage could not be defined for these workers. After these exclusions, the number of observations for manuals is 246,811.

Figure 6 shows the hourly wage distribution for manual workers in the sample, in 2008. The various minimum wages are indicated by the vertical lines A–E. The spikes at these rates suggest that minimum wages are indeed binding for manuals in the retail industry. The spikes are quite distinct, with no smoothing around the minimum rates, which indicates that wages are measured with little error. Between 2 and 8 per cent of workers are on each of the five minimum wages and more than one fourth (28 per cent) are on either of them.[8] The distribution of monthly wages pertaining to non-manuals, shown in Figure 7, is quite different and closer to the textbook version of wage distributions in competitive labour markets. A mere 0.5 per cent of these workers are on either of the two minimums. The spikes that do appear in the figure can be explained by clustering at round number salaries, like 20,000 or 25,000 SEK, rather than clustering at minimum wages (at A and B). This suggests that minimum rates are not binding for lower-level non-manuals.

As in previous empirical analyses of the employment effects of minimum wages conditional on the worker's position in the wage distribution, the data set is partitioned into four groups, (see, e.g., Abowd et al., 2000). The idea behind this exercise is to identify workers affected by minimum wage changes – the "treatment" group – and a suitable "control" group, close to the treatment group in the wage distribution but assumed not to be affected by minimum wage changes.

For increasing real minimum wages, i.e., $mw_t < mw_{t+1}$, the four groups are defined as follows:

Below:	$w_t < mw_t$
Treatment:	$mw_t \leq w_t < mw_{t+1}$
Control:	$mw_{t+1} \leq w_t < 1.05(mw_{t+1})$
High:	$w_t \geq 1.05(mw_{t+1})$,

[8] There is a large spike to the right of and close to the highest minimum wage (E). A possible explanation for this spike is that many workers, after more than 3 years' experience, are awarded common and negotiated wage increases over and above the minimum wage increase. High attrition among workers would then smooth out the wage distribution to the right of this spike. Another possibility is that some employers set wages slightly above the highest minimum wage in order to attract more productive workers.

where *w* is the real wage and *mw* is the real minimum wage, both deflated by the output price.

With increasing minimum wages, and under the assumption that workers are paid their marginal product, a worker belonging to the treatment group is at peril of being laid off unless his or her productivity increases enough in order to match the higher minimum. Workers in the control group, with slightly higher wages (arbitrarily set at a maximum of 5 per cent higher in the chosen specification), are however not directly affected by the minimum wage increase. Assuming constant marginal productivity, the hypothesis, in line with the competitive model, is that workers in the treatment group, but not in the control, are laid off as a result of the minimum wage increase. Micro data on wages typically include some workers who, for various reasons, are paid less than the minimum wage. These workers are assigned to the "below" group and thus do not form part of the treatment group.

Table 1 shows summary statistics for the four groups of manual workers in the sample (with some information also on non-manuals). It is apparent that many manual workers in retail are affected by minimum wage changes as 41 per cent of observations belong to the treatment group. Around 5 per cent of observations are sub-minimum. These observations may be explained by some employers not (yet) having adjusted wage levels to the recent collective agreement or measurement errors, most likely occurring in the tenure variable. If tenure is underestimated, due to some firms being absent in the data, the minimum wage rate pertaining to these workers will also be underestimated. Despite the narrowly defined wage interval (5 per cent) relative to other studies in the field, the control group among manual workers contains many observations (19 per cent). Given the compressed wage structure in Sweden, this observation is not out of line with expectations.

Real minimum wage increases, measured as $\log mw_{t+1} - \log mw_t$, are higher, on average, in the treatment group than in other groups. The average increase among the treated manual workers was 7.9 per cent, with a variation (not shown in the table) between 0.1 and 14.8 per cent. There is thus substantial variation in minimum wage increases, both over time and across worker groups.

Many manual workers are part-timers in the retail industry and their usual hours worked per week ranges between 27 and 31 on average, depending on sample group. In general, the lower-paid work fewer hours. For example, individuals in the treatment group work over 5 hours less per week than those in the control. The change

in hours among all workers is negative (partly by construction of the variable, since separated workers are assigned zero hours). For workers remaining in the industry, changes are positive but relatively small, less than 1 hour.

Turnover is very high, with 37 per cent of manuals separating in the industry each year in the treatment group (recall that the measure relates to exits from the industry, and excludes transitions between firms within the industry).[9] A worker's attachment to the job is likely to influence mobility. There is unfortunately no direct information on the use of fixed-term contracts, which is widespread in the industry, but there are other variables in the data which could help capture the degree of attachment to the job. Besides part-time status and the hourly wage, there is a variable in the data set indicating whether the worker is salaried and there is also information on the share of unsocial hours pay of total pay. Salaried workers are typically less mobile while workers with much unsocial hours pay are likely to be students working mainly during evenings and weekends, with little long-term attachment to the job. Among the treated manuals, around 11 per cent are salaried and 19 per cent of total pay is compensation for unsocial hours, on average. Here, the variation among workers is large, with many having no such pay at all and some earning almost all their pay from working unsocial hours.

