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**Working Paper**

## Negotiated wage increases and the labor market outcomes of low-wage workers: Evidence from the Swedish public sector

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**IFAU**

Institute for Evaluation of Labour  
Market and Education Policy

**Negotiated wage increases and the  
labor market outcomes of  
low-wage workers:  
evidence from the Swedish public sector**

Tove Eliasson  
Oskar Nordström Skans

WORKING PAPER 2014:10

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# Negotiated wage increases and the labor market outcomes of low-wage workers: evidence from the Swedish public sector\*

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## Abstract

This paper investigates the impact of a collective agreement stipulating a one shot increase in establishment-specific wage levels in a public-sector setting where wages otherwise are set according to individualized wage bargaining. The agreement stipulated that wages should increase in proportion to the number of low-paid females within each establishment. We find that actual wages among incumbents responded to the share of females with a wage below the stipulated threshold, conditional on the separate effects of the share of low wage earners, and the share of females. We find clear evidence of path-dependence in wages, covered workers remained on higher wage levels 4 years after the agreement took effect. The increase in wages resulted in a reduced probability of exit among young workers with relatively good grades and a lower frequency of new hires at the establishment level.

Keywords: Collective bargaining, wage growth, turnover, wage rigidity, hours of work, labor costs

JEL codes: J23, J31, J52, J63

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# 1 Introduction

Collective bargaining plays a central role for the setting of wages in most OECD countries (Freeman, 2007). In particular, this tends to be true for the low skilled segments of the labor market. However, wages in most OECD countries are set in a complex multi-stage process where sectoral collective agreements and local bargaining tend to coexist. As a consequence, the impact of collective wage agreements on actual outcomes, including wages, are therefore often unknown a priori, also to the unions and employer representatives that sign the agreements.<sup>1</sup> In this paper we provide empirical evidence on how wage increases as stipulated in a collective agreement for public-sector blue-collar workers affect key outcomes at the individual and establishment level.

It appears obvious that changes in workers' wage growth induced by collective agreements could have substantial effects on fundamental labor market outcomes such as labor turnover, working hours and the long run wage trajectories of the covered workers as well as the skill-composition of covered organizations.<sup>2</sup> Despite of this, we have found very few recent empirical studies on the topic. In particular, we have found very few recent studies for Sweden, from where we draw our data. The absence of studies is likely to reflect empirical challenges arising from the fact that the content of collective agreements should reflect expectations regarding the outcomes we are interested in.<sup>3</sup> In addition, it is often difficult to match the content of collective agreements to high-quality individual-level data (Forslund et al., 2012).

The intervention we study in this paper (see below for details) was motivated by a union initiative with the aim of increasing wages among low-paid women. To achieve this end, a collective agreement stipulated additional, establishment-level, wage increases in proportion to the number of female workers earning below a pre-specified wage threshold. We use a panel data covering the universe of covered workers and document that actual establishment-level wages increased as stipulated by the agreement, and analyze the extent to which this increase affected the long run outcomes of the employees.

The previous literature contains a host of empirical studies of how wages and other outcomes are affected by wage setting institution, such as, e.g., studies of the union wage premium, or the wage impact of works councils. In terms of more pre-

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<sup>1</sup>A good example is Böckerman and Uusitalo (2009) who finds that a sectoral exception from minimum wages in Finland had no effects since wages at the local level failed to respond.

<sup>2</sup>See e.g. Salop and Salop (1976); Neumark and Wascher (2008)

<sup>3</sup>See for example the literature on "wage drift", (Holden, 1998; Hibbs and Locking, 1996) and the literature on "wage cushions", e.g. Cardoso and Portugal (2005).

cisely documenting the impact of wage changes on economic outcomes within a stable institutional setting, the previous literature is however almost exclusively focused on the impact of minimum wages. This literature has, on the other hand, resulted in hundreds of empirical studies using a multitude of outcomes. The minimum wage literature is dominated by studies from the US, Schmitt (2013) provides a recent overview. But as noted by Neumark and Wascher (2008), the effects of collectively agreed minimum wages may differ from legally imposed minimum wages since the bargaining process may take into account adverse effects of minimum wage changes. Rare examples from a collective agreement context include Skedinger (2006, 2011) and Böckerman and Uusitalo (2009).

The empirical focus on minimum wages in the broader literature is a natural response to the fierce political debate regarding the appropriate level of minimum wages in the US (in particular). However, to the extent that we are interested in how wage setting practices affect the labor market outcomes of low-skilled workers in a European industrial-relations context, it is important to note that a one-sided focus on the minimum wage will provide us with a very partial answer. In general, a very small share (mostly very young) of the low-skilled workers tend to be covered by the minimum wages (Garnero et al., 2013), whereas the coverage of collective agreements is near universal within sizable segments of many European labor markets. As a consequence, bargained wage increases are likely to be relevant for significant fractions of the workforce. In a Swedish context, Forslund et al. (2012) documented that actual wages for young entrants are far above the sectoral minimum wages within many bargaining areas. Yet, the wage costs of these workers are still affected by collective bargaining through local agreements on entry wages and (potentially) through both sectoral and local agreements on wage increases. In contrast to archetypical state level minimum wages, the decentralized element within contemporary European bargaining institutions also means that workers are able to move across sectors in response to changes in collective agreements. In this paper, we try to derive first-order evidence of the effects of collectively agreed wage hikes within such a setting.<sup>4</sup>

Our empirical case is based on a very unusual collective agreement. The agree-

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<sup>4</sup>Our analysis is also related to other strands of the economic literature. The literature on the effects of changes in labor costs, induced by subsidized labor or changes in payroll taxes, provides another way of studying labor costs changes similar to the collectively agreed wage growth we study here. Examples include Benmarker et al. (2009); Huttunen et al. (2012); Kramarz and Philippon (2001). A large literature focus on the inter temporal dependence of wages by either analyzing real or nominal wage rigidities, or by documenting the impact of past economic conditions on current wages (following Beaudry and DiNardo (1991); see Grant (2002) for a more recent study).

ment stipulated that average wage increases among incumbent workers should be a function of the interaction between gender and the previous wage of incumbent employees within the establishment. This agreement was the result of a very turbulent discussion within the Swedish Confederation of Labor Unions (LO) which resulted in a general agreement to focus the attention towards wage increases for low paid women in the 2006-2007 round of wage negotiations across the Swedish economy. The exact implementation of this target differed greatly across sectors however. The most literal interpretation, which we study in this paper, was implemented by the Swedish Municipal Workers' Union in their agreement with the Swedish Association of Local Authorities and Regions (Medlingsinstitutet, 2007).<sup>5</sup> This agreement (HÖK) stipulated an extra scope for wage increases amounting to 400 SEK (200 SEK) for each woman earning below 20 000 SEK during 2007 (2008). Since the general principle for wage determination within the bargaining was (and is) that negotiated (aggregate) wage increases should be distributed locally in accordance with individual performances, the agreement did not stipulate that the extra wage increase necessarily needed to be paid to the women that earned below this threshold, instead it was stipulated that average (local) wage increases should reflect the number of low-paid women.

The design of the agreement allows us to study its impact on actual wage growth and subsequent economic outcomes while separately controlling for wages and gender since the eligibility is determined by the interaction of these variables. Within this paper, we focus on the impact on wage trajectories, hours worked and exit probabilities. We also explore changes in hiring practices at the establishment level. Throughout, we rely on population-wide micro-level panel data capturing all workers covered by the agreement.

Our results show that the agreement had an effect on actual wage growth along the lines suggested by the agreement, inducing additional wage growth at establishments which employed a high fraction of women earning below the threshold. In other words, the agreements did induce a substantial change in the relationship between wage growth and the interaction between initial wages and gender composition at the workplace. The impact on wage increases among the covered incumbents had effects which lasted at least four years after the initial impact, with no signs of reversion within our four year follow-up period. This result implies that the impact of this one-time shock to individual wages had persistent effects on individual

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<sup>5</sup>The agreement between the Swedish Paper Worker Union and the the Swedish Forest Industries Federation was designed similarly.



wages.<sup>6</sup>

These results suggest that one-time interventions through collective agreements can serve as a tool to achieve long-lasting changes in the wage distribution. This is a question of direct importance for wage setting unions (and policy-makers) who may perceive observable wage differentials between, e.g., men and women as a reflection of historical discrimination. Notably, the rationale for such interventions (and the result of the evaluation) will crucially hinge on whether wages are set according to contemporary market forces or if they remain a function of past wages, and hence also of past events.

