Employment effects of a payroll tax cut – Evidence from a regional tax subsidy experiment

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Abstract

The Finnish government implemented a temporary exemption of employer social insurance contributions for the employers that are located in high unemployment areas in the Northern Finland. The payroll tax exemption was designed as an experiment that aimed to evaluate employment effects of a regional payroll tax reduction. As a result of the experiment payroll taxes were reduced by 3 – 6 percentage points for three years beginning in January 2003.

In this paper we evaluate the employment and wage effects of this regionally targeted payroll tax reduction. We compare the employment and wage changes in the target region to the employment and wage changes in a control region that has similar unemployment rate and similar industry structure than the target region. As finding exactly similar control regions is not possible, we adopt a matching procedure by choosing, for each firm in the target region, a matched pair from the control region. We perform propensity score matching and use the estimated propensities as balancing scores to create a control group of firms that is similar to the treatment group in all the observable pre-treatment characteristics. We then estimate the effect of the payroll tax reduction using difference-in-differences estimators, essentially comparing employment and wage changes between the matched pairs after the start of the experiment. We report results from both nearest neighbour and kernel matched comparison groups.

To enhance the transparency of the evaluation we have created the matched firm pairs from the plant database of Statistics Finland and designed and published the evaluation method before any data on employment or wage outcomes were available in January 2004. We will follow the employment change of the selected firms based on annual tax reports that will be available in March 2004. Detailed information on the wage responses will be added to the report in May 2004 based on the payroll data of the members of Employers Federation of the Service Industries.

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

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

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1 Introduction

Cutting payroll taxes has become a standard proposal to promote employment. For example, in Employment Outlook 2003 the OECD suggests targeted payroll tax cuts for the groups that are in the weakest position in the labour market. Typical target groups are low-wage workers, youth, disabled and long-term unemployed. Regional employment subsidies or tax reductions targeted in the high unemployment regions have also been used, for example, in Norway and Sweden, and from the beginning on 2003 in Northern Finland.

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

Past evidence on the incidence of payroll taxes is mixed. Studies that rely in time-series or cross-country variation in national payroll tax produce widely varying estimates of tax incidence. An important problem in such approaches is omitted variables bias. In time-series studies, there may be simultaneous changes in other variables that determine wages. In cross-country studies it is difficult to control for the differences in wage-setting institutions and other factors that may be correlated with the level of taxation and employment. More promising approach for evaluating the effects of payroll taxes is to examine changes in the cost of various mandated employer benefits that vary across firms over time or, for example, across the states in the US. Prime examples of such studies include Bohm and Lind (1993) who evaluate 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 the changes in pension contributions in Chile, and Johansen and Klette (1998) who examine the effects of regionally differentiated payroll taxes in Norway. These studies typically find that the changes in the payroll taxes are almost completely shifted into wages with little or no employment effects.

In this paper we evaluate the employment effects of a government program that abolished employer contributions to the national pension scheme and to the national health insurance system for three years in targeted high unemployment regions. Prior to the experiment, these employer contributions varied between 2.95 and 6 percent of wages, depending on the capital intensity and the size of the firm. From January 1st 2003, all private employers in 20 target municipalities are exempt from these social security contributions.

The payroll-tax cut was implemented in fourteen municipalities in Northern Finland and in six municipalities on islands along the western coast. 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 potential effects of implementing the policy nationwide. First, the payroll tax cuts are financed by increased 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, the regional experiment may have substitution effects with labour shifting to target region from the rest of the country. This might be beneficial in the sense that one motivation behind the payroll tax cut was to boost employment in the disadvantaged regions, but naturally 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 wages are largely determined by national bargaining. However, this point should not be over-emphasized: firm-level and individual bargaining over wages is important even in the centralized bargaining regime. During the period 1992 – 2000 wage drift (i.e. wage increases that exceed the union bargains) has been responsible for 38 % of the average wage growth. (Piekkola and Marjanen 2003). Finally, a temporary program 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 the firms to adjust their demand for labour. On the other hand, many employment contracts in the target region are far shorter than three years. For example, seasonal employment in the ski resorts makes up a significant fraction of employment in Lapland. It is also possible that the tax exemption will be continued after the experimental period.

Regionally targeted program has also several benefits compared to 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 control regions are truly similar, the inferences on the employment effects based on comparison on the employment (and wage) changes between the treatment and comparison regions provide much more reliable estimates on the effects of the payroll tax cut than time-series variation in the payroll-taxes ever could do.

