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EMPLOYMENT AND WAGE EFFECTS OF A PAYROLL-TAX CUT-EVIDENCE FROM A REGIONAL EXPERIMENT*

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Abstract: In this paper we evaluate the effects of a regional experiment that reduced payroll-taxes by 3–6 percentage points for three years in Northern Finland. We match each firm in the target region with a similar firm in a comparison region and estimate the effect of the payroll-tax reduction by comparing employment and wage changes within the matched pairs before and after the start of the experiment.

According to our results the reduction in the payroll-taxes led to an increase in wages in the target region. The point estimates indicate that the increase in wages offset roughly half of the impact of the payroll tax cut on the labor costs. The remaining labor cost reduction had no significant effects on employment.

Keywords: Payroll-tax, Labor demand, Tax incidence, Propensity score matching

JEL-codes: J18, J23, J38, J58, J65, J68

KORKEAMÄKI, OSSI – UUSITALO, ROOPE: EMPLOYMENT AND WAGE EFFECTS OF A PAYROLL-TAX CUT-EVIDENCE FROM A REGIONAL EXPERIMENT Helsinki, VATT, Valtion taloudellinen tutkimuskeskus, Government Institute for Economic Research, 2008 (C, ISSN 0788-5016 (nid.), ISSN 1795-3359 (PDF) No 443). ISBN 978-951-561-767-5 (nid.), ISBN 978-951-561-768-2 (PDF).

Tiivistelmä: Tutkimuksessa arvioidaan Lapin sotu-maksukokeilun työllisyys-vaikutuksia. Kokeilu laski työn sivukuluja kolmeksi vuodeksi 3-6 prosentti-yksiköllä 14 Lapin kunnassa. Vaikutusarvio tehtiin vertaamalla kokeilualueen yritysten työllisyys- ja palkkamuutoksia vertailualueen yritysten muutoksiin ennen kokeilua ja kokeilun aikana. Vertailussa käytettiin kaltaistettujen parien menetelmää. Työn sivukulujen alentaminen johti kokeilualueen yrityksissä jonkin verran vertailualueen yrityksiä nopeampaan palkkojen nousuun. Palkkojen nousu vastasi noin puolta sotumaksujen alentamisen tuomasta työvoimakustan-nusten laskusta – jäljelle jäänyt työvoimakustannusten lasku ei johtanut työllisyyden kasvuun kokeilualueen yrityksissä.

Asiasanat: Työnantajamaksut, työvoiman kysyntä, verotuksen kohtaanto, kaltaistetttujen parien menetelmä

Contents

1. Introduction

A reduction of payroll-taxes lowers wage costs and hence boosts the demand for labor. Its effect on employment depends on the incidence of the payroll-taxes. If the tax cut leads to higher wages that entirely offset the reduction in taxes, the tax cut has no effect on employment.

Past evidence on the incidence and on the employment effects of payroll-tax changes is mixed. Studies that rely on cross-country or time-series variation in the national payroll-tax produce widely varying estimates of tax incidence. An important problem in such approaches is the omitted variables bias. In the cross-country studies it is difficult to control for all the differences in wage-setting institutions. These unobserved across-country differences may be correlated with the differences in the level of taxation and employment. In the time-series studies, there may be simultaneous changes in other variables that affect wages and employment. For example, Hamermesh (1993) summarizes this literature by noting that "the estimates of tax shifting vary across the entire admissible rage and even outside it" and concludes that "it is impossible to draw any firm conclusions about the incidence of payroll tax from these studies".

A more promising approach is to examine the effects of changes in taxes or other mandatory employer contributions when these changes differ across otherwise similar firms. Following this approach Gruber (1994) evaluates the effects of mandated maternity benefits in the US and Gruber (1997) the effects of changes in the mandatory pension contributions in Chile. Anderson and Meyer (1997) and Murphy (2007) examine the incidence of unemployment insurance taxes in the US. In all these cases the changes in the payroll tax rates vary between the firms because of a different composition of their labor force or because of the tax rates depend on firm characteristics. Another approach that is more directly related to our study examines the effects of regional policies that create different changes in the payroll tax rates across firms that are located in different regions but that are otherwise comparable. Prime examples include Bohm and Lind (1993) who evaluate employment effects of regional wage subsidies in the Northern Sweden, Johansen and Klette (1998) who examine the effects of regional differences in payroll-taxes in Norway, and Bennmaker, Mellander and Öckert (2007) who evaluate the effects of a recent regional wage subsidy scheme in Sweden. These studies typically find that the changes in the payroll-taxes are mostly shifted into wages with little effect on labor costs or employment.

In this paper we evaluate employment and wage effects of a regional experiment in the Northern Finland. This experiment abolished employer contributions to the National Pension Scheme and to the National Health Insurance for firms located in the targeted high unemployment regions. Prior to the 2003, these employer

contributions varied between 2.95 and 6 percent of the wage bill, depending on the capital intensity and the size of the firm. From January 1st 2003, all private employers in the 20 target municipalities located in the Northern Finland and on the islands along the western coast were exempt from these social security contributions for three years. In this paper we focus on the effects in the Northern Finland where over 90 percent of the eligible firms are located.

A regionally targeted program has several benefits compared to an across the board cut in taxes. Perhaps the main benefit for the policy makers is that the effects of a regional program are substantially easier to evaluate. The employment change in the target region can be compared to similar regions that are not affected by the tax cut. If the target and comparison regions are truly similar, the estimates for the employment effects that are based on the differences in the employment and wage changes between the treatment and the comparison regions provide much more reliable estimates on the effects of the payroll-tax cut than time-series or cross-section variation in the payroll-taxes could ever do.

In this paper we evaluate the effects of the payroll-tax cut using firm-level data on employment and individual data on wages. Our main results are based on a comparison of employment changes in the target region firms and the employment changes in the firms located in a control region that is as similar as possible in terms of unemployment rate, industry structure and the composition of the labor force. We end up comparing the target region firms in the Northern Finland to the firms located in other high unemployment areas in the Northern and Eastern Finland.

Comparison of the employment changes across regions still creates problems if the regions are not quite similar in all relevant characteristics. For example, an industry-specific boom might have different effects in different regions depending on the industry structure of the region. To make the treatment and the comparison regions more comparable we adopt a matching procedure to identify comparable firms (or rather plants) in the treatment and control regions. We then evaluate the effects of the payroll-tax cut by comparing the firms that are located in different regions but that are otherwise similar in all observed pre-treatment characteristics.

2. The experiment

Payroll-taxes in Finland consist of employer contributions to the Employees' Pension Scheme, the Unemployment Insurance, the National Pension Insurance, the National Health Insurance, and the Employment Accident Insurance. The tax rates of various components vary across sectors and by firm size[1]. According to Statistical Yearbook of the Social Insurance Institution the average payroll-tax rate was 23.86 % in 2002.

In March 2002, the Finnish government agreed to a temporary removal of employer contributions to the National Pension Insurance and the National Health Insurance for firms that operated in the twenty target municipalities. The removal of these contributions lowered the payroll-taxes for the eligible firms by 4.1 percentage points, on average. The program was designed as an experiment with a stated aim to evaluate the effect of a cut in the payroll-taxes on employment in the target region. The payroll-tax exemption lasted for three years from January 1st 2003 to December 31st 2005. In December 2005, the government extended the duration of the experiment to the end of 2009.