We also study the extent to which the agreement affected the hours worked and the probability to remain within the establishment, which serve as channels of possible adjustment for the employer as labor costs change, as has been noted in the literature on minimum wages (Hirsch et al., 2011). The results show that, as wage growth increased, hours worked appear to have increased less than within other workplaces at the same time. We also find that the overall separation rate remained unaffected, while for young (more mobile) workers with grades above the median the separation probability declined. This final result suggests that higher wages allowed the employers to retain mobile workers with relatively good outside options. Further results suggest that the number of new hires at the workplace level decreased as a consequence of the rising wages.

The paper is structured as follows. Section 2 presents the institutional background and, in particular, the 2007 agreement that we study empirically. Section 3 describes the data. Section 4 present the first stage results on actual wages of covered workers. Sections 5 documents the long-run impacts. Section 6 concludes.

## 2 Institutional background

Since 1997, Sweden has a system of pattern bargaining where the "industrial agreement" for blue and white collar workers within the manufacturing sector serves as the focal point for sectoral agreements that are to follow. The (formal) relationship between sectoral agreements and final wages differ greatly between sectors. In contrast to many other countries, public sector wages are (again, at least formally) set with an element of individual bargaining within all major agreements. National wage scales were abandoned throughout the Swedish public sector already in the early 1990s (OECD, 1996).

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<sup>6</sup>Path dependence of wages have previously been documented in other settings, see e.g. Beaudry and DiNardo (1991).

Our analysis focus on the impact of the 2007 sectoral agreement between the Swedish Municipal Workers' Union (Kommunal) and the Swedish Association of Local Authorities and Regions (SKL). The agreement covers more than 400,000 workers, which is more than any other agreement on the Swedish labor market. The agreement covers some of the lowest paid jobs in the Swedish economy and most employees are female (about 16 percent male employees, varying slightly over the years). The main occupations are within basic services such as child care and health care.<sup>7</sup>

In normal years the (wage part of the) agreement has three main components. First, it stipulates a minimum wage. This minimum wage is so low that it only is paid to about 10 percent of young first-time employees and it has no direct impact on the wages of incumbent workers or older entrants (see Forslund et al. (2012)). Second, the agreement (sometimes) stipulates a lowest possible wage increase which is payable to all incumbent employees. This minimum amount is typically far below the average wage increase. Finally, it stipulates a required (average) rate of wage increases which should be calculated according to the number of full-time equivalent employees on permanent contracts. This average should be distributed over all employees according to local, in principle individualized, bargaining. Wages are not allowed to discriminate against those on temporary contracts. Table 1 shows the central elements of the agreements and the outcomes between 2004 and 2011.

## 2.1 The 2007 agreement

The different unions within the blue collar confederation (LO) have, to a varying degree, made attempts to coordinate their bargaining strategies before the negotiations concerning the industrial agreement. The bargaining round resulting in the 2007 municipal agreement was preceded by heated discussions between different unions within the LO confederation where unions dominated by low wage females, including the Swedish Municipal Workers' Union, were pushing for a general emphasis on lifting the wages of low wage females during the upcoming negotiations. As a result, many of the 2007 agreements across the Swedish economy resulted in larger increases in minimum wages than in average wages and larger wage increases in sectors with large shares of low-paid female workers (Konjunkturinstitutet, 2007).

The municipal agreement covering 2007 to 2009 was signed in April 2007. The agreement implied that workers, instead of wage increases, should receive two lump sum payments (5400 SEK in total) during 2007. The motivation for this odd solution

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<sup>7</sup>See a description of the occupations covered by the agreement in table A1 the appendix.

Table 1: Details of wage agreement for municipal and county blue collar workers 2004-2011

Year	Required average wage increase	Guaranteed individual increase	in-wage	Change in minimum wage - workers 19 or older	Change in minimum wage - workers with at least 1 year experience	Year specific details
2004	2 %	0		0	7.7 % *	5 % average wage increases among child care workers
2005	510 SEK	175 SEK ☐		3.1%	0	
2006	500 SEK	175 SEK ☐		3%	7,1 %	
2007	0	0		0	0	5400 SEK lump sum
2008	1400 SEK	0		0	0	400 SEK per female earning below 20,000 SEK
2009	720 SEK	175 SEK		10,9%	13.3%	200 SEK per female earning below 20,000 SEK
2010	490 SEK	100 ☐		2.5 %	2.26 %	
2011	480 SEK	100 ☐		2.45 %	2.21 %	

\*Calculated as increase from the level of minimum wage for worker 19 or older 2003. The latter minimum wage definition did not exist until 2004. ☐ Negotiated centrally, and can be overruled by local negotiations.

was a perceived problem with lags in the local negotiations. By skipping one round of local negotiations, the partners hoped that the 2008 round of local negotiations would be completed already in January 2008.

The second unusual part of the agreement, which we study in this paper, was a specific focus on low paid females *within* the agreement. On top of the normal required average wage increases (on average 1400 SEK/month and full-time employee), it was stipulated that an additional 400 SEK/month should be added to the required average increases for each *female* earning below a full-time equivalent of 20,000 SEK/month. Thus, during 2008, the share of low wage females mattered for wage increases according to collective agreements. The third year of the agreement (2009), had a similar construction but instead added 200 SEK/month for each low paid female on top of the general increase of 720 SEK/month. Minimum wages were kept unchanged during 2007 and 2008, and there were no guarantees for individual wage increases for these years. For 2009, the minimum wage increased by 10.9 percent<sup>8</sup> and the all workers were guaranteed a wage increase of at least 175

<sup>8</sup>The minimum wage for workers with at least one year of experience increased with 13.3 percent in 2009

SEK/month.

Notably, while 20,000 SEK was a very low wage in many sectors at the time, this was not the case within this particular part of the labor market. Therefore, a large share of the covered workers within this agreement are considered low-wage earners when using this cut-off (see wage distribution in section 3).

### 3 Data and descriptive statistics

The paper primarily relies on wage data from the "Structure of Wages Statistics" originally collected from the municipalities and counties by the National Mediation Office. These data allow us to separate between workers covered by blue and white collar agreements and hence identify our population of interest. The data set contains wages, occupations and working hours in November during 2003-2011 for all employees within the municipalities and counties covered by the agreement. Wages are reported as monthly "full-time equivalents" which means that they are adjusted to take into account differences in working hours (i.e. they measure hourly wages multiplied by normal monthly full-time hours within the agreement). The wages we use in this paper are "base wages", which includes all persistent wage components (including, e.g. supplements for managerial work), but not time-varying supplements such as overtime compensation.

Individuals can be followed over time through an anonymized indicator variable. Place of work (or "establishment") is added by Statistics Sweden using data from Swedish tax authorities that are available for all years except 2011. To these data we add basic demographic characteristics and final grades from compulsory schools (age 16) which are available for all individuals graduating from 1988 onwards.

We place two initial restrictions on the data: First, we use one observation per worker and year. Here we give precedence to the observation with the highest annual earnings according to Statistics Sweden (if there are multiple entries) and the observation with the highest wage if there are multiple observations for the same employer. Second, we remove workers with missing establishment codes. These are all employees without a physical place of work, including many within the home care sector.