In this paper we evaluate the effects of the payroll tax cuts by comparing the employment changes in the target region to the carefully selected control regions that are as similar as possible in terms of unemployment rates, net-migration, industry structure and the composition of the labour force. We end up comparing the target region in the Northern Finland to a group of municipalities in other high unemployment areas in the Northern and Eastern Finland. For the targeted municipalities on islands we select comparison group from other similar municipalities on the western coast.

Comparison of 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 the control regions. By finding each firm in the treatment region a twin firm with similar characteristics from the control region, and by including only these comparable firms in the control group, we can create a control group that is similar in the pre-treatment characteristics. We then evaluate the effects of the payroll tax cut by comparing firms that are similar in all observable characteristics. In practice, we create the comparison groups by propensity score matching. We use both nearest neighbour and kernel-matched comparison groups, and then calculate the effects of the tax cut by comparing the employment change in the target firms to the matched comparison group.

2 The experiment

Payroll taxes in Finland consist of 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 across the firm size. In 2002, for the private sector employers the contribution to the National Health Insurance was 1.69 % and to the Employment Accident Insurance 1.00 %. For calculating the National Pension Insurance contributions the firms are divided into three categories based on the depreciation and the fraction of depreciation of the total payroll. (The intention is to support labour intensive firms). The contribution rates in these categories were 1.35, 3.55 and 4.45. The Unemployment Insurance contributions are progressive, contribution rate being 0.7 % of wages for wages up to 840 940 euros and 2.7 % of the wages exceeding this threshold. The Employee's Pension Scheme has a relatively complicated fee structure. In large firms the contributions depend on the age of the employees and on the number of previous employees receiving early retirement benefits. Smaller firms pay a flat rate of 17.32 %. The average total 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. The removal of these contributions lowered payroll taxes, depending on the firm size, by, on average, 4.1 percentage points in the firms that were located in the four northernmost economic regions of Finland and in the firms located on municipalities on islands. 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 targeted municipalities.

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. Also private households were eligible if the work was done in the target municipalities. The maximum annual reduction was 30 000 euros. Due to restriction in the EU-legislation agriculture, fishing, and transport were excluded from the experiment. An important restriction is also that municipal employers are not eligible for the exemption[1].

The payroll-tax exemption will last for three years. It was implemented from the January 1st 2003 and will be in force up to December 2005. Possible extensions after the year 2005 will be discussed in conjunction of the government budget proposal for the year 2006 in March-April 2005. As the payroll-tax exemption may have anticipatory effects, it is useful to note that the regional tax exemption was first suggested by a working group that gave 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 estimated number of eligible firms in the government bill was 3 500 and the estimated annual budgetary cost of the payroll-tax exemption 8 million euros. The experiment was financed by temporarily raising the National Health Insurance contributions of the employers outside the target region by 0.014 percentage points.

All the target municipalities are located in high unemployment areas. However, the geographical borders of the target area are somewhat arbitrary. There are other regions outside the target area with comparable, or 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 to only the three northernmost municipalities Inari, Utsjoki and Enontekiö. In their final report the working group proposed two alternatives: one involving just these three municipalities and one involving also nine other municipalities in Northern Finland.

[1] Of all employed in the target region 59.8 % worked in private firms, 40.1 % in the public sector as a whole and 28.6 % were municipal employees (same figures at national level were 71.9 %, 28.1 % and 21.6 %). XX % of employed were excluded due to the exclusion of certain industries, most importantly the exclusion of firms producing agricultural products.

After the working group rendered its proposal, but before the government gave its proposal to the parliament, two more municipalities in Lapland and six municipalities on islands were added to the tax exemption region. On the other hand, the working group would have granted a tax exemption also to the municipal employers. The final proposal was a compromise that excluded all public sector employers with the exception of state-owned companies.

Getting tax exemption was made rather easy for the participating employers. The employer had to file a starting declaration to the local tax office. The employers could then simply deduct the tax exempt amount from its monthly employer contributions. The employers also had to add an attachment to the annual notification listing the wages of the employees that were paid within the calendar year and the reductions of social security contributions due to the tax exemption. The ease of participation was reflected in high take-up rates. By November 2003, 86 percent of the eligible employers with more than five employees in 2002 had filed a starting declaration. The fraction of participating firms with less than five employees was smaller but more uncertain, because not all these firms paid any wages during 2003.

3 Empirical strategy

A regionally targeted payroll tax reduction offers better chances to evaluate the incidence of payroll taxes and the possible employment effects than across the board reduction of employer contributions. The key benefit is that in a regional experiment, the wage and employment changes in the eligible firms can be compared to similar firms outside the target region. By carefully selecting comparison areas, it is more likely that other simultaneous changes in the economic environment have similar effects in the treatment and control regions. Instead of simple before – after -comparisons it is possible to compare wage and employment changes across target and control regions and base the inferences on the difference in differences.