As the payroll-tax exemption may have anticipatory effects, it is useful to note that the tax exemption was first suggested by a working group that presented its report in December 2001. The law was a part of the government budget proposal for the year 2003 that was agreed upon within the government in March 2002. The government gave the proposal to the parliament in September 2002. The parliament accepted the budget proposal and the president signed the law on the payroll-tax exemption in December 2002. The payroll-tax exemption was also widely discussed in press during the spring 2002. It is, therefore, possible that firms who anticipated the tax exemption could have altered their employment already before the start of the program in January 2003. However, it is unlikely that any employment effects could have occurred before March 2002 since the nature of the program was very much an open question until then.

All private employers and state-owned enterprises that had a "permanent place of business" in the twenty target municipalities were eligible for the tax exemption. The maximum annual reduction was 30 000 euros per firm. To comply with the EU-legislation regulating state-aid that may distort competition within the Union,

[1] In 2002, the private sector employers contributed 1.69 % of the wage bill to the National Health Insurance, and 1.00 % to the Employment Accident Insurance. For calculating the National Pension Insurance contributions the firms are divided into three categories based on their size and capital intensity. The contribution rates in these categories were 1.35, 3.55 and 4.45. The Unemployment Insurance contributions are progressive, the contribution rate being 0.7 % of wage bill for wages up to 840 940 euros and 2.7 % of the wages exceeding this threshold. The Employees' Pension Scheme has a relatively complicated fee structure. In the large firms pension contributions vary with the age of the employee and are partially experience-rated and depend on the number of previous employees receiving early retirement benefits. Small firms pay a flat rate of 17.32 %.

agriculture, fishing, and transport industries were excluded from the experiment. An important restriction is also that local governments were not eligible for the exemption.

Prior to the beginning of the experiment the government estimated that 3500 firms would be eligible for the exemption, and that the budgetary cost of the experiment would be eight million euros. To cover the costs without cutting benefits financed by payroll taxes the experiment was financed by temporarily raising the National Health Insurance contributions for the employers outside the target region by 0.014 percentage points.

All the target municipalities were located in high unemployment areas. However, the geographical borders of the target area were somewhat arbitrary. There were other regions outside the target area with comparable, and even higher, unemployment rates. The target municipalities were selected through a political process and there is no obvious reason why just these municipalities were selected. In fact, the original task of the working group that proposed the tax exemption was limited to measures that would be targeted only to the three northernmost municipalities. In their final report, the working group proposed two alternatives: one involving only these three municipalities and another involving also nine other municipalities in the Northern Finland. After the working group rendered its final report, but before the government gave its proposal to the parliament, two more municipalities in Lapland and six municipalities on islands along the western coast were added to the tax exemption region. Eventually the target area covered the entire province of Lapland except its capital region around Rovaniemi and an industrial region around Kemi-Tornio. On the other hand, the working group would have granted a tax exemption also to the local governments employers. The final proposal was a compromise that excluded all public sector employers with the exception of state-owned enterprises[2].

Applying for the tax exemption was made easy for the participating employers. The employers were only required to file a starting declaration to the local tax office. The employers could then simply deduct the tax exempt amount from their monthly employer contributions. The additional requirement was that the employers also had to report tax exemptions in detail in their annual report to the tax administration. The ease of participation was reflected in high take-up rates. According to our calculations, all eligible employers with at least 50 employees, 90 percent of the eligible employers with at least five employees and 75 percent of the firms with 2–4 employees had filed a starting declaration by December 2003.

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[2] State-owned enterprises are government agencies that operate in the market and compete with the private firms. The largest such agencies in the target area are Destia that builds and maintains roads and Metsähallitus that mainly maintains state-owned forests and national parks.

Most firms that applied for the tax exemption were very small. The median firm had only four employees. Only ten percent of the firms had more than twenty, and 2.5 percent more than fifty employees. In terms of employment and payroll-tax bill these "large" firms naturally represent a much higher share. The largest industries were business services, retail trade, hotels and restaurants and construction. In total, the experiment involved 2334 firms with 17 099 employees during the first year. According to our calculations the reduction of payroll-tax revenue due to the experiment was 4.2 million euros in the first year.

Table 1 Participating firms according to size

Firm size (number of full-time employees)	N firms	N Employees	Payroll-tax deduction
0	456		31 955
1	424	424	84 075
2–4	659	1 836	382 585
5–9	369	2 451	686 321
10–19	237	3 202	931 020
20–49	139	4 153	1 157 750
50–99	37	1 544	600 498
101–250	10	1 578	289 497
> 250	3	911	63 555
Total	2 334	17 099	4 227 256

Source: Authors calculations from data provided by the National Board of Taxes

3. Tax incidence and the Finnish wage bargaining system

According to the textbook model the incidence of payroll tax cut depends on the relative elasticities of labor demand and labor supply. A typical empirical finding is that labor supply is relatively inelastic and that the workers therefore bear the cost of tax increases. Most often these incidence results are presented within the context of competitive labor markets. However, the Finnish wage determination system differs substantially from the competitive market model. Below we describe the main features of the system focusing on its potential effects on tax incidence.

Wage bargaining in Finland involves a high degree of co-ordination between the different unions and the employer organizations. A framework agreement is typically negotiated at a national level between the union and employer federations on a one- or two-year basis. After central agreement has been reached, the individual unions and the respective employer organizations bargain over wages separately in each industry. These contracts determine a general wage increase applied to all wages in the sector and a wage schedule determining a minimum pay in each task. The industry-specific collective labor agreements are also binding for the non-union members in the industries where the union contract is "representative". Since union density is roughly 70 per cent, most industries have a representative contract. There are no statutory minimum wages in Finland.

Even though union bargaining occurs at the national level, there is room for regional variation in wages, as well as, wage variation across firms and across workers within firms. Local bargaining between individual workers or their local union organization and the firms may lead to outcomes that deviate from the national contracts. The employer can naturally pay more and if both local parties agree, and as long as the minimum provisions are not violated, even less than what is agreed in the national contract. Wage drift defined as wage increase exceeding what is agreed in the union contracts has historically accounted for approximately 40 percent of the wage growth. This fraction has declined over time but was still 35 percent between 1992 and 2000 (Uusitalo 2005). More recently firm-specific arrangements such as profit sharing have become more important leading to an increase in across firm variance in wages (Uusitalo and Vartiainen 2007).

The implications of national wage bargaining for the tax incidence are not entirely clear. On one hand one might claim that wage changes are determined at the national level and that a regional payroll-tax subsidy scheme has little or no impact on wages. On the other hand the importance of local bargaining and the fact that the employee and the employer can freely agree on wage increases

exceeding what is agreed in the union contract, may lead to a situation where the payroll tax cut leads to a wage increase. Even in this case tax shifting may be different than in a national scheme if the workers are mobile and due to factor mobility the elasticity of local labor supply larger than elasticity of labor supply in the whole country (Murphy 2007).