As could be seen from table 1 there are clear differences in the agreement for 2007 compared to the previous and following years. In 2007 all wage growth was paid as a lump sum and therefore the base wage which we use for studying wage changes remained at the exact same level for 80 % of workers between 2006 and

Table 2: Wage dispersion 2006 and wage growth dispersion 2005-2006

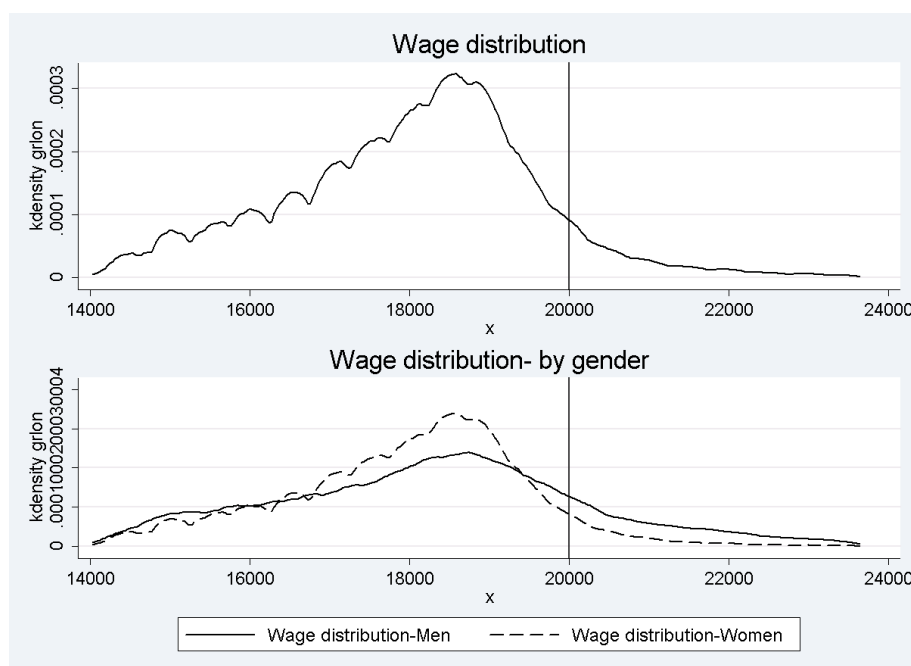
Panel A. Wage Dispersion					
	Mean (Ln SEK)	Variance (Ln SEK)	90/10	50/10	Wage gap $\varpi$ (ln SEK)
Total SEK	9.7932	0.0091	1.2608	1.1579	-0.0269
Organization		0.0085	1.2484	1.1516	-0.0243
Workplace		0.0067	1.2202	1.1286	-0.0022
Local Occupation		0.0064	1.2106	1.1252	-0.0028
Job		0.0048	1.1715	1.0977	0.0069
<i>Within large occupations</i>					
Child-care workers	9.7814	0.0078	1.2556	1.1605	-0.0109
Assistans nurses, hospital assistants	9.8210	0.0051	1.1859	1.1151	0.0032
Home based personal care workers	9.7435	0.0093	1.2767	1.1639	0.0099
Attendants, psychiatric care	9.8073	0.0085	1.2544	1.1611	0.0011
Panel B. Wage Growth Dispersion					
	Mean	Variance	90-10	50-10	Wage gap
Total SEK	0.0334	0.0006	0.0410	0.0146	0.0003
Organization		0.0005	0.0305	0.0096	0.0003
Workplace		0.0005	0.0323	0.0103	0.0002
Local Occupation		0.0005	0.0312	0.0096	-0.0002
Job		0.0004	0.0289	0.0091	-0.0003
<i>Within large occupations</i>					
Child-care workers	0.0325	0.0005	0.0381	0.0137	-0.0005
Assistans nurses, hospital assistants	0.0323	0.0004	0.0374	0.0140	-0.0010
Home based personal care workers	0.0364	0.0008	0.0455	0.0138	-0.0002
Attendants, psychiatric care	0.0338	0.0007	0.0463	0.0191	0.0008

Note. P90/P10 and P50/P10 are calculated within each organization/Workplace/Local Occupation/Job. Mean of ratios presented here. In Panel B P90-P10 and P50-P10 is used instead as some P10 equals 0).  $\varpi$  Wage gap is defined as the estimate in simple wage regression controlling only for female dummy and Age. Summary statistics presented for the unrestricted data, which means the whole population of covered workers excluding only observations with unidentified workplaces.

2007.<sup>9</sup> For this reason we will consistently use wage changes between 2005 or 2006 (or before that) whenever we study the evolution before the agreement of interest (covering changes between 2007 and 2008).

<sup>9</sup>Figure A1 in the appendix shows the share of all workers who remained at their workplace but who did not change their base wage between year t and year t+1 for years 2003 to 2009.

Figure 1: Wage dispersion 2007



Note. Plotted for unrestricted data excluding only workers with unidentified workplaces.

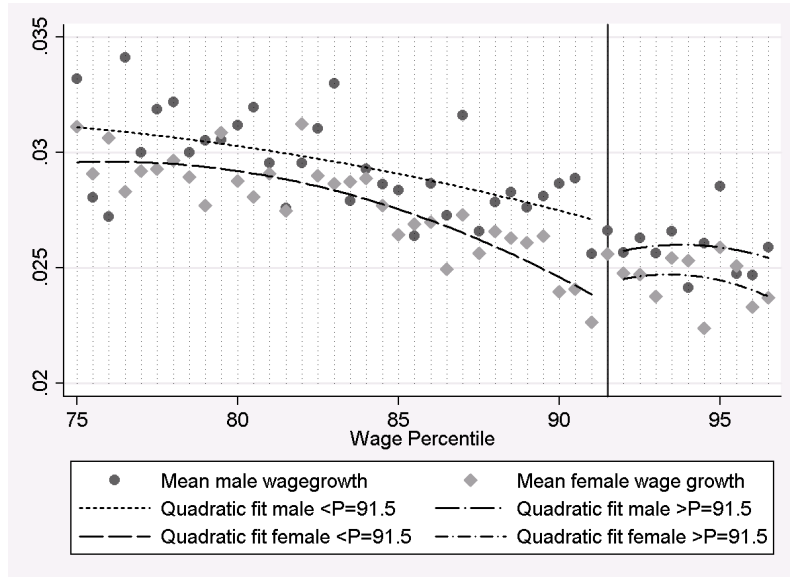
In table 2 we describe the dispersion of wages and wage increases as well as wage differences between men and women before the agreement was struck. As is evident from the table, wages within these jobs are fairly compressed and there are very small wage differences between men and women: On average men earn 2.7 percent more than women, which reflects the fact that the municipal sector is the sector which has the smallest gender wage gap in the Swedish economy (Konjunkturinstitutet, 2007). The ratio between the 90th and the 10th percentile is 26 percent. We also see that most of the overall wage dispersion within municipalities, occupations, establishments and even jobs (occupation-establishment combinations) remains, whereas gender differences appear to primarily originate from differences across occupations.

A similar pattern pertains to wage increases. The average wage increase was 3.3 percent with a 90-10 ratio of 4.1 percent. Most of this dispersion remains within workplaces and within the occupations. Differences in wage growth between men and women are virtually non-existent.

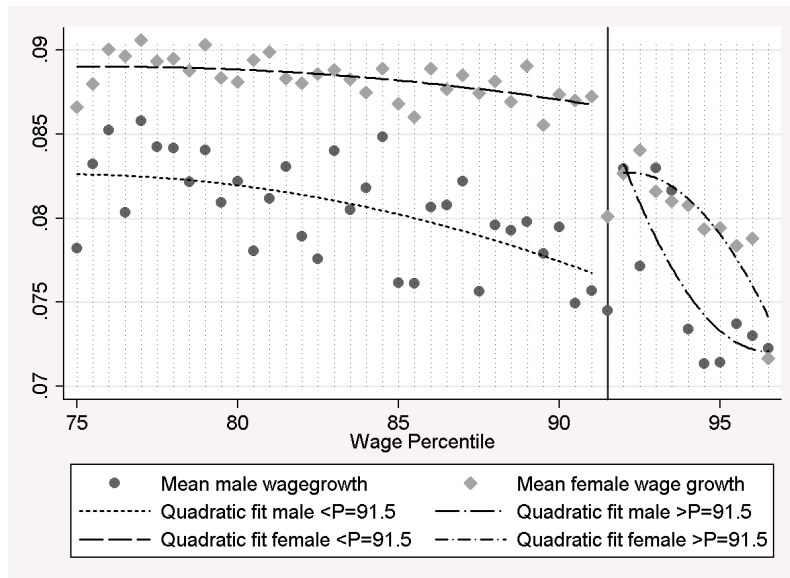
In figure 1, we show the wage distribution in November 2007. Since the targeted increase we are interested in was supposed to take effect at the turn of the year, this

Figure 2: Wage increases by wage percentile

(a) 2005-2006



(b) 2007-2008



Note. Plotted for unrestricted data excluding only observations with unidentified workplaces. The vertical line shows the cut-off for 20,000 SEK in 2007.

is a reasonable proxy for the target distribution. The figure shows that most of the wage distribution falls below the "low wage" threshold of 20,000 SEK, emerging at the 91st percentile of the distribution of base wages. The average base wage within

our sample is 18,050 SEK.

