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EMPLOYMENT EFFECTS OF A PAYROLL-TAX CUT: EVIDENCE FROM A REGIONAL TAX EXEMPTION 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 in Northern Finland for three years. We match each firm in the target region with a similar firm in the control 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 payroll taxes led to somewhat faster wage growth in the target region. The increase in wages offset roughly half of the impact of the payroll-tax cut on the labour costs. The remaining labour cost reduction had no significant effects on employment.

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

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

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 työvoimakustannusten laskusta – jäljelle jäänyt työvoimakustannusten lasku ei johtanut työllisyyden kasvuun kokeilualueen yrityksissä.

Asiasanat: työnantajan sivukulut, sotu-maksukokeilu, työn kysyntä, verotuksen kohtaanto, propensity score matching

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

Contents

1 Introduction

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

Past evidence on the incidence and employment effects of payroll-tax changes is mixed. Studies that rely on time-series or cross-country 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 time-series studies, there may be simultaneous changes in other variables that affect wages and employment. 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. Therefore, 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. Prime examples of such studies include Bohm and Lind (1993), who evaluate the employment effects of regional wage subsidies in Northern Sweden, Gruber (1994), who evaluates the effects of mandated maternity benefits in the US, Gruber (1997), who examines the changes in the mandatory pension contributions in Chile, and Johansen and Klette (1998), who examine the effects of regional differences in payroll taxes in Norway. These studies typically find that the changes in payroll taxes are almost completely shifted into wages with little effect on labour costs or employment.

In this paper we evaluate the employment and wage effects of a regional experiment in 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 experiment, these employer contributions varied between 2.95 and 6 per cent of the wage bill, depending on the capital intensity and size of the firm. From January 1st 2003, all private employers in the 20 target municipalities located in 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 Northern Finland where over 90 per cent of the eligible firms are located.

A regionally targeted programme has several benefits compared with an across the board cut in taxes. Perhaps the main benefit for policy makers is that the effects of a regional programme are substantially easier to evaluate. The employment change in the target region can be compared with similar regions that are not affected by the tax cut. If the target and control 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 off 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 wages. Our main results are based on a comparison of employment changes in the target-region firms with the employment changes in the firms located in a control region that is as similar as possible in terms of the unemployment rate, the industry structure and the composition of the labour force. We end up comparing the target region firms in Northern Finland with the firms located in other high unemployment areas in Northern and Eastern Finland.

Comparison of the employment changes across regions still creates problems if the regions are not quite similar in all the 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 similar in the observed pre-treatment characteristics.

2 The experiment

Payroll taxes in Finland consist of contributions to the Earnings-related 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]. The total average payroll-tax rate was 23.86 % in 2002 (Statistical Yearbook of the Social Insurance Institution, Finland).

In March 2002, the Finnish government agreed to a temporary removal of employer contributions to the National Pension Scheme 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 programme was designed as an experiment with a stated aim to evaluate the effect of a cut in the payroll taxes on employment. The payroll-tax exemption continued for three years from January 1st 2003 to December 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 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 the press during the spring of 2002. It is, therefore, possible that firms who anticipated the tax exemption could have already altered their employment before the start of the programme in January 2003. However, it is unlikely that any employment effects could have occurred before March 2002, since the nature of the programme was very much an open question until then.

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[1] In 2002, 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 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 the wage bill for wages up to 840 940 euros and 2.7 % of the wages exceeding this threshold. The earnings-related pension scheme has a relatively complicated fee structure. In 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 %.

All private employers and state-owned firms 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 EU, agriculture, fishing, and the transport industries were excluded from the experiment. There is an important restriction that municipal employers are 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. The experiment was financed by temporarily raising the National Health Insurance contributions for 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 these very 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 also involving nine other municipalities in Northern Finland. After the working group submitted 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. On the other hand, the working group would also have granted a tax exemption to the municipal employers. The final proposal was a compromise that excluded all public sector employers with the exception of state-owned enterprises.