4. Empirical strategy

Our estimates are based on differences in employment and wage changes between the firms eligible for tax exemption and a control group. We created the control group by a two-stage procedure. We first selected the "counties" (NUTS 4 -level sub-regional units) that were most comparable to the target region in terms of unemployment rates, industrial structure and workforce characteristics. We based the selection on the regional statistics published in "Seutukunta- ja maakuntakatsaus 2002" by Statistics Finland. The target region had a high unemployment rate and little manufacturing or other industrial activity. The share employed in agriculture was much higher and the average level of education much lower than in the rest of the country. To create a comparable control region we excluded from the control region other non-target regions in Lapland because they were administrative centers with above average education level (Rovaniemi) or major manufacturing regions (Kemi-Tornio). Instead, we included high unemployment areas from Eastern Finland just south of the target region. Also in choosing the comparison area we excluded regions with major cities so that the comparison area would resemble the target area also in its industry composition (see map in Figure 1). Our judgment is that the choice of comparison areas was rather successful. As shown in Table 2, the target and control regions have similar unemployment and employment rates, reasonably similar industry distribution and a similar population structure. In all these dimensions the target and control regions deviate substantially from the national average.

Figure 1 *Target and comparison regions in the Finnish pay-roll tax cut experiment*

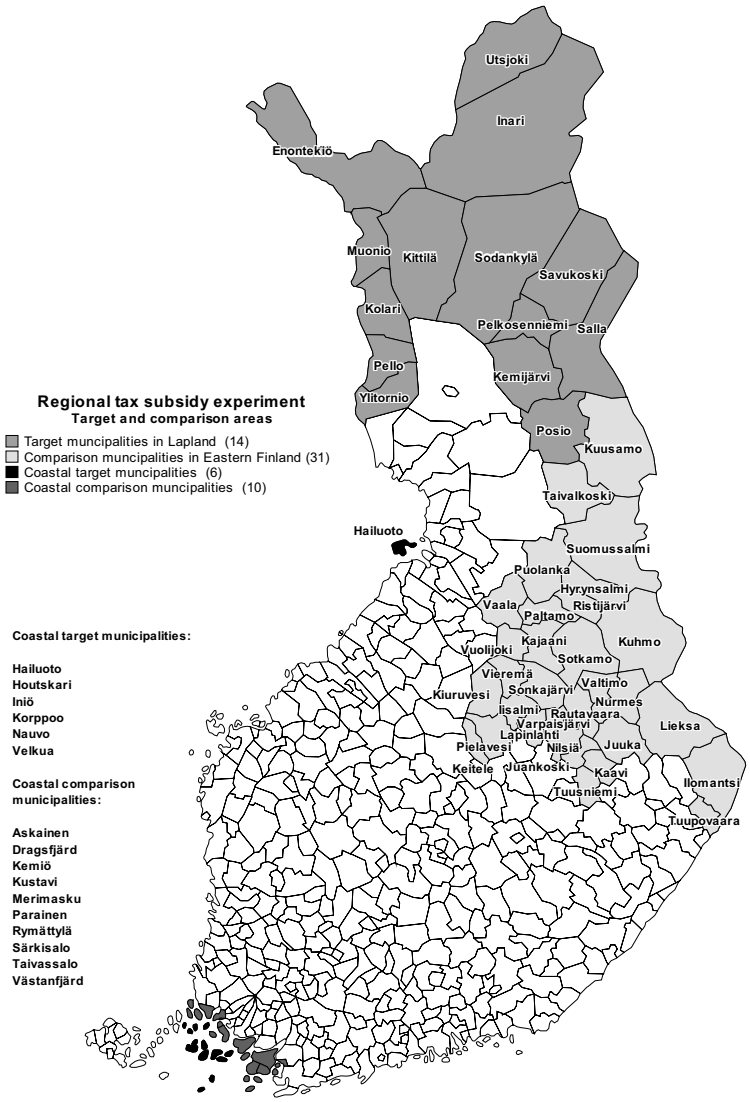


Table 2 Comparison of target and control regions in 2002

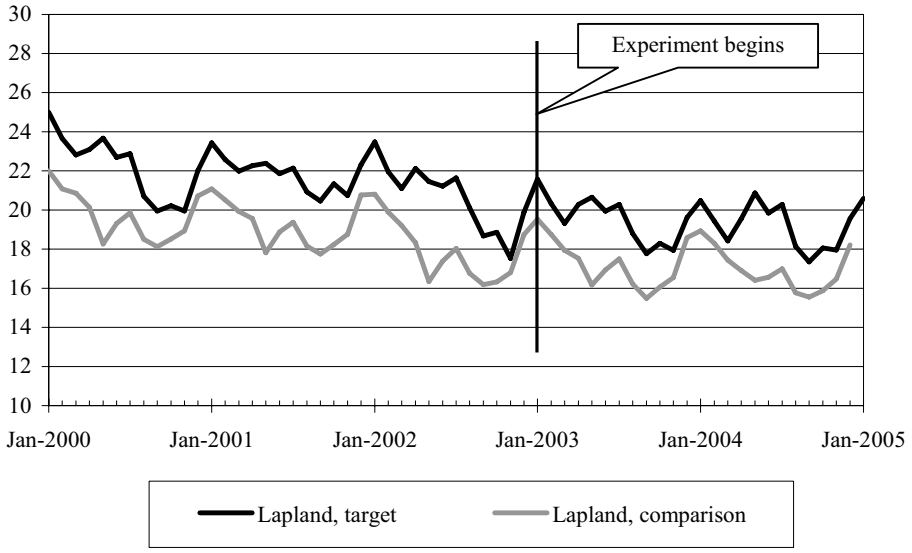
	Regions in Lapland		All Finland
	Target	Comparison	
Population			
Total population	64 979	238 325	5 194 901
Population density 1)	0.84	4.72	17.06
Degree of urbanisation 2)	53.28	61.30	83.30
Percent Swedish	0.18	0.07	5.60
Percent Pensioners	27.33	28.57	21.87
Dependency ratio	1.97	1.96	1.30
Secondary education, % 3)	37.85	37.72	36.10
University level education, %	14.85	15.53	23.30
Employment			
Employment rate, %	52.33	53.18	64.16
Unemployment rate, %	23.56	21.27	12.34
Municipal employees, %	22.95	20.56	14.12
Agriculture and fishing, %	11.74	13.39	4.68
Manufacturing, %	8.90	15.96	19.38
Hotels and restaurants, %	6.46	2.90	3.05
Trade, %	9.35	9.39	12.01
Municipal finance			
State grants, € / person	1 782	1 498	706
Tax revenue, € / person	2 085	2 022	2 715

Notes: 1) inhabitants / km^2, 2) Indicates the proportion of population living in built-up areas (%), 3) Persons aged 15 or over who have a degree from a senior secondary school, vocational or professional education institution, or from a university.

In addition to having similar population structure and similar industry composition, the aggregate economic development in the target and the comparison regions has been remarkably similar before the experiment. For example, the unemployment rate has a very similar downward trend in the target and comparison regions. (Figure 2).

The comparison of unemployment rates in Figure 2 also indicates that the payroll tax exemption did not have a major impact on unemployment - there is no clear difference between the target and the comparison regions after the beginning of the experiment in January 2003. It would also be interesting to compare changes in employment between the regions, but the available data sources offer limited possibilities for doing that in a reliable way. Since the target region represents only some 1.3 % of Finnish population, the sample sizes in national surveys, such as the Labor Force Survey, become dismally small. Eventually, the problem can be solved by computing regional employment changes based on register data, but currently only data up to 2003 are available.

Figure 2 *Unemployment rates in the target and the comparison regions 2000–2005*



Employment weighted average of the municipality level unemployment rates reported by the Ministry of Labour. These unemployment rates are calculated by dividing the number of unemployed job seekers in the unemployment register by the number of people in the labour force calculated from administrative data in the end of year t–2.

