Regional payroll tax cuts and individual wages: Heterogeneous effects of worker ability and firm productivity*

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Abstract

This paper exploits a payroll tax reform in Norway and applies matched employer-employee register data on individual wages to study the incidence of payroll taxation. The contribution is to allow for heterogeneous wage effects of payroll tax cuts based on unobserved worker ability and firm productivity (measured by estimated worker and firm fixed effects, respectively). Using the difference-in-difference approach, the estimates show that on average, about 30% of the labor cost reduction is shifted to employees through higher wages and the degree of tax shifting increases gradually during the first three years after the reform. Among high-productivity firms, the degree of tax shifting is stable at 40 – 50% throughout the post-reform period. In low-productivity firms, none of the labor cost reduction is shifted to employees in the short term but there is a delayed wage response in the medium term. On average, the wage effect of reduced payroll taxes is twice as large in high-productivity compared to low-productivity firms. The difference in wage response is mainly between firms, rather than between workers within firms, although low-ability workers in low-productivity firms miss out on the wage gain from the reduced labor costs. The analysis does not find any robust employment effect of the payroll tax cut, neither in low- nor high-productivity firms.

Keywords: Payroll tax cut; individual wages; matched employer-employee data; heterogeneous effects

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1. Introduction

This paper exploits a regionally differentiated payroll tax reform in Norway and applies matched employer-employee register data on individual wages to study the incidence of payroll taxation. The contribution is to study heterogeneous wage effects of reduced payroll taxes across low- and high-quality firms and across low- and high-ability workers within firms. Following the method innovated by Abowd et al. (1999), worker and firm fixed effects are estimated simultaneously, and the fixed effect coefficients are used as measures of unobserved worker ability and firm productivity, respectively. 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. These studies estimate an average wage response to payroll tax changes and do not capture important heterogeneities across low- and high-quality workers and firms.

The broad finding in the literature is that payroll tax changes are partly shifted to employees through wages, while the employment response is limited. Based on Swedish firm-level data for the private sector, Bennmarker et al. (2009) show that 1 percentage point reduction in the payroll tax rate increases the average wage bill per employee by about 0.25%. They find no employment effect among firms that existed both before and after the reform, but accounting for entry and exit of firms gives 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. Gruber (1997) applies data for manufacturing firms in Chile and finds full shifting of payroll taxes to wages and no significant effect on employment. Based on plant-level data for the main manufacturing export industries in Norway, Gavrilova et al. (2017) document about 50% tax shifting following an industry-specific increase in the payroll tax rate. 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.

A few studies consider heterogenous wage effects of payroll taxation across workers or firms. Murphy (2007) applies individual level data from the US Current Population Survey during 1992 – 2002 to study the incidence of state unemployment insurance taxes on wages. Since the data consists of independent cross-sections and workers cannot be followed over time, the data is aggregated to the state level while separating between three demographic groups. The main finding is that increased taxes are partly passed on to employees through wages, and the effect is larger for less mobile worker groups

(married women and youths) than for more mobile groups (prime-aged men).[1] Saez et al. (2019) apply matched employer-employee data to study payroll tax cuts targeted to young workers in Sweden. They find no wage effect for young treated workers relative to older untreated workers, but rather a general wage increase in heavily treated firms. Further, their analysis documents positive employment effects for young workers. Based on matched employer-employee data for Norway, Dale-Olsen (2018) and Ku et al. (2020) study heterogenous effects of payroll taxation according to workers' position in the wage distribution and firm size, respectively.

This paper contributes to the limited literature on heterogenous wage effects of payroll taxation and is the first to study heterogeneity based on worker ability and firm productivity. Based on a regionally differentiated payroll tax reform, the difference-in-difference approach is used to estimate the incidence of taxation. Treatment municipalities face a 4.2 percentage point cut in the payroll tax rate (equal to 3.8% reduction in labor costs). The dataset consisting of about 172,000 observations during 1998–2003 gives information on individual wages, worker characteristics, and firm affiliation. The matched employer-employee data allows for simultaneous estimation of worker and firm fixed effects, which are used to rank workers based on the quality of the firm where they are employed (measured by firm fixed effects) or their unobserved ability (measured by worker fixed effects). Workers are divided into separate groups, which enables an analysis of heterogeneous wage effects of the payroll tax cut across low- and high-quality workers and firms.[2]

The results show that on average, about 30% of the labor cost reduction is shifted to employees through higher wages and the degree of tax shifting increases gradually during the first three years after the reform. Among workers employed in high-productivity firms, 1% reduction in labor costs generates 0.4 – 0.5% wage increase, while the average wage response in low-productivity firms is down to 0.22%. The degree of tax shifting is twice as large in high-productivity compared to low-productivity firms. While tax shifting in the most productive firms is stable throughout the post-reform period, the delay in wage response following the payroll tax cut is driven by low-productivity firms, where none of the labor cost reduction is shifted to employees in the short term. Workers with high unobserved abilities seem to gain somewhat more from the payroll tax cut compared to low-ability workers, although the difference between the two groups of workers is not significant. Looking at low- and high-productivity firms separately shows that within the most productive firms, all workers gain from the

[1] Other studies using tax variation across firms and states in the US to estimate tax incidence include Gruber (1994) and Anderson and Meyer (1997, 2000).

[2] It should be noted that the analysis estimates short- to medium-term impacts of the reform as it is based on only three years of post-reform data.

payroll tax cut and the difference in wage effect between low- and high-ability workers is not significant. In low-productivity firms, however, the average wage effect is relatively lower and low-ability workers are shown to miss out on the wage gain from reduced labor costs. This implies that the payroll tax cut contributes to increased wage inequality within municipalities. The findings are robust to alternative model specifications, and the assumption of parallel wage trends in treatment and control municipalities prior to the reform is fulfilled.

Aggregating the individual level data to the firm level allows for an investigation of employment effects of the payroll tax cut. The analysis is based on about 35,000 firm-year observations of firm size (number of workers) and focuses on employment effects along the extensive margin. The analysis does not find any robust employment effect of the payroll tax cut, neither in low- nor high-productivity firms. Lack of parallel employment trends between treatment and control municipalities in the years before the reform limits the confidence in any employment response to the payroll tax cut.

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 worker ability and firm productivity. Employment effects are investigated in section 4, and interpretations of the findings are given in section 5. Section 6 offers concluding remarks.

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. 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. The tax rate varies from 0% in peripheral municipalities in the most northern parts of Norway (zone 5) to 14.1% in densely populated municipalities in the south (zone 1). Municipalities in zones 2 – 4 face tax rates of 10.6%, 6.4% and 5.1%, respectively. [3]

[3] 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 municipalities. At the same time, the differentiation was extended to seven regional zones and the determination of tax zone changed from employees' resident location to the firm's location.