Applying for the tax exemption was made easy for participating employers. The employers were only required to file an initial declaration with 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 per cent of the eligible employers with at least five employees and 75 per cent of the firms with 2–4 employees had filed an initial declaration by December 2003.

Most firms that applied for the tax exemption were very small. The median firm had only four employees. Only ten per cent of the firms had more than twenty, and 2.5 per cent more than fifty employees. In terms of employment and the

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. The reduction in payroll taxes 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	1836	382 585
5–9	369	2451	686 321
10–19	237	3202	931 020
20–49	139	4153	1 157 750
50–99	37	1544	600 498
101–250	10	1578	289 497
> 250	3	911	63 555
Total	2334	17 099	4 227 256

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

3 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 the 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 demanded that the firm had only one plant, so that its location and hence the eligibility for the tax exemption could be determined accurately. We found 1592 such firms in the target area and 4265 firms in the control area[2].

The main disadvantage with the establishment register data is that employment numbers are 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 the employer's annual notification that all employers are required to submit to the regional tax office. The annual notification includes all wages and salaries paid during the calendar year. The payments are itemised by employee, and the summary form contains the number of recipient itemisations. This number equals the number of employees that have received some wages or salaries from the firm during the year. Naturally, the number of itemisations is only a rough measure of the average employment in the firm. On the other hand, the total wage bill (i.e. the product of hours worked and the average hourly wage) is accurately reported.

The tax data therefore provides a reliable estimate of whether the payroll-tax deduction had an impact on the 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 of 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 organisations. In Finland there are two large employer organisations: the Confederation of Finnish

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

Industry and Employers (TT) and the Employers' Federation of the Service Industries (PT)[3]. Most large employers are members of one of these organisations. Together, these organisations cover approximately 60 per cent of private sector employment.

Both TT and PT wage surveys contain individual data on all workers in all their member firms. PT gathers data from the October and TT from the December of each year. 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.

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

4 Empirical strategy

Our estimates are based on differences in employment growth rates between the target region firms and a control group. We created the control group by a two-stage procedure. We first selected the "counties" (NUTS4-level sub-regional units) that were most comparable with 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. We excluded from the control region other non-target regions in Lapland because they were administrative centres with an above-average education level (Rovaniemi) or major manufacturing regions (Kemi-Tornio). Instead, we included areas from Eastern Finland just south of the target region (see map in Figure 1). Our judgement 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, a reasonably similar industry distribution and a similar population structure. In all these dimensions the target and control regions deviate substantially from the national average. To minimize the temptation to re-define the control region ex-post, we fixed the design and published the setup before any data on employment effects became available in January 2004 (Korkeamäki & Uusitalo 2004).