If the control area is truly similar to the target area, the development in the control area can be used as a valid counterfactual estimate of what would have happened in the target area in the absence of the payroll-tax reduction. Careful selection of the comparison region is a necessary pre-condition for the validity of this assumption. While focusing on the employment changes "differences away" pre-existing differences between the target and control regions, it is still possible that the target and the control regions experience different shocks or display different pre-existing trends in employment or wages. In particular, a different industrial structure may lead to different timing of the business cycle in the control and the target regions.

To further enhance the comparability of the target and the comparison regions we matched each firm from the target region with a similar firm or firms from the comparison region. We first split the data into seven main industries using the industry classification in the Labor Force Survey and then applied matching methods to create treatment and comparison croups within these industry classes.

In practice, we estimated logit-models within each industry explaining whether the firm was located in the target region. The explanatory variables were the payroll tax bracket, (log)number of employees, (log)total earnings of the

employees, (log)total sales of the firm (all measured in 1999, 2000, and 2001) and a set of three-digit industry codes. The logit-estimates were then used for calculating each firm the predicted probability of being located in the target region, i.e. the propensity score.

Each target region firm is then matched with its nearest neighbor (or neighbors) from the comparison region. We used a genetic matching method (Diamond and Sekhon 2005) that uses both the covariates and the propensity score to create matched samples. The genetic matching procedure starts with a weighting scheme identical to Mahalanobis distance. The weight matrix is then iteratively changed using an evolutionary search algorithm (Sekhon and Mebane 1998) until no further improvement in match quality is attained (see Diamond and Sekhon for details on match quality criteria). In our case this method yields a better match quality with respect to almost all matching variables than simple propensity score matching.

In this evaluation we will follow the effects during the first two years of the program. We account for potential anticipatory effects by creating matched samples based on data from the end of 2001, before any information on the program was made public. To minimize the temptation to re-define the control group ex-post, we fixed the design and published the setup before any data on employment effects became available in January 2004. (Korkeamäki and Uusitalo 2004) The effects of the payroll tax exemption were then evaluated in a transparent way by simply comparing the changes in wages and employment in the treatment and control firms after January 2003. Our last observation date is December 2004. The last year of the experiment is left out because of the changes in the comparison area; the firms in ten municipalities in the Kainuu County that belong to our comparison area became eligible for a similar payroll-tax exemption in 2005 as a part of a regional self-government experiment. This new experiment may contaminate the results of the original experiment but it should not be a major issue up to the end of 2004, because adding payroll-tax cut to the Kainuu regional self-government experiment was a last-minute change in legislation that was announced only in December 2004.

5. Data

We created the matched sample of target and control firms based on the data from the Register of Enterprises and Establishments by Statistics Finland. This register includes data on sales, wage bill, and (imputed) employment of each plant in Finland. Each plant can also be located to a certain municipality. There were 2809 firms in the target area and 7544 firms in the control area. We restricted the sample to the private sector firms that had a positive turnover, paid at least some wages and employed at least one worker in 2001. We also required that the firm has only one plant, so that its location and hence the eligibility for the tax exemption can be determined accurately. We found 1592 such firms in the target area and 4265 firms in the control area[3].

The main disadvantage with the establishment register data is that the number of employees in the firm is imputed based on the wage bill, composition of employment and average wages for various employee groups. It is not clear whether the changes in these imputed numbers capture the changes in employment, changes in wages, or perhaps changes in the imputation procedure. Fortunately, comprehensive data on the employment and earnings outcomes was available from the Finnish Tax Administration. Data are based on employer's annual notification that all employers are required to submit to the local tax office. The annual notification includes all wages and salaries paid during the calendar year. The payments are itemized by employee, and the summary form contains the number of recipient itemizations. This number equals the number of employees that have received some wages or salaries from the firm during the year. Naturally, the number of itemizations is only a rough measure of the average employment in the firm. On the other hand, the total wage bill that forms the tax base (i.e. the product of hours worked and the average hourly wage excluding payroll taxes) is accurately reported.

The tax data therefore provides a reliable estimate on whether the payroll-tax deduction had an impact of total wage bill. If the total wage bill increased due to the experiment, there must have been an effect on either wages or employment. Reliable estimates on the incidence of payroll-tax changes require more detailed information on wages and hours. There is no single database where this information could be gathered for all firms. The best available sources of data on wages and hours are the wage statistics of the employer organizations. In Finland there are two large employer organizations: Confederation of Finnish Industry and Employers (TT) and Employers Federation of the Service Industries (PT)[4].

[3] The reduction of the sample size is mainly due to dropping firms that had no paid employees in 2001. Many of these firms still had positive turnover. As a robustness check we included these firms in the sample, but this had no real effect on the results.

[4] These two employer organisations merged in 2004. We use data up to 2004 when the wage surveys were still conducted separately.

Most large employers are members of one of these organizations. Both TT and PT wage surveys contain individual data on all workers in all their member firms. Both surveys contain detailed information on monthly or hourly wages and regular weekly hours. In addition, there are a number of background variables on the employees including sex, tenure, occupation and industry. More detailed description of the data is presented in the Appendix.

6. Results

In the following, we first display evidence that matching balances the characteristics of the firms in the target and the control regions. Then we proceed by presenting the results on the employment changes in the target and comparison regions. We conclude this section with the analysis on wage effects.

6.1 Covariate balancing

Table 3 reports the means of the variables used in matching separately in the target and comparison regions, and in the matched treatment and control groups. In the rightmost column, we also report the national averages of the same variables. According to the table, the differences between the firms in the target and control regions are rather small to begin with and matching removes most of the remaining differences. A comparison between the treatment and the comparison regions and the national average reveals that both regions differ from the national averages and that our comparison region is a substantially more similar to the treatment region than the whole country.

Table 3 Covariate balancing

Means, all variables in log's	Target firms	Matched targets	Matched controls	Control region	National average
Employment 2001, SF	1.06	1.06	1.06	1.12	1.26
Employment 2000, SF	1.00	1.00	1.01	1.06	1.27
Employment 1999, SF	0.92	0.92	0.93	0.97	1.25
Employment 2001, TA	1.66	1.66	1.63	1.74	n.a.
Employment 2000, TA	1.49	1.50	1.50	1.58	n.a.
Wage sum 2001, TA	9.65	9.64	9.67	9.74	10.16
Wage sum 2000, TA	8.62	8.64	8.72	8.71	10.13
Wage sum 1999, SF	7.57	7.58	7.60	7.59	10.05
Turnover 2001	11.10	11.12	11.13	10.95	12.16
Turnover 2000	10.36	10.37	10.39	10.08	12.11
Turnover 1999	9.50	9.50	9.57	9.23	12.04
Industry distribution of firms (percent of firms)					
Manufacturing	13.69	13.46	13.46	15.80	11.47
Construction	13.63	13.84	13.84	15.03	13.16
Trade	20.92	21.11	21.11	21.55	16.62
Hotels and restaurants	12.44	12.37	12.37	7.67	4.48
Transport	12.19	12.05	12.05	9.31	10.35
Business services	13.25	13.46	13.46	14.11	20.71
Other services	13.88	13.71	13.71	16.53	22.28
National Pension Insurance contribution rate					
I (2.95 %)	96.48	96.81	96.81	97.07	92.79
II (5.15 %)	1.01	1.02	1.02	0.98	2.87
III (6.05 %)	2.51	2.17	2.17	1.95	4.34
N Firms 2001	1592	1430	1 430	4 265	136 434
N Employees 2001, TA	12 318	11 034	10 190	39 111	1 318 654

