

Stokke, Hildegunn

**Conference Paper**

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56th Congress of the European Regional Science Association: "Cities & Regions: Smart, Sustainable, Inclusive?", 23-26 August 2016, Vienna, Austria

**Provided in Cooperation with:**

European Regional Science Association (ERSA)

*Suggested Citation:* Stokke, Hildegunn (2016) : Regional payroll tax cuts and individual wages: Heterogeneous effects across education groups, 56th Congress of the European Regional Science Association: "Cities & Regions: Smart, Sustainable, Inclusive?", 23-26 August 2016, Vienna, Austria, European Regional Science Association (ERSA), Louvain-la-Neuve

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# **Regional payroll tax cuts and individual wages: Heterogeneous effects across education groups<sup>\*</sup>**

Hildegunn E. Stokke

Department of Economics, Norwegian University of Science and Technology

[hildegunnes@svt.ntnu.no](mailto:hildegunnes@svt.ntnu.no)

## *Abstract*

The empirical evidence on the incidence of payroll taxation is primarily based on the average wage bill of firms and tends to find partial tax shifting to wages and limited effects on employment. This paper exploits a payroll tax reform in Norway and applies matched employer-employee register data on individual wages to investigate the heterogeneity of labor market effects of reduced payroll taxes across education groups. Based on the difference-in-difference approach, I find that 1% reduction in labor costs generates 0.3% wage increase. Among low-educated workers the degree of tax shifting equals 40-50%, while there is no significant wage effect among college-educated workers. Lower payroll taxes generate an employment expansion along the extensive margin, driven by an increase in the number of low-educated workers, mainly in services. The findings imply that the absolute value of the labor demand elasticity decreases with the level of education.

Keywords: Payroll tax cut, individual wages, heterogeneous effects, education

JEL codes: H22, J23, J31, J38

Date: September 21, 2016

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<sup>\*</sup> I appreciate discussions at the 2015 Norwegian Research Forum on Taxation, the 2015 Meeting of the Urban Economics Association (UEA), the 2016 Meeting of the Society of Labor Economists (SOLE), the 2016 Louis-André Gérard-Varet (LAGV) Conference in Public Economics, the 2016 European Meeting of the Urban Economics Association (UEA), and the research seminar at Statistics Norway, and in particular comments from Rolf Aaberge, Ådne Cappelen, Tarjei Havnes, Lars Kirkebøen, Andreas Kostøl, Stefan Leknes, Jarle Møen, Jørn Rattsø, Bjarne Strøm, Gaute Torsvik, Mark van Duijn, Russell Weinstein, and Jeffrey Zax. I am grateful for the cooperation of Statistics Norway.

## 1. Introduction

Regionally differentiated payroll taxes represent an important policy tool with the intention of stimulating employment in lagging regions. Existing empirical evidence on the incidence of payroll taxation is primarily based on firm-level data on the average wage bill rather than individual wages. The broad finding in the literature is that payroll tax reductions are partly shifted to employees through higher wages, while the employment response is limited. This paper exploits a payroll tax reform in Norway and applies matched employer-employee register data on individual wages to investigate the heterogeneity of labor market effects of reduced payroll taxes across education groups. To my knowledge, this is the first analysis of individual wage and employment effects of payroll tax cuts based on workers' level of education. The findings can shed light on potential differences in the labor markets for low- and high-educated workers with respect to labor demand and labor supply elasticities.

Bohm and Lind (1993) apply the difference-in-difference approach to study employment effects of a payroll tax reform in northern Sweden. They focus on manufacturing firms, and find no significant effect on employment along the extensive margin (number of workers). In a more recent study based on Swedish firm-level data for the private sector, Bennmarker et al. (2009) find no employment effects among firms that existed both before and after the reform. Their wage analysis conclude that 1 percentage point reduction in the payroll tax rate increases the average wage bill per employee by about 0.25%. When accounting for entry and exit of firms, they find indications of positive employment effects while the wage effect becomes insignificant. Korkeamäki and Uusitalo (2009) take advantage of a payroll tax cut in Finland, and find evidence of about 50% tax shifting, while the remaining labor cost reduction has no significant effects on employment. Based on data for manufacturing firms in Chile, Gruber (1997) documents full shifting of payroll taxes to wages and no significant effect on employment. Cruces et al. (2010) take advantage of regionally differentiated payroll taxes in Argentina, and find only partial tax shifting but still without any effect on employment. Studies using tax variation across firms and states in the US (Gruber 1994; Anderson and Meyer 1997, 2000; Murphy 2007) find that taxes are mainly passed on to employees through wages and with limited effects on employment. Some contrarian evidence is provided by Saez et al. (2012), who take advantage of a cohort-based payroll tax reform in Greece and find that higher employer-paid payroll taxes are fully carried by the employer rather than being shifted to employees. Existing analyses of payroll taxes in Norway mainly focus on the manufacturing sector, and include Johansen and Klette (1997), Dyrstad and Johansen (2000), Carlsen and Johansen (2005), and

Gavrilova et al. (2015).<sup>1</sup> Based on firm-level data, Johansen and Klette (1997) find that 60-100% of payroll tax changes are shifted to employees through wages. The recent study by Gavrilova et al. (2015) finds that on average employees pay for 66% of an increase in payroll taxes. They find that firms increase their labor demand on the intensive margin, while there is no effect on the number of employees.

I exploit the Norwegian payroll tax reform in 2000, where treatment regions face a 4.2 percentage point cut in the payroll tax rate (equal to 3.8% reduction in labor costs). Based on matched employer-employee register data during 1997-2003, I use the difference-in-difference approach to estimate the incidence of payroll taxation. The wage effects are estimated based on 154,000 worker-year observations of individual daily wages, while employment effects on the extensive margin are estimated from 37,000 firm-year observations of firm size (number of workers). The dataset allows for an investigation of heterogeneous wage and employment effects of payroll tax cuts across education groups. While the aggregate results indicate that about 30% of the reduction in labor costs is shifted to employees through higher wages (consistent with Benmarker et al., 2009), there are important differences across education groups. Among low-educated workers, 1% reduction in labor costs generates 0.4 – 0.5% wage increase, while there is no significant wage effect among workers with college education. Most studies of payroll taxation focus on manufacturing firms, and tend to conclude that employment effects are limited. I study employment effects along the extensive margin, and allow for heterogeneous effects across sectors and education groups. I find that the payroll tax cut generates 3% increase in employment, which corresponds to a labor demand elasticity of about 0.8. The employment expansion at the firm level is driven by an increase in the number of low-educated workers, mainly in services. The findings imply that the absolute value of the labor demand elasticity decreases with the level of education.

The rest of the paper is organized as follows. Section 2 discusses the Norwegian payroll tax reform, the administrative register data on individual wages and the econometric strategy. Section 3 presents the wage effect of reduced payroll taxes, including heterogeneous effects based on workers' level of education. Employment effects are investigated in section 4, and interpretations of the findings are given in section 5. Section 6 offers concluding remarks.

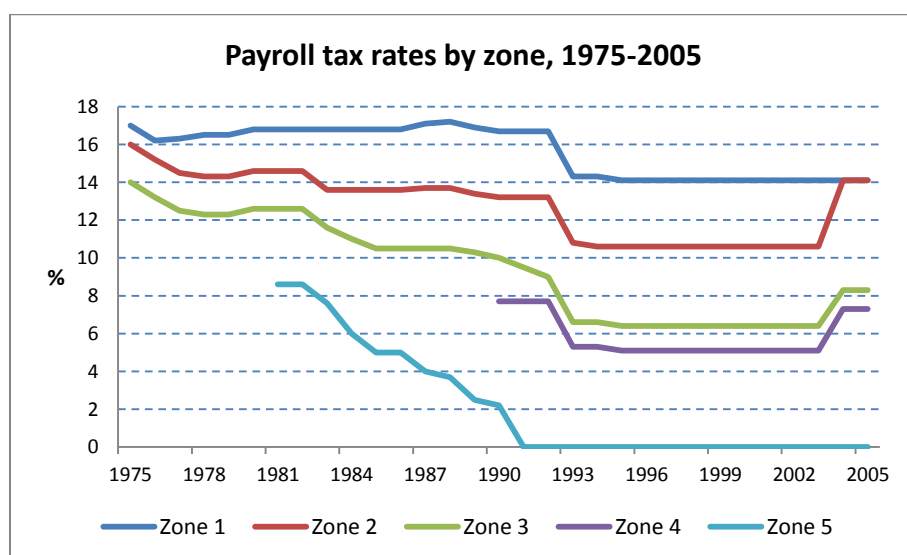
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<sup>1</sup> Dyrstad and Johansen (2000) and Carlsen and Johansen (2005) include the payroll tax rate as control variable in regional wage equations for manufacturing. Based on their long-run estimates, changes in payroll taxes are not shifted to employees, while short-run estimates are more mixed and indicate some degree of tax shifting.