Figure 2a and 2b shows wage increases by gender and (half) wage percentiles during 2005-2006 and 2007-2008 for an interval including wages 1,000 SEK above or below the low wage cut-off of 20,000 SEK in 2007. The percentiles of the wage distribution which this interval translates to are also plotted for the wage growth between 2005-2006 for comparison. As is evident, wage increases for females within this interval close to the low-wage cut-off point were on average much higher relative to men during the second period than during the first period. It is also evident that the shift in gender differences is particularly pronounced for those earning below 20,000 SEK.<sup>10</sup> Notably, however, there is a strong relationship between individual wages and the fraction of low-wage (below 20,000) women at the establishment as shown in Figure A2 in the appendix. For this reason we will study the separate effects of the own treatment status, as well as of the composition of the workplace.

## 4 The first stage impact: wage growth during 2007-2008

The agreement specifies an extra wage increase for each female earning below 20,000 SEK. According to the agreement, actual wage increases should be determined at a level where performance can be monitored, but local partners have large degrees of freedom concerning how this should be implemented in practice. Our initial conjecture was that wage increases either were allocated by individual treatment status (being a low paid female), or by the treatment intensity (share of low paid females) at the establishment level. In order to study the impact on other outcomes, we first need to separate between these two hypotheses since if, e.g., the impact depended on the individual treatment status, we expect the wage of low paid women to respond more to the intervention than others in other dimensions as well. A third alternative hypothesis is that the wages were smoothed across the entire organization (municipality or county) which would make it difficult to study the impact on other outcomes.

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<sup>10</sup>The higher wage growth overall between 2007 and 2008 than between 2005 and 2006 is due to that wage increases 2007 were paid as a lump sum and thus base wages does not change between 2006 and 2007.



## 4.1 Restrictions

For the empirical analysis we add four restrictions on our data.<sup>11</sup> Firstly, we exclude all individuals with very few contracted working hours (less than 25 % of full time), as the wage growth of these individuals are likely to be subject to a different wage setting scheme.<sup>12</sup> We furthermore remove outliers in terms of wage growth (1 percent in each tail). We thereafter exclude newly employed individuals (defined as individuals who have not been employed in the organization any of the last two years). We do this as a proxy for workers on permanent contracts, but we also present results from a robustness check where new entrants are added to the data and the results are completely unchanged. Lastly, we focus our baseline wage growth analysis on workers remaining within the original establishment since these are the ones that are directly treated by the agreement. For long-run estimates we require that workers remain within the organization (i.e. Municipality or County). Again, robustness exercises suggest that the restriction to establishment-level stayers is unimportant for the conclusions.

## 4.2 Effect of individual treatment status

To study the impact of the agreement we first model the wage increase as an individual process with an individual-level treatment  $T$  taking the value one for females earning below 20,000 SEK and 0 for all males and high wage women. This allows us to identify the effect of the agreement using a cross-sectional differences-in-differences strategy controlling (separately) for the direct impacts of both gender and initial wages.

Our outcome of interest is the individual wage growth ( $\Delta wi$ ). Since this outcome is derived by taking first differences at the individual level, we will, throughout the analysis, account for persistent wage differences across workers (i.e. the model corresponds to traditional "individual fixed effects" models specified in log levels). We control for gender by a dummy ( $D_i^{Female}$ ). Our controls for initial wages are defined by a set of dummies for each percentile (denoted by  $\pi^p$ ) in the (initial) wage distribution. Using percentile-dummies allows us to contrast the estimated outcomes of the agreement with patterns during preceding years.<sup>13</sup> Formally we estimate:

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<sup>11</sup>For a description of how sample sizes changes with each restriction, see table A2 in the appendix.

<sup>12</sup>The estimated effects on wages are robust to relaxing this restriction.

<sup>13</sup>The wage percentiles are calculated on the wage distribution in the unrestricted data. We focus this model on the sample range between 19,000 and 21,000 SEK in 2007 which translates to the 75th to 96th percentile of the 2007 wage distribution. Thus, when studying pre-treatment

$$\Delta w_i = \sum_{p=75}^{96} \phi^p \pi_i^p + \delta D_i^{Female} + \gamma T_i + \beta X_i + \varepsilon_i, \quad (1)$$

where  $T_i = I[\pi_i^p < 91; D_i^{Female} = 1]$ .

The vector  $X_i$  include controls for education level, a dummy for being foreign born, a set of controls for working hours measured in percent of full-time work.<sup>14</sup> as well as dummies for different tenure levels<sup>15</sup> and a set of fixed effects defined either at the organization (Municipality/County) or establishment level. We estimate the model on the sample range between 19,000 and 21,000 SEK in 2007 which translates to the 75th to 96th percentile of the 2007 wage distribution.

The results are presented in table 3. Columns 1-4 show an individual-level effect of being a low paid woman. Column 1 controls for demographics and wage percentile, while column 2 adds organization (employing municipality or county) fixed effects. Importantly, the fact that the effects remain if we control for organization fixed effects implies that the wages are indeed not smoothed across the entire organization. The estimated effect of being a low paid female, compared to being a highly paid female or a low paid male, is about 0.26 % (approx. 50 SEK) when including organization fixed effects.

As shown in Figure A2 in the appendix, individual treatment status is very highly correlated with the treatment intensity at the establishment level. To identify the individual level impact, net of the impact of the establishment-level treatment intensity, we re-estimate the model after including establishment fixed effects. The results of this exercise, presented in column 3, shows that the estimate decreases radically and becomes insignificant once we control for these establishment fixed effects. This implies that the wage effect of being a low wage woman is to a large degree driven by employment in workplaces with a large share of low wage women. Notably, this is fully in line with the spirit of the agreement which stipulated that *average* wage increases should be a function of the fraction of low wage females.<sup>16</sup>

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years, we focus on individuals at the same place in the wage distribution, i.e; between the 75th and 96th wage percentile of that year.

<sup>14</sup>The distribution of individually contracted hours as a fraction of full-time work is displayed in figure A3 in the appendix. There are three noticeable spikes are at 50, 75 and 100 percent of full time and we therefore control for three dummies corresponding to these spikes, as well as for a linear function of individually contracted hours.

<sup>15</sup>Tenure is defined as number of years at the workplace. Dummies for each year of tenure, truncated at 5 years.

<sup>16</sup>Controlling for establishment level treatment (see section 4.3) instead of using establishment fixed effects in the model corresponding to column 3 of Table 3 show similar results.

Table 3: Test of individual level treatment

	(1)	(2)	(3)	(4)
		t=2007		t=2005
	$\ln w_{t+1} - \ln w_t$	$\ln w_{t+1} - \ln w_t$	$\ln w_{t+1} - \ln w_t$	$\ln w_{t+1} - \ln w_t$
Female*Below wage percentile 91	0.00307*** (0.00118)	0.00262** (0.00109)	0.00124 (0.000986)	-0.000910** (0.000444)
Observations	61,471	61,471	61,471	60,260
R-squared	0.070	0.123	0.431	0.712
Fixed effects		Organization	Establishment	Establishment
Clusters	Organization	Organization	Organization	Organization

Note. Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Estimation of equation 1. Individual level controls include dummies for wage percentile, gender, age, age squared, education level dummies, a dummy for being foreign born, tenure level dummies, three dummies for spikes in, and a linear control for, contracted hours of work as fraction of full time.

We find no corresponding effects when reestimating the model for the 2005-2006 period (column 4) using the same percentile range and same controls as in column 3. Instead we here find a very small (significant) negative effect of being a low wage woman. This indicates that before this special agreement low wage women had slightly lower wage increases than both low wage men and high wage females.

### 4.3 Effects of the establishment level treatment intensity

Next, we turn to estimating models using the establishment level treatment intensity (*ETI*) as the main variable of interest. We calculate this fraction (by establishment) as the share of all employees that are female and earned less than 20,000 SEK in 2007. The mean share of female low wage workers in 2007 is 0.78 with a standard deviation of 0.24.<sup>17</sup> In addition, we calculate the share of females (*FS*) and the share of low wage workers overall (*LWS*).<sup>18</sup> We then estimate the impact on the individual wage growth of the establishment level wage increases controlling for these shares. Formally,

<sup>17</sup>We find similar means and standard deviations of "treatment intensities" for all years in our data if we using a low-wage cut-off defined by the percentile corresponding to 20,000 SEK in 2007 for the other years.

<sup>18</sup>All establishment level variables are defined by individuals who have working hours more than 25 percent of full time, and who are not recent entrants in the organization. As the wage setting scheme is based on full time equivalents, ETI, FS and LWS are calculated from the total number of full time employments among the workers. This means that two half-time workers are counted as one full time employed worker.