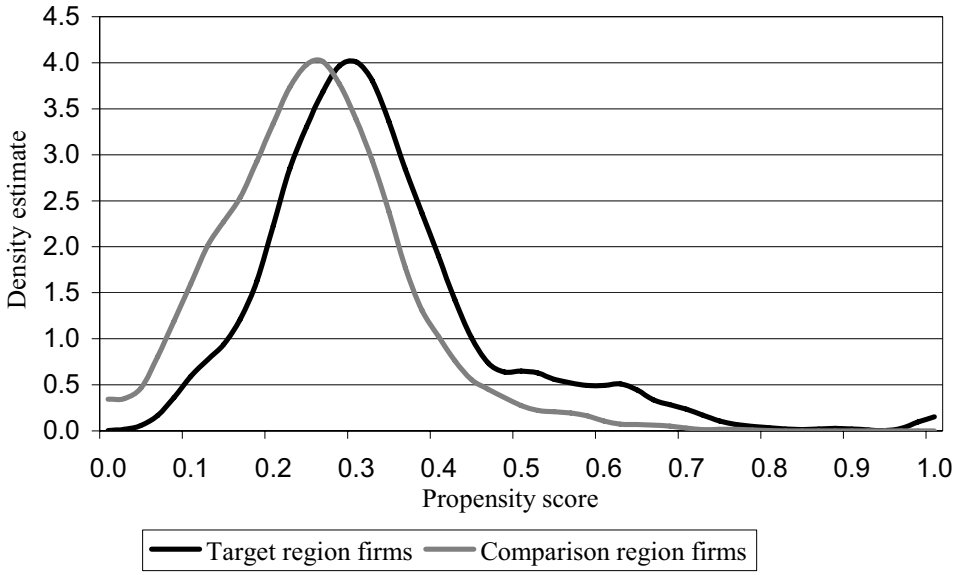
Notes: For the ease of comparison, we calculated the control group mean displayed in the table using nearest neighbour matching.

In the table the industry distribution is reported at a one-digit level. In the actual matching procedure, we use a more detailed industry classification adding 116 three-digit industry codes to the logit-models.

The national averages are calculated from the firm register of Statistics Finland for firms with positive employment, wage sum and turnover. SF = Employment figure supplied by Statistics Finland, estimated man-years. TA = Employment figure supplied by Tax Administration.

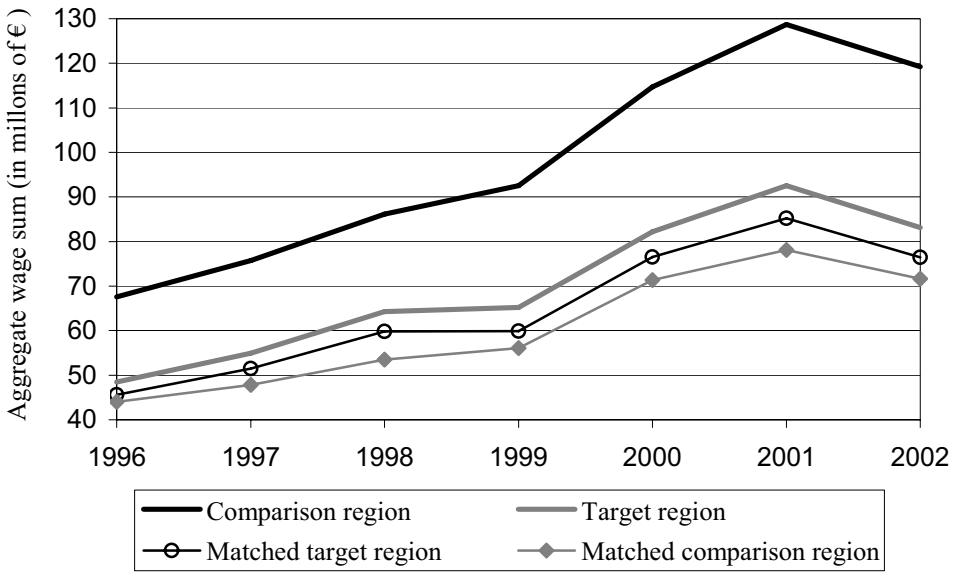
Given the similarity of the target and control regions, there are few strong predictors in the logit-model that is used to explain whether the firm is located in the target region. This is also reflected in the distribution of the propensity score that is rather similar in the target and comparison regions. This also implies that finding regions of common support is not a major problem: a large fraction of firms in both regions have estimated propensity scores between 0.1 and 0.5.

Figure 3 *Estimated propensity score densities for target and control region firms*



As a final check on the comparability between the treatment and the matched control groups we examine pre-experiment trends in the key variables. Figure 4 presents this comparison for the aggregate wage sum. It appears that the firms in the comparison region were larger in the beginning of the period (1996) and have experienced somewhat more rapid growth during the last years of the 1990s. However, the growth in the matched control firms has been very similar to the treatment firms. Note that we use only data from years 1999–2001 is matching and creating the control group, so the similarity in the growth rates before this period is not "forced" into the data, but reflects the similarity between the treatment and the matched control firms. Similar analyses on the long-term trends of mean firm size and aggregate employment did not detect major differences either.

Figure 4 *Development of the aggregate wage bill in the target region firms, comparison region firms and matched (5 nearest neighbours) firms.*



Notes: Single plant firms that existed in the end of 2001. Comparison region figures are weighted to correspond to the number of firms in the target region at 2001, i. e. weight = number of target firms / number of comparison firms.

6.2 Employment and wage sum responses to the regional payroll-tax experiment

The main purpose of the payroll tax exemption was to promote employment in the target region. Therefore, also our main outcome measure is the change in the absolute number of employees in a firm. We prefer absolute changes to relative changes because this way employment changes add up to the total effect of the experiment and no weighting is necessary. However, the qualitative results were similar when we use changes in log employment as an outcome measure and weight the estimates by the firm size in 2001.

To reduce noise in employment numbers, we exclude the workers who receive only ancillary income from the firm and concentrate on the employees in their principal employment. Even this measure is naturally imperfect because it does not capture the variation in working hours.

Our main findings on employment effects are reported in Table 4. The first two columns report the average changes in employment in the treatment and control groups. The third column labeled "Treated-Controls" is our estimate for the program effect. In each case we first report annual changes. In the lower section

of the tables, we also calculated two-year changes just before experiment 2000–2002 and after the start of the experiment 2002–2004.

The first observation to note from Table 4 is a strong employment growth before 2001 and a strong decrease after 2001 that occurs in both the treatment and the control groups. For example, between 2000 and 2001, employment grew by 0.57 persons in an average treatment group firm. Between 2001 and employment decreased on average by 0.37 persons in the same firms. This pattern is largely due to the entry and exit of firms. Our sample consists of firms that existed in the end of 2001. The firms that exit before the end of 2001, or enter after 2001, are not included in data. On the other hand, exits after 2001 contribute to the average growth rate with large negative changes, and firms that enter before 2001 with large positive changes.