## 2. The Norwegian payroll tax reform, data, and econometric strategy

Since 1975, Norway has had regionally differentiated payroll taxes at the local government level, with the intention of stimulating employment growth in the periphery. Payroll taxes are levied on employers alone, and the tax zone is determined by the employees' resident municipality, not by the firm's location. Figure 1 illustrates the development in payroll tax rates by zone during 1975-2005. Initially, there were three different zones with tax rates varying from 14% to 17%. The degree of differentiation increases over time, both with respect to number of zones and variation in tax rates. Since 1990, municipalities are divided into five different payroll tax zones, and during 1995-2003 the tax rates within each zone are constant<sup>2</sup>. The tax rate varies from 0% in peripheral municipalities in the most northern parts of Norway (zone 5) to 14.1% in cities in the south (zone 1). Municipalities in zones 2-4 face tax rates of 10.6%, 6.4% and 5.1%, respectively.

Figure 1: Norwegian payroll tax rates by zone, 1975-2005



I exploit a payroll tax reform enforced by the Norwegian government effective from January 1<sup>st</sup> 2000, where 53 municipalities changed tax zone.<sup>3</sup> I focus on 32 municipalities that went from zone 2 to zone 3 facing a 4.2 percentage point reduction in the payroll tax rate. Since municipalities are small and part of larger economic regions the analysis is performed with labor market regions as the local

<sup>2</sup> During 2004-2006 tax rates increased gradually in zones 2-4 due to European Economic Area (EEA) regulations. In 2007, payroll taxes were again allowed to differ across regions. At the same time, the differentiation was extended to 7 regional zones and the determination of tax zone changed from employees' resident location to the firm's location.

<sup>3</sup> In 2000, Norway consisted of 435 municipalities. Of the 53 municipalities affected by the reform, 14 municipalities faced an increase in the payroll tax rate (moving from zone 2 to zone 1), while the remaining 39 municipalities moved to a zone with lower tax rate.

government level.<sup>4</sup> Based on information about commuting flows between municipalities, Statistics Norway divides Norway into 89 travel-to-work areas, capturing functional regions understood as common labor markets. I consider a region to be affected by the payroll tax cut if all municipalities in the region are part of the change from payroll tax zone 2 to zone 3 in 2000. This gives five regions covering 23 of the 32 municipalities affected by the payroll tax cut.<sup>5</sup>

The main methodological challenge is that regions affected by the payroll tax cut are not randomly chosen, but follow from a political process targeting lagging regions. It is hard to find valid instruments for such policy changes. My approach is to use difference-in-difference estimation, where the control group is defined as regions remaining in tax zone 2 during the entire period of study (eight regions).<sup>6</sup> Prior to the reform in 2000, both treatment and control regions have payroll tax rate of 10.6%, while in the post-reform years the treatment group faces 4.2 percentage point lower tax rate than the control group. The analysis focuses on the years 1997-2003. No other payroll tax reform occurred during this period, neither to tax rates within zones or to the definition of zone borders. Importantly, no other modifications of regional policies were observed in this period, and no compensations were offered to regions not affected by the payroll tax cut.

While existing evidence on the incidence of payroll taxation is mainly based on firm-level data on the average wage bill, I apply administrative register data of individual wages. The employment register links workers and firms and gives information on work contracts for all employees, including the length of the contract and the type of contract.<sup>7</sup> Data on the exact number of hours worked per week is not available for the full period of study. In order to have as precise measure of wages as possible, I focus on full-time workers. Under the assumption that workers on full-time contracts work five days per week, the number of days worked per year can be inferred from the contract length.<sup>8</sup> This is combined with data on annual wage income from the tax register to give a measure of daily wages. The education register gives information about the level of education for each worker, which allows

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<sup>4</sup> As a robustness check, the analysis is also performed at the municipal level, and the findings are broadly consistent.

<sup>5</sup> The remaining nine municipalities are excluded from the analysis.

<sup>6</sup> There are 12 regions in tax zone 2 during the entire period of study. Four of these (Høyanger, Odda, Sogndal/Årdal, Sunndalsøra) are dominated by one or two large firms, which gives an industrial structure that differs from the treatment regions. I exclude these four regions from the control group, but do a robustness analysis where all 12 regions are included.

<sup>7</sup> The employment register separates between three contract types: Full-time contracts with at least 30 hours work per week, long part-time contracts with 20 – 29 hours work per week, and short part-time contracts with less than 20 hours work per week.

<sup>8</sup> As a check of robustness, the analysis is also performed with workers on long part-time contracts included. In this case, the number of days worked each year is calculated under the assumption that these workers on average work three days per week. The main findings remain.

me to investigate heterogeneous wage effects of payroll tax cuts across education categories (primary-, secondary-, and college-educated workers).<sup>9</sup>

The original dataset consists of about 558,000 worker-year observations. Workers above 61 years of age face lower payroll tax rates than other workers, and are therefore excluded from the analysis (25,000 observations). To have comparable age compositions across education groups, I exclude workers below 25 years of age (103,000 observations). Due to special wage settings, workers in the resource-based sectors of agriculture, fishing, and forestry are excluded (12,000 observations). Further, since public sector wages are determined by national regulation, workers in public administration, education, and health care are excluded. This reduces the dataset by 143,000 observations. As mentioned above, I concentrate on workers on full-time contracts. Workers with more than two contracts during a year, as well as workers with one full-time and one part-time contract, are excluded. Workers with two full-time contracts are excluded if the overlap between the two contracts exceeds three months. I also exclude workers whose contract length is less than 300 days. These restrictions reduce the dataset by about 90,000 observations. Missing data on annual wage income or level of education, together with the exclusion of workers that are in the dataset for a single year, further excludes about 29,000 observations. Finally, to avoid extreme observations, I exclude the bottom 1% of the wage distribution. The final dataset includes about 154,000 worker-year observations with 49,000 residing in treatment regions and the remaining 105,000 in control regions. Workers are allocated to almost 10,000 different firms and 54 industries.

To study the employment response to lower payroll taxes, I take advantage of the matched employer-employee register data to generate a dataset at the firm level. The original individual-level data is aggregated to the firm level to give a measure of firm size. In the wage analysis, sample restrictions are imposed on the data to have precise wage measures. Many of these restrictions are redundant in the analysis of employment effects at the firm level. Firm size is measured by the number of workers at each firm (aggregate and by education category) and includes workers of all ages and all contract types (part-time and full-time). Workers on short-term contracts (less than three months) and workers with more than two contracts are excluded (since they work for several firms), while workers with two contracts are allocated to their main employer. I further ignore firms in the primary and public sectors. The final dataset consists of about 37,000 firm-year observations with 13,000 located in treatment regions and the remaining 24,000 in control regions. Firm characteristics include industry affiliation, region of location, and workforce composition.

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<sup>9</sup> Secondary education corresponds to workers that have completed at least one year of secondary education, while college education includes workers with at least one year at university/college.

Table 1. Descriptive statistics

	Treatment regions	Control regions
<i>Panel A: Individual-level data</i>		
Worker-year observations	48,756	105,058
Average daily wage (in 2000 NOK)	754	823
Age composition		
25-34 years old	0.25	0.264
35-49 years old	0.478	0.485
50-61 years old	0.272	0.251
Share of immigrants	0.033	0.052
Share of male workers	0.79	0.781
Education composition		
Primary education	0.222	0.213
Secondary education	0.673	0.663
College education	0.105	0.123
Sector composition		
Secondary sectors	0.535	0.555
Tertiary sectors (services)	0.465	0.445
<i>Panel B: Firm-level data</i>		
Firm-year observations	12,638	24,439
Average firm size	6.4	7.0
Age composition		
< 35 years old	0.354	0.395
35-49 years old	0.365	0.359
> 49 years old	0.281	0.246
Share of immigrants	0.044	0.061
Share of male workers	0.599	0.602
Education composition		
Primary education	0.277	0.289
Secondary education	0.614	0.59
College education	0.109	0.121
Sector composition		
Secondary sectors	0.309	0.296
Tertiary sectors (services)	0.691	0.704
Share of part-time workers	0.289	0.267

Notes: The individual- and firm-level data cover all full-time workers and firms, respectively, in the private sector in five treatment regions and eight control regions during 1997-2003 (excluding workers and firms in the primary sectors). The average firm size corresponds to the number of employed workers in the firm (both full-time and part-time).

Factors determining a region's payroll tax zone include geography (distance to cities), demography (population growth, female/youth shares), and regional development (income p.c., unemployment rate). Treatment and control regions are comparable along these dimensions. The two groups consist of sparsely populated peripheral regions facing outmigration in the years prior to the reform.<sup>10</sup> The average unemployment rate during 1997-1999 is about 2% in both treatment and control regions. Descriptive statistics for the individual-level data and the firm-level data are given in panels A and B of Table 1, respectively. As seen from panel A, both treatment and control regions consist of roughly

<sup>10</sup> In 2000, the average (treatment and control) region had about 15,000 inhabitants.