I exploit a payroll tax reform enforced by the Norwegian government effective from January 1st, 2000, where 32 municipalities went from zone 2 to zone 3 facing a 4.2 percentage point reduction in the payroll tax rate.[4] Due to annual wage negotiations and time lags in hiring decisions, it is not expected to see an impact on wages or employment until 2001, which acts as the reform year in the analysis. Municipalities are part of larger economic regions, and based on commuting flows between municipalities, Statistics Norway divides Norway into 89 travel-to-work areas understood as common labor markets. Of the 32 municipalities affected by the payroll tax cut, 23 municipalities are part of a region where all municipalities in the region face the same reduction in the payroll tax rate. In the analysis, I focus on these municipalities, while the remaining nine municipalities are excluded.[5]

The main methodological challenge is that municipalities affected by the payroll tax cut are not randomly chosen but follow from a political process targeting lagging areas. It is hard to find valid instruments for such policy changes. My approach is to use difference-in-difference estimation, where the control group includes municipalities that remain in tax zone 2 during the entire period of study (56 municipalities).[6] Prior to the reform, both treatment and control municipalities have payroll tax rate of 10.6%, while in the post-reform years the treatment group faces 4.2 percentage points lower tax rate than the control group. The analysis focuses on the years 1998–2003. 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 municipalities 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 duration of the contract and the type of contract.[7] Data on the exact number of hours worked per week is only available for a subsample of workers, so to have as precise measure of wages as possible, I focus on full-time workers. The number of days worked per year can be inferred from the duration of

[4] In 2000, Norway consisted of 435 municipalities, and in total, 53 municipalities were 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.

[5] As a check of robustness, the analysis is also performed based on all 32 municipalities, and the findings are consistent (documented in Table A.1 in the appendix and discussed in section 3).

[6] Consistent with the choice of treatment municipalities, the control group is restricted to municipalities that are part of an economic region where all municipalities in the region remain in tax zone 2 during the entire period of study.

[7] 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.

the contract, which combined with data on annual wage income from the tax register give a measure of daily wages. There is also information on the age, gender, immigrant status, industry affiliation, firm affiliation, and home municipality of all individuals. The dataset includes almost 29,000 private sector workers every year during the period 1998–2003, giving a total of 171,991 observations.[8] Workers can enter and leave the labor market during the six-year period, and in total 36,889 different workers are included (8,723 residing in treatment municipalities and the remaining 28,166 in control municipalities). Workers are allocated to 6,767 different firms and 47 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 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. The final dataset consists of 6,767 firms and 35,432 firm-year observations during 1998–2003. Almost 30% of the firms are located in treatment municipalities and the remaining 70% in control municipalities. Firm characteristics include industry affiliation, municipality of location, and workforce composition.

Table 1 about here

Factors determining a municipality'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 municipalities are comparable along these dimensions.

[8] I exclude workers in the primary industries (agriculture, fishing, and forestry), as well as public sector workers. Workers above 61 years of age face lower payroll tax rates than other workers and are therefore dropped from the analysis. I also exclude workers below 25 years of age. This gives a dataset of about 270,000 worker-year observations. The tax register gives information on total annual earnings, rather than separate earnings for each work contract. Workers with more than two contracts during a year, as well as workers with one full-time and one part-time contract, are excluded. For workers with two full-time contracts, a maximum of three months of overlap between the contracts is allowed. I also exclude workers whose contract duration is less than 300 days during a year. These restrictions reduce the dataset by about 51,000 observations. Missing data on annual earnings, level of education, or industry affiliation, together with exclusion of workers that move between treatment and control municipalities, further excludes approximately 3,000 observations. Individuals residing in treatment or control municipalities but working in firms located outside these municipalities are excluded since the estimation of firm fixed effect will be misleading (the majority of workers in these firms are not part of the dataset). This applies to offshore workers as well as workers with long-distance commute to Oslo and other large cities and amounts to about 36,000 observations. Workers that are observed in a single year are excluded, since worker fixed effects cannot be estimated (about 6,000 observations). To avoid extreme observations, I exclude the bottom 1% of the wage distribution, which gives the final dataset of 171,991 observations.

The two groups consist of sparsely populated peripheral municipalities facing outmigration in the years prior to the reform and with an average unemployment rate of about 2%. 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, the age, ethnic, gender, and education compositions of the labor force are similar in the two groups of municipalities. About 4% of workers are immigrants, 77% are male, 27% are below 35 years of age, and almost half belong to the middle age group (35-49 years old). College-educated workers account for 10-12% of the work force, while the rest is roughly equally split between low- and secondary-educated workers. Compared to treatment municipalities, the control group employs a larger share of the labor force in manufacturing (59% vs. 53%).[9] As seen from panel B, firms in treatment and control municipalities have similar size and composition of their workforce. The average firm has about eight workers, of which 64% are male, 5% are immigrants, 12% are college-educated, and almost 20% are part-time workers. The lower share of male workers in the firm-level dataset compared to the individual level data follows from the inclusion of part-time workers. In both treatment and control municipalities, 68% of firms are in services and the remaining 32% in manufacturing.

The first part of the analysis applies the individual-level data to estimate wage effects of lower payroll taxes. The estimation is based on a difference-in-difference approach with log wages as the dependent variable. Since the estimation is at the individual level, wages of 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. New workers may enter the panel after the reform and if new entries correlate with the treatment this may bias the treatment effect. To account for this, worker fixed effects are included in the regression. As a first start, the wage effect of payroll taxes is estimated as an average post-reform effect:

$$\ln w_{ijrst} = \alpha_1 P_t + \alpha_2 T_r \cdot P_t + X_{it}\delta + \gamma_j + \eta_r + \varphi_{st} + \mu_i + \varepsilon_{ijrst} \qquad (1)$$

where w_{ijrst} is the daily wage income for worker *i* employed in firm *j*, in municipality *r*, industry *s*, and year *t*, T_r is a dummy that equals 1 if the municipality 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 2001 onwards).[10] Firm, municipal, industry-year, and worker fixed effects are represented by γ_j, η_r, φ_{st} and μ_i, respectively.

[9] As a check of robustness, the analysis is also performed based on an alternative control group with sector composition more comparable to the treatment group (documented in Table A.1 in the appendix and discussed in section 3).
[10] Due to the inclusion of worker fixed effects, the non-interacted treatment dummy is dropped from the regression.

Industry-year fixed effects capture industry-specific trends. The vector of time-varying worker characteristics (X_{it}) includes dummies for age (5-year intervals). The error term is given by ε_{ijrst} and δ is a vector of parameters. The coefficient of the interaction term between the treatment dummy and the dummy for post-reform years (α_2) captures the difference in daily wages between treatment and control municipalities in the years after the payroll tax cut compared to the pre-reform period.

The static fixed-effects specification in equation (1) has its drawbacks when dealing with unbalanced panels, heterogenous treatment effects, and violations of the common-trend assumption (Borusyak and Jaravel 2017; Gibbons et al. 2019; Schmidheiny and Siegloch 2019). My preferred specification therefore applies the event-study design with a full set of leads and lags around the treatment effect (with year 2000 as the base year):

$$\ln w_{ijrst} = \sum_{k \neq 2000} \rho_k D_{rt}^k + X_{it}\delta + \gamma_j + \eta_r + \varphi_{st} + \mu_i + \varepsilon_{ijrst} \quad (2)$$

where D_{rt}^k is a dummy indicating that the worker is employed in a treatment municipality and the time is year *k*, where $k = [1998, 2003]$ and $k \neq 2000$, and ρ_k is the corresponding year-specific coefficient. The specification thus includes interaction terms between the treatment dummy and each year pre- and post-reform (except the base year). The advantage of the event-study design is that it reveals the dynamics of the wage response by separating between short- and medium-term effects of the payroll tax cut. Further, the difference-in-difference approach assumes that treatment and control municipalities follow parallel trends in the outcome variable during the pre-reform period, which can be tested based on the significance of the pre-reform coefficients.