Figure 1 Target and comparison regions

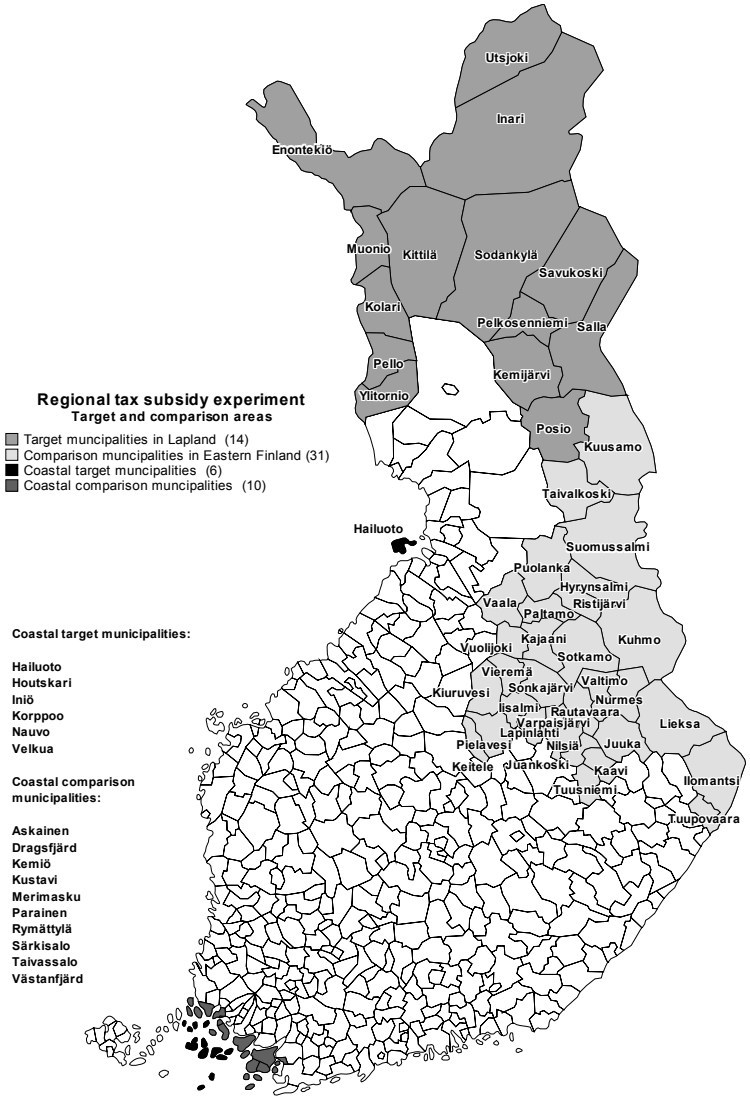


Table 2 Comparison of target and control regions

	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
Per cent Swedish	0.18	0.07	5.60
Per cent 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 educ, %	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	1782	1498	706
Tax revenue, € / person	2085	2022	2715

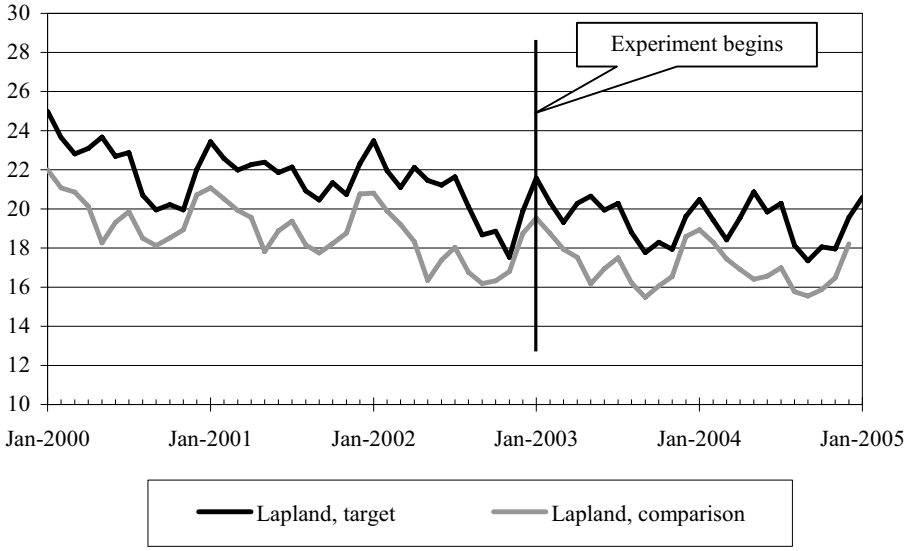
Notes: 1) inhabitants / km^2, 2) Indicates the proportion of the population living in built-up areas (%), 3) Persons aged 15 or over who have a degree from a senior secondary school, vocational or professional educational institution, or from a university.

In addition to having a similar population structure and a similar industry composition, the aggregate economic development in the target and the comparison regions was remarkably similar before the experiment. For example, the unemployment rate has been around 20 per cent in both regions. The unemployment rate in the comparison region has been slightly lower but the downward trend in the unemployment rate is very similar to that in the target region (Figure 2).