22% primary-educated workers, 67% with secondary education, and 11% college-educated workers. Further, the age, ethnic, gender, and sector compositions of the labor force are similar in the two groups of regions. About 80% of full-time workers are male, 25% are below 35 years of age, almost half belong to the middle age group (35-49 years old), 55% are employed in secondary sectors (dominated by manufacturing), and 45% in services. As seen from panel B, firms in treatment and control regions have similar size and composition of their workforce. The average firm has about 7 workers, of which 60% are male, 5% are immigrants, 12% are college-educated, and almost 30% are part-time workers. The lower share of male workers in the firm-level dataset follows from the inclusion of part-time workers. About 70% of firms are in services, and the remaining 30% in secondary sectors.

The first part of the analysis applies the individual-level data to estimate wage effects of lower payroll taxes, both aggregate and within three education categories. Consistent with Korkeamäki and Uusitalo (2009), I focus on workers who appear in the data in two consecutive years and use the change in log daily wages as dependent variable.<sup>11</sup> This is a way to account for unobserved individual level variation in wages. Since the estimation is at the individual level, wage changes in big firms have large weights in the estimates. An important advantage of the matched employer-employee dataset is the opportunity to control for firm-specific shocks by including firm fixed effects in the regression. The identification of wage effects of payroll taxes is based on variations of the following regression:

$$\Delta \ln w_{ijsrt} = \alpha_0 + \alpha_1 T_r + \alpha_2 P_t + \alpha_3 T_r \cdot P_t + X_{it} \delta + \gamma_j + \eta_r + \mu_s + \rho_t + \mu_s \cdot \rho_t + \varepsilon_{ijsrt} \quad (1)$$

where  $\Delta \ln w_{ijsrt}$  is the change in log daily wage income from year  $t-1$  to year  $t$  for worker  $i$  in firm  $j$  in industry  $s$  located in region  $r$ ,  $T_r$  is a dummy that equals 1 if the labor market region is part of the treatment group facing lower payroll tax rate, and  $P_t$  is a dummy that equals 1 in the post reform years (from 2000 onwards). Firm, industry, regional, and year fixed effects are represented by  $\gamma_j$ ,  $\mu_s$ ,  $\eta_r$ , and  $\rho_t$ , respectively. Industry times year fixed effects capture industry-specific trends. The vector of worker characteristics in year  $t$  ( $X_{it}$ ) includes dummies for age (5-year intervals), education level (primary, secondary, college), immigrant status (native, western immigrant, non-western immigrant), and gender. The error term is given by  $\varepsilon_{ijsrt}$ ,  $\alpha_0$  is a constant and  $\delta$  is a vector of parameters. The main interest is the interaction term between the treatment dummy and the

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<sup>11</sup> As a check of robustness, I use an alternative specification with log wage level as dependent variable.

dummy for post reform years. The parameter  $\alpha_3$  captures the difference in wage growth between treatment and control regions in the years after the payroll tax cut compared to the pre-reform period. I also consider specifications with year-specific effects, where interaction terms between the treatment dummy and each year after the reform are included.

The second part of the analysis applies the firm-level data to estimate employment effects of lower payroll taxes. I focus on employment expansions along the extensive margin and use the change in log firm size as dependent variable, where firm size is measured by the number of workers in the firm. The analysis is done both aggregate and within education categories, and is based on variations of the following regression:

$$\Delta \ln size_{jsrt} = \beta_0 + \beta_1 T_r + \beta_2 P_t + \beta_3 T_r \cdot P_t + Y_{jt} \varphi + \eta_r + \mu_s + \rho_t + \mu_s \cdot \rho_t + \tau_{jsrt} \quad (2)$$

where  $\Delta \ln size_{jsrt}$  is the change in log number of workers from year  $t-1$  to year  $t$  in firm  $j$  in industry  $s$  located in region  $r$ . The vector of firm characteristics in year  $t$  ( $Y_{jt}$ ) includes a range of controls for workforce composition based on age, gender, immigrant status, level of education, and contract type. Other explanatory variables are defined in relation to equation (1). The regression includes dummies for treatment regions and reform years, as well as regional, industry, year, and industry times year fixed effects. The error term is given by  $\tau_{jsrt}$ ,  $\beta_0$  is a constant and  $\varphi$  is a vector of parameters. The parameter  $\beta_3$  is our main interest and captures the difference in employment growth between treatment and control regions in the years after the payroll tax cut compared to the pre-reform period.

The methodological approach assumes that treatment and control regions follow parallel trends in the outcome variables during the pre-reform period. Average annual wage and employment growth during 1997 – 2003 are illustrated in Figures 2 and 3, respectively.<sup>12</sup> In both groups of regions, wage growth in the pre-reform period first increases and then decreases. In the reform year of 2000, wage growth in treatment regions tends to flatten out, while wage growth in control regions continues the decreasing trend. Employment growth decreases during 1997 – 2000 in both treatment and control regions. Immediately following the reform, employment growth increases more in control regions, but from 2001, employment growth is decreasing in control regions and increasing in treatment

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<sup>12</sup> Average employment growth is weighted by firm size, consistent with the weighting of observations by firm size in the regressions.

regions. Roughly similar patterns are found within the three education groups, as illustrated in Appendix Figures 1 and 2.

Figure 2: Average annual wage growth 1997-2003, treatment vs. control regions

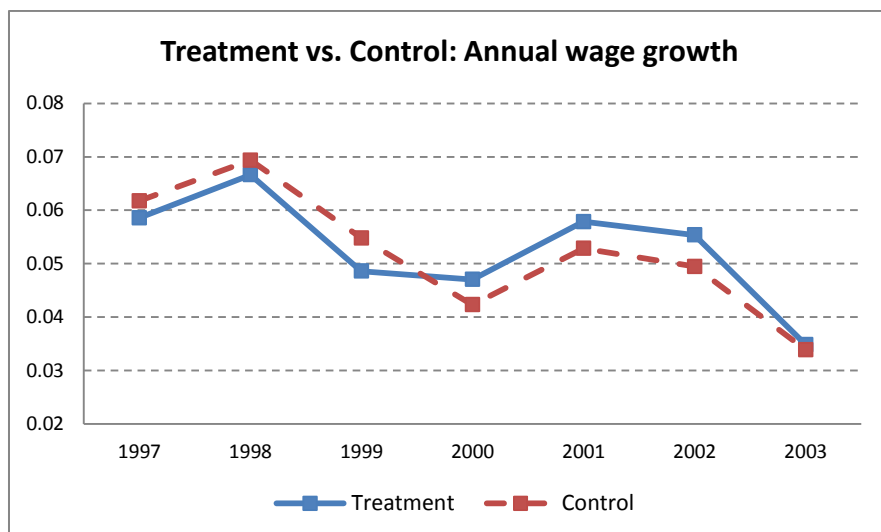
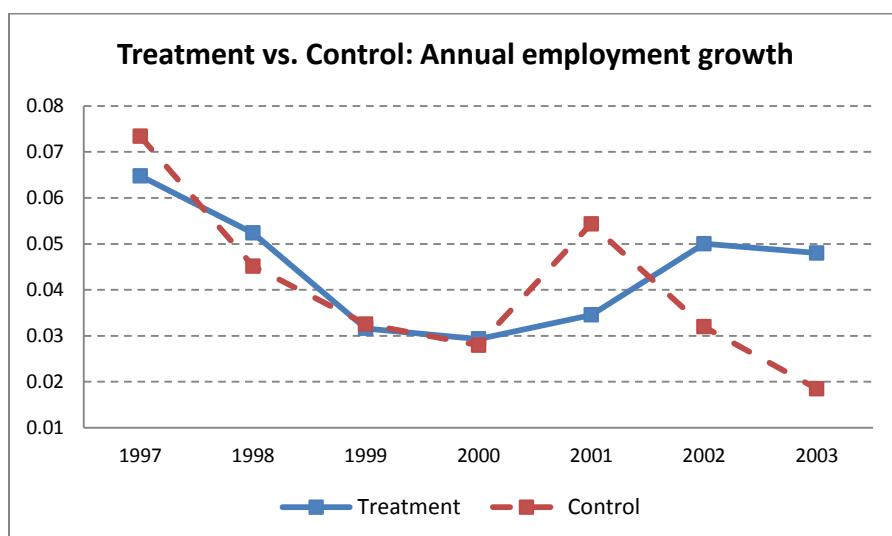


Figure 3: Average annual employment growth 1997-2003, treatment vs. control regions