Equations (1) and (2) employ the method innovated by Abowd et al. (1999) estimating two-way worker and firm fixed effects, where the fixed effects are identified by workers switching firms. By ranking workers based on the estimated worker and firm fixed effects, I study the heterogeneity of the wage effect of reduced payroll taxes across low- and high-quality workers and firms, respectively. Worker fixed effects represent a measure of unobserved ability of workers, while firm fixed effects give a measure of firm productivity.

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 log firm size as

dependent variable, where firm size is measured by the number of workers in the firm.[11] The analysis starts out estimating the employment effect of payroll taxes as an average post-reform effect, but the preferred specification follows the event-study design:

$$\ln size_{jrst} = \beta_0 + \beta_1 T_r + \sum_{k \neq 2000} \rho_k D_{rt}^k + Y_{jt}\lambda + \eta_r + \varphi_{st} + \tau_{jrst} \quad (3)$$

where $size_{jrst}$ is the number of workers in firm *j* in municipality *r*, industry *s*, and year *t*. 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 equations (1) and (2). The regression includes municipal and industry-year fixed effects. The error term is given by τ_{jrst}, β_0 is a constant and λ is a vector of parameters. Since a given percentage change in employment can be hard to compare across small and large firms, the observations are weighted by firm size.[12] The parameters ρ_k are our main interest and capture the difference in employment between treatment and control municipalities in year *k* compared to the base year.

3. Heterogeneous wage effects of payroll tax cut

Table 2 documents the impact of the payroll tax reform on individual wages based on the difference-in-difference approach described by equations (1) and (2) of section 2. Column (1) gives the average wage effect of the payroll tax cut in the post-reform years. The interaction term between the treatment dummy and the dummy for post-reform years is significant at the 5% level with a coefficient of 0.01. The increase in wages from the pre-reform period to the post-reform period is one percentage point higher in treatment municipalities compared to control municipalities. The interpretation is that 4.2 percentage point reduction in the payroll tax rate (equivalent to 3.8% reduction in labor costs) generates 1% higher wages (average effect during the post-reform years).[13] Column (2) reports the preferred difference-in-difference specification, where interaction terms between the treatment dummy and each year pre- and post-reform are included. The wage effect is significant in all three years after the reform with the coefficient increasing gradually from 0.007 in the short term to 0.015

[11] Lower payroll taxes can also generate an employment expansion at the municipal level through entry of new firms (rather than increased firm size). Such effects are ignored in the present analysis.
[12] Solon et al. (2015) offer a discussion of weighting in regression analysis.
[13] 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.

after three years (with an average coefficient of 0.011). On average, 1% reduction in labor costs generates 0.29% wage increase, so that 29% of the labor cost reduction is shifted to employees through higher wages. The estimates show that the degree of tax shifting is higher in the medium term than in the short term. The estimation further reveals that none of the pre-reform interaction terms are significant. This confirms the assumption of parallel wage trends in treatment and control municipalities prior to the reform and indicates that the findings are not driven by long-term trends. The estimated coefficients and corresponding 95% confidence intervals are illustrated in Figure 1.

Table 2 and Figure 1 about here

The estimated wage effect documented above is consistent with previous studies of payroll taxation in Nordic countries. The Swedish analysis of Bennmarker et al. (2009) finds that the average wage bill per employee increases by 0.25% per percentage point reduction in the payroll tax rate. The recent analyses by Dale-Olsen (2018) and Ku et al. (2020) study the impact of payroll taxation in Norway based on a reform occurring during 2004–2006, where tax rates increased gradually in zones 2 – 4. The reform was enforced by EU regulations independently of local labor market conditions, which creates exogenous variation in the payroll tax rate. The main drawback to the use of the later reform is that it was quickly reversed (by 2007 the tax rates were back to the pre-reform levels). Ku et al. (2020) find that one percentage point increase in the payroll tax rate reduces wages by 0.32% in general, and by 0.46% in large firms (although the wage effects are imprecisely estimated and statistically insignificant). Dale-Olsen (2018) documents 60-80% tax shifting following the payroll tax increase in 2004, while he finds no significant impact on wages when the tax increase is reversed a couple of years later.

The robustness of the results is investigated and documented in Table A.1 in the appendix. The share of workers employed in manufacturing is higher in control municipalities than in treatment municipalities (as documented in Table 1). Column (1) of Table A.1 applies an alternative control group, excluding 17 municipalities that are dominated by one or two large manufacturing firms. This gives an industrial structure that is more comparable to the treatment group. Column (2) extends the treatment group from 23 to 32 municipalities by including municipalities that are part of a labor market region where some of their neighbor municipalities are unaffected by the reform. To check if small firms are driving the results, column (3) excludes firms with only one full-time employee during the period of study. This amounts to about 25% of the firms, but only 5% of the workers and 4% of the observations. The increase in daily wages following the reduction in payroll taxes identified in the main analysis could be due to increased hourly wages and/or increased number of hours worked. To check whether the

result is driven by wage changes along the intensive margin, the regression in column (4) is based on a subsample of workers where data on hours worked per week is available for the period 1998–2003. This amounts to about 60% of the full sample and as documented in Table A.2 in the appendix, the characteristics of the subsample correspond well with the full sample of workers (as given in Table 1).[14] Further, the correlation between the daily wage and the hourly wage equals 0.95. As seen from Table A.1, the estimated wage effect of reduced payroll taxes is robust to the alternative control and treatment groups, is unaffected by the exclusion of small firms, and is independent of whether daily or hourly wages are used as dependent variable. The common-trend assumption for the pre-reform period holds in all robustness tests. Finally, I run a placebo test for the period 1995–2000, with an imagined payroll tax reform happening in 1998. The average wage effect of the (imagined) reform is insignificant and in the full dynamic specification with interaction terms between the treatment dummy and each year pre- and post-reform, none of the interaction terms is significant at the 5% level. The estimates are available from the author.[15]

The matched employer-employee dataset allows for the simultaneous estimation of worker and firm fixed effects. I use the estimated fixed effects to study heterogenous wage effects of lower payroll taxes across groups of workers defined based on the quality of the firm where they are employed (measured by firm fixed effects) or their unobserved ability (measured by worker fixed effects). Workers are ranked according to the value of the corresponding fixed effect and divided into two separate groups. The distribution of firm fixed effects ranges from -1.48 to 1.57 with mean and standard deviation across workers equal to zero and 0.17, respectively. About 45% of the worker-year observations are in firms with positive fixed effect coefficient, 37% are in firms with negative fixed effect coefficient, while the remaining 18% of the observations are in firms with fixed effect coefficient equal to zero. I separate between workers employed in low-productivity firms (fixed effect equal to or lower than zero, bottom 55%) and workers employed in high-productivity firms (positive fixed effect, top 45%).[16]

As documented in panel A of Table 3, the impact of the payroll tax cut on wages differs across low- and high-productivity firms. Among workers employed in the most productive firms, the wage effect is significant and large in all three years after the reform with the coefficient in the range 0.014 – 0.019

[14] In the full dataset, the observations are allocated equally across the 6-year period with 16-17% of the observations in each year. In the subsample with data on hourly wages, the share of observations is down at 12% in 1998, while each of the other years accounts for 15-19% of the observations.