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 across 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, different industrial structure may lead to different timing of the business cycle in control and target regions. To minimize the differences

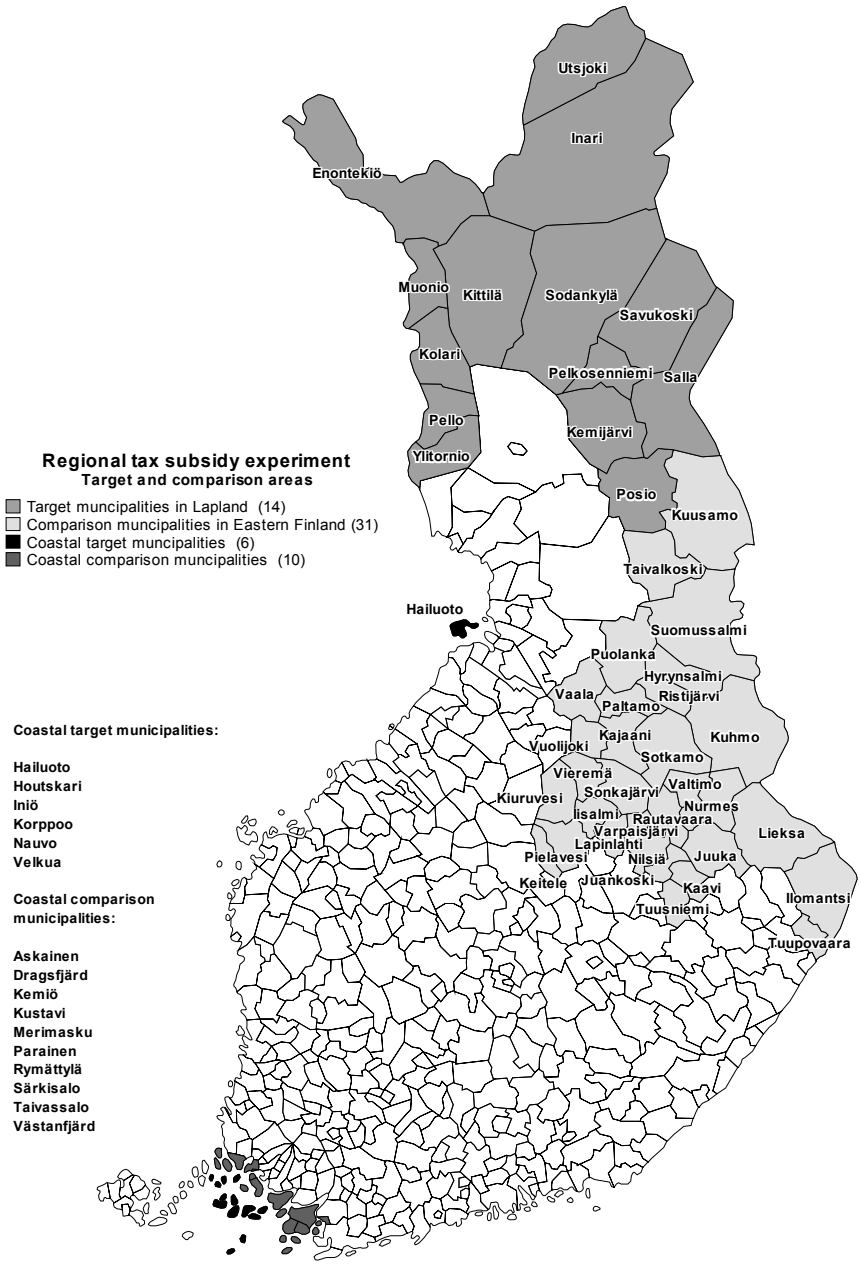
between target and comparison regions it is necessary to select into comparison group regions that have similar pre-treatment unemployment rates and similar industry-structure.

The target area in the payroll tax exemption evaluated in this paper comprises of twenty municipalities in high unemployment areas. Even if the tax exemption was introduced as an experiment, the target area was definitely not chosen randomly. Rather the payroll tax cut was intended to bolster employment in the most disadvantaged areas. The final selection of target municipalities was a result of intense political bargaining. Still, it is not obvious why just these municipalities were selected. There are other regions with comparable or even higher unemployment rates that were excluded from the target area[2].

In this paper we select the control group by a two-stage procedure. We first select NUTS 4 -level economic regions that are most comparable to the target region in terms of unemployment rates, urbanization, shares of employment in agriculture, manufacturing, and services and the fraction of hotels and restaurants of all business enterprises. (Compared to the national average the target region is characterized by high unemployment, low urbanization, a large agricultural sector and a small manufacturing sector. Tourism is a large and growing industry in the target region). We select the comparison regions based on data in Seutukunta- ja maakuntakatsaus 2002 by Statistics Finland. The six target municipalities on islands are part of several NUTS 4 regions. These regions also include major cities in the mainland. We chose to select the comparison region for these municipalities from other small municipalities from the western coast.