Working hours should be influenced by available variables, such as age, gender, occupation and wage. However, family-related variables, in particular the presence of small children in the household, are among probable determinants of working hours, at least among females, that are missing in the data set.

Table 1 includes descriptive statistics for lower-level non-manuals, who have been assigned to the four groups based on the fictitious minimum wage structure for manuals (the table reports statistics only for the treatment and the control groups). Although relatively fewer workers among non-manuals are found in the treatment and control groups, turnover characteristics are very similar. For example, 38 per cent in the treatment group are separated next period, which is close to the figure for manuals. An important difference, though, is that non-manuals rarely work part-time and that they put in longer hours than manuals, about 34 hours per week on average. The lower incidence of part-time work among non-manuals may be explained by their

[9] Since it is likely that some workers in the industry are employed only part of the year and then return to their employer and that there are workers who are temporarily absent, due to, e.g., parental leave, these figures represent an upper bound of "real " turnover.

tasks being less tied to unsocial shop hours and fluctuating customer demand than is the case for manual workers. Table 1 also shows that non-manuals tend to be somewhat older than manuals in the corresponding groups.

4. Econometric specification

For workers within each group *k* (Below, Treatment, Control and High) with increasing minimum wages, the interacted variables $I_k(\log mw_{t+1} - \log mw_t)$ are defined, where *I* is a dummy that takes on the value 1 if the worker belongs to group *k = 1, 2, 3, 4* and zero otherwise. The increase in minimum wages, within parenthesis above, is the same for all *k*, given age and experience.

In the regression model in which separations are investigated, the worker either is separated from the job in the next period (*Y=1*) or is not (*Y=0*). For ease of computation and interpretation, the estimations are implemented through OLS (but experiments with binomial logit will also be performed).[10] For increasing minimum wages, we have

$$Y_t = \alpha + \sum_{k=1}^{3} \beta_k I_{k,t} (\log mw_{t+1} - \log mw_t) + X_t^{'} \gamma + \varepsilon_t , \qquad (1)$$

where *X* is a vector of additional explanatory variables assumed to influence worker transitions (with individual indices suppressed). As in most of the related literature, these controls include the real hourly wage (deflated by cpi), age, age squared, job tenure, the number of employees in the firm and its square and dummies for gender, occupation, year and region. In my specification, I add dummies for salaried work, part-time work and the share of unsocial hours of total pay in order to capture attachment to the job. The equation error is denoted by ε.

In the regressions with changes in hours (*H*) as the dependent (and continuous) variable, the same estimation technique and basically the same sets of variables are used as in (1). With increasing minimum wages, $\Delta H = H_{t+1} - H_t$, where workers non-employed at *t+1* are assigned zero hours.

The difference between the coefficients for the treatment and the control group ($\beta_2 - \beta_3$) is interpreted as the effect among the treated of increasing minimum wages

[10] Angrist and Pischke (2009, p. 107) argue that it is "fairly robustly true" that marginal effects are similar in empirical applications of linear and non-linear regression models.

on separations or hours in (1). This interpretation of the coefficients relies on a number of assumptions. Firstly, there should be no unobserved variables that are correlated with being low-paid, on the one hand, and employment transitions and hours, on the other hand. Secondly, the control group should not be affected by changes in minimum wages. If wage spillovers exist, due to production technology or relative wage consideration, this assumption is violated. Production technology may imply that different types of labour are substitutes or complements. In addition, the estimation technique used in this paper requires that increases in minimum wages have a larger impact on actual wages in the treatment than in the control group. These assumptions will be discussed in more detail in the next section, which presents the econometric results.

5. Econometric results

Table 2 shows regression results for the impact of minimum wage increases on employment among manual workers. All individuals have been assigned to one of the four groups "below", "treatment", "control" and "high", with the latter as a reference category. The group dummies are interacted with the increase in minimum wages, as in equation (1). Of particular interest is the test for equality of the coefficients for the treatment and control groups, that is, whether $\beta_2 - \beta_3 = 0$.

Besides the interacted group dummies, all specifications in the table include worker and firm-specific controls as well as region and year dummies (which are unreported for brevity). The first three columns relate to separations. In the second column, the sample is restricted to a balanced panel of firms and the third column adds firm-specific effects to the specification. The first regression shows that separations increase more in the treatment group than in the control. According to the coefficients, one percentage point's increase in the real minimum wage is associated with an increase in the separation rate among the treated that is 0.136 percentage points higher than in the "high" group. The coefficient for the control group is –0.171, suggesting that substitution takes place between the treated and the control.[11] The difference between the two coefficients is 0.307, which taken at face value could be interpreted as a causal effect of minimum wages on separations among the treated. This effect is not far off the corresponding elasticity for Swedish hotels and

[11] This interpretation is in line with Persson (1999), who finds that substitution between worker categories, especially young workers and adult females, is important in Swedish retail and wholesale trade.

restaurants reported in Skedinger (2006), which is about 0.5. The unreported worker and firm-specific controls mostly work according to expectations. Increasing age, tenure and wage contribute significantly to fewer separations. Variables proxying for attachment to the job – part-time work, salaried position and unsocial hours pay in relation to total pay – all indicate that less attachment is associated with more separations.