[15] A set of tables describing alternative model specifications is available as an external online appendix: https://sites.google.com/site/hildegunnestokke/.

[16] The alternative split, where workers in firms with zero fixed effect coefficient are included in the upper part of the distribution, is documented in the external online appendix. The main findings remain.

(given in column (4)). The increase in wages from the base-year to the post-reform years is 1.4 – 1.9 percentage points higher in treatment municipalities compared to control municipalities. Since the payroll tax cut resulted in 3.8% lower labor costs, this implies that 1% reduction in labor costs generates 0.4% – 0.5% wage increase both in the short and the medium term. The average degree of tax shifting in high-productivity firms is 45%. For workers in the lower part of the firm fixed effects distribution, the first-year effect on wages is insignificant and close to zero, meaning that none of the labor cost reduction is shifted to employees in the short term. The second- and third year effects are significant at the 10% and 5% levels, respectively, with coefficient values that are lower than the corresponding estimates for workers employed in high-productivity firms. On average during the post-reform period, 22% of the labor cost reduction is shifted to employees through higher wages. The degree of tax shifting is twice as large in high-productivity compared to low-productivity firms. While tax shifting in the most productive firms is stable throughout the post-reform period, the delay in wage response following the payroll tax cut is driven by low-productivity firms. Table A.3 in the appendix reformulates the estimated models to test for the statistical significance of the differences between the two groups of workers. The findings show that in the short term, the estimated wage effect of reduced payroll taxes is significantly higher among workers employed in high-productivity firms compared to workers in low-productivity firms. As a check of robustness, I consider an alternative division of workers into three equal-sized groups based on the firm fixed effects distribution. The difference in wage effect between the bottom and top third of workers confirms the main finding of heterogenous wage effects of payroll tax cuts based on firm quality (see external online appendix).

Table 3 about here

In panel B of Table 3, workers are ranked according to their unobserved abilities and divided into two equal-sized groups.[17] Among high-ability workers (top half of the fixed effects distribution), the wage effect of reduced payroll taxes is significant in all post-reform years, with the coefficient increasing from 0.007 in the first year to 0.017 in the third year. The wage effect for low-ability workers is somewhat lower in magnitude and less significant compared to high-ability workers (the third-year effect is the only significant at the 10% level). However, as seen from the second column of Table A.3 in the appendix, the difference in wage effect between low- and high-ability workers is not significant. An alternative separation of workers into three equal-sized groups generates consistent findings (see external online appendix). I also consider the wage effect of reduced payroll taxes for low- and high-

[17] The worker fixed effect distribution ranges from 4.68 to 8.53 with mean and standard deviation equal to 6.47 and 0.29, respectively.

ability workers within low- and high-productivity firms, respectively, as documented in Table 4. As seen from the number of observations in the four worker groups, low- and high-ability workers are well represented in both types of firms. The findings show that low-ability workers in low-productivity firms miss out on the wage gain from the reduced labor costs, while within high-productivity firms, both low- and high-ability workers benefit from the payroll tax cut. This implies that the payroll tax cut contributes to increased wage inequality within municipalities.[18]

Table 4 about here

4. Employment response to payroll tax cut

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 (3) in section 2. As seen from column (1) of Table 5, the interaction term between the treatment dummy and the dummy for post-reform years is significant and equals 0.052. The increase in employment from the pre-reform to the post-reform period is 5.2 percentage points higher in treatment municipalities compared to control municipalities. However, the more flexible specification in column (2) reveals that the interaction term between the treatment dummy and one of the pre-reform years is negative and significant. The assumption of parallel employment trends between treatment and control municipalities in the years before the reform does not hold. Further, the post-reform interaction terms are low in value and not significant at the 5% level. The lack of any employment response to the payroll tax cut is in line with most of the previous studies on payroll taxation in Nordic countries (Bennmarker et al. 2009; Korkemäki and Uusitalo 2009). Dale-Olsen (2018) finds no short-run employment effect of a payroll tax rise, but in the long run, there is evidence of increased inflow into retirement and disability benefit receipt. Some contrarian evidence is provided in the recent analysis by Ku et al. (2020), who find that local employment decreases by 1.37% following a one percentage point increase in the payroll tax rate.

Table 5 about here

The robustness of the (lack of) employment effect is investigated in Table A.4. Column (1) applies an alternative control group with industrial structure more comparable to the treatment group (as

[18] To further check for potential differences across workers, the external online appendix offers comparisons of wage effects of reduced payroll taxes across groups of workers based on observable worker characteristics, notably gender, age and level of education. The broad picture is no significant difference in the estimated wage effects across these worker groups.

explained in relation to Table A.1). In column (2), the treatment group is extended to include all 32 municipalities affected by the payroll tax cut, and in column (3), small firms (average firm size over the period equal to 2 or less) are excluded from the analysis. 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. In column (4), I redefine firm size and concentrate on the employment expansion among full-time workers. The first two columns of Table A.4 show a significant employment effect in the first year after the reform, but in all specifications, there is still significant pre-reform interaction terms. The lack of parallel employment trends pre-treatment limits the confidence in any employment response to the payroll tax cut.

The heterogeneity in wage effects of reduced payroll taxes across low- and high-productivity firms (documented in panel A of Table 3) motivates an analysis of heterogenous employment effects across firms. I use the firm fixed effects estimated in column (1) of Table 2 to separate between low- and high-productivity firms. Among the 6,767 firms in the dataset, about 30% have positive fixed effects coefficient, 25% have negative coefficient, while the remaining 45% of firms have fixed effect coefficient equal to zero. Based on two alternative splits between low- and high-productivity firms, the results document no significant employment response to lower payroll taxes, neither in low- nor high-productivity firms (see Table 6). While the wage effect of payroll tax cuts differs between low- and high-productivity firms, the employment effect is lacking for all firms.

Table 6 about here

The analysis above considers employment expansions along the extensive margin (number of workers within firms). Alternatively, the payroll tax cut can affect employment through the intensive margin by increasing the worktime of existing employees. I do not have information on the exact number of hours worked per week for all workers, but the dataset at the individual level allows for an analysis of movements between contract types (short part-time, long part-time, and full-time contracts, as described in footnote 7 in section 2). Based on the difference-in-difference approach, I study whether the reduction in payroll taxes increases the probability of a contract upgrade (movement from part-time to full-time contract, or from short part-time to long part-time contract). Overall, 1.3% of the worker-year observations involve an upgrade in the type of contract, but there is no significant difference between treatment and control municipalities when comparing the pre-reform and post-

reform periods. Given the data available, I do not find any evidence of employment expansions along the intensive margin.[19]

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 elasticities of labor demand and labor supply. The lack of any significant employment effect of the payroll tax cut is consistent with inelastic labor supply (very steep supply curve). In this case, labor supply does not respond much to wage changes. This can be understood from limited pool of available workers locally (low unemployment rate) combined with low regional labor mobility. The municipalities affected by the reform are typically remote regions characterized by outmigration in the years before the reform. Regional labor mobility is highest among young, high-educated individuals and they tend to move towards urban areas. Positive and significant wage effect of lower payroll taxes indicate elastic labor demand and a possible understanding of the heterogeneity in wage response across low- and high-productivity firms is related to differences in demand elasticities. The more elastic labor demand is (flatter demand curve), the larger is the wage effect of reduced labor costs. This is consistent with high-productivity firms being more able to substitute towards labor as this factor becomes relatively cheaper. In addition, the scale effect could be larger in the most productive firms giving more pronounced output expansion following the labor cost reduction. This is illustrated in Figure 2, where the upward shift in the labor demand curve gives relatively larger wage increase in high-productivity firms with more elastic labor demand.