### 3. Payroll tax cuts and individual wages

Table 2 documents the impact of the payroll tax reform in 2000 on individual wages based on the difference-in-difference approach described in equation (1) in section 2. Column (1) gives the average wage growth effect of the payroll tax cut in the post-reform years. Our main interest is the interaction term between the treatment dummy and the dummy for post-reform years, which is

significant at the 1% level with a coefficient of 0.007. The change in annual wage growth from the pre-reform period to the post-reform period is 0.7 percentage point higher in treatment regions compared to control regions. The interpretation is that 4.2 percentage point reduction in the payroll tax rate (equivalent to 3.8% reduction in labor costs) generates 0.7 percentage point higher wage growth per year (average effect during the post-reform years 2000-2003).<sup>13</sup>

Table 2. Impact of payroll tax cut on individual wage growth

Dependent variable	(1) $\Delta \ln w$ Secondary + Services	(2) $\Delta \ln w$ Secondary + Services	(3) $\Delta \ln w$ Secondary + Services	(4) $\Delta \ln w$ Secondary	(5) $\Delta \ln w$ Services
Treatment	0.004 (0.0067)	0.004 (0.0067)	0.005 (0.0078)	0.001 (0.0116)	0.008 (0.0082)
Post 2000	-0.042*** (0.0073)				
Treatment x Post 2000	0.007*** (0.0018)				
Treatment x 1998			-0.001 (0.0026)		
Treatment x 1999			-0.002 (0.0039)		
Treatment x 2000		0.005* (0.0024)	0.004 (0.0035)	0.01*** (0.0029)	0.000 (0.0044)
Treatment x 2001		0.01*** (0.003)	0.009** (0.0035)	0.017*** (0.0046)	0.002 (0.0043)
Treatment x 2002		0.008* (0.004)	0.007 (0.0045)	0.012*** (0.0038)	0.004 (0.0053)
Treatment x 2003		0.004* (0.0024)	0.003 (0.0035)	0.005* (0.0023)	0.004 (0.0032)
Observations	153,814	153,814	153,814	84,363	69,451
Obs. treatment	48,756	48,756	48,756	26,081	22,675
Obs. Control	105,058	105,058	105,058	58,282	46,776
R <sup>2</sup>	0.14	0.14	0.14	0.10	0.17

Notes: The regressions in columns (1) – (3) are based on yearly data for all full-time workers in the private sector during 1997-2003, excluding workers in the primary sectors (agriculture, fishing, and forestry). Columns (4) and (5) offer separate regressions for workers in the secondary sectors (dominated by manufacturing) and tertiary sectors (services), respectively. Public sector workers are excluded in all regressions. The dependent variable is the change in log daily wages from one year to the next. All regressions include regional fixed effects, industry fixed effects, year fixed effects, industry x year fixed effects, firm fixed effects, age controls, dummies for education level, immigrant status, and gender, as well as a constant term. Robust standard errors clustered at the regional level are given in parenthesis. \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10 percent level, respectively.

Column (2) reports year-specific effects for the post-reform period. The wage growth effect is strongest in 2001 with a coefficient of 0.01, which is significant at the 1% level. In other years, the estimated effects are positive and significant at the 10% level. The persistence in the wage growth

<sup>13</sup> The 4.2 percentage point reduction in the payroll tax rate from an initial level of 10.6% corresponds to 3.8% reduction in labor costs:  $(1.106w - 1.064w)/1.106w = 0.038$ .

effect could reflect lags in wage adjustments. Under the assumption of equal wage levels in treatment and control regions prior to the reform, the accumulated effect in 2003 is 2.7% higher wages in treatment regions, which would imply that about 70% of the labor cost reduction of 3.8% is shifted to employees. However, since the estimated wage growth effects in all years except 2001 are only significant at the 10% level, this probably exaggerates the degree of tax shifting. When considering effects that are significant at the 5% level or lower, the payroll tax cut contributes to 1% higher wages in treatment regions, which implies close to 30% tax shifting. This interpretation of the findings is confirmed by the more flexible difference-in-difference specification in column (3), where interaction terms between the treatment dummy and pre-reform years are included. The estimated coefficient for 2001 equals 0.009 and is significant at the 5% level, while the wage growth effect is not significant in any of the other years (not even at the 10% level). This implies that 1% reduction in labor costs generate about 0.3% wage increase (30% tax shifting). The estimation reveals that none of the pre-reform interaction terms are significant. This confirms the assumption of parallel wage growth trends in treatment and control regions prior to the reform, and indicates that the findings are not driven by long-term trends.

The finding of 30% tax shifting is consistent with the Swedish analysis of Bennismarker et al. (2009), who show that the average wage bill per employee increases by 0.25% per percentage point reduction in the payroll tax rate (based on firms that existed both before and after the reform). While the present paper and Bennismarker et al. (2009) use data from most sectors of the economy (excluding only public and primary sectors), many other studies on the incidence of payroll taxation focus on the manufacturing sector. As discussed in section 1, these studies tend to find much higher degree of tax shifting. Columns (4) and (5) of Table 2 present separate effects of the payroll tax cut for the secondary sector (dominated by manufacturing) and the tertiary sector (services), respectively. The estimations reveal a strong positive and significant wage effect of reduced payroll taxes in the secondary sector. The accumulated effect in 2002 is an increase in wages of 3.9%, implying full shifting of the labor cost reduction to employees in the form of higher wages. The finding is robust to the more flexible specification including pre-reform interaction terms. When it comes to services, however, there is no significant effect on wages following the payroll tax reform.

While the lack of any wage effect in services is consistent with the analysis of young workers in the Swedish retail industry by Skedinger (2014), it contradicts the findings of Korkeamäki and Uusitalo (2009). They use individual wage data for a sample of large Finnish firms, and find about 50% tax shifting in services, but no significant wage effects in manufacturing. To check whether my findings are driven by firm size, I estimate wage effects separately for small firms (less than 10 full-time

workers; the aggregate median firm size measured across workers) and large firms (at least 10 full-time workers).<sup>14</sup> About 2/3 of workers in services are employed in small firms, with no significant wage effect from lower payroll taxes. Consistent with Korkemäki and Uusitalo (2009), there are some indications of positive wage effects among workers in large service sector firms (about 30% tax shifting). In the secondary sector, the positive wage effect of lower payroll taxes is driven by large firms.

The rich administrative register data on individual wages and worker characteristics allows me to investigate possible heterogeneous effects of payroll tax cuts across workers' level of education. Table 3 documents the wage effects of reduced payroll taxes for three education groups; primary, secondary, and college. The year-specific effects on wage growth during the post-reform years are given in columns (1), (3), and (5) for primary-, secondary-, and college-educated workers, respectively. As a check of robustness, the more flexible specification with interaction terms between the treatment dummy and pre-reform years are reported in columns (2), (4), and (6).

For primary-educated workers, the change in wage growth from the pre-reform period to the reform year of 2000 is 1.4 percentage points higher in treatment regions compared to control regions. The effect is significant at the 1% level, and indicates that the payroll tax cut generates an increase in wages of primary-educated workers of 1.4%, which means that 37% of the labor cost reduction is shifted to workers through higher wages. The regression in column (2) confirms the finding, and again, none of the pre-reform interaction terms are significant. For secondary-educated workers, the payroll tax cut generates a wage growth effect of 1 percentage point in 2001 and 0.9 percentage points in 2002, significant at the 1% and 5% level, respectively. Under the assumption of equal wage levels in treatment and control regions prior to the reform, the accumulated effect in 2002 is 1.9% higher wages in treatment regions, which implies 50% tax shifting. The finding is robust to the inclusion of pre-reform interaction terms, as documented in column (4). Interestingly, as seen from columns (5) and (6), there is no significant effect of the payroll tax cut on wages of college-educated workers. This finding is not driven by sector affiliation; the lack of wage effect among college-educated workers is present both in secondary sectors and in services. Further, the difference in wage effect between low- and high-educated workers is consistent across firm size.<sup>15</sup>

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<sup>14</sup> The estimation results are available from the author upon request.

<sup>15</sup> In large firms, there are indications of positive wage effects from lower payroll taxes also for college-educated workers, but the estimated effect is only significant at the 10% level and not robust to alternative specifications. The estimation results are available from the author upon request.