The regressions with a balanced panel of firms in the second and third columns result in a loss of observations of 8 per cent in the sample and a loss of 29 per cent of the firms. Without firm fixed effects, the estimated effect β_2–β_3 is identical to the one in the larger sample in the first column, and it increases somewhat, to 0.370, as firm fixed effects are introduced.

Thus the results so far in Table 2 align with expectations from the competitive model in that increases in the minimum wages are associated with more separations. What about the effect of minimum wages on hours worked? The regressions in Columns (4) – (9) of Table 2 attempt to answer this question, using the same specifications as for separations. To begin with, in Columns (4) – (6), all workers are considered regardless of employment status (with non-employed workers assigned zero hours). In Columns (7) – (9), only workers employed in both periods are included in the regressions.

The estimated effects of increasing minimum wages on hours for all workers is –0.043 when firm fixed effects are imposed, but otherwise insignificant, implying that hours decrease at the most by around 2.5 minutes for each percentage point's increase in the real minimum wage, which is a modest effect. Nor did remaining workers change their hours much, according to the rightmost regressions in Table 2. The estimate is positive, but insignificant.

Why is the impact on hours so small? As noted in Table 1, the treated put in fewer hours than the control, and a further look at the data shows that is true also for separated workers in the two groups. Apparently the reduction of hours caused by the increase in separations among the treated is counteracted by the fact that relatively fewer hours are lost among them.

One crucial assumption in the econometric framework used in this paper is that there should be no unobserved variables that affect employment transitions (or hours) and the likelihood of belonging to the treatment or control group. It is, for example, conceivable that turnover is higher among the low-paid for structural

reasons, unrelated to changes in the minimum wages. The presence of a group of lower-level non-manuals in the retail industry – with less binding minimum wages – offers a possibility to test whether other factors than changes in minimum wages contribute to employment transitions. The two groups of workers are similar as far as turnover at given wages and skills are concerned (see Table 1). However, since work patterns in terms of hours are quite different it seems not useful to include the effect on hours in the examination.

Table 3 includes both manual and non-manual workers and imposes the minimum wage structure pertaining to manual workers on all workers. This means that the non-manuals have been assigned fictitious minimum wages and the purpose behind this exercise is to investigate to what extent the results for separations and changes in hours in Table 2 for manuals can be explained by the presence of structural turnover, not accounted for by the explanatory variables.

Before estimation, observations for lower-levels non-manuals potentially affected by changes in *actual* minimum wages were filtered out. That is, with $mw_t < mw_{t+1}$, observations fulfilling the condition that $w_t < mw_{t+1}$, where mw denotes the real minimum wage for non-manuals, were excluded. As expected, this procedure eliminated few observations: 1.4 per cent of the 44,762 observations for non-manuals.

According to Table 3, where the additional coefficients β_4 - β_7 refer to non-manuals, the impact of increases in minimum wages on the "treated" among non-manuals is small and the difference β_5 - β_6 is insignificant (and even negative), which lends support to the assumptions underlying the econometric model.

Another concern with the regressions so far is that the estimation technique used in this paper requires that increases in the minimum wages for manuals have a larger impact on actual wages among manuals in the treatment than in the control group. This assumption is tested, and receives support, in the first column of Table 4. According to the results, real wage growth among manuals increases by 0.144 percentage points more in the treatment group than in the control when real minimum wages increase by 1 percentage point. The conjecture that increases in minimum wages among manuals affect wage growth among "treated" non-manuals, through interdependence in the production process or relative wage considerations, is however rejected, as demonstrated in the second column of Table 4.

Table 5 presents additional robustness checks for manual workers (for brevity, only the difference β_2–β_3 is reported). These checks deal with measurement errors in

the experience variable, heterogeneous effects across groups as well as alternative estimation methods.

As discussed previously, experience within the industry may be measured with error and minimum wages are partly based on this experience. Alternative estimations attempt to minimize the influence of possibly mismeasured experience. For workers above the age of 18 and with at least three years of recorded experience the minimum wage is known with certainty, since minimum rates do not increase with additional years of experience. The first row of Table 5 presents estimates for a subsample of workers with a minimum of three years' experience within the industry. A drawback with this exercise is that the subsample is scarcely representative in an industry with large turnover and minimum wages could have a larger impact among workers with less experience. On the whole, the basic results are strengthened. For separations the estimate is larger than in Table 2 (0.497) and there is now a distinct negative effect on overall hours (–0.084), which speaks for the possibility that measurement errors have biased results downwards in the original estimations.