Figure 2 about here

Applying the method innovated by Zoutman et al. (2018), the variation in the payroll tax rate is used to estimate labor demand and labor supply elasticities. The labor supply elasticity follows from regressing firm size on the average wage per employee excluding payroll tax, while the labor demand elasticity is based on a regression of firm size on the average firm wage including payroll tax.[20] In both cases, a transformation of the payroll tax rate serves as an instrument for the respective daily wage

[19] The estimation results are available from the author upon request.

[20] The estimation applies the firm-level data used in section 4 and the average firm wage is calculated from the individual-level data used in section 3. Due to stricter sample restrictions on the individual-level data (to have precise wage measures) compared to the firm-level data, some firm-year observations are dropped due to missing wage data. The estimation is based on a total of 32,408 observations.

rate (excluding or including payroll tax). The regressions include controls for workforce composition at the firm, as well as municipal and industry-year fixed effects. The findings are documented in Table 7. Column (1) gives the reduced form effect of the payroll tax on log firm size. The coefficient is negative and significant, indicating that a reduction in the payroll tax rate increases the number of employees at the firm level. The estimations of the labor supply and labor demand elasticities are given in columns (2) – (3) and (4) – (5), respectively. In the supply-side equations, neither the first stage estimation nor the labor supply elasticity estimated in the second stage is significantly different from zero. This is consistent with the interpretation given above where the labor supply curve is very steep and labor supply does not respond much to wage changes. There is considerable variation in the size of labor supply elasticities in the literature, as documented in the meta-analysis of Bargain and Peichl (2016). The elasticity is highest among married women and single mothers (mean values of 0.43 and 0.59, respectively), while most estimates for men and childless single women lie in the range between zero and 0.3.

Table 7 about here

From the demand-side, the first stage estimation gives a positive and significant effect of the transformed payroll tax rate on the wage rate including payroll tax and the F-statistic for instrument strength is about 32, well above the rule of thumb for weak instruments in the test suggested by Stock and Yogo (2005). The labor demand elasticity is estimated to -1.37 and is significant at the 5% level. Separating between firms based on productivity (defined from estimated firm fixed effects) gives a labor demand elasticity of -0.71 and -1.51 in low- and high-productivity firms, respectively. This indicates that the labor demand curve is flatter for the most productive firms, although the estimated elasticities are not statistically significant. As shown in the meta-regression analysis of Lichter et al. (2015), most estimates of labor demand elasticities range between zero and minus one but estimates below minus one are not unusual. Based on the same method as in this paper, Gavrilova et al. (2017) find a labor demand elasticity of -3.2.

6. Conclusions

This paper studies the incidence of payroll taxation based on matched employer-employee register data on individual wages for private sector workers in Norway. Exploiting a payroll tax reform and using the difference-in-difference approach, the estimates show that on average, about 30% of the labor cost reduction is shifted to employees through higher wages and the degree of tax shifting increases gradually during the first three years after the reform. The contribution of the paper is to allow for

heterogeneous wage effects of the payroll tax cut based on unobserved worker ability and firm productivity (measured by estimated worker and firm fixed effects, respectively). The main finding is that the wage effect of the reduced payroll tax is twice as large in high-productivity compared to low-productivity firms. In the most productive firms, the degree of tax shifting is stable at 40 – 50% throughout the post-reform period. In low-productivity firms, none of the labor cost reduction is shifted to employees in the short term but there is a delayed wage response in the medium term. The difference in wage effect is mainly between firms, rather than between workers within firms, although low-ability workers in low-productivity firms miss out on the wage gain from the reduced labor costs. This implies that the payroll tax cut contributes to increased wage inequality within municipalities. The analysis does not find any robust employment effect of the payroll tax cut, neither in low- nor high-productivity firms. It is of interest to pursue the potential heterogeneity in employment responses to payroll tax cuts further, preferably based on rich firm-level data.

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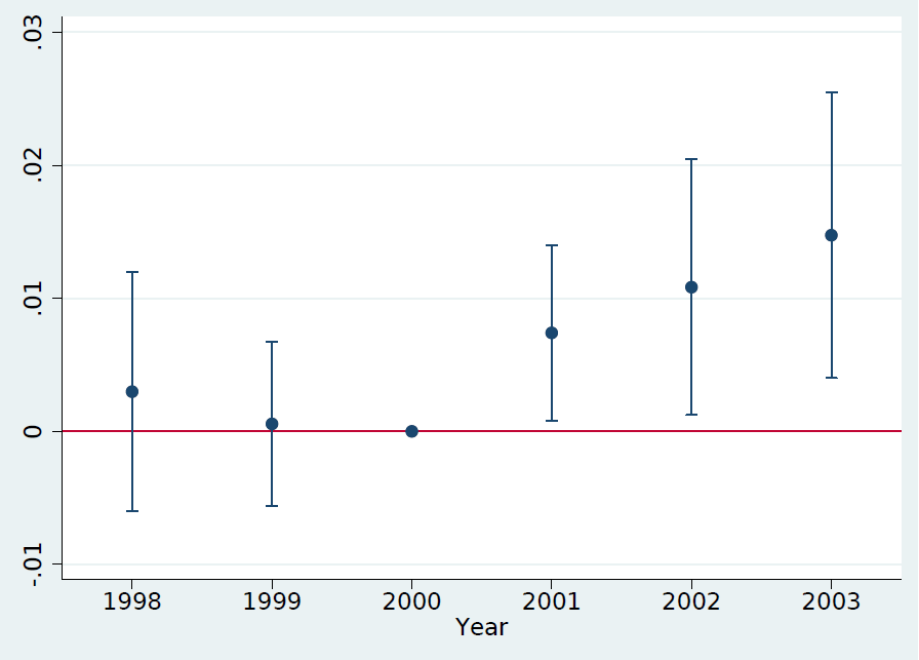
Fig. 1 Event study estimates of payroll tax cut on individual wages



Notes: The figure plots the estimated coefficients and 95 percent confidence intervals of interacted year and treatment fixed effects in the regression of log daily wages given in column (2) of Table 2.