Table 3. Impact of payroll tax cut on individual wage growth by level of education

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Education group	$\Delta \ln w$ Primary	$\Delta \ln w$ Primary	$\Delta \ln w$ Secondary	$\Delta \ln w$ Secondary	$\Delta \ln w$ College	$\Delta \ln w$ College
Treatment	-0.003 (0.0197)	-0.003 (0.0197)	0.003 (0.0071)	0.003 (0.0079)	0.029*** (0.0081)	0.04*** (0.0101)
Treatment x 1998		0.007 (0.0049)		-0.001 (0.0032)		-0.013 (0.0109)
Treatment x 1999		-0.006 (0.0066)		0.000 (0.0037)		-0.016 (0.0099)
Treatment x 2000	0.014*** (0.0043)	0.014** (0.005)	0.003 (0.0031)	0.003 (0.0045)	0.006 (0.0083)	-0.005 (0.0061)
Treatment x 2001	0.01 (0.0073)	0.01 (0.0074)	0.01*** (0.0025)	0.01** (0.0034)	0.001 (0.0089)	-0.01 (0.0101)
Treatment x 2002	0.013 (0.0102)	0.013 (0.0106)	0.009** (0.0032)	0.008* (0.0038)	0.000 (0.0099)	-0.011 (0.0142)
Treatment x 2003	0.001 (0.0054)	0.001 (0.0058)	0.005 (0.0033)	0.005 (0.0041)	-0.000 (0.0087)	-0.011 (0.0116)
Observations	33,253	33,253	102,495	102,495	18,066	18,066
Obs. treatment	10,829	10,829	32,801	32,801	5,126	5,126
Obs. control	22,424	22,424	69,694	69,694	12,940	12,940
R <sup>2</sup>	0.19	0.19	0.14	0.14	0.29	0.29

Notes: The dependent variable is the change in log daily wages from one year to the next. All regressions include regional fixed effects, industry fixed effects, year fixed effects, industry x year fixed effects, firm fixed effects, age controls, dummies for immigrant status and gender, as well as a constant term. Robust standard errors clustered at the regional level are given in parenthesis. \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10 percent level, respectively.

The findings in Table 3 are robust to a range of alternative model specifications. Table 4 documents the results with the log wage level (rather than the change in log wages) as dependent variable.<sup>16</sup> The main picture is that the heterogeneity in wage response between low- and high-educated workers remains. Among primary-educated workers, the estimated wage increase from the pre-reform period to 2002 is 1.7% higher in treatment regions compared to control regions (although the effect is only significant at the 13% level). As seen from column (3), the payroll tax cut generates a wage increase of 1.5% for workers with secondary education. The effect is significant at the 1% level, and robust to the more flexible specification in column (4). Among college-educated workers, there is no significant effect on wages following the labor cost reduction.

<sup>16</sup> It should be noticed that the number of observations is higher in Table 4 than in Table 3. This follows since workers that are in the dataset in non-consecutive years are included when the log wage rate is used as dependent variable.

Table 4. Impact of payroll tax cut on individual wage level

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Education group	ln $w$ Primary	ln $w$ Primary	ln $w$ Secondary	ln $w$ Secondary	ln $w$ College	ln $w$ College
Treatment	-0.028 (0.0296)	-0.026 (0.0289)	-0.034** (0.0155)	-0.035** (0.0156)	0.067* (0.0321)	0.07* (0.036)
Treatment x 1998		0.005 (0.0062)		-0.001 (0.0038)		0.006 (0.0086)
Treatment x 1999		-0.01 (0.0056)		0.002 (0.0065)		-0.012 (0.0087)
Treatment x 2000	0.002 (0.0055)	0.000 (0.007)	0.000 (0.0026)	0.001 (0.0051)	-0.000 (0.0118)	-0.003 (0.013)
Treatment x 2001	0.005 (0.0103)	0.003 (0.0115)	0.009* (0.0044)	0.01 (0.0068)	0.01 (0.0108)	0.007 (0.0119)
Treatment x 2002	0.017 (0.0105)	0.015 (0.011)	0.015*** (0.0047)	0.015** (0.0064)	0.009 (0.0117)	0.006 (0.0138)
Treatment x 2003	0.011 (0.0126)	0.01 (0.0133)	0.012 (0.0067)	0.012 (0.0085)	0.008 (0.0094)	0.005 (0.0099)
Observations	39,065	39,065	118,942	118,942	22,913	22,913
Obs. treatment	12,717	12,717	38,168	38,168	6,584	6,584
Obs. control	26,348	26,348	80,774	80,774	16,329	16,329
R <sup>2</sup>	0.67	0.67	0.63	0.63	0.74	0.74

Notes: The dependent variable is log daily wages. All regressions include regional fixed effects, industry fixed effects, year fixed effects, industry x year fixed effects, firm fixed effects, age controls, dummies for immigrant status and gender, as well as a constant term. Robust standard errors clustered at the regional level are given in parenthesis. \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10 percent level, respectively.

The results are robust to the use of an alternative control group. In the main analysis, the control group is defined as regions that remain in tax zone 2 during the entire period of study, but as argued in section 2 (footnote 6), four regions with different industrial structure compared to the treatment regions are excluded. Appendix Table 1 documents the wage growth effects across education groups when these four regions are included in the control group. Based on the preferred specification given in columns (1), (3), and (5), the degree of tax shifting equals about 40% and 60% for primary- and secondary-educated workers, respectively, while the wage response among college-educated workers remains insignificant. Controlling for firm size rather than including firm fixed effects does not alter the main findings, but tend to strengthen the degree of tax shifting among low-educated workers (documented in Appendix Table 2). The payroll tax cut generates an accumulated wage increase of 3.2% and 2.7% for primary- and secondary-educated workers, respectively, and still no wage effect for college-educated workers. Finally, I run a placebo test for the period 1995-99, with an imagined payroll tax reform happening in 1997, and find no significant wage effect in any year, neither on aggregate nor within education groups.<sup>17</sup>

<sup>17</sup> The estimation results are available from the author upon request.

#### 4. Payroll tax cuts and employment

The analysis of employment effects of the payroll tax cut concentrates on expansions along the extensive margin, and is based on the difference-in-difference approach outlined in equation (2) in section 2. The analysis covers the period 1997-2003 and applies about 37,000 observations of annual growth in the number of workers within firms (including both full-time and part-time workers). Since a given percentage change in employment can be hard to compare across small and large firms, the observations are weighted by firm size.<sup>18</sup> As seen from the estimated year-specific effects in column (1) of Table 5, there is no significant effect of the payroll tax reform on employment growth the first two years. However, the effects in 2002 and 2003 are positive and significant at the 10% and 5% level, respectively. The delay in employment response can be related to lags in hiring decisions and capacity constraints due to the business cycle (peak in 2000-2001 with very low unemployment). The estimated coefficient for 2003 indicates that employment growth is 3 percentage points higher in treatment regions compared to control regions (relative to the pre-reform period). Under the assumption of equal employment levels in treatment and control regions prior to the reform, the payroll tax cut (equal to 3.8% reduction in labor costs) generates 3% increase in employment. This implies a labor demand elasticity of about 0.8. The estimation is robust to the more flexible specification in column (2), and none of the pre-reform interaction terms are significant.

Most studies of payroll taxation focus on manufacturing firms, and tend to conclude that employment effects are limited (see for instance Gruber, 1997). Columns (3) and (4) of Table 5 document the employment response to the payroll tax cut for firms in the secondary sector (mainly manufacturing) and firms in services, respectively. The estimated coefficient for 2003 is roughly similar across sectors (around 3%), but the positive employment effect is only significant within the service sector.

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<sup>18</sup> Solon et al. (2015) offer a discussion of weighting in regression analysis.

Table 5. Impact of payroll tax cut on employment growth

Dependent variable	(1)	(2)	(3)	(4)
Sector	$\Delta \ln size$ Secondary + Services	$\Delta \ln size$ Secondary + Services	$\Delta \ln size$ Secondary	$\Delta \ln size$ Services
Treatment	0.009 (0.0058)	-0.001 (0.0149)	0.035*** (0.0082)	0.008 (0.0064)
Treatment x 1998		0.017 (0.0137)		
Treatment x 1999		0.012 (0.0223)		
Treatment x 2000	0.01 (0.015)	0.02 (0.022)	0.013 (0.0273)	0.007 (0.0187)
Treatment x 2001	-0.017 (0.0176)	-0.007 (0.0164)	-0.031 (0.0368)	-0.007 (0.0148)
Treatment x 2002	0.024* (0.0117)	0.034 (0.02)	0.022 (0.0224)	0.025* (0.012)
Treatment x 2003	0.03** (0.0103)	0.041* (0.02)	0.026 (0.0297)	0.033*** (0.0092)
Observations	37,077	37,077	11,133	25,944
Obs. treatment	12,638	12,638	3,910	8,728
Obs. control	24,439	24,439	7,223	17,216
R <sup>2</sup>	0.05	0.05	0.07	0.05

Notes: The regressions are based on yearly data on firms in the private sector during 1997-2003, excluding firms in the primary sectors (agriculture, fishing, and forestry). Columns (3) and (4) offer separate regressions for firms in the secondary sectors (dominated by manufacturing) and tertiary sectors (services), respectively. Public sector firms are excluded in all regressions. The dependent variable is the change in log firm size, where firm size is measured as number of workers (both part-time and full-time). All regressions include regional fixed effects, industry fixed effects, year fixed effects, industry x year fixed effects, and a range of controls for workforce composition at the firm level based on age, gender, immigrant status, level of education, and contract type, as well as a constant term. The observations are weighted by firm size. Robust standard errors clustered at the regional level are given in parenthesis. \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10 percent level, respectively.