The comparison of unemployment rates in Figure 2 also indicates that the experiment did not have a major impact on the unemployment rate. 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 regions represent only some 1.3 % of the Finnish population, the sample sizes in

national surveys, such as the Labour 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



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 at 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 Labour Force Survey and then applied matching methods to create treatment and comparison groups 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 (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. This logit estimation gives 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 neighbour (or neighbours) 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 Mebane1998) 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 programme was made public. We then compare the treated and control firms for both the pre-programme period from December 2001 to December 2002 and for the programme period 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 control area; the firms in ten municipalities in Kainuu County that belong to our control 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 the 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 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 and wage changes in the target and comparison regions. We conclude this section with some further results and robustness checks.

5.1 Covariate balancing

Table 3 reports the means of the variables used in matching separately in the target and control regions, and in the matched treatment and control groups. In the column farthest right, 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 with the national average reveals that both regions differ from the national averages and that the treatment region is a substantially closer comparison with the treatment region than the whole country.

Table 3 Covariate balancing

Means, all variables in log's	Target firms	Matched targets	Matched controls	Control Group	National average
Employment 2001, SF	1.06	1.06	1.06	1.12	1.26
Employment 2000, SF	1.00	1.02	1.03	1.06	1.27
Employment 1999, SF	0.92	0.94	0.95	0.97	1.25
Employment 2001, TA	1.66	1.66	1.64	1.75	n.a.
Employment 2000, TA	1.49	1.52	1.52	1.58	n.a.
Wage sum 2001, TA	9.65	9.67	9.70	9.74	10.16
Wage sum 2000, TA	8.62	8.79	8.84	8.71	10.13
Wage sum 1999, SF	7.57	7.80	7.81	7.59	10.05
Turnover 2001	11.10	11.21	11.20	10.95	12.16
Turnover 2000	10.36	10.56	10.54	10.08	12.11
Turnover 1999	9.50	9.72	9.76	9.23	12.04
Industry distribution of firms (percentage 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
N Firms 2001	1592	1430	1430	4265	136 434
N Employees 2001, TA	12 318	11 034	10 190	39 111	1 318 654

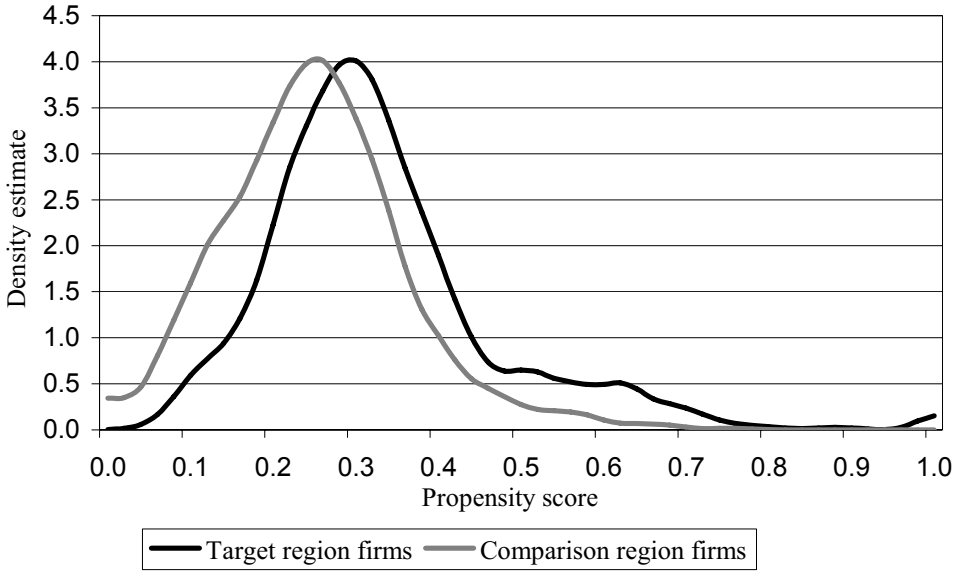
Notes: For ease of comparison, we calculated the control group mean displayed in the table using nearest neighbour matching.

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.