Fig. 2 Wage effect of payroll tax cut in low- and high-productivity firms

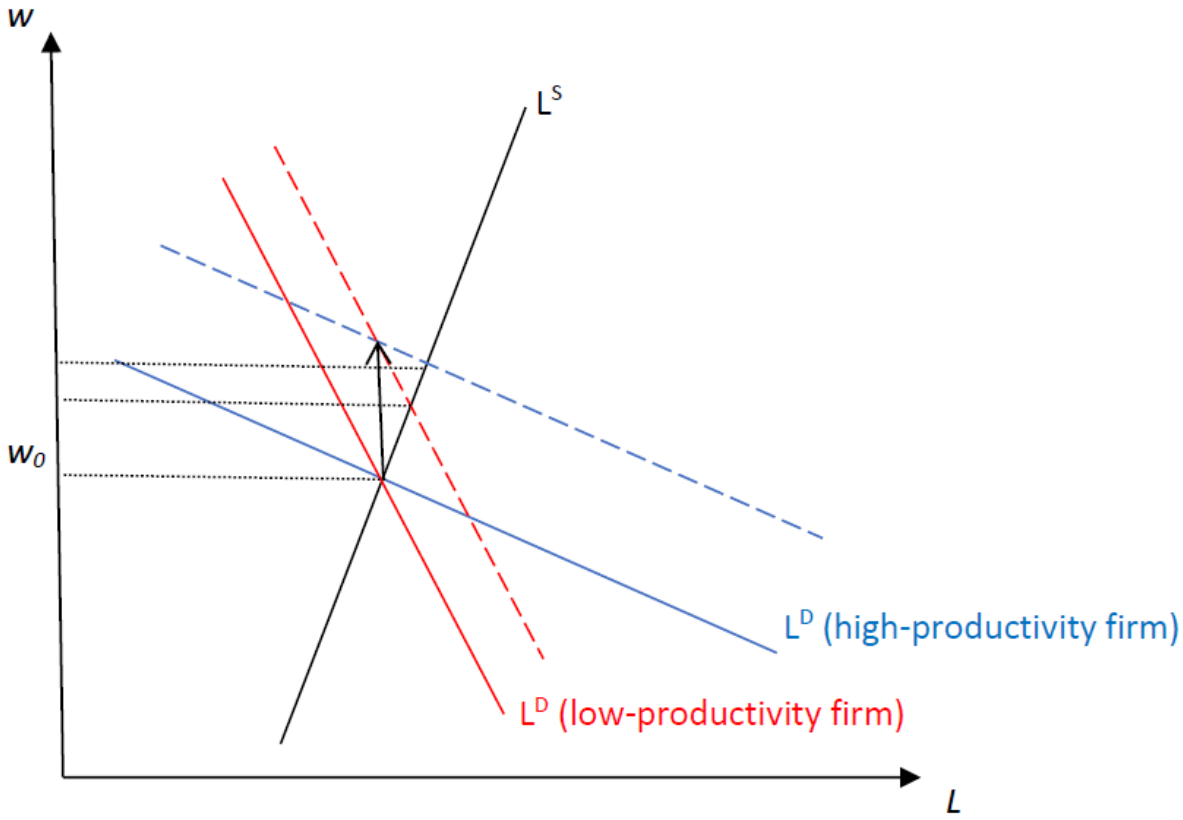


Table 1. Descriptive statistics

	Treatment municipalities	Control municipalities
Panel A: Individual-level data		
Worker-year observations	40,137	131,854
Average daily wage (in 2000 NOK)	737	814
Age composition		
25-34 years old	0.265	0.271
35-49 years old	0.47	0.479
50-61 years old	0.265	0.25
Share of immigrants	0.036	0.049
Share of male workers	0.768	0.771
Education composition		
Low educated	0.45	0.414
Secondary educated	0.45	0.46
College educated	0.1	0.126
Sector composition		
Manufacturing	0.529	0.589
Services	0.471	0.411
Panel B: Firm-level data		
Firm-year observations	9,501	25,931
Average firm size	7.1	8.4
Age composition		
< 35 years old	0.339	0.38
35-49 years old	0.397	0.387
> 49 years old	0.264	0.233
Share of immigrants	0.045	0.055
Share of male workers	0.643	0.638
Education composition		
Low educated	0.49	0.481
Secondary educated	0.392	0.387
College educated	0.118	0.132
Sector composition		
Manufacturing	0.32	0.315
Services	0.68	0.685
Share of part-time workers	0.194	0.179

Notes: The individual- and firm-level data cover all full-time workers and firms, respectively, in the private sector in 23 treatment municipalities and 53 control municipalities during 1998-2003 (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).

Table 2. Impact of payroll tax cut on individual wages

Dependent variable	ln *w* (1)	ln *w* (2)
Post 2001	0.217***	
	(0.0113)	
Treatment x Post 2001	0.01**	
	(0.0045)	
Treatment x 1998		0.003
		(0.0045)
Treatment x 1999		0.001
		(0.0031)
Treatment x 2001		0.007**
		(0.0033)
Treatment x 2002		0.011**
		(0.0048)
Treatment x 2003		0.015***
		(0.0054)
Observations	171,991	171,991
Obs. Treatment	40,137	40,137
Obs. Control	131,854	131,854
R^2	0.35	0.35

Notes: The regressions are based on yearly data for all full-time private sector workers during 1998-2003, excluding workers in the primary sector (agriculture, fishing, and forestry). The dependent variable is log daily wages. The regressions include time-varying worker characteristics, as well as municipal, industry-year, firm, and worker fixed effects. Robust standard errors clustered at the municipal level are given in parenthesis. ***, ** and * indicate significance at the 1, 5 and 10 percent level, respectively. The R^2 reported is within workers.

Table 3. Heterogenous wage effects of reduced payroll taxes: firm quality and worker ability

Panel A: Ranking of workers based on firm productivity (estimated firm fixed effects)				
	Employed in low-productivity firm (firm FE ≤ 0)		Employed in high-productivity firm (firm FE > 0)	
	(1)	(2)	(3)	(4)
Post 2001	0.205***		0.233***	
	(0.008)		(0.0242)	
Treatment x Post 2001	0.007		0.014**	
	(0.0047)		(0.0057)	
Treatment x 1998		0.001		0.009
		(0.0046)		(0.0075)
Treatment x 1999		0.000		0.002
		(0.0031)		(0.0057)
Treatment x 2001		0.002		0.018***
		(0.0033)		(0.0047)
Treatment x 2002		0.009*		0.014**
		(0.0048)		(0.0068)
Treatment x 2003		0.014**		0.019**
		(0.0062)		(0.0077)
Observations	93,777	93,777	76,843	76,843
Obs. Treatment	28,022	28,022	11,795	11,795
Obs. Control	65,755	65,755	65,048	65,048
R^2	0.32	0.32	0.40	0.40
Panel B: Ranking of workers based on worker ability (estimated worker fixed effects)				
	Low-ability workers (bottom 50%)		High-ability workers (top 50%)	
	(1)	(2)	(3)	(4)
Post 2001	0.193***		0.238***	
	(0.0154)		(0.0131)	
Treatment x Post 2001	0.009*		0.012**	
	(0.0055)		(0.0046)	
Treatment x 1998		-0.000		0.002
		(0.0056)		(0.005)
Treatment x 1999		0.001		-0.001
		(0.0045)		(0.0034)
Treatment x 2001		0.007		0.007**
		(0.0048)		(0.0034)
Treatment x 2002		0.009		0.015***
		(0.0059)		(0.0054)
Treatment x 2003		0.013*		0.017**
		(0.0066)		(0.0064)
Observations	85,784	85,784	85,878	85,878
Obs. Treatment	23,559	23,559	16,498	16,498
Obs. Control	62,225	62,225	69,380	69,380
R^2	0.28	0.28	0.44	0.44

Notes: The dependent variable is log daily wages. In panel A, workers are ranked based on the quality of the firm where they are employed, measured by the firm fixed effect coefficients estimated in column (1) of Table 2. Workers employed in firms with fixed effect coefficient equal to zero are included in the lower part of the distribution. In panel B, workers are ranked based on their unobserved ability, measured by the worker fixed effects coefficients estimated in column (1) of Table 2. All regressions include time-varying worker characteristics, as well as municipal, industry-year, firm, and worker fixed effects. Robust standard errors clustered at the municipal level are given in parenthesis. ***, ** and * indicate significance at the 1, 5 and 10 percent level, respectively. The R^2 reported is within workers.