Table 4: Test of establishment level treatment

	(1)	(2)	(3)
	Full data, t=2007		Narrow data, t=2007
	$\ln w_{t+1} - \ln w_t$	$\ln w_{t+1} - \ln w_t$	$\ln w_{t+1} - \ln w_t$
ETI	0.0172*** (0.00375)	0.0160*** (0.00345)	0.0174*** (0.00382)
Observations	116,765	116,765	39,341
R-squared	0.136	0.155	0.132
Fixed effects		Organization	Organization
Clustered	Organization	Organization	Organization

Note. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Estimation of equation 2. Individual level controls include dummies for wage percentile, gender, age, age squared, education level dummies, a dummy for being foreign born, tenure level dummies, three dummies for spikes in, and a linear control for, contracted hours of work as fraction of full time. Workplace level controls include share low paid, share female and mean log wage level.

$$\Delta w_i = \sum_{p=1}^{100} \phi^p \pi_i^p + \delta D_i^{Female} + \beta X_i + \mu \bar{w}_j + \gamma^e ETI_i + \Psi[FS_i^E, LWS_i^E] + \varepsilon_i. \quad (2)$$

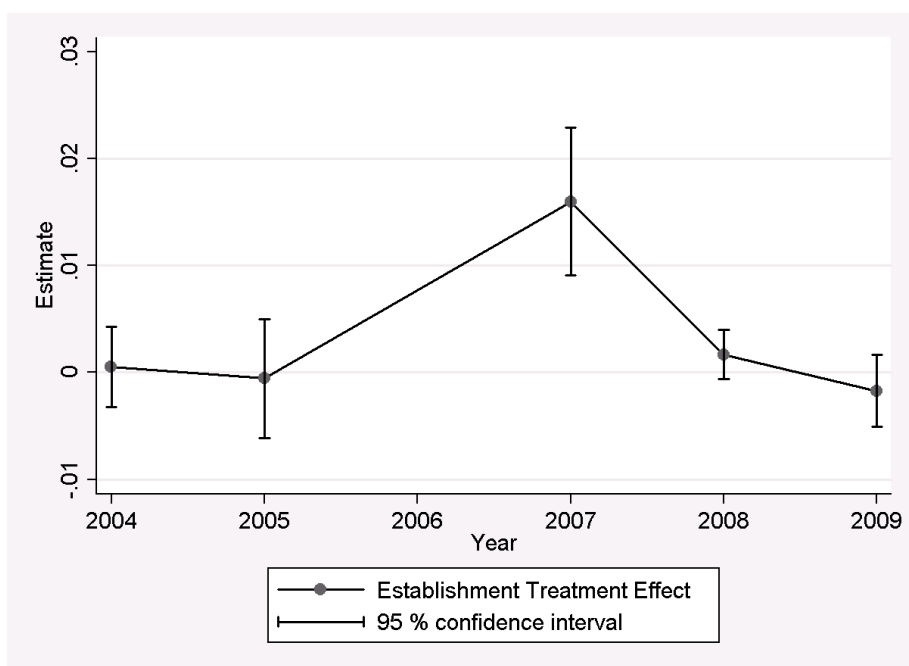
The model controls for the same individual level controls as equation (1) through  $X_i$  as well as the establishment mean log wage ( $\bar{w}_j$ ) in 2007. When estimating this model, we do not restrict the analysis to individuals within the narrow wage range defined above, but include all covered workers, regardless of initial wage. We do restrict the analysis, though, by focusing on establishments with common support in terms of both gender and wages, implying that all establishments should have shares of low wage workers and males strictly between zero and one.<sup>19</sup>

The results presented in table 4 suggest that the agreement had a significant impact on actual wages. Extrapolating from the share-estimates suggest that wages increased by an amount which is fairly close to the stipulated 400 SEK if the share of low wage females increased from 0 to 1, keeping constant the direct impact of the share of females and the share of low wage workers.<sup>20</sup> Column 1 estimates the model without organization fixed effects. Adding organization dummies in column 2 only has a marginal effect on the estimate of interest.

<sup>19</sup>In general, the results are robust to relaxing this restriction, as seen in section 5.5.

<sup>20</sup>When using change in Wage (SEK) as the regression outcome we get an estimate of 346 SEK.

Figure 3: Establishment treatment effect 2004-2009



In column 3, we estimate the same model, but for the narrow range of individuals with pre-period wages of between 19,000 and 21,000 to compare to the earlier estimates of the individual level effect. The effect is noticeably stable considering that we remove two thirds of the data (standard errors remain largely unaffected since we cluster on organization).

To ensure that our models are not capturing generic differences in wage growth between establishments with a high share of low wage females and other establishments, we have also re-estimated the model using data from several years (compare with column 2 in table 4 above). Here, we define workers with wages below wage percentile 91 as earning low wages when calculating low-wage shares and treatment intensities for other years. The results for two leading and lagged years respectively are presented in figure 3.<sup>21</sup> As is evident, the results suggest that 2007-2008 is a true outlier in terms of the estimates. The fact that we fail to find any effects in the succeeding years suggest that the ambition to target these particular establishments disappeared once new agreements were struck.

Overall, the results of this section suggest that the agreement of interest had an impact on the wage growth of the individuals employed in establishments with a larger share of low wage females. This additional wage growth does not differ

<sup>21</sup>As explained above, we cannot use data for 2006 since very few workers had their wages revised during 2007.

significantly between low wage females and other females or low-wage workers within the same establishments. Therefore, apart from learning about the actual outcome of the agreed wage change, we can use this property of the agreement to examine further outcomes for the covered workers.

## 5 Post-agreement outcomes for the covered individuals

### 5.1 Wage trajectories

To document the impact of the agreement beyond the direct effects on the workers employed by the covered establishments we first follow the career trajectories of the covered workers. Clearly, the implications of a one time change in the wage growth distribution differs depending on whether the effects survives over a number of years. The extent to which a one time wage-push persists over time is first-order diagnostic over how sensitive the contemporary wage distribution is to historical wage agreements.

In order to study the impact on wage trajectories, we replace the 1 year differences of equation (2) above by 2, 3 and 4 year differences which capture the total accumulated wage growth from 2007 to 2009, 2010 and 2011 respectively. These estimates should be stable if the total gain from the treatment intensity in 2007 remained during the years to follow. If, on the other hand, other workers are catching up, estimates should go towards zero and if wage growth begets future wage growth, the estimates should grow over time.

Here it should also be noted that interpretation of, in particular, the 2-year difference (i.e. from 2007 to 2009) will reflect the impact of the treatment intensity in 2008, but also the fact that each female that remained below the 20,000 SEK threshold in 2008 provided an extra wage increase of 200 SEK during 2009 (thus, treatment intensities may be correlated).<sup>22</sup>

We estimate the model for the year-by-year sample of workers remaining within the establishment the first year, and workers remaining within the organization 2, 3, and 4 years after 2007. We also estimate the model for a balanced sample of workers remaining within the organization during all four years. The results presented in

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<sup>22</sup>Although Figure 3 shows that the effect of low wage females in the workplace during 2008 had an insignificant effect of wage growth between 2008 and 2009, it should be noted that the analysis of the figure used a threshold defined from percentiles and not from nominal 20,000 SEK as in the agreement.

Table 5: Long term effects of establishment level treatment

Panel A. Balanced data (4 years stayers in organization), t=2007				
	(1)	(2)	(3)	(4)
	$\ln w_{t+1} - \ln w_t$	$\ln w_{t+2} - \ln w_t$	$\ln w_{t+3} - \ln w_t$	$\ln w_{t+4} - \ln w_t$
ETI	0.0165*** (0.00403)	0.0185*** (0.00469)	0.0177*** (0.00555)	0.0203*** (0.00587)
Mean outcome	.09	.13	.15	.18
Observations	82,942	82,942	82,942	82,942
R-squared	0.154	0.205	0.250	0.224
Panel B. Unbalanced data, t=2007				
ETi	0.0160*** (0.00345)	0.0200*** (0.00442)	0.0181*** (0.00544)	0.0217*** (0.00553)
Mean outcome	.09	.13	.15	.18
Observations	116,765	115,680	107,263	102,179
R-squared	0.155	0.201	0.232	0.210
Fixed effects	Organization	Organization	Organization	Organization
Clusters	Organization	Organization	Organization	Organization

Note. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. We only condition on staying in the establishment the first year. Subsequent years we condition on staying in the organization. Estimation of equation 2. Individual level controls include dummies for wage percentile, gender, age, age squared, education level dummies, a dummy for being foreign born, tenure level dummies, three dummies for spikes in, and a linear control for, contracted hours of work as fraction of full time. Workplace level controls include share of low paid, share female and mean log wage level.

table 5 shows that the agreement had lasting effects on the wages of covered workers. Indeed we find little evidence of a declining effect. The fact that the wage growth is very similar for those who stayed all the four years (see panel A) as for those in the year-by-year sample (panel B) suggests that the results are not driven by exit of workers with lower wage growth. The table also presents the mean wage growth over 1,2, 3 and 4 years respectively.<sup>23</sup>

In principle, the model could also be estimated using an IV strategy, where the initial wage growth was instrumented by the treatment intensity in 2007. This would require the additional assumption that the agreement only affected the long run evolution of wages through its short-run wage impact, an assumption which seems fairly uncontroversial (apart from the complication arising from the 2009-component). Since the short run (first stage) and long run (reduced form) estimates are of a very similar magnitude, it is however straightforward to infer that the corresponding IV-estimates would be close to unity.