Rows 2–5 consider heterogeneity in treatment effects across groups. It is of some interest to check whether the results are different when analysing just young workers, aged 18–24, as is done in the second row. The results reveal that primarily the negative effects on overall hours (–0.131) are more pronounced among the young, but the effect on separations is also slightly larger than for workers in general. A way to potentially reduce unobserved heterogeneity between the treatment and control groups is to let those within the relevant wage interval who have previously been treated form the control group for the treated in year *t* (known as "staggered treatment"). The estimates, in row 3, point to stronger effects on separations and overall hours than those reported in Table 2. In rows 4–5, robustness to different wage intervals is tested, using a slightly narrower interval (2.5 per cent) and a slightly wider one (7.5 per cent). The results indicate that the effect on separations decreases somewhat as the wage interval is widened.

The results so far only consider affected workers versus those slightly above the minimum wage. However, it is possible that those individuals whose wage is further below the minimum wage prior to an increase face increased probabilities of separation. An alternative specification that allows for this is to use a wage gap measure, which is the percentage increase in the wage required to take the individual up to the new minimum (see, for example, Currie and Fallick, 1996, and Zavodny,

2000). Workers whose wages were previously above the new minimum are assigned a zero value of the wage gap variable and thus form an implicit control group. Workers below the initial minimum wage are excluded from the sample, for comparability with previous estimates. The wage gap estimates, in row 6 of Table 5, do not diverge much from the results in Table 2. Unlike the latter, however, wage gap estimates do not provide information about substitution effects.

Finally, the regression for separations was rerun using logit. The estimated effect turns out to be close to the corresponding figure reported in Table 2, as expected.

6. Conclusions

This paper has examined the effects of increases in real minimum wages on separations and hours worked in the Swedish retail sector during 2001–05. The econometric framework relies on the identification of workers affected by minimum wage increases, depending on their position in the wage distribution, and contrasts outcomes for these workers to those for a control group of workers, with slightly higher wages.

The boom experienced in the industry during the period analysed implies that the cards have been stacked against finding strong results on employment from minimum wage increases. Despite this, the initial analysis, for manual workers, suggests that separations do increase as minimum wages increase. The findings also indicate that substitution between worker groups in response to changes in minimum wages is important in retail; for example, as minimum wages increase more workers are separated in the treatment group, but fewer workers are separated in the control. The median worker is located in the control group, according to Table 1. Thus the results showing disemployment effects in the treatment group and the opposite outcome in the control seem consistent with a union model in which members bargain over a minimum wage that maximizes the utility of the median voter (Booth, 1984).

In general, though, total hours and hours per remaining worker do not change much as minimum wages increase, but there is a distinct negative effect on overall hours among the young. On the whole, the loss of hours due to more separations among the treated seems to be mitigated by the fact that these workers initially put in relatively fewer hours. This result suggests that analyses that deal with employment consequences of increasing minimum wages solely by examining worker flows, but

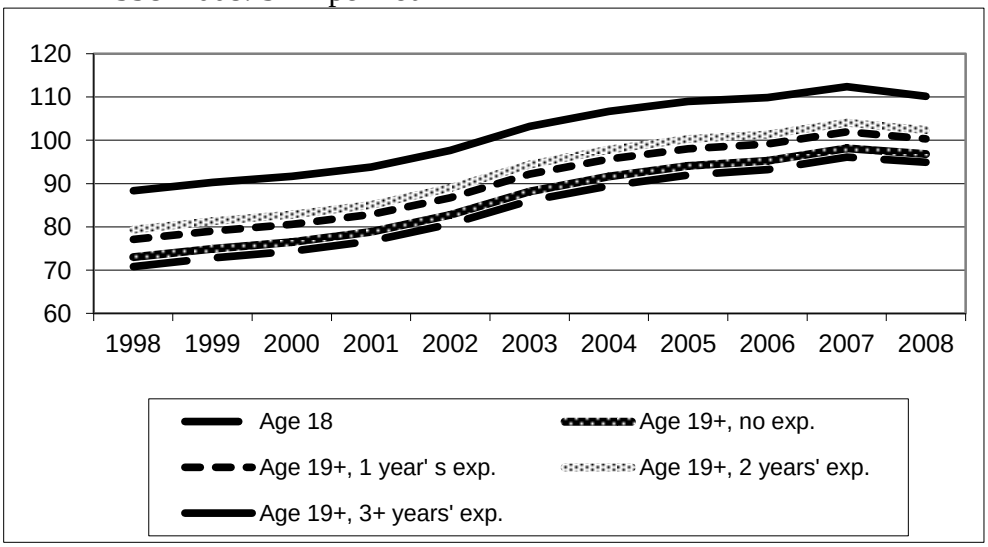
disregard hours, may exaggerate the overall decline in employment to the extent that job losses are concentrated among low-paid, part-time workers.

The assumptions underlying the model were tested by including non-manual workers in the analysis. Lower-level non-manuals in the industry exhibit turnover characteristics similar to those of manuals at given wages, but differ in one important respect: minimum wages are much lower and not binding. By imposing the minimum wage structure pertaining to manuals on non-manuals it was possible to examine whether the findings obtained for manuals are indeed attributable to increases in minimum wages – and not to something else. As the fictitious minimum wages had only a small impact on separations among "treated" non-manuals, the underlying assumptions were supported.

The main conclusions turned out to be fairly robust to numerous checks, including experiments with different subsamples of workers, different wage intervals and wage gap estimates.