The lack of wage effects among college-educated workers following the payroll tax cut (documented in Table 3) motivates an analysis of employment effects within education groups. The findings are presented in Table 6. The year-specific effects on employment growth during the post-reform years are given in columns (1), (3), and (5) for primary-, secondary-, and college-educated workers, respectively. As a check of robustness, the more flexible specification with interaction terms between the treatment dummy and pre-reform years are reported in columns (2), (4), and (6). The main finding is that the employment expansion at the firm level (documented in Table 5) is driven by an increase in the number of low-educated workers. As seen from column (1), the payroll tax cut generates an increase in the number of primary-educated workers of 5.7%, consistent with a labor demand elasticity of 1.5. Similar, the number of secondary-educated workers increases by 2.6%, while there is no significant effect on the employment of college-educated workers.

Table 6. Impact of payroll tax cut on employment growth by level of education

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Education group	$\Delta \ln size$ Primary	$\Delta \ln size$ Primary	$\Delta \ln size$ Secondary	$\Delta \ln size$ Secondary	$\Delta \ln size$ College	$\Delta \ln size$ College
Treatment	0.021 (0.0058)	0.002 (0.0146)	-0.002 (0.0044)	-0.006 (0.0137)	0.022** (0.0099)	0.022 (0.03)
Treatment x 1998		0.035 (0.0199)		0.009 (0.0135)		-0.015 (0.028)
Treatment x 1999		0.021 (0.0228)		0.003 (0.0225)		0.015 (0.0455)
Treatment x 2000	0.016 (0.0211)	0.035 (0.0227)	0.009 (0.0122)	0.013 (0.0212)	-0.001 (0.0263)	-0.001 (0.04)
Treatment x 2001	0.001 (0.0212)	0.02 (0.0256)	-0.015 (0.0229)	-0.011 (0.0222)	-0.011 (0.0263)	-0.01 (0.0298)
Treatment x 2002	0.006 (0.0145)	0.025 (0.0184)	0.017* (0.0096)	0.021 (0.0188)	0.008 (0.0211)	0.008 (0.0419)
Treatment x 2003	0.057*** (0.018)	0.076*** (0.0235)	0.026** (0.0092)	0.03 (0.018)	0.012 (0.0223)	0.012 (0.0386)
Observations	20,901	20,901	30,488	30,488	10,272	10,272
Obs. treatment	7,060	7,060	10,490	10,490	3,296	3,296
Obs. control	13,841	13,841	19,998	19,998	6,976	6,976
R <sup>2</sup>	0.07	0.07	0.05	0.05	0.08	0.08

Notes: The regressions are based on yearly data on firms in the private sector during 1997-2003, excluding firms in the primary sectors (agriculture, fishing, and forestry). The dependent variable is the change in log number of workers (both part-time and full-time) by level of education. All regressions include regional fixed effects, industry fixed effects, year fixed effects, industry x year fixed effects, and a range of controls for workforce composition at the firm level based on age, gender, immigrant status, and contract type, as well as a constant term. The observations are weighted by firm size within the respective education group. Robust standard errors clustered at the regional level are given in parenthesis. \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10 percent level, respectively.

The findings in Tables 5 and 6 are robust to a range of alternative specifications. Appendix Table 3 shows the estimated effects when I use employment level (rather than employment growth) as dependent variable. This increases the number of observations compared to the estimations in Table 6 since firms that are in the dataset in non-consecutive years are included. The main findings remain. The reduction in payroll taxes generates an employment expansion along the extensive margin, which is driven by low-educated workers. The main analysis measures firm size by the number of workers independent of contract type, while controlling for the share of workers in the firm on part-time contracts. As a check of robustness, I redefine firm size and concentrate on the employment expansion among full-time workers. The main findings remain.<sup>19</sup>

<sup>19</sup> The estimation results are available from the author upon request.

## 5. Interpretation of the findings

According to standard neoclassical labor market theory, lower payroll taxes imply lower labor costs and give an upward shift in the labor demand curve. In perfect market equilibrium the implication is higher employment and higher wages. The magnitude of effects depends on the elasticity of labor demand and the elasticity of labor supply. The labor demand response following the reduction in labor costs determines the size of the demand curve shift and thus the magnitude of the impact on wages and employment. For a given elasticity of labor demand, perfectly inelastic labor supply (vertical supply curve) implies that employment is unaffected while wages increase. If labor supply is perfectly elastic (horizontal supply curve), wages are unaffected while employment increases.

A possible understanding of the heterogeneity in wage and employment responses across education groups is related to differences in demand and supply elasticities in the respective labor markets. Positive and significant wage and employment effects for low-educated workers indicate elastic labor demand and elastic labor supply for these workers. This is illustrated in Figure 4, where the upward shift in the labor demand curve gives higher wages and increased employment. As documented in sections 3 and 4, the wage effects among low-educated workers is mainly in the secondary sectors, while the employment expansion is more pronounced in services. This indicates that the labor supply elasticity differs across sectors, and the findings are consistent with more elastic labor supply in services (flatter labor supply curve). A possible understanding is high degree of worker mobility between firms and industries in the service sector, while the secondary sector has lower mobility due to industry-specific requirements with respect to training (different types of apprenticeships).

Figure 4: Impact of payroll tax cut for low-educated workers

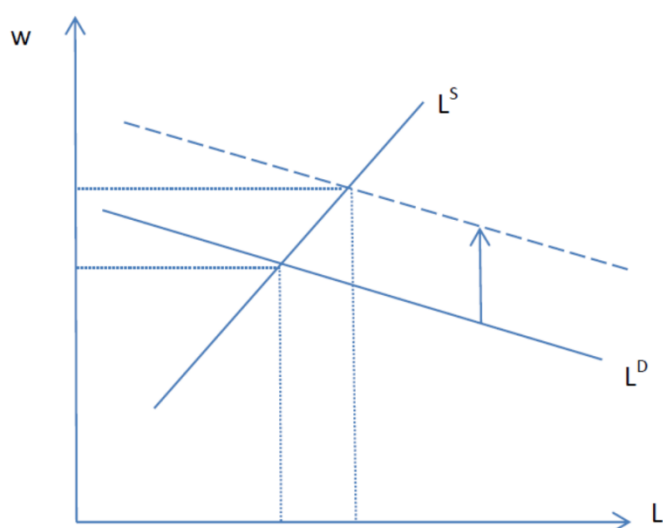
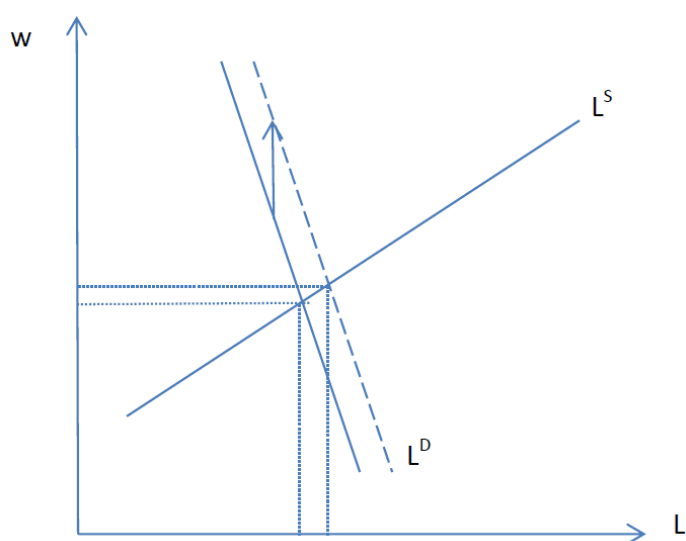


Figure 5: Impact of payroll tax cut for college-educated workers



The lack of any wage or employment effects among college-educated workers is consistent with inelastic labor demand (steep labor demand curve), as seen from Figure 5. Low elasticity of demand among the highly educated can be understood from capital-skill complementarity and lack of opportunities to substitute away from college-educated workers. This is consistent with empirical studies finding that the absolute value of the labor demand elasticity decreases with the level of education (Hamermesh, 1993).

Institutional factors like degree of union membership could potentially contribute to the difference in wage response between low- and high-educated workers. Nergaard (1999) presents union densities based on the labor force survey of Statistics Norway in the 2<sup>nd</sup> quarter of 1998. The survey covers about 10,000 workers and on aggregate 57% are members of a trade union. The degree of union membership is much higher in the public sector (83%), while 43% of workers in the private sector are organized. Among private sector workers, the degree of union membership is remarkably similar across education groups (40% for primary, 44% for secondary and 43% for college). This implies that differences in union density cannot explain the identified heterogeneity in wage effect of the payroll tax cut across education groups. When it comes to the difference in wage effect between sectors, the degree of union membership could be of relevance. Among private sector workers in services, 35% are part of a trade union, compared to 56% of workers in secondary sectors.