A more important observation from Table 4 is that employment growth has been rather similar in the treatment and the control groups. The differences in the growth rates reported in the third column are in most cases smaller than the standard error of the estimate, and in no case anywhere close to being statistically significant. According to these results the payroll-tax experiment has not had a significant effect on employment in the target region. In addition to statistical significance it is interesting to assess the economic significance of the point estimates. According to Table 4 the two-year change in employment after the start of the experiment (2002–2004) was on average 0.103 persons larger in the treatment group. Given that there are 1430 firms in the treatment group the total employment effect of the tax cut amounts to 147 new jobs or 1.3 percent increase in employment.

Table 4 *Effect of payroll-tax cut on employment*

	Treated	Controls	Treated-Controls	Std. Error
Change in average number of employees				
2000–2001	0.565	0.550	0.016	0.124
2001–2002	-0.372	-0.289	-0.083	0.131
2002–2003	0.003	0.017	-0.014	0.160
2003–2004	0.204	0.092	0.111	0.160
2000–2002	0.200	0.261	-0.062	0.159
2002–2004	0.218	0.115	0.103	0.219

Notes: The estimates in tables 4, 5 and 6 are (our favoured) five nearest neighbours estimates, estimated using GenMatch procedure as described in Section 4. Standard errors are robust to heteroskedastic treatment effect.

As noted before, tax data is not ideal for measuring the changes in employment. On the other hand, any changes in the wage bill (average hourly wage × sum of hours) that form the tax base should be accurately reported. In Table 5 we calculate the effect of the payroll-tax cut on the wage bill in the target and control firms. Now the estimates have mostly the "right" sign indicating stronger wage bill growth in the treatment group after the start of the experiment in 2003. The wage bill increase was 1125 euros larger in the treatment group in the first year after the experiment. There was a slight difference also in the second year so that the two-year increase in wage bill was 1728 euros (about 2.7 percent of an average target area firm wage bill) larger in the treated firms. Also these estimates are far from being statistically significant.

Table 5 Effect of payroll-tax cut on wage bill

	Treated	Controls	Treated-Controls	Std. Error
Average change in wage bill, €				
2000–2001	5 328	4 689	639	850
2001–2002	1 026	1 661	-635	945
2002–2003	2 263	1 137	1 125	1 597
2003–2004	1 666	1 063	603	1 222
2000–2002	6 354	6 350	4	1 276
2002–2004	3 929	2 201	1 728	2 142

Notes: The estimates in tables 4, 5 and 6 are (our favoured) five nearest neighbours estimates, estimated using GenMatch procedure as described in Section 4. Standard errors are robust to heteroskedastic treatment effect.

6.3 The effects by firm type

One could argue that the effect of payroll tax cut might differ across firms. For example, firms paying below average wages may be more responsive to wage costs if the own price demand elasticity of low-skill workers is higher than that of high-skill workers. There could also be different effects in the small and in the large firms. At least the effect is likely to be smaller in the largest firms that paid more than the deductible maximum of 30 000 euros in payroll-taxes. For these firms payroll tax cut is a lump-sum reduction in taxes and marginal changes in employment should not be affected by the tax rate. Finally, the size of the payroll tax cut depends on the pre-experiment tax-bracket and one might expect larger effects in the firms that face larger payroll tax reductions.

To examine these issues we first split the sample into quartiles defined according to the average wage in the firm and calculated the effects separately in each quartile. We then calculated the effects of the payroll tax cut separately for the

firms that paid less than 25 000 euros in payroll taxes in 2001 and that hence were well below the maximum tax deduction. Finally, we calculated the effects for the firms that were in the lowest payroll tax bracket. (The number of firms in the higher brackets was too small for meaningful calculations).

Table 6 reports the results of these experiments. No clear patterns appear. The effect of the payroll tax cut on employment seems to be highest in the 2nd wage quartile. The effect seems also to be higher than full sample average in the small firms that are in the lowest payroll tax bracket and in the firms that pay less that 25 000 euros in payroll taxes. The effects on the wage bill change appear rather similar though now the largest positive effects appear in the 3rd wage quartile. Due to large standard errors associated with all sub-sample estimates not much can be concluded from these numbers.

Table 6 Effect of payroll tax cut by firm type

	Treatment control difference in			
	Employment change 2000 – 02	Employment change 2002 – 04	Wage bill change 2000 – 02	Wage bill change 2002 – 04
Full sample	-0.062 (0.159)	0.103 (0.219)	4 (1 276)	1 728 (2 142)
By wage quartile				
1st (lowest)	-0.027 (0.139)	-0.210 (0.133)	-2 086 (604)	-281 (612)
2nd	-0.119 (0.173)	0.724 (0.204)	218 (1 122)	2 666 (1 280)
3rd	-0.264 (0.273)	0.457 (0.272)	1 124 (2 083)	9217 (3 226)
4th (highest)	-0.245 (0.177)	0.000 (0.296)	55 (1 999)	356 (4 826)
Firms in lowest payroll tax bracket	-0.122 (0.121)	0.160 (0.159)	66 (881)	1 264 (1 274)
Firms paying less than 25 000 € in payroll taxes	-0.122 (0.134)	0.026 (0.175)	559 (138)	-455 (1 775)

6.4 The effect on wages

To have a closer look at the incidence of the payroll tax cut we examined its effect on hourly wages. As noted before wage data is available only for the subset of (large) firms that belong to one of the two employer organizations. These two organizations have slightly different surveys and different wage concepts. The manufacturing sector data is also divided into the white-collar and the blue-collar worker files according to whether the employees receive hourly wages or monthly salaries. To avoid the need of ad hoc adjustments for different

measurements, we report also these estimates separately. For the service sector workers and for blue-collar workers in manufacturing we have data for the period from 2000 to 2004, for the white-collar workers in manufacturing only for 2001–2004.

While the firm is a natural unit of observation when measuring changes in employment, it is more straightforward to use individual wages to estimate average wage growth. Our wage equation estimates are reported in Table 7. In each case we create a measure that accounts for the variation in working hours. For the workers that receive monthly salaries we divide monthly salary by usual hours. For workers that are paid by hour we divide total wages during the last quarter of the year by total hours during the same period. We estimate the wage equations using all wage components (including various bonuses). To account for unobserved individual-level variation in wages we use data for the employees who appear in the data in the two consecutive years and use change in real log wage as a dependent variable.

All wage equations include the usual control variables: age, education and gender. We also include an indicator for supervisory or trainee status when available, and add a full set of two-digit occupational dummies in the wage equations. The equations include year fixed-effects as well as a fixed-effect for being located in the target region. The effect of the payroll-tax cut is identified from the interaction between year 2003 and target region indicators. The coefficient of this interaction can be interpreted as the difference in wage growth rate between the employees in the target and control regions due to the start of the experiment. Note that the interaction between year 2004 and the target region should be zero unless wage adjustments involve long lags since there were no changes in payroll taxes between 2003 and 2004.