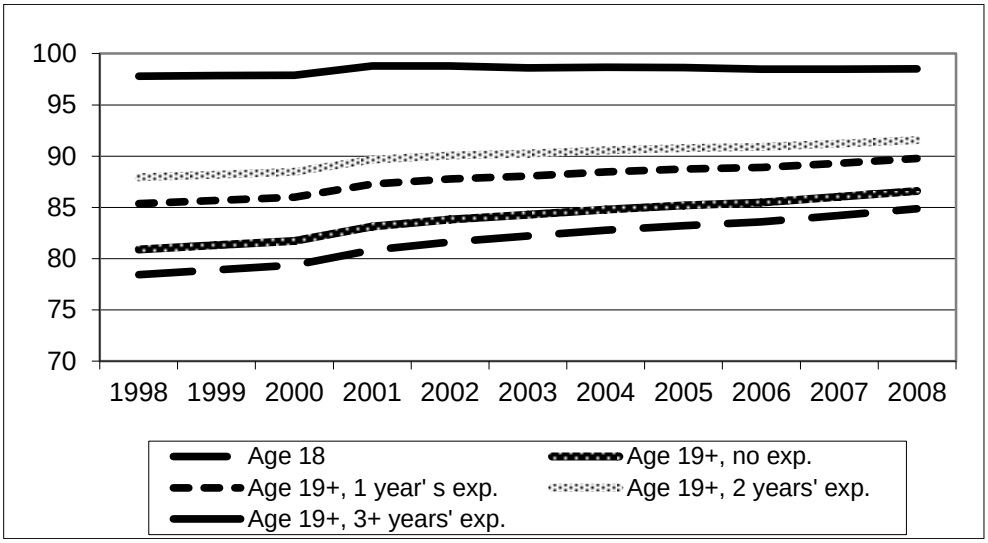
The purpose of this paper has been to identify effects of increases in minimum wages for a narrowly defined group, namely those directly affected by these changes. Hence it is beyond the scope of this paper to assess the impact on overall employment in the retail sector of the reduction of payroll taxes in 2007–08. This issue will be explored in future research, which will also take into account the additional reduction of payroll taxes in 2009, with the intention of considering more broadly defined worker groups and not just those affected by changes in minimum wage costs.

Figure 1. Real minimum wages for manual workers, by age and experience, 1998–2008. SEK per hour



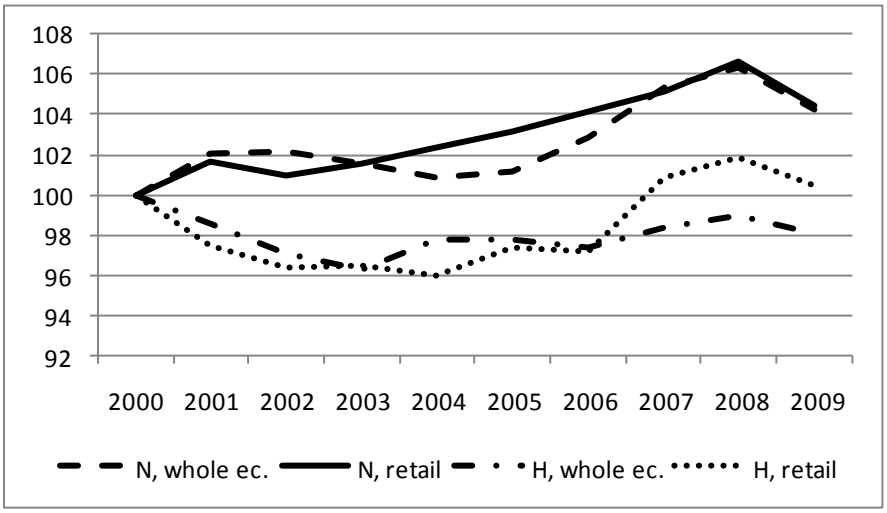
Note: Minimum wages deflated by the output price in retail, index 2008=1.
Source: Collective agreements (minimum wages); Statistics Sweden, National Accounts (output prices).

Figure 2. Minimum wage bites for manual workers, by age and experience, 1998–2008. Per cent