## 6. Conclusions

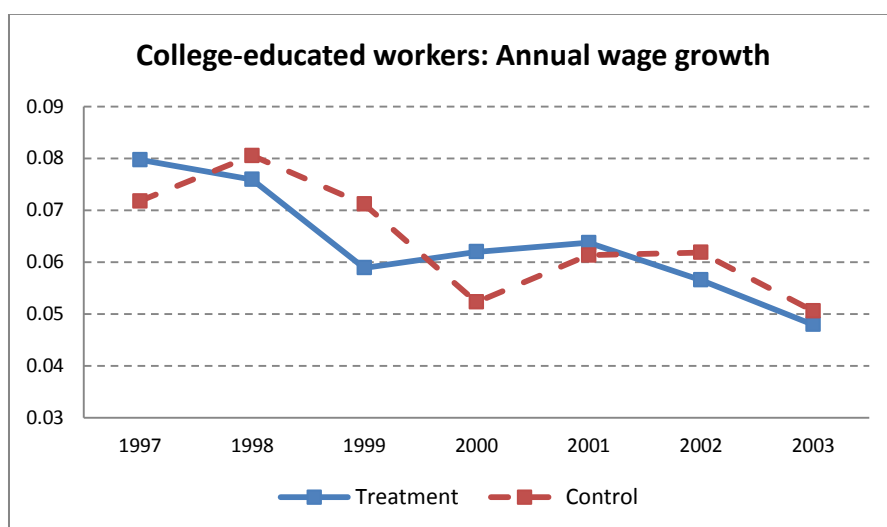
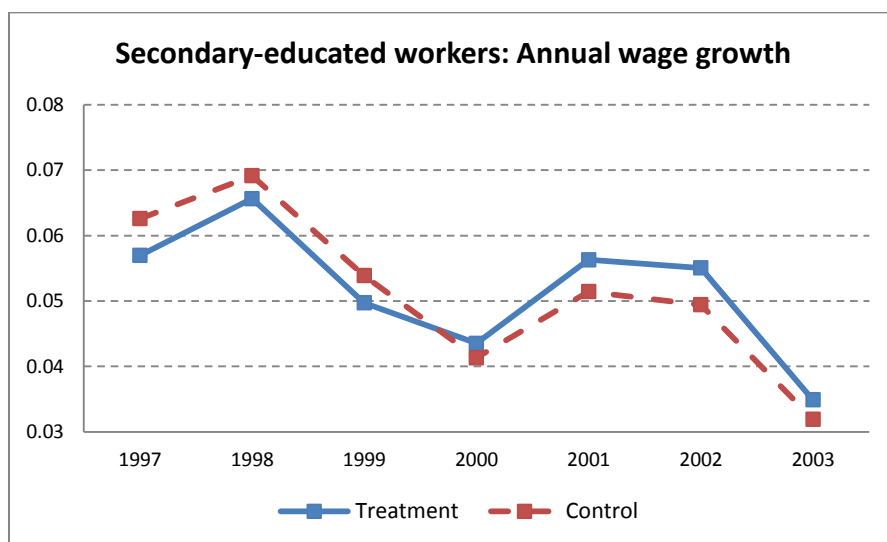
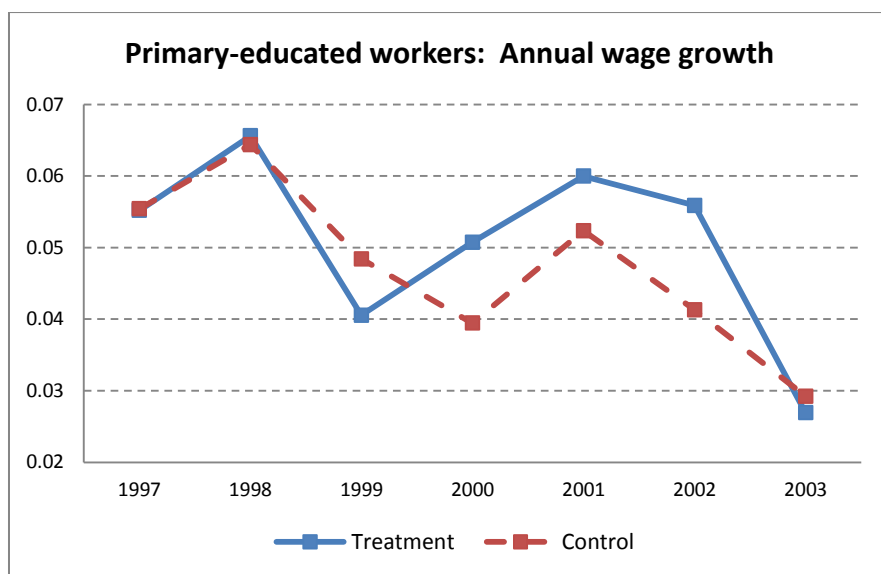
While the empirical evidence on the incidence of payroll taxation is primarily based on the wage bill of firms, this paper applies matched employer-employee register data on individual wages for private sector workers in Norway. The dataset allows for an investigation of heterogeneous wage and employment effects of payroll tax cuts across education groups. Exploiting a payroll tax reform and using the difference-in-difference approach, the aggregate results indicate that about 30% of the reduction in labor costs is shifted to employees through higher wages. The wage response differs across education groups. Among low-educated workers, 1% reduction in labor costs generates 0.4 – 0.5% wage increase, while there is no significant wage effect among workers with college education. The estimation results also reveal differences across sectors. While the degree of tax shifting is close to 100% in the secondary sector (dominated by manufacturing), services have no significant wage effect from lower payroll taxes. The analysis of employment effects focuses on expansions along the extensive margin, and finds that the payroll tax cut generates an employment increase of 3%, equivalent to a labor demand elasticity of about 0.8. The employment expansion at the firm level is driven by an increase in the number of low-educated workers, in particular in services. The findings imply that the absolute value of the labor demand elasticity decreases with the level of education.

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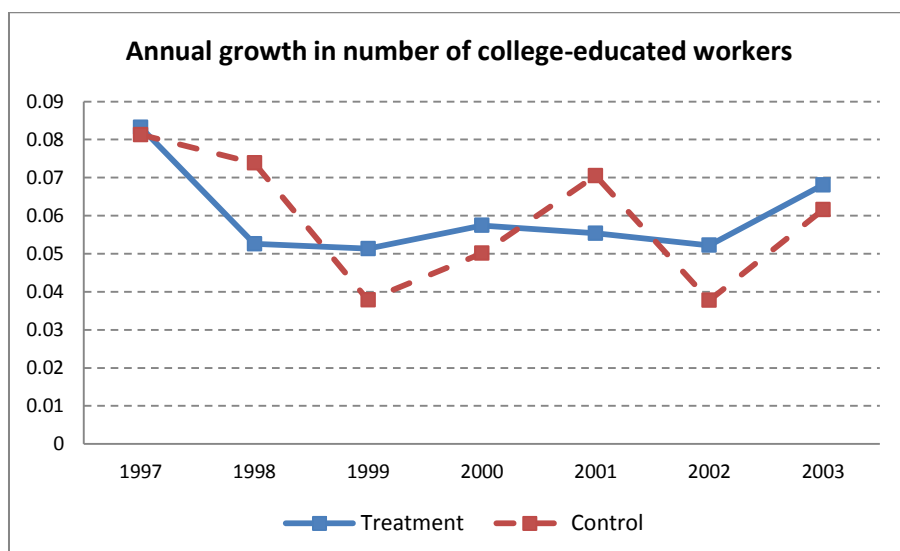
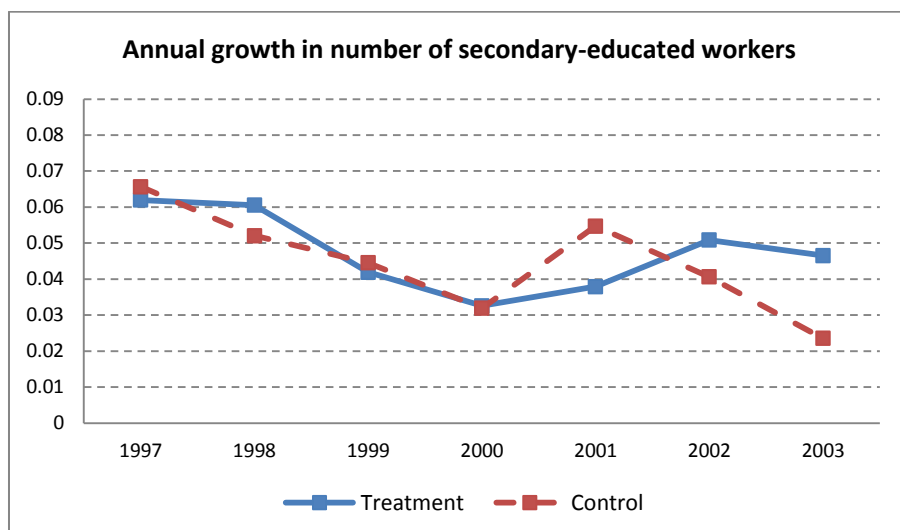
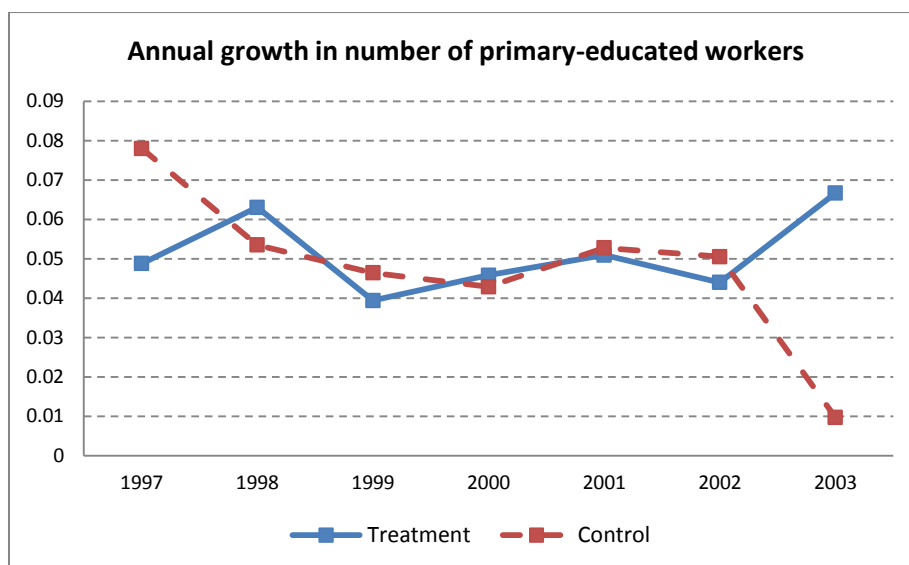
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Appendix Figure 1: Average annual wage growth 1997-2003 by education group, treatment vs. control regions