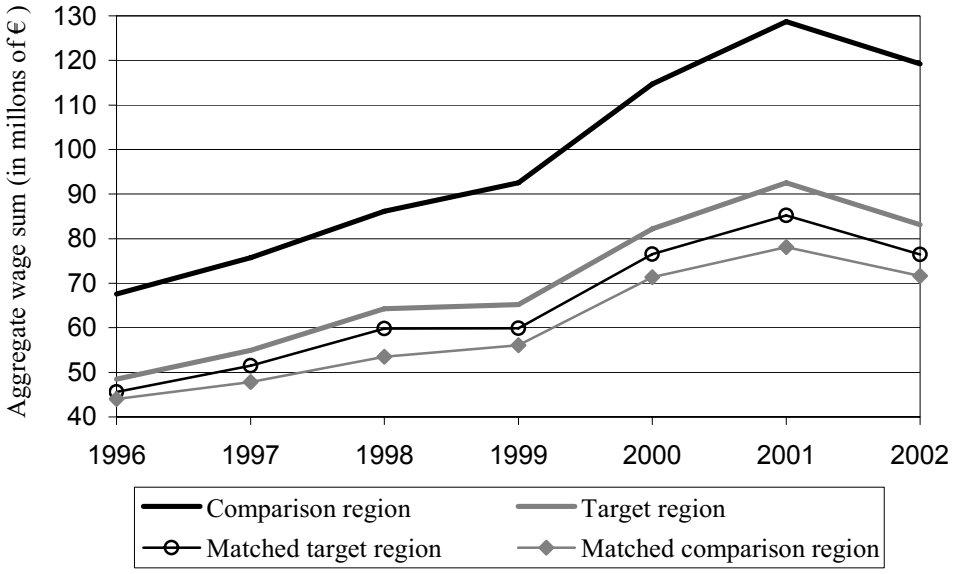
Given the similarity of the target and control regions, there are few strong predictors in the logit-model that is used to explain whether the firm is located in the target region. This is also reflected in the distribution of the propensity score that is rather similar in the target and comparison regions. 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



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 control 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 the years 1999–2001 for matching, 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 of 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



Notes: Single plant firms that existed at 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.

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

Our main response variable is the change in the absolute number of employees in a firm. We prefer absolute changes to relative changes because in 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 measured outcomes using relative employment growth.

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 mean growth rates in the treatment and control groups. The third column labelled "Treated–Controls" is our estimate for the programme effect. In each case we first report annual changes. In the lower section of the tables, we also calculated two-year changes just before the experiment of 2000–2002 and after the start of the experiment of 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 has grown by

0.57 persons in an average treatment group firm. Between 2001 and employment decreased on average by 0.36 persons in the same firms. This pattern is largely due to the entry and exit of firms. Our target population is the firms that existed at the end of 2001. The firms that exit before 2001, or enter after 2001, are not included in the 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, 0.067 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 96 new jobs or 0.8 per cent growth 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	0.570	0.537	0.033	0.122
2001–2002	-0.356	-0.204	-0.152	0.130
2002–2003	0.001	-0.006	0.008	0.147
2003–2004	0.198	0.148	0.050	0.164
2000–2002	0.221	0.328	-0.107	0.156
2002–2004	0.210	0.143	0.067	0.216

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 bootstrapped using 500 replications.

As noted before, tax data is not ideal for measuring the changes in employment. On the other hand, any changes in the wage bill that form the tax base should be accurately reported in the tax data. 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. Interestingly, the point estimate for the treatment effect is highest in the first year of the experiment when the wage bill grew, on average, 1324 euros more in the treated firms. In the second

year of the experiment the wage bill growth was very similar in the target and the control groups so that the two-year change was 1255 euros (about 2 per cent) larger in the treated firms. These estimates are also 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	5412	4714	698	831
2001–2002	1127	1880	-753	924
2002–2003	2278	955	1324	1343
2003–2004	1678	1747	-69	1263
2000–2002	6539	6594	-55	1298
2002–2004	3956	2701	1255	1994

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 bootstrapped using 500 replications.