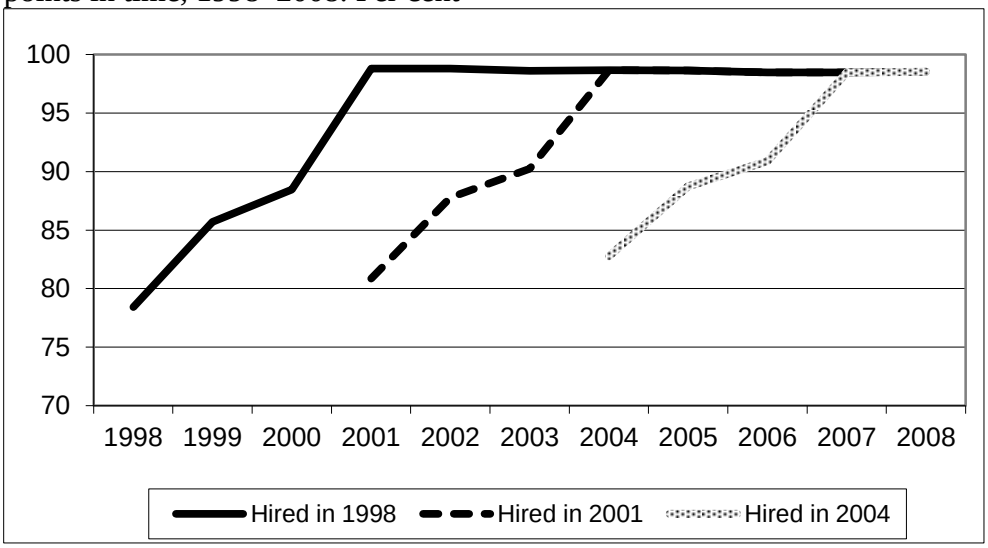
Note: Minimum wages as a percentage of median wages for manuals in the industry.
Source: Own calculations.

Figure 3. Employment (N) and hours per worker (H) in retail and in the whole economy, 2000–09. Index 2000=100



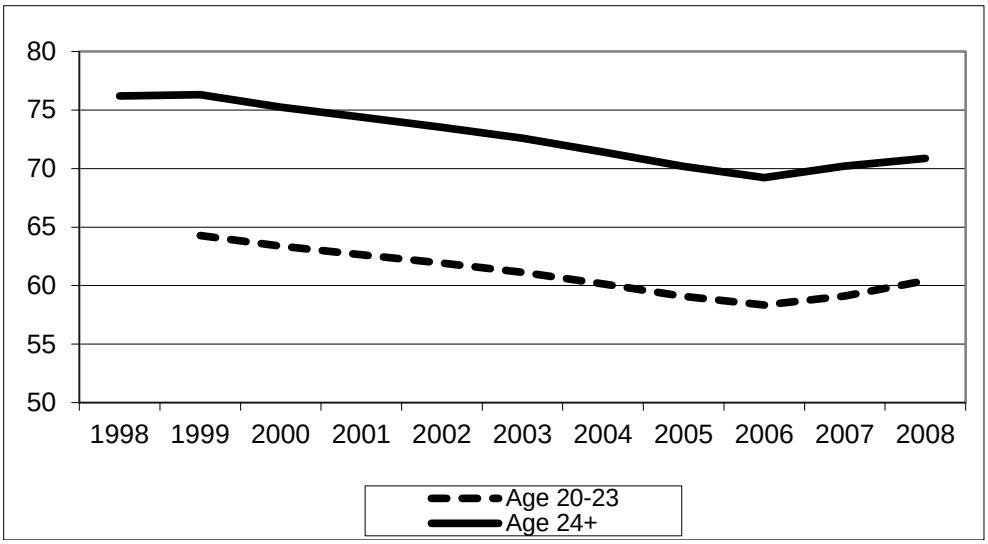
Source: Statistics Sweden, National Accounts.

Figure 4. Minimum wage paths for manual workers, hired as 18-year-olds at different points in time, 1998–2008. Per cent



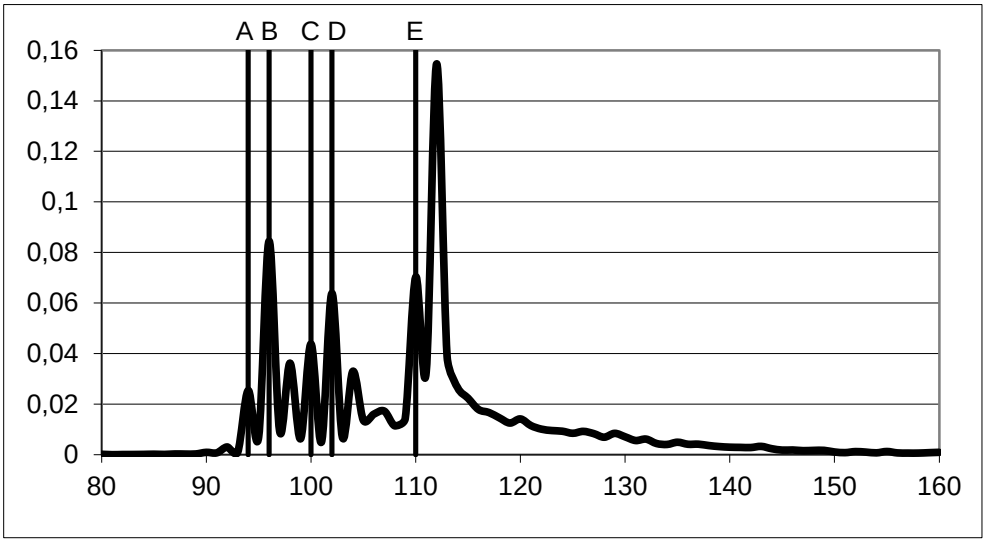
Note: Minimum wages for workers continuing in employment, as a percentage of median wages for manuals in the industry.
Source: Own calculations.