Table 7 Wage effects

	Service sector		Manufacturing, salaried		Manufacturing, blue-collar	
Year 2002	-0.005	-0.006			0.008	-0.002
	(0.006)	(0.005)	n.a.	n.a.	(0.025)	(0.023)
Year 2003	-0.009	-0.010	-0.012	-0.012	-0.040	-0.046
	(0.004)	(0.004)	(0.013)	(0.012)	(0.024)	(0.024)
Year 2004	-0.009	-0.011	-0.012	-0.011	0.013	0.007
	(0.005)	(0.004)	(0.015)	(0.014)	(0.022)	(0.019)
Target region	-0.003	-0.007	-0.011	-0.004	-0.000	-0.041
	(0.007)	(0.004)	(0.010)	(0.004)	(0.020)	(0.013)
Target region × 2002	-0.001				-0.073	
	(0.010)	—	n.a.	n.a.	(0.029)	—
Target region × 2003	0.016	0.020	0.015	0.008	0.025	0.066
(Treatment effect)	(0.010)	(0.010)	(0.011)	(0.006)	(0.028)	(0.028)
Target region × 2004	-0.010		0.014		-0.047	
	(0.010)	—	(0.015)	—	(0.029)	—
R2	0.028	0.027	0.070	0.070	0.042	0.040
N obs.	9 972	9 972	2 493	2 493	9 721	9 721
N indiv. t-region	746	746	108	108	408	408
N indiv. c-region	3 134	3 134	1 028	1 028	3 133	3 133
N firms t-region	81	81	8	8	11	11
N firms c-region	255	255	39	39	45	45

Notes: The dependent variable in all regressions is the change in log hourly wages including overtime, benefits (taxable value), and provision payments. All equations include gender, age, age squared, dummies for occupations (67 for manufacturing 41 for service sector). Service sector regression has additional dummy variables for trainees, supervisors, and managers. Both manufacturing sector regressions have controls for education level. Robust standard errors are calculated taking into account clustering by firm. From manufacturing sector we have excluded one large target region firm that shut down during the observation period. n.a. = not available due to lack of data

According to results in Column 1 of Table 7, service sector wage growth seems to have been very similar in the target and control regions before the experiment started. In 2003, when the payroll taxes were cut, wages grew 1.6 percent faster in the target region though the estimate is not statistically different from zero. The point estimates also suggest that wage growth was slightly slower in the target region in 2004 but also these estimates are insignificant.

The specification including all interaction terms reported in Column 1 effectively compares the differences in wage growth between the reform year 2003 and the base year 2001. In Column 2 we report results from a specification that restricts all the other interactions except the interaction between 2003 and the target group to zero, effectively comparing wage changes in the reform year to all other years. The estimate is now slightly higher and statistically significant indicating that the tax exemption led to 2 percent faster wage growth among the employees in the

treatment group. This result is robust to small changes in the model specification such as restricting the sample to occupations that are present both in target and control groups, measuring occupations at three-digit level or excluding bonuses from the wage measure.

In the manufacturing sector the number of target group firms is smaller and the results are sensitive to whether one large firm that closed down during the period is included in the data or not. The estimates are also generally less robust to small changes in specification. In fact, the largest wage changes in manufacturing – such as the relative decline in wages in 2002 – seem to be unrelated to the reform.

Above, we estimated all wage equations at the individual-level. This may be problematic since changes in the large firms have a large weight in the estimates. If there are firm-specific shocks, the results may be driven by the shocks that occur in some large firms. To reduce the weight of these large firms we experimented with re-weighting the data so that each firm gets the same weight. Except for the blue-collar workers (where one large firm dominated the results), this re-weighting had only a minor effect. In particular, the result that the service sector wages grew slightly faster in the target region was robust to re-weighting.

7. Concluding comments

Well designed policy experiments may provide valuable information for the policy makers on the effects of taxation on wages and employment. In an ideal case, estimates based on regional experiments are more reliable than estimates based on cross-country comparisons or time-series data. Estimates from these experiments could then be used for cost-benefit analysis and as a basis for future tax policy. The main problem in small scale experiments tends to be a small number of observations. Measurements of the employment changes of firms are noisy and pinning down the effects of reasonably small changes in payroll taxes would require a large experiment.

The Finnish payroll tax experiment reduced payroll taxes by 4.1 percent, on average. If the estimates for the sub-sample of firms for which wage data is available can be generalized to all firms, about half of the effect of the payroll tax reduction on the labor costs was offset by faster wage growth in the firms that were eligible for the payroll tax cut. The remaining two percent decrease in the labor costs did not have a significant effect on employment, but the estimates are not very precise due to small sample size. Still, our point estimates of tax incidence are somewhat different than earlier results by Gruber (1994, 1997) and Johansen and Klette (1998) according to which reductions in payroll taxes are almost entirely shifted to wages. These studies imply that labor supply is less elastic than labor demand while according to our estimates the demand and supply elasticities are roughly equal. According to our point estimates that the tax cut increased employment by 1.3 percent indicating that labor demand elasticity would be around 0.6, well within the range of earlier estimates. Unfortunately, the confidence bands around this estimate are too wide to give much guidance to future tax policy.

Targeting the cut in the payroll-taxes to narrowly defined regions, and the temporary nature of the tax cut, naturally limits the extent to which the results can be generalized to the potential effects of permanently reducing payroll-taxes in the whole country. First, the payroll-tax experiment was financed by increasing payroll-taxes in the rest of the country. In a national scheme, the budgetary cost would need to be financed by raising other taxes. Second, a regional experiment may have substitution effects if the firms reallocate labor to the target region from the rest of the country. This might be beneficial in the sense that a partial motivation behind the regional payroll-tax cut was to boost employment in the disadvantaged regions. However, this limits the usefulness of the results from the experiment in predicting the effects from a national program. Third, also the incidence of the tax cut may be different in a regional program since the union contracts are negotiated at the national level. Any nation-wide changes in the payroll-taxes may have an impact on the outcome of these negotiations while a regional program that affects only a small share of

employers has little weight in the national bargaining. Finally, a temporary program is likely to create smaller employment effects than a permanent reduction of the payroll-taxes. Three years may not be a sufficiently long period for the firms to adjust their demand for labor to a relatively small change in the labor costs.

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Appendix

The datasets used

Here are short descriptions of the three main datasets used in the study, namely the employer's annual notification data from the Finnish Tax Administration that was used for estimating employment outcomes of the experiment, the Register of Enterprises and Establishments from Statistics Finland that was used (alongside the tax data) to form the target and comparison groups, and the individual wage data from the Confederation of Finnish Industry and Employers (TT) and Employers Federation of the Service Industries (PT) that was used to estimate the wage effects.

The register data on enterprises and establishments

Statistics Finland maintains the Register of Enterprises and Establishments. It covers all enterprises, corporations and self-employed persons that are liable to pay value added tax or have paid employees. Most of the data are based on administrative registers. To complement information in the administrative registers Statistics Finland conducts direct inquiries to enterprises. The inquiries request information on the enterprise's industry, employment, turnover and establishment structure. Inquiries are sent annually to most multi-establishment enterprises and single-establishment enterprises with over 20 employees. Other enterprises receive inquiries less often on rotation basis. New enterprises receive the inquiry soon after they start operating.

Tax administration data on employment and wages

Data on employment and earnings are based on the employer's annual notification from the Finnish Tax Administration. The annual notification includes all wages and salaries paid during the calendar year and the number of wage recipients. Data covers all firms with paid employees.

The tax data also has an indicator of whether a firm has applied for payroll tax exemption and the date it has submitted its starting declaration. This information was used to ascertain that most the eligible firms promptly participated in the experiment. Tax data was available for all firms in our initial sample and was merged to it with unique firm identifier.

Wage data from the employer organisations

Wage data is based on the registers of two large employer organisations: Confederation of Finnish Industry and Employers (TT) and Employers Federation of the Service Industries (PT). Most large employers are members of one of these organisations and they cover approximately 60 percent of private sector employment. The data is collected mainly in order to monitor wage growth in the manufacturing and service sectors. Official statistics on wage growth in these sectors are mainly based on these data. The data also serve as an information base for collective wage bargaining between unions and employer organizations.