Table 4. Heterogeneous wage effect of reduced payroll taxes: Low- and high-ability workers within low- and high-productivity firms

Panel A: Workers employed in low-productivity firms				
	Low-ability workers		High-ability workers	
	(1)	(2)	(3)	(4)
Post 2001	0.175***		0.221***	
	(0.0144)		(0.0118)	
Treatment x Post 2001	0.006		0.011**	
	(0.0059)		(0.0046)	
Treatment x 1998		-0.003		0.001
		(0.0066)		(0.0049)
Treatment x 1999		-0.001		0.000
		(0.0046)		(0.0038)
Treatment x 2001		-0.001		0.005
		(0.0051)		(0.0036)
Treatment x 2002		0.006		0.015***
		(0.0064)		(0.0052)
Treatment x 2003		0.012		0.018***
		(0.0082)		(0.0064)
Observations	43,410	43,410	50,225	50,225
Obs. Treatment	14,867	14,867	13,119	13,119
Obs. Control	28,543	28,543	37,106	37,106
R^2	0.25	0.25	0.42	0.42
Panel B: Workers employed in high-productivity firms				
	Low-ability workers		High-ability workers	
	(1)	(2)	(3)	(4)
Post 2001	0.197***		0.288***	
	(0.0268)		(0.0305)	
Treatment x Post 2001	0.016**		0.018*	
	(0.006)		(0.0094)	
Treatment x 1998		0.008		0.000
		(0.0082)		(0.0089)
Treatment x 1999		0.006		-0.011*
		(0.0071)		(0.0062)
Treatment x 2001		0.021***		0.014
		(0.0063)		(0.0087)
Treatment x 2002		0.018**		0.014
		(0.0076)		(0.0097)
Treatment x 2003		0.02**		0.015
		(0.0088)		(0.0125)
Observations	41,705	41,705	35,059	35,059
Obs. Treatment	8,519	8,519	3,259	3,259
Obs. Control	33,186	33,186	31,800	31,800
R^2	0.35	0.35	0.49	0.49

Notes: The dependent variable is log daily wages. The definitions of the different worker groups are given in the notes to Table 3. All regressions include time-varying worker characteristics, as well as municipal, industry-year, firm, and worker fixed effects. Robust standard errors clustered at the municipal level are given in parenthesis. ***, ** and * indicate significance at the 1, 5 and 10 percent level, respectively. The R^2 reported is within workers.

Table 5. Impact of payroll tax cut on employment

Dependent variable	ln *size* (1)	ln *size* (2)
Treatment	0.668***	0.691***
	(0.1248)	(0.1257)
Post 2001	-0.264***	
	(0.092)	
Treatment x Post 2001	0.052**	
	(0.0206)	
Treatment x 1998		-0.067**
		(0.0285)
Treatment x 1999		-0.000
		(0.0209)
Treatment x 2001		0.028*
		(0.0164)
Treatment x 2002		0.018
		(0.0232)
Treatment x 2003		0.044
		(0.0347)
Observations	35,432	35,432
Obs. Treatment	9,501	9,501
Obs. Control	25,931	25,931
R^2	0.62	0.62

Notes: The regressions are based on yearly data on firms in the private sector during 1998-2003, excluding firms in the primary sectors (agriculture, fishing, and forestry). The dependent variable is the log firm size, where firm size is measured as number of workers (both part-time and full-time). All regressions include municipal and industry-year fixed effects, as well as a range of controls for workforce composition at the firm level based on age, gender, immigrant status, level of education, and contract type. The observations are weighted by firm size. Robust standard errors clustered at the municipal level are given in parenthesis. ***, ** and * indicate significance at the 1, 5 and 10 percent level, respectively.

Table 6. Heterogenous employment effects of reduced payroll taxes based on firm quality

	Low-productivity firms		High-productivity firms	
	Firm FEs < 0	Firm FEs ≤ 0	Firm FEs ≥ 0	Firm FEs > 0
	(1)	(2)	(3)	(4)
Treatment	1.356***	0.142**	0.226**	-0.036
	(0.1258)	(0.0623)	(0.101)	(0.3207)
Treatment x 1998	-0.026	-0.031	-0.088	-0.099
	(0.0272)	(0.0225)	(0.0564)	(0.0993)
Treatment x 1999	0.012	-0.003	0.000	0.027
	(0.0233)	(0.0192)	(0.0279)	(0.0418)
Treatment x 2001	0.033	0.023	0.029	0.035
	(0.0233)	(0.0178)	(0.0262)	(0.0348)
Treatment x 2002	0.012	0.001	0.031	0.023
	(0.0311)	(0.0221)	(0.0309)	(0.0397)
Treatment x 2003	0.066	0.048	0.033	0.014
	(0.0449)	(0.0315)	(0.0505)	(0.0621)
Observations	10,515	27,130	24,917	8,302
Obs. Treatment	3,063	7,610	6,438	1,891
Obs. Control	7,452	19,520	18,479	6,411
R^2	0.47	0.41	0.72	0.77

Notes: The dependent variable is log firm size. Firms are ranked based on their productivity, measured by the firm fixed effect coefficients estimated in the wage regression in the first column of Table 2. All regressions include municipal and industry-year fixed effects, as well as a range of controls for workforce composition at the firm level based on age, gender, immigrant status, level of education, and contract type. The observations are weighted by firm size. Robust standard errors clustered at the municipal level are given in parenthesis. ***, ** and * indicate significance at the 1, 5 and 10 percent level, respectively.

Table 7. Estimation of labor supply and labor demand elasticity

Dependent variable	Log firm size	Supply-side Wage rate excl. payroll tax	Log firm size	Demand-side Wage rate incl. payroll tax	Log firm size
	(1)	(2)	(3)	(4)	(5)
Payroll tax rate	-1.22**	-0.111		0.889***	
	(0.5327)	(0.1572)		(0.1572)	
Labor supply elasticity			11.01		
			(15.4936)		
Labor demand elasticity					-1.372**
					(0.6682)
Observations	32,408	32,408	32,408	32,408	32,408
F-Statistic			0.498		32.075

Notes: The dependent variable is shown at the top of each column. The first column gives the reduced form effect of the payroll tax rate on log firm size, where the payroll tax is transformed as log (1 + payroll tax rate). Columns (2) – (3) and (4) – (5) report the first and second stage estimations for the supply-side and the demand-side, respectively. The estimations are based on 2SLS, where the wage rate (excluding or including payroll tax) is instrumented by the transformed payroll tax rate. The estimated coefficient in the second stage gives the corresponding elasticity. All regressions include controls for workforce composition, as well as municipal and industry-year fixed effects. The observations are weighted by firm size. Robust standard errors clustered at the municipal level are given in parenthesis. ***, ** and * indicate significance at the 1, 5 and 10 percent level, respectively.