Figure 5. Minimum wage bites for lower-level non-manual workers, by age, 1998–2008. Per cent



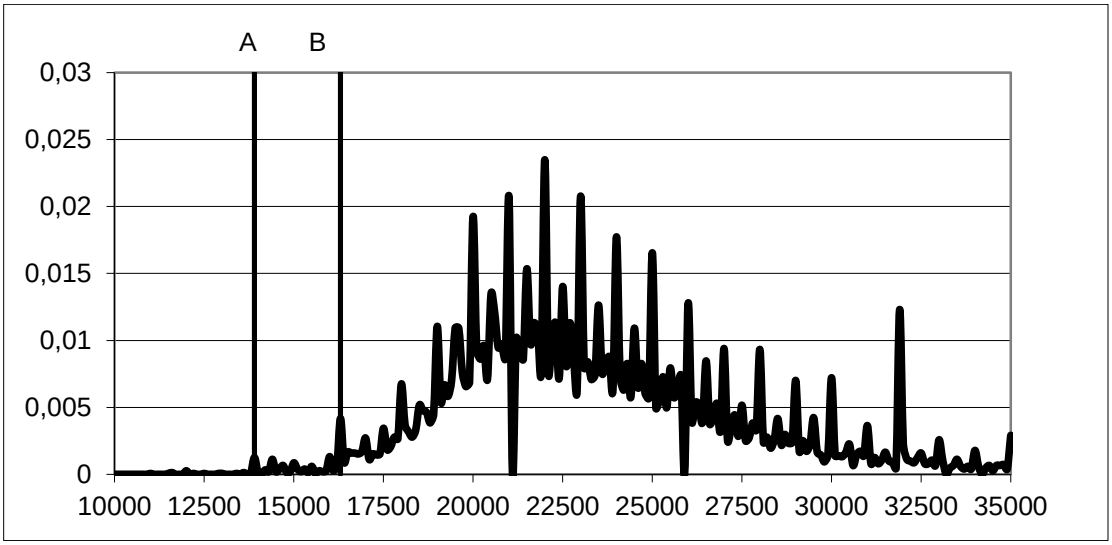
Note: Minimum wages as a percentage of median wages in the industry for non-manuals in occupations requiring secondary education.
Source: Own calculations.

Figure 6. Wage distribution for manual workers, 2008. SEK per hour



Note: Minimum wages are indicated by vertical lines: A = 18-year-olds; B = 19-year-olds or over without experience; C = 19-year-olds or over with 1 year's experience; D = 19-year-olds or over with 2 years' experience; E = 19-year-olds or over with at least 3 years' experience.
Source: Own calculations.

Figure 7. Wage distribution for lower-level non-manual workers, 2008. SEK per month



Note: Minimum wages are indicated by vertical lines: A = 20–23-year-olds; B = 24-year-olds or over.
Source: Own calculations.

Table 1. Descriptive statistics for manual workers (M) and lower-level non-manuals (NM), by position in the wage distribution, 2001–05

Variable	Below M	Treated		Control		High M
		M	NM	M	NM	
Separated	0.390	0.365	0.381	0.208	0.278	0.248
Weekly hours	26.77	24.17	34.43	29.65	35.59	30.99
Change in hours, all workers	–9.75	–7.94	–12.56	–5.59	–9.55	–6.85
Change in hours, remaining workers	0.34	0.70	0.19	0.28	–0.08	0.35
Increase in real hourly minimum wage	0.066	0.079	0.078	0.048	0.061	0.058
Real hourly wage, SEK	87.30	92.87	95.97	103.99	103.25	113.24
Age	25.66	26.81	29.47	37.32	35.52	36.66
Male	0.307	0.260	0.262	0.194	0.238	0.353
Occupation: Salesperson	0.884	0.852	0.235	0.858	0.159	0.915
Job tenure: < 1 year	0.306	0.480	0.452	0.155	0.272	0.328
1 year	0.324	0.182	0.153	0.146	0.152	0.239
2 years	0.149	0.149	0.220	0.168	0.166	0.099
≥ 3 years	0.222	0.188	0.175	0.531	0.409	0.334
Part-time	0.450	0.527	0.173	0.371	0.128	0.299
Salaried	0.222	0.110	0.687	0.225	0.867	0.431
Unsocial hours pay of total pay	0.186	0.193	0.039	0.134	0.027	0.109
No. employees	782	750	680	775	670	776
No. obs.	12,479	100,646	1,298	47,320	2,447	86,366
Per cent of total obs	5.1	40.8	2.9	19.2	5.5	35.0

Note: Minimum wage structure for manuals is imposed on both manuals and non-manuals.