TT surveys its member firms each December. The wage records are collected separately for manual (hourly paid, blue-collar) workers and salaried (monthly paid, white-collar) workers. PT's survey is conducted in October. All surveys contain detailed information on monthly or hourly wages and regular weekly hours of all workers in the firm excluding only top management and the owners of the firm. In addition, there are a number of background variables on the employees including age, sex, tenure, schooling, and detailed occupation and industry. The wage information originates directly from the payroll records of the companies and the data can be considered highly accurate - recall or rounding errors typically occurring in surveys of individual workers should be non-existent.

We could not use our original sample of firms from the Statistics Finland to identify the workers in target and comparison firms due to different establishment identifiers used in the wage data. However the data includes a location code for each respondent (typically establishment) and selected records in all establishments that had workers solely in the target area or solely in the comparison area. Descriptive statistics for the service sector and manufacturing are reported in the appendix table 2.

Appendix Table 1 From all firm register records at 2001 to single plant firms with positive employment, wage sum and turnover

	Target region			Comparison region		
Eligible industry, number of firms in SF 2001 firm table		2809			7543	
	Median	Mean	Sum	Median	Mean	Sum
Employment, TA	1	5.74	16 121	1	7.36	55 480
Employment, SF	1	2.62	7363	1	3.70	27 904
Wage sum, TA	2876	49 243	138.3 M	3 221	75 681	570.9 M
Turnover	58 200	382 674	1074.9 M	57 964	484 936	3658.4 M
AND number of plants = 1		2732			7258	
Employment, TA	1	4.51	12 318	1	5.39	39 111
Employment, SF	1	2.00	5469	1	2.52	18 460
Wage sum, TA	2364	34 144	93.2 M	2321	48 476	351.8 M
Turnover	56 060	274 077	748.8 M	55 504	294 800	2140.0 M
AND turnover > 0		2692			7200	
Employment, TA	1	4.58	12 318	1	5.44	39 132
Employment, SF	1	2.03	5465	1	2.56	18 444
Wage sum, TA	2645	34 652	93.3 M	2455	48 867	351.8 M
Turnover	57 348	278 150	748.8 M	56 229	297 211	2140.2 M
AND TA wage sum > 0 & TA employment > 0		1592			4265	
Employment, TA	3	7.74	12 318	4	9.17	39 132
Employment, SF	1	3.62	845	2	3.89	16 605
Wage sum, TA	18 486	58 252	92.7 M	21 550	81 380	347.3 M
Turnover	112 368	433 103	689.5 M	111 959	455 200	1942.3 M

Notes: SF = Statistics Finland supplied man-year estimates, TA = Tax Administration supplied numbers on the total number of employees on payroll over a one year period.

Appendix Table 2 Descriptive statistics for the employer union datasets used in wage regressions. Means and standard errors in (parenthesis) are for the year 2001.

Data	Service sector		Manufacturing blue-collar		Manufacturing white-collar	
Region	Target	Control	Target	Control	Target	Control
Hourly wage	10.86	11.25	13.42	11.59	16.15	14.29
	(0.113)	(0.067)	(0.192)	(0.063)	(0.371)	(0.163)
Share of women	0.75	0.75	0.50	0.21	0.38	0.37
	(0.018)	(0.008)	(0.018)	(0.007)	(0.028)	(0.017)
Age	38.38	41.73	40.11	39.06	40.06	42.87
	(0.392)	(0.186)	(0.388)	(0.201)	(0.576)	(0.326)
Tenure	5.77	11.19	11.36	11.10	11.04	13.93
	(0.299)	(0.200)	(0.357)	(0.194)	(0.598)	(0.373)
Years of education	-	-	10.56	10.57	12.73	12.46
			(0.040)	(0.018)	(0.100)	(0.063)
Manager	0.02	0.01	-	-	0.47	0.23
	(0.006)	(0.002)			(0.029)	(0.015)
Working time, hours/week	37.04	36.80	39.95	39.70	37.88	37.96
	(0.063)	(0.039)	(0.037)	(0.039)	(0.135)	(0.095)
Firm size	15.84	53.36	271.31	211.02	285.94	180.41
	(0.429)	(1.165)	(5.282)	(2.531)	(9.193)	(5.492)
N obs	594	2726	754	2999	290	830

Appendix Table 3 Covariate balancing with different matching methods

	Bias before matching	Bias after matching (reduction, %)				
Variables used in matching, in log's		p-score, 1-nn	p-score, 5-nn	p-score, kernel	GenMatch, 1-nn	GenMatch, 5-nn
Employment 2001, SF	-8.9	1.5 (82.9)	2.6 (70.3)	1.8 (79.4)	0.3 (97.2)	1.7 (81.3)
Employment 2000, SF	-7.0	2.8 (59.2)	2.2 (68.5)	1.6 (76.5)	-1.1 (84.3)	0.4 (94.3)
Employment 1999, SF	-6.1	2.2 (63.8)	2.2 (64.1)	1.8 (70.3)	-0.8 (86.7)	-0.1 (97.8)
Employment 2001, TA	-9.5	2.4 (74.7)	1.7 (82.5)	1.5 (84.7)	3.3 (65.7)	5.0 (47.5)
Employment 2000, TA	-8.5	4.2 (50.6)	2.0 (76.2)	1.6 (80.7)	-0.2 (98.1)	2.0 (76.1)
Wage sum 2001, TA	-4.8	1.4 (71.2)	1.9 (60.5)	1.5 (68.1)	-1.3 (76.8)	0.3 (93.3)
Wage sum 2000, TA	-2.3	2.9 (-26.7)	2.7 (-16.9)	2.2 (1.8)	-2.3 (2.2)	-0.7 (68.6)
Wage sum 1999, SF	-0.4	1.9 (-414.8)	1.7 (-353.6)	1.4 (-289.3)	-0.5 (-24.8)	-0.8 (-121.0)
Turnover 2001	4.6	1.4 (70.1)	4.5 (1.2)	2.5 (45.9)	-0.3 (94.4)	-0.7 (85.3)
Turnover 2000	6.5	3.6 (44.5)	2.9 (54.7)	1.2 (81.4)	-0.6 (90.8)	-1.0 (84.9)
Turnover 1999	5.5	2.0 (63.6)	2.7 (51.6)	1.1 (79.4)	-1.3 (72.1)	-0.5 (90.2)
Payroll tax bracket I	-3.6	-2.5 (31.3)	-1.1 (13.1)	-1.9 (47.5)	-0.4 (90.1)	-4.1 (-11.9)
Payroll tax bracket II	0.2	0.6 (-217.7)	0.9 (-335.3)	1.5 (-619.9)	0.0 (100.0)	5.0 (-2360.8)
Payroll tax bracket III	3.8	2.2 (43.7)	0.3 (91.0)	0.9 (76.8)	0.0 (100.0)	1.3 (67.4)

Notes: The standardised bias is the difference of the sample means in the treated and non-treated (full or matched) sub-samples as a percentage of the square root of the average of the sample variances in the treated and non-treated groups (Rosenbaum and Rubin, 1985).
SF = Employment figure supplied by Statistics Finland, estimated man-years.
TA = Employment figure supplied by Tax Administration, # of employees on payroll over a year.
Payroll tax bracket I = tax reduction 2.95, II = tax reduction 5.15, III = tax reduction 6.05 percent of the wage bill.

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