## 5.2 Hours of work

Next we turn to the impact of the wage trajectories on the number of hours worked by the covered employees. The agreement under study provides a very unusual example of an exogenous shock to workers' wages which in principle could allow us to present evidence on the hours elasticity to changes in wages.<sup>24</sup> However, although we are able to document the causal impact of wage changes on changes in hours worked, our estimates must be interpreted as a (partial) equilibrium outcome. We are unable to separate whether the changes in hours worked depend on the behavior of workers or employers. In particular it should be noted that the Municipal workers' union considers the reduction of involuntary part-time work within the sector as one of its key objectives, which indicates that part-time workers may be constrained in their hours of work.

As our measure of hours of work we use individually contracted hours measured

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<sup>23</sup>The high wage growth the first year is a result of an extra scope for wage increases during that year, this phenomena is related to the zero wage growth during the preceding year (2006 to 2007).

<sup>24</sup>From the minimum wage literature it is not clear what to expect in terms of effects on hours of work, see Neumark and Wascher (2008) for a general discussion. For Sweden, Skedinger (2011) show a modest average reduction in hours of work from an increased minimum wage in the retail sector, although the decline is larger amongst the young. Skedinger (2011) concludes that the modest average decline in hours in part is due to selective separation, as many marginal workers (i.e. employed very few hours) exited their jobs due to the minimum wage increase. As our specifications are estimated in first-differenced form with detailed controls for initial hours, we are less exposed to these types of selection effects.



as a fraction of full-time work, usually 40 hrs per week. We use the model of equation (2), with the change in percentage of full time, between 2007 and subsequent years as, the dependent variable.<sup>25</sup> Note that the model includes very detailed controls for initial working hours.

The results presented in table 6 show that the effect on contracted hours of work is negative, but also that they are estimated with fairly poor precision.<sup>26</sup> The negative effect could either be due to income effects dominating substitution effects as in the traditional labor supply setting, or due to cost saving reductions in hours from the demand side. Notably, the mean contracted fraction of full time is increasing within the study population, which suggests that the negative estimates can be interpreted as smaller increases in contracted hours of work for the covered workers. Comparing the estimates for the change in fraction of full time to the wage growth estimates, we see that the elasticity between wages and hours is near -1. As wages are full time equivalents, this elasticity can be interpreted as a net effect on gross disposable income close to 0.

Around 50 percent of the remaining workers do not change their hours of work at all. We have therefore also estimated models where we let the outcome be the probability of increasing or decreasing hours of work. As outcomes we use dummies taking on the value 1 if the employee has increased (decreased, in corresponding models) her hours of work between 2007 and 2008 and the value zero if hours remained unchanged or decreased (increased). Results, presented in table A3 in the appendix, are well in line with the results of table 6, in particular considering that average hours were increasing within the study population: A higher treatment intensity is associated with a lower probability of increasing hours of work, whereas the probability of decreasing contracted hours is unchanged.<sup>27</sup>

### 5.3 Separations

Next we turn to the impact on the separation rates of covered workers. Here, we follow in the tracks of the minimum wage literature which to a large extent has

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<sup>25</sup>Outliers (1 percent in each tail) in change in hours worked are dropped.

<sup>26</sup>These results are, in contrast to other main results in the study, sensitive to the inclusion of extreme values. If these are included these estimates are no longer significant.

<sup>27</sup>Data also contain information on actual hours of work during the survey month. This variable deviates from contracted hours due to overtime and absence and thus provides a noisier measure of hours. Using this variable we find results that are sensitive to the choice of empirical model: We find positive effects of the establishment treatment intensity on changes in actual hours worked, but also an increased probability of decreasing actual hours of work. Our impression is that contracted hours is a less noisy and hence more suitable variable for studying the response of hours worked.

Table 6: Effect of treatment intensity in 2007 on changes in (individually contracted) hours since 2007

	(1)	(2)	(3)	(4)
	$h_{t+1}^C - h_t^C$	$h_{t+2}^C - h_t^C$	$h_{t+3}^C - h_t^C$	$h_{t+4}^C - h_t^C$
ETI	-0.958** (0.414)	-0.102 (0.510)	-0.928* (0.528)	-0.367 (0.673)
Mean outcome	.9	1.14	1.5	1.4
<i>Mean individually contracted hours as a fraction of full-time (2007): 86.6 %</i>				
Observations	115,249	113,981	105,636	101,057
R-squared	0.126	0.167	0.203	0.212
Fixed effects	Organization	Organization	Organization	Organization
Clusters	Organization	Organization	Organization	Organization

Note. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. We only condition on staying in the establishment the first year. Subsequent years we condition on staying in the organization. Estimation of equation 2. Outcome variable is the change in contracted hours worked as fraction of full time between year t and subsequent years. Individual level controls include dummies for wage percentile, gender, age, age squared, education level dummies, a dummy for being foreign born, tenure level dummies, three dummies for spikes in, and a linear control for, contracted hours of work as fraction of full time year t. Workplace level controls include share of low paid, share female and mean log wage level.

focused on the impact of changes in the minimum wage on the rate of separations. Examples include Kramarz and Philippon (2001) and Abowd et al. (1997) who find disemployment effects of minimum wage increases for France and US. For Sweden, Skedinger (2011) used data from the retail sector, finding evidence of substitution (in terms of separations) between workers affected by the minimum wage and those unaffected. A main reason for focusing on separations rather than entrants is that the target population (workers at risk) is well-defined when analyzing separations, but not when analyzing recruitments. This is particularly true when analyzing changes in sectoral agreements since workers may change their sectoral allocation of search efforts in response to sector-specific changes in wage levels.

Here we, again, estimate a straightforward version of equation (2) where we use a dummy taking the value one for individuals who leave the establishment as the outcome.<sup>28</sup> The results, presented in Table 7, column 1 suggest that the average exit rate remained unaffected.

Since mobility among older workers tends to be fairly low, we have also explored the exit rates among younger workers. Here, we focus on workers aged 36 and younger which implies that we study the cohorts for which we have data on compulsory school grades. Results, presented in column 2, suggest that the average exit rate for this group also remained unaffected.

Finally, we turn to the selectivity of workers who remain within their jobs.<sup>29</sup> Here we focus on the young workers and characterize the workers by their percentile ranked grades from the end of compulsory school (age 16). This is an indicator which previous research has shown to be strongly related to the incidence and duration of unemployment among young workers in Sweden (OECD, 2008), and we therefore use it as an indicator of the workers' market values. In column 3, we interact the estimate of interest with an indicator for having grades below the median at the end of compulsory school. The results suggest large differences in the impact of separation probabilities depending on position in the grade distribution. Workers with lower than median grades have significantly larger probability of exiting the establishment due to a higher treatment intensity.<sup>30</sup>

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<sup>28</sup>This implies that separations are defined as individuals working in an establishment during November during year  $t$  (2007) but not in November during  $t+1$ . The dummy variable is set to missing (i.e. the observation is removed) if the entire establishment has disappeared.

<sup>29</sup>We have also estimated models analyzing separation probabilities separately for females and males, as well as for native born and foreign born. We found no significant differences in estimated effects.