Table 2. Employment regressions for manual workers

	Separations			Hours – all			Hours – remaining workers		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
β_1: Below	0.543 (4.93)	0.547 (4.79)	0.500 (4.69)	–0.232 (2.79)	–0.241 (2.80)	–0.143 (2.60)	–0.027 (0.71)	–0.029 (0.73)	–0.002 (0.05)
β_2: Treatment	0.136 (2.78)	0.124 (2.46)	0.132 (2.72)	0.004 (0.21)	0.004 (0.23)	0.004 (0.24)	0.003 (0.34)	0.005 (0.45)	0.010 (1.05)
β_3: Control	–0.171 (3.41)	–0.183 (3.47)	–0.238 (4.86)	–0.000 (0.02)	0.006 (0.26)	0.047 (2.35)	–0.010 (0.79)	–0.007 (0.53)	–0.002 (0.21)
Panel of firms	N	Y	Y	N	Y	N	Y	Y	Y
Firm fixed effects	N	N	Y	N	N	N	N	N	Y
Test: $\beta_2-\beta_3=0$	0.307 [0.000]	0.307 [0.000]	0.370 [0.000]	0.004 [0.881]	–0.002 [0.937]	–0.043 [0.079]	0.013 [0.240]	0.011 [0.307]	0.012 [0.230]
R-squared	0.079	0.081	0.103	0.034	0.035	0.085	0.009	0.010	0.027
No. observations	246,811	227,046		245,704	226,084		172,883	159,775	
No. firms	2,273	1,611		2,273	1,611		2,244	1,590	

Note: The regressions consider workers aged 18–64 and are estimated by OLS over the period 2001–05. All regressions control for the real wage (deflated by cpi), age and its square, number of employees in the firm and its square, share of unsocial hours pay of total pay and dummies for region, year, gender, tenure, occupation , salaried work and part-time work (except in the hours regressions). A constant (not reported) is included in all regressions. Coefficients are divided by 100 in the hours regressions. Absolute t-values, robust for clustering of standard errors at the firm level, within parentheses. P-values within brackets.

Table 3. Employment regressions including both manual (M) and lower-level non-manual (NM) workers

	Separations		
	(1)	*(2)*	*(3)*
β_1: Below (M)	0.504 (4.41)	0.498 (4.19)	0.498 (4.73)
β_2: Treatment (M)	0.098 (1.72)	0.078 (1.31)	0.117 (2.16)
β_3: Control (M)	–0.196 (4.05)	–0.210 (4.12)	–0.262 (5.34)
β_4: Below (NM)	1.855 (4.96)	1.896 (4.14)	1.315 (2.97)
β_5: Treatment (NM)	0.397 (2.08)	0.334 (1.66)	0.287 (1.48)
β_6: Control (NM)	0.481 (2.53)	0.469 (2.32)	0.481 (2.84)
β_7: High (NM)	–0.192 (1.24)	–0.291 (1.77)	–0.188 (1.56)
Panel of firms	N	Y	Y
Firm fixed effects	N	N	Y
Test: β_2–β_3=0	0.295 [0.000]	0.288 [0.000]	0.379 [0.000]
Test: β_5–β_6=0	–0.084 [0.642]	–0.135 [0.470]	–0.195 [0.301]
R-squared	0.087	0.088	0.116
No. observations	290,943	266,168	
No. firms	3,178	2,281	

Note: See notes to Tables 1 and 2.

Table 4. Wage growth regressions

	Manuals	Non-manuals
β_1: Below	0.635 (12.33)	0.260 (1.96)
β_2: Treatment	0.283 (24.21)	0.033 (0.79)
β_3: Control	0.139 (8.30)	0.034 (1.52)
Panel of firms	N	N
Firm fixed effects	N	N
Test: $\beta_2-\beta_3=0$	0.144 [0.000]	–0.001 [0.974]
R-squared	0.112	0.047
No. observations	176,042	36,265
No. firms	2,533	1,401

Note: Real wage growth is defined as log w_{t+1} – log w_t, where *w* is the real wage (deflated by cpi). All regressions control for age and its square, number of employees in the firm and its square and dummies for region, year, gender, tenure, occupation, salaried work and part-time work. See also notes to Tables 1 and 2.

Table 5. Robustness checks for employment regressions with manual workers

	Separations	Hours – all	Hours – remaining workers
1. Experience ≥ 3 years	0.497 [0.000] {85,409}	–0.084 [0.048] {85,180}	0.008 [0.637] {71,051}
2. Age 18–24	0.403 [0.000] {91,464}	–0.131 [0.002] {90,867}	–0.026 [0.240] {54,561}
3. Staggered treatment	0.646 [0.000] {183,589}	–0.171 [0.001] {182,745}	–0.005 [0.857] {128,185}
4. Wage interval 2.5 %	0.376 [0.000] {246,811}	–0.025 [0.400] {245,704}	0.012 [0.307] {172,883}
5. Wage interval 7.5 %	0.190 [0.000] {246,811}	0.019 [0.410] {245,704}	0.007 [0.472] {172,883}
6. Wage gap	0.388 [0.000] {234,332}	0.022 [0.594] {233,289}	–0.005 [0.823] {165,329}
7. Logit	0.262 [0.000] {246,811}	–	–

Note: The estimated effects refer to β_2–β_3 in specifications based on columns (1), (4) and (7) in Table 2, except in the wage gap regressions (see text for details). The estimation period is 2001–05, except in the staggered treatment regressions in which the period is 2002–05. P-values within square brackets, number of observations within curly brackets. See also notes to Table 2.

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