Appendix Figure 2: Average annual employment growth 1997-2003 by education group, treatment vs. control regions



Appendix Table 1. Robustness: Wage growth effects with alternative control group

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Education group	$\Delta \ln w$ Primary	$\Delta \ln w$ Primary	$\Delta \ln w$ Secondary	$\Delta \ln w$ Secondary	$\Delta \ln w$ College	$\Delta \ln w$ College
Treatment	0.002 (0.0268)	0.004 (0.0274)	0.019*** (0.0044)	0.021*** (0.0045)	0.032*** (0.0101)	0.047*** (0.0106)
Treatment x 1998		0.004 (0.0059)		-0.005 (0.0031)		-0.017 (0.0112)
Treatment x 1999		-0.01* (0.0055)		-0.001 (0.0031)		-0.021* (0.0098)
Treatment x 2000	0.014*** (0.0036)	0.011** (0.0044)	0.004 (0.0027)	0.002 (0.0037)	0.008 (0.007)	-0.006 (0.0071)
Treatment x 2001	0.013* (0.0062)	0.01 (0.0065)	0.01*** (0.0022)	0.008** (0.003)	-0.000 (0.008)	-0.014 (0.0101)
Treatment x 2002	0.008 (0.0098)	0.006 (0.0101)	0.007** (0.0031)	0.005 (0.0036)	-0.001 (0.0093)	-0.015 (0.0148)
Treatment x 2003	0.001 (0.0043)	-0.002 (0.0049)	0.006** (0.003)	0.004 (0.0034)	0.006 (0.0081)	-0.008 (0.011)
Observations	43,723	43,723	140,902	140,902	25,849	25,849
Obs. treatment	10,829	10,829	32,801	32,801	5,126	5,126
Obs. control	32,894	32,894	108,101	108,101	20,723	20,723
R <sup>2</sup>	0.18	0.18	0.13	0.13	0.26	0.26

Notes: The dependent variable is the change in log daily wages from one year to the next. All regressions include regional fixed effects, industry fixed effects, year fixed effects, industry x year fixed effects, firm fixed effects, age controls, dummies for immigrant status and gender, as well as a constant term. Robust standard errors clustered at the regional level are given in parenthesis. \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10 percent level, respectively.

Appendix Table 2. Robustness: Wage growth effects without firm fixed effects

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Education group	$\Delta \ln w$ Primary	$\Delta \ln w$ Primary	$\Delta \ln w$ Secondary	$\Delta \ln w$ Secondary	$\Delta \ln w$ College	$\Delta \ln w$ College
Treatment	0.000 (0.0025)	0.002 (0.0038)	0.001 (0.0017)	0.001 (0.003)	0.005 (0.0035)	0.015* (0.007)
Treatment x 1998		0.003 (0.0062)		-0.000 (0.0034)		-0.012 (0.0107)
Treatment x 1999		-0.009 (0.0073)		0.000 (0.0035)		-0.015 (0.0118)
Treatment x 2000	0.015*** (0.0045)	0.013** (0.0053)	0.003 (0.0035)	0.003 (0.0045)	0.01 (0.0085)	0.000 (0.0047)
Treatment x 2001	0.01 (0.0068)	0.008 (0.0074)	0.01*** (0.0029)	0.01** (0.0035)	0.005 (0.006)	-0.005 (0.0074)
Treatment x 2002	0.017** (0.0067)	0.015* (0.0075)	0.009** (0.003)	0.009** (0.0035)	-0.003 (0.0072)	-0.012 (0.0117)
Treatment x 2003	0.001 (0.0042)	-0.001 (0.0064)	0.008*** (0.0023)	0.008** (0.0035)	-0.001 (0.0063)	-0.01 (0.0083)
Observations	33,253	33,253	102,495	102,495	18,066	18,066
Obs. treatment	10,829	10,829	32,801	32,801	5,126	5,126
Obs. control	22,424	22,424	69,694	69,694	12,940	12,940
R <sup>2</sup>	0.03	0.03	0.02	0.02	0.05	0.05

Notes: The dependent variable is the change in log daily wages from one year to the next. All regressions include regional fixed effects, industry fixed effects, year fixed effects, industry x year fixed effects, age controls, dummies for immigrant status and gender, linear and quadratic terms for log firm size, as well as a constant term. Robust standard errors clustered at the regional level are given in parenthesis. \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10 percent level, respectively.

Appendix Table 3. Robustness: Impact of payroll tax cut on number of workers in a firm (by level of education)

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Education group	<i>ln size</i> Primary	<i>ln size</i> Primary	<i>ln size</i> Secondary	<i>ln size</i> Secondary	<i>ln size</i> College	<i>ln size</i> College
Treatment	-0.251*** (0.057)	-0.27*** (0.0552)	-0.05 (0.0625)	-0.052 (0.0633)	0.406*** (0.0399)	0.413*** (0.0393)
Treatment x 1998		0.014 (0.0268)		-0.018 (0.0202)		-0.034 (0.0333)
Treatment x 1999		0.043 (0.0408)		0.023 (0.0171)		0.012 (0.0226)
Treatment x 2000	0.031 (0.0233)	0.051 (0.0418)	-0.002 (0.0172)	-0.000 (0.0216)	0.058 (0.0375)	0.05 (0.0344)
Treatment x 2001	0.061* (0.0295)	0.08 (0.045)	0.042*** (0.0131)	0.044** (0.0155)	0.003 (0.0457)	-0.004 (0.0413)
Treatment x 2002	0.002 (0.0284)	0.021 (0.0388)	0.043 (0.0245)	0.045* (0.0219)	0.024 (0.0305)	0.017 (0.0285)
Treatment x 2003	0.061 (0.0396)	0.08 (0.0475)	0.069* (0.0335)	0.071** (0.0286)	0.064 (0.0477)	0.057 (0.0462)
Observations	24,470	24,470	34,139	34,139	12,419	12,419
Obs. treatment	8,258	8,258	11,761	11,761	3,999	3,999
Obs. control	16,212	16,212	22,378	22,378	8,420	8,420
R <sup>2</sup>	0.42	0.42	0.41	0.41	0.39	0.39

Notes: The dependent variable is the log number of workers (both part-time and full-time) by level of education. All regressions include regional fixed effects, industry fixed effects, year fixed effects, industry x year fixed effects, and a range of controls for workforce composition at the firm level based on age, gender, immigrant status, and contract type, as well as a constant term. The observations are weighted by firm size within the respective education group. Robust standard errors clustered at the regional level are given in parenthesis. \*\*\*, \*\* and \* indicate significance at the 1, 5 and 10 percent level, respectively.