<sup>30</sup>The results are very similar if we instead define separations at the organization level. The differences between the high and low skilled remain, with the workers with grades above the

Table 7: Effects on separations 2007-2008

	(1)	(2)	(3)	(4)	(5)
	Without new entrants			With new entrants	
	Pr(exit)	Pr(exit)	Pr(exit)	Pr(exit)	Pr(exit)
ETI	0.0159 (0.0562)	-0.00477 (0.0751)	-0.145 (0.0995)	-0.0334 (0.0693)	-0.171** (0.0849)
Lower grades		-0.0259*** (0.00831)	-0.0257*** (0.00776)	-0.0336*** (0.00724)	-0.0332*** (0.00695)
Lower grades*ETI			0.204* (0.116)		0.222** (0.0858)
Mean separation rate	.21	.35	.35	.41	.41
Observations	147,835	24,394	24,394	34,346	34,346
R-squared	0.114	0.113	0.114	0.141	0.141
Fixed effects	Organization	Organization	Organization	Organization	Organization
Clusters	Organization	Organization	Organization	Organization	Organization

Note. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Estimation of equation 2. Outcome variable is a variable taking on the value 1 if the workers leaves the establishment between year t and t+1, and 0 otherwise. In the specifications where ETI is interacted with Lower grades (column 3) also the variables Share of Female and Share of Low paid are interacted. Individual level controls include dummies for wage percentile, gender, age, age squared, education level dummies, a dummy for being foreign born, tenure level dummies, three dummies for spikes in, and a linear control for, contracted hours of work as fraction of full time. Workplace level controls include share of low paid, share female and mean log wage level.

As many young workers are recent entrants (i.e. starting their job between 2005 and 2007), who are excluded in our base sample, we have re-estimated the model on an extended sample where these workers are included. Results presented in column 4 and 5 of table 7, are very similar to the models for the base sample, but the estimates are more precise.

Overall, these results suggest that the young stock of employees become more selected when wages goes up as those young workers who have better outside option find stronger incentives to stay within their jobs, or as employers become more selective in who, among the young, they allow to stay. This result is well in line with recent research on the effects of minimum wages hikes on separations, which indicates that employers substitute low skilled workers with slightly more skilled labor in response to minimum wage increases (Neumark and Wascher, 2008).

## 5.4 Establishment-level responses

In order to be able to, at least tentatively, analyze the impact on hirings, we have explored empirical models defined at the workplace level. These models control for individual level variables aggregated at the workplace level year 2007, as well as industry dummies (see equation (3)).<sup>31</sup> As outcome variables we use the normalized (by the stock of employees during 2007) number of hires in 2008, normalized separations in 2007, as well as the normalized change in workplace size between 2007 and 2008. Estimates are weighted by workplace size the initial year to reflect the population distribution. Formally we estimate:

$$Y_j = \gamma ETI_j + \Psi[FS_j, LWS_j, \bar{X}_j, \bar{w}_j, \text{Industry}_j] + \varepsilon_j. \quad (3)$$

The results presented in table 8 suggest small positive but far from statistically significant effects on average separations which is well in line with the average effect of individual exits presented above.<sup>32</sup> More importantly, we also find negative effects on establishment level hirings (see table 8). As a consequence, the final column shows that the net effect on employment is negative. This suggests that the higher wage growth induced by the agreement had negative effects on employment, but through reduced hires and not through increased separations.

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median reducing their separation probability. Mean separations rates are about 7 percentage point lower when defined by organization.

<sup>31</sup>As with earlier workplace level variables, these variables are aggregated over all permanent employees at the workplace.

<sup>32</sup>Here we include recent entrants, as well as individuals with low hours of work. The estimates are therefore not directly comparable to the estimate in column 1 of table 7.

Table 8: Effects on hires and employment- workplace level, t=2007

	(1)	(2)	(3)
	Separations, year t	Hires, year t+1	Change in size, year t+1-year t
ETI	-0.0132 (0.0541)	-0.144** (0.0678)	-0.131* (0.0746)
Observations	2,486	2,486	2,486
R-squared	0.480	0.402	0.189
Fixed effect	Organization	Organization	Organization
Clustered	Organization*Industry	Organization*Industry	Organization*Industry

Note. Robust standard error in parenthesis. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  Estimations of equation 3. The outcome variables are normalized by the workplace size year t, why they should be understood as relative changes. Controls here include individual level controls aggregated at the workplace level, as well as dummies for industry affiliation. Estimates are weighted by workplace size year t. Outliers in normalized total hirings, normalized total separations and normalized change in workplace (1 percent in each tail) are excluded.

## 5.5 Robustness

All our models presented above control for permanent differences in wage levels (and hours) between different workers through individual fixed effects which are implicitly accounted for by taking individual-level first differences. However, the estimates may be biased by the selective separations rates for the young (documented above) if these are related to differences in individual-specific wage or hours *trajectories* (i.e. trends). For this reason we have re-estimated the key models while excluding the group of young workers (aged 36 or less) for which we found selective separation rates.<sup>33</sup> These results are presented in Panel A of table 9. Here we see that the positive wage growth effect from being in an establishment with a higher share of low wage females in year 2007 remains largely unaffected when the young workers are excluded (columns 1 and 2). The same is true for the negative effect on hours worked (column 3). This supports the notion that the wage trajectories as well as the reduction in hours worked is not caused by selective separation rates among the young.

Panel B of the same table instead shows how the wage and hours estimates are influenced by including recent entrants in the estimation sample. Overall, earlier estimated effects are unaffected (somewhat, but insignificantly, larger for hours) when including this group.

<sup>33</sup>We also worry less about selection amongst the older workers since they are considerably less mobile overall.

Finally, our main data set excludes workplaces which either do not have both men and women, contain only low wage or high wage employees in order to isolate the impact without letting the other controls be affected by outliers. In panel C, we explore the sensitivity of our estimates related to wage growth, changes in hours and separation rates to this restriction. The results implies that the wage and separation estimates remain robust to relaxing this sample restriction. A possible concern is that we find no significant effect on hours worked when relaxing this restriction. A potential explanation may be that the composition of part-time workers, and the evolution of working hours, is very different within all-male or all-female establishments.

As further robustness tests, we have also estimated the effect on individual wage growth during the first year, as presented in table 4, within a broader sample which also include those that change establishments within the organization (Municipality or County). The estimates are completely robust.<sup>34</sup> Overall, the effect of share of low paid females at the workplace on individual wage growth is robust to inclusion of further workplace level controls and more flexible controls for the workplace wage level defined by share of female and male workers within each decile of the overall wage distribution.<sup>35</sup>

## 6 Conclusions

This paper has focused on a documentation of the impact of negotiated wage increases on actual wages, worker career trajectories and separation rates as well as establishment-level separation and hiring responses. To this end, we have exploited the impact of a Swedish collective agreement for blue-collar workers employed by municipalities and counties. Our results show that the agreement had a substantial first-order effect on the wages of workers within the covered establishments. The wage increases appear to be lasting over at least four years for covered workers.

When studying labor input responses, we first find evidence of reduced hours of work (relative to other workers) which could imply that employers adapt to the wage hike by reducing labor input along the intensive margin. In a context where unions consider involuntary part-time work as a major concern, this is likely to be a negative side-effect from the workers' perspective. The negative impact on hours

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<sup>34</sup>The estimate is 0.0152 with a standard error of 0.00345.

<sup>35</sup>Added workplace level controls are aggregates of individual level controls among the employees who are not new entrants; share with at least high school education, share with low tenure, share foreign born and mean age.

Table 9: Sensitivity analysis, t=2007

	(1)	(2)	(3)	(4)
	$\ln w_{t+1} - \ln w_t$	$\ln w_{t+3} - \ln w_t$	$h_{t+1}^C - h_t^C$	Pr(Exit)
<i>Panel A. Without Young Workers</i>				
ETI	0.0142*** (0.00323)	0.0141*** (0.00523)	-0.920** (0.450)	
<i>Mean values</i>	.089	.152	.798	
Observations	101,149	92,501	100,008	
R-squared	0.150	0.214	0.114	
<i>Panel B. With New Entrants</i>				
ETI	0.0155*** (0.00343)	0.0188*** (0.00564)	-1.381*** (0.438)	
<i>Mean values</i>	.09	.154	1.057	
Observations	126,483	115,867	124,545	
R-squared	0.149	0.234	0.142	
<i>Panel C. With workplaces without common support of females and low wage employees</i>				
ETI	0.0138*** (0.00222)	0.0162*** (0.00303)	-0.0538 (0.254)	-0.0496 (0.0321)
<i>Mean values</i>	.092	.157	.889	.212
Observations	224,226	209,059	221,513	285,846
R-squared	0.150	0.240	0.120	0.107
Fixed effects	Organization	Organization	Organization	Organization
Clusters	Organization	Organization	Organization	Organization

Note. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Estimations of equation 2. We condition on staying in the establishment the first year. For column 2 we condition on staying in the organization. Outcome variable in column 1 (2) is the wage growth between year t and t+1 (t+3). Outcome variable in column 3 is the change in contracted hours of work between year t and year t+1. In column 4 the outcome variable is a dummy taking on the value 1 if the workers leaves the workplace and 0 otherwise. Individual level controls include dummies for wage percentile, gender, age, age squared, education level dummies, a dummy for being foreign born, tenure level dummies, three dummies for spikes in, and a linear control for, contracted hours of work as fraction of full time year t. Workplace level controls include share of low paid, share female and mean log wage level.



is large enough to greatly reduce the positive impact on workers' gross disposable income. We also see an increased selectivity amongst the young workers who remain within their jobs as well as indications of a reduced number of new hires in workplaces with a higher treatment intensity. Jointly, the estimates imply that employers reduce their use of labor inputs, and benefit from a relative skill upgrading, when wages are increased. At the establishment-level, we find evidence consistent with falling hiring rates as a result of the agreement.