Appendix: Additional tables

Table A.1. Robustness of the wage effect of reduced payroll taxes

	Alternative control group (1)	Alternative treatment group (2)	Excluding small firms (3)	Subsample with hourly wages (4)
Treatment x 1998	0.004	0.003	0.004	-0.000
	(0.0047)	(0.0039)	(0.0046)	(0.0045)
Treatment x 1999	0.001	-0.004	0.001	0.003
	(0.0032)	(0.0036)	(0.003)	(0.0042)
Treatment x 2001	0.007**	0.008**	0.007**	0.007*
	(0.0033)	(0.0032)	(0.0033)	(0.0037)
Treatment x 2002	0.013**	0.014***	0.01*	0.013**
	(0.0052)	(0.0048)	(0.005)	(0.0063)
Treatment x 2003	0.016***	0.015***	0.013**	0.016**
	(0.0057)	(0.005)	(0.0056)	(0.0065)
Observations	124,550	182,944	164,912	104,678
Obs. Treatment	40,137	51,078	38,230	24,875
Obs. Control	84,413	131,866	126,682	79,803
R^2	0.33	0.35	0.36	0.29

Notes: Column (1) uses an alternative control group, where 17 municipalities with different industrial structure compared to the treatment municipalities are excluded. Column (2) extends the treatment group to include municipalities that went from tax zone 2 to zone 3 while other municipalities in their labor market region were unaffected by the reform. This increases the number of treatment municipalities from 23 to 32. In column (3), firms with only one full-time employee are excluded. In columns (1) – (3), the dependent variable is log daily wages, while column (4) is based on a subsample of workers with log hourly wages as dependent variable. All regressions include time-varying worker characteristics, as well as municipal, industry-year, firm, and worker fixed effects. Robust standard errors clustered at the municipal level are given in parenthesis. ***, ** and * indicate significance at the 1, 5 and 10 percent level, respectively. The R^2 reported is within workers.

Table A.2. Descriptive statistics for subsample of workers with data on hourly wages

	Treatment municipalities	Control municipalities
Worker-year observations	24,875	79,803
Average hourly wage (in 2000 NOK)	142.2	157.8
Age composition		
25-34 years old	0.267	0.275
35-49 years old	0.439	0.448
50-61 years old	0.294	0.277
Share of immigrants	0.037	0.052
Share of male workers	0.756	0.763
Education composition		
Low educated	0.465	0.426
Secondary educated	0.433	0.451
College educated	0.102	0.123
Sector composition		
Manufacturing	0.504	0.591
Services	0.496	0.409

Notes: The data cover all full-time workers in the private sector with data on hourly wages during 1998-2003, which account for about 60% of the full sample of workers.

Table A.3. Alternative model formulations to check for significant difference in wage effect between groups of workers defined based on firm productivity or worker ability

	Ranking of workers based on firm productivity (1)	Ranking of workers based on worker ability (2)
Treatment x High-quality firm/worker	0.039	
	(0.0397)	
Treatment x 1998	0.001	-0.002
	(0.005)	(0.0055)
Treatment x 1999	0.000	-0.001
	(0.0033)	(0.0045)
Treatment x 2001	0.002	0.006
	(0.0033)	(0.0047)
Treatment x 2002	0.01*	0.008
	(0.0051)	(0.0058)
Treatment x 2003	0.015**	0.011
	(0.0065)	(0.0066)
Treatment x 1998 x High-quality firm/worker	0.006	0.007
	(0.008)	(0.0052)
Treatment x 1999 x High-quality firm/worker	0.000	0.003
	(0.0067)	(0.0051)
Treatment x 2001 x High-quality firm/worker	0.017***	0.003
	(0.0057)	(0.0053)
Treatment x 2002 x High-quality firm/worker	0.004	0.008
	(0.0076)	(0.0059)
Treatment x 2003 x High-quality firm/worker	0.002	0.008
	(0.0095)	(0.0077)
Observations	171,991	171,991
Obs. Treatment	40,137	40,137
Obs. Control	131,854	131,854
R^2	0.35	0.35

Notes: The dependent variable is log daily wages. The regressions check for significant differences in the wage effect of reduced payroll taxes across groups of workers ranked based on firm productivity in column (1) and worker ability in column (2). In column (1), the interaction terms are with the variable *High-quality firm*, which is a dummy that equals one if the worker is employed in a firm in the upper part of the firm fixed effects distribution, as defined in panel A of Table 3. In column (2), the interaction terms are with the variable *High-quality worker*, which is a dummy that equals one if the worker belongs to the top half of the worker fixed effects distribution. All regressions include interaction terms between year dummies and the dummy for high-quality firm or worker (in column (1) and (2), respectively). Further, time-varying worker characteristics, as well as municipal, industry-year, firm, and worker fixed effects are included. The inclusion of firm fixed effects implies that the dummy *High-quality firm* is dropped from the regression in column (1), and the inclusion of worker fixed effects implies that the dummy *High-quality worker* and the interaction term between treatment region and high-quality worker are dropped from the regression in column (2). Robust standard errors clustered at the municipal level are given in parenthesis. ***, ** and * indicate significance at the 1, 5 and 10 percent level, respectively. The R^2 reported is within workers.

Table A.4. Robustness of the employment effect of reduced payroll taxes

Robustness test	Alternative control group (1)	Alternative treatment group (2)	Excluding small firms (3)	Only full-time workers (4)
Treatment	0.526***	1.099***	0.72***	0.53***
	(0.0485)	(0.1457)	(0.1503)	(0.05)
Treatment x 1998	-0.05**	-0.051**	-0.065**	-0.063**
	(0.019)	(0.0257)	(0.0286)	(0.0263)
Treatment x 1999	-0.003	0.005	0.000	0.01
	(0.0208)	(0.0178)	(0.0207)	(0.0201)
Treatment x 2001	0.039**	0.034**	0.027	0.033*
	(0.0156)	(0.014)	(0.0166)	(0.0175)
Treatment x 2002	0.03	0.029	0.014	0.026
	(0.0238)	(0.0208)	(0.0227)	(0.0236)
Treatment x 2003	0.059*	0.06*	0.04	0.048
	(0.0315)	(0.0322)	(0.0354)	(0.0353)
Observations	28,299	38,168	26,575	34,886
Obs. Treatment	9,501	12,242	7,125	9,338
Obs. Control	18,798	25,926	19,450	25,548
R^2	0.45	0.62	0.65	0.68

Notes: The dependent variable is log firm size. Column (1) uses an alternative control group, where 17 municipalities with different industrial structure compared to the treatment municipalities are excluded. Column (2) extends the treatment group to include municipalities that went from tax zone 2 to zone 3 while other municipalities in their labor market region were unaffected by the reform. This increases the number of treatment municipalities from 23 to 32. In column (3), small firms are excluded (average firm size over the period equal to 2 or less). Column (4) redefines firm size to include only full-time workers. All regressions include municipal and industry-year fixed effects, as well as a range of controls for workforce composition at the firm level based on age, gender, immigrant status, level of education, and contract type. The observations are weighted by firm size. Robust standard errors clustered at the municipal level are given in parenthesis. ***, ** and * indicate significance at the 1, 5 and 10 percent level, respectively.