5.3 The effects by firm type

One could argue that the effect of the 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 elasticity of low-skill workers is higher than that of high-skill workers. There could also be different effects in small and large firms. At least the effect is likely to be smaller in the firms that paid more than the deductible maximum of 30 000 euros in payroll taxes. For these firms the 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 naturally 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 split the sample into quartiles defined according to firm average wage and calculated the effects separately in each quartile. We also 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 also seems to be higher than the 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 2000 – 02
Full sample	-0.107 (0.156)	0.067 (0.216)	-55 (1298)	1255 (1994)
By wage quartile				
1st (lowest)	-0.026 (0.184)	-0.192 (0.223)	-1769 (709)	-508 (806)
2nd	-0.120 (0.271)	0.885 (0.388)	298 (1892)	3217 (2886)
3rd	-0.085 (0.318)	0.245 (0.401)	815 (2573)	9130 (4061)
4th (highest)	-0.052 (0.296)	-0.459 (0.601)	-228 (3201)	-4927 (10669)
Firms in lowest payroll tax bracket	-0.167 (0.136)	0.159 (0.182)	-104 (1062)	1354 (1395)
Firms paying less than 25 000 € in payroll taxes	-0.105 (0.156)	0.143 (0.256)	107 (1377)	1563 (3938)

5.4 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 organisations. For the service sector workers (PT) we have data for the period from 2001 to 2004, for manufacturing (TT) only for 2002–2004.

Since PT and TT wage data come from different surveys and the information content is slightly different it is natural to report the estimates separately. TT wage 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 to adjust for a different measurement, we also report these estimates separately.

While the firm is a natural unit of observation when measuring changes in employment, it is more natural to use individual wages to estimate average wage

growth. Our wage equation estimates are reported in Table 6. In each case we create a measure that accounts for the variation in working hours. In PT and TT white-collar worker data we divide monthly salary by the usual hours. For TT blue-collar data we divide the total wages during the last quarter by the total hours during the same period. We estimate the wage equations using all wage components (including various bonuses). We use data for all employees who appear in the data in the two consecutive years and use real log wage growth as a dependent variable.

All wage equations include the usual control variables: age, education, tenure and gender. We also include an indicator for supervisory or trainee status when available, and add a full set of occupational dummies in the wage equations. We also separately report estimates where we restrict the sample to those occupations that occur both in the target and the control region firms.

The estimates 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 the wage growth rate between the employees in the target and control regions due to the start of the experiment.

Table 7 Wage effects

	Service sector		Manufacturing, salaried		Manufacturing, blue-collar	
Model	ΔW2	ΔW2c	ΔW2	ΔW2c	ΔW2	ΔW2c
Year 2002	-0.006 (0.003)	-0.003 (0.003)	—	—	—	—
Year 2003	-0.010** (0.003)	-0.012** (0.003)	-0.013* (0.005)	-0.014** (0.006)	-0.050** (0.008)	-0.018 (0.011)
Year 2004	-0.011** (0.003)	-0.010** (0.003)	-0.013* (0.006)	0.016* (0.006)	0.004 (0.006)	0.016 (0.009)
Target region	-0.008 (0.005)	-0.008* (0.005)	-0.007 (0.009)	-0.008* (0.009)	-0.039* (0.016)	-0.014 (0.020)
Target region × 2002	-0.002 (0.008)	-0.004 (0.009)	—	—	—	—
Target region × 2003	0.016* (0.007)	0.018* (0.007)	0.014 (0.008)	0.015 (0.008)	0.057** (0.019)	0.025 (0.021)
Target region × 2004	-0.008 (0.007)	-0.010 (0.007)	0.012 (0.010)	0.014 (0.010)	-0.005 (0.017)	-0.016 (0.019)
R2	0.043	0.040	0.089	0.090	0.054	0.038
N obs.	9972	8631	2493	2341	7680	4312
N indiv. t-region	746	746	108	108	645	645
N indiv. c-region	3134	2696	1028	964	2655	1306
N firms t-region	81	81	8	8	10	10
N firms c-region	255	241	39	39	40	32
N occupations	178	93	100	85	173	63