Although the particular agreement we are analyzing appear quite unique in its seemingly arbitrary allocation of wage increases, our conjecture is that there is considerable room for more empirical work on how collective agreements affect the outcomes of covered workers and establishments. In order to reduce transaction costs, social partners (just as legislators) often resort to fairly arbitrary cut-offs when signing collective agreements and these cut-offs can provide very useful foundations for econometric evaluations. Yet, in particular since the wages of low skilled workers across Europe, at least formally, are determined through collective agreements, it is striking how little we know about the real impact on these agreements on the outcomes of the covered groups. We thus believe that a further investigation of the consequences of negotiated wage increases is a promising avenue for future research on key topics within the core of labor economics such as documenting the nature and extent of wage rigidities, the allocative power of establishment-level wages and spill-over effects of wages between groups.

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## A Appendix

Table A1: Occupational Distribution 2006

	Unrestricted data		Restricted data	
	Mean values	Share males	Mean values	Share males
Legislators, senior officials and managers	0.001	0.345	0.002	0.365
Professionals	0.004	0.419	0.003	0.478
Technicians and associate professionals	0.018	0.450	0.015	0.498
Clerks	0.003	0.640	0.003	0.636
Service workers and shop sales workers (not the below)	0.023	0.458	0.022	0.374
Cooks	0.029	0.098	0.028	0.130
Child-care workers	0.162	0.097	0.080	0.162
Assistant nurses and hospital ward assistants	0.309	0.066	0.420	0.072
Home-based personal care and related workers	0.187	0.105	0.166	0.116
Attendants, psychiatric care	0.124	0.237	0.113	0.324
Skilled agricultural and fishery workers	0.006	0.770	0.007	0.796
Craft and related trades workers (not the below)	0.011	0.968	0.013	0.965
Building caretakers	0.023	0.910	0.023	0.932
Plant and machine operators and assemblers	0.009	0.871	0.010	0.790
Elementary occupations (not the below)	0.009	0.822	0.014	0.823
Helpers and cleaners in offices, hotels etc.	0.035	0.069	0.042	0.076
Helpers in restaurants	0.047	0.047	0.041	0.047

Note. Unrestricted data refers to the raw data over the covered workers, excluding only observations without identified workplace. The restricted data refers to the data used from section 4.3 and onwards, which is the most restricted data used.

Table A2: Sample restrictions

	# Obs 2007
Before restrictions	433,022
<i>After imposed restriction:</i>	
Drop unidentified workplaces	359,532
Drop if hours worked < 25 percent	336,227
Drop 1 percent outlier in wage growth in each tail	330,751
Drop new entrants	289,620
Drop workplaces without common support in share males and share low paid	149,248

Note. Numbers of observations for the year 2007 remaining in sample after restrictions imposed. The numbers are similar for other years.

Figure A1: Share of remaining workers with zero nominal wage growth



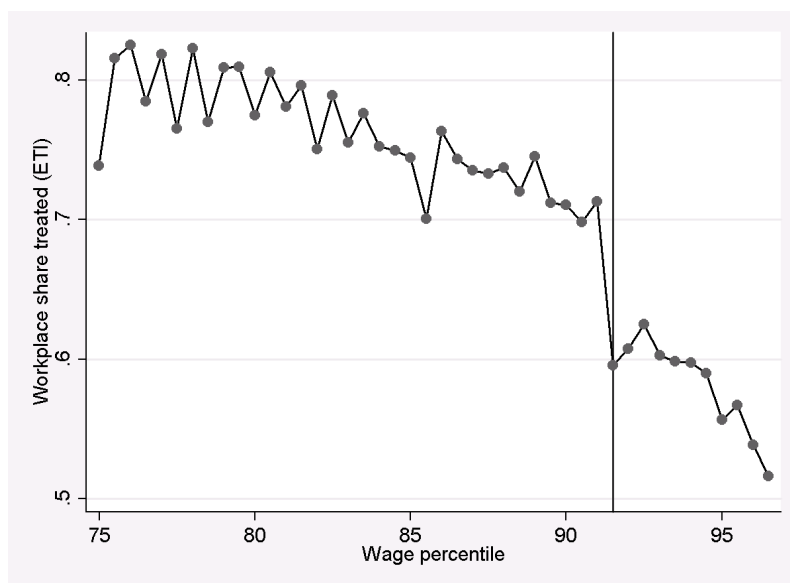
Note. Plotted for unrestricted data excluding only observations with unidentified workplaces.

Table A3: Effects on probability of increasing and decreasing hours worked as fraction of full time, t=2007

VARIABLES	(1) $\Pr(h_{t+1}^C > h_t^C)$	(2) $\Pr(h_{t+1}^C < h_t^C)$
ETI	-0.0335* (0.0172)	0.0111 (0.0140)
Mean outcome	.113	0.067
Observations	115,249	115,249
R-squared	0.185	0.039
Fixed effects	Organization	Organization
Clusters	Organization	Organization

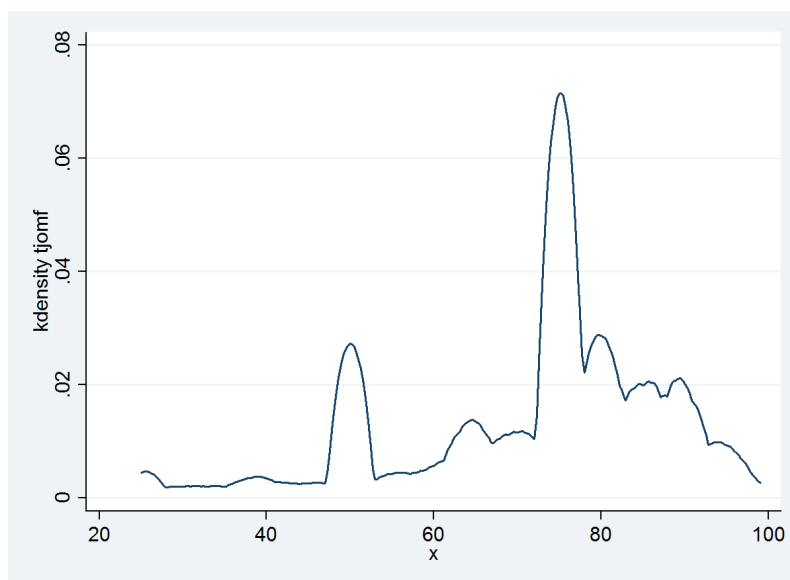
Note. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1 Estimation of equation 2. Outcome variable is dummy for increasing contracted hours of work (column 1), and dummy for decreasing contracted hours of work (column 2). Individual level controls include dummies for wage percentile, gender, age, age squared, education level dummies, a dummy for being foreign born, tenure level dummies, three dummies for spikes in, as well as linear control for, contracted hours as fraction of full time. Workplace level controls include share of low paid, share female and mean log wage level.

Figure A2: Establishment treatment intensity by wage percentile



Note. Plotted for unrestricted data excluding only observations with unidentified workplaces. The vertical line shows the cut-off for 20,000 SEK in 2007.

Figure A3: Distribution over contracted hours of work as fraction of full time



Note. 46 percent are employed at 100 percent. The spikes in the distribution here are at 50 percent of full time (5 percent of observations), and at 75 percent of full time (12 percent of observations). 50, 75, and 100 are the spikes controlled for in the regressions.

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