Notes: Model ΔW2 for changes in log hourly wage including overtime, benefits (taxable value), and provision payments, model ΔW2c restricted to common support of occupations over regions. All equations include gender, age, age squared, years of education, tenure, tenure squared, dummy variables for trainees, supervisors, and managers and a dummy for each occupation. Standard errors that are robust to clustering within firms are in parentheses below the parameter estimates. From the manufacturing sector we have excluded one large target region firm that shut down during the observation period.

In the service sector, the 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 almost two per cent faster in the target region. This difference is statistically significant. The difference between the target and control regions is not significantly different in 2004, though the point estimates suggest that wage growth was slightly slower in the target region.

In the manufacturing sector the number of firms is smaller and the results are sensitive to whether a large firm that closed down during the period is included in the data or not. The estimates are also generally less precise and mostly insignificant, but the point estimates are of similar magnitude to those in the service sector.

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 re-estimated the models, giving each firm 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.

6 Concluding comments

Well designed policy experiments may provide valuable information on the effects of taxation on employment for the policy-makers. 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 the 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 per cent 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 labour costs was offset by faster wage growth in the target region. The remaining two per cent reduction in the labour costs did not have a significant effect on employment growth. The insignificant result is mainly due to small sample size. According to the point estimate the tax cut increased employment by 0.8 per cent indicating that labour demand elasticity would be around 0.4, 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 generalised 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 labour 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, the incidence of the tax cut may also be different in a regional programme since the wage increases are largely determined in national bargaining between unions and employer organisations. Any nation wide changes in the payroll taxes may have an impact on the outcome of these negotiations, while a regional programme that affects only a small share of employers has little weight in the national bargaining. Finally, a temporary programme is likely to create smaller employment effects than a permanent reduction in the payroll taxes. Three years may not be a sufficiently long period for firms to adjust their demand for labour to a relatively small change in the labour costs.

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Appendix

Changes in the Formation of the firm sample

Table 1 *From all firm register records 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.

Covariate balancing and average treatment effect estimates with different matching methods

Table 2 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 (96.7)	0.2 (97.6)
Employment 2000, SF	-7.0	2.8 (59.2)	2.2 (68.5)	1.6 (76.5)	-0.6 (90.9)	-0.6 (91.9)
Employment 1999, SF	-6.1	2.2 (63.8)	2.2 (64.1)	1.8 (70.3)	-0.7 (88.5)	-0.4 (93.3)
Employment 2001, TA	-9.5	2.4 (74.7)	1.7 (82.5)	1.5 (84.7)	2.88 (69.7)	4.5 (52.4)
Employment 2000, TA	-8.5	4.2 (50.6)	2.0 (76.2)	1.6 (80.7)	-0.1 (99.2)	1.7 (80.4)
Wage sum 2001, TA	-4.8	1.4 (71.2)	1.9 (60.5)	1.5 (68.1)	-1.9 (61.0)	-0.3 (94.4)
Wage sum 2000, TA	-2.3	2.9 (-26.7)	2.7 (-16.9)	2.2 (1.8)	-1.3 (41.3)	-0.5 (79.0)
Wage sum 1999, SF	-0.4	1.9 (-414.8)	1.7 (-353.6)	1.4 (-289.3)	-0.3 (-22.8)	-0.2 (46.5)
Turnover 2001	4.6	1.4 (70.1)	4.5 (1.2)	2.5 (45.9)	0.26 (94.3)	0.5 (88.4)
Turnover 2000	6.5	3.6 (44.5)	2.9 (54.7)	1.2 (81.4)	0.47 (92.7)	-0.7 (88.9)
Turnover 1999	5.5	2.0 (63.6)	2.7 (51.6)	1.1 (79.4)	-0.84 (84.8)	0.1 (98.7)

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 (formulae from 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.

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