[2] The economic region with highest unemployment rate in 2001 was Kehys-Kainuu just south of the target region. In fact, proposals of extending payroll tax exemptions to Kainuu are currently being discussed.

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



We did not follow any formal procedure in selecting the comparison region but made an attempt to find regions that are as comparable as possible in all the aspects mentioned above. Therefore, we excluded other non-target regions in Lapland because they were administrative centres with a large university (Rovaniemi) or major manufacturing cities (Kemi-Tornio). Instead, we included areas from Eastern Finland just south of the target region where the unemployment rate is high and tourism is an important industry. (See the map) Our preliminary judgement is that the choice of comparison areas was rather

successful. To minimize the temptation to alter the choice ex-post, we fixed the design and published the setup before any data from employment effects became available in January 2004.

To further enhance the comparability of target and comparison regions we took the Establishment Register of Statistics Finland and matched each firm in the target area with a similar firm in the comparison region. In practice, we first split the data into main industries[3] using the classification in the Labour Force Survey and then estimated logit-models within each industry explaining whether the firm was located in the target region. The explanatory variables were the number of employees in 1999, 2000 and 2001, the total earnings of the employees in 1999, 2000 and 2001, the total sales of the firm in 2001 and a three-digit industry code[4]. We then found the closest match from the comparison region for each firm in the target region using the predicted probability from the model. In addition to this nearest neighbour matching, we also estimated models where each target firm was matched to five nearest neighbours and a model where the control was a weighted average of control firms and the weights were determined by the distance in the probabilities according to an Epanechnicov kernel.

It should be noted that the motivation of the matching approach differs slightly from the usual approach used in the program evaluation literature. For example, Deheija and Wahba (1999) describe propensity score matching stating that "conditional on the propensity score the assignment to treatment is essentially random". In the case of regional programs the location of the firm fully determines the eligibility for the tax exemption – there is no randomness. Still by choosing only matched pairs to the comparison group the basic features of the matching procedure remain intact. Matching guarantees that only firms that are comparable in the observable characteristics are compared. No functional form restrictions on the effects of contextual variables are required and the analysis can be restricted to "common support" by excluding firms that cannot be matched[5]. A better description of the matching procedure is that using the propensity scores the distributions of the firms in the observable characteristics are balanced so that the distribution of observable characteristics is similar in the treatment and the control group.

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[3] In the target and comparison regions we use a classification into 6 main industries at the probability estimation stage. Matching is done strictly within a finer industry classification with 20 classes.

[4] The number of employees, total wages and sales were entered in logarithms.

[5] For example, if in some industry there are firms only in the target region, including industry dummies in the logit equation predicts "failure" with certainty. Imposing the common support condition by requiring that $P(D = 1 \mid X) < 1$ where $D = 1$ indicates that the firm is located in the target region drops these observations from the analysis.

As a final note in this section, we discuss the timing of potential effects. The working group that proposed the tax cut handed in its report in December 2001. The decision to implement the program was effectively made in April 2002 when the government decided on the budget for the year 2003. It 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. We examine potential anticipatory effects by basing the matching on data from the end of 2001, before any information on the program was made public. We then compare the treated and control firms for both the pre-program period from January 2002 to December 2002 and for the program period after January 2003.

The tax exemption was planned to be temporary and to last for three years. In this evaluation we will follow the effects during the first two years of the program and finalize the analysis in the spring of 2005. By then most firms that will alter their employment will probably have done so already. The employment changes during the final year of the experiment and the effects of ending of the program will be left for later generations of researchers.

4 Data

We created the matched data on target and control firms based on data from the Register of Enterprises and Establishments of Statistics Finland. This database should ideally contain information on every unit that contributes to GNP, but the annual tables that we have used are somewhat more restricted in their scope. We have concentrated on private sector firms that have had a clearly positive turnover, have paid wages and employed someone, and that are within industries eligible for the tax cut. Furthermore, we required that the firms have one plant only, so that its location can be determined accurately. We found 2 811 (247) such firms in the northern (south coast) target area and 7 583 (1 551) firms in the control area. After removing observations with missing information on some of the required control and response variables we were left with 1 513 () firms in the target area and 3 994 () firms in the comparison area.

Comprehensive data on the employment and earnings outcomes was available from the Finnish Tax Administration. The data is based on employer's annual notification that all employers are required to submit to the regional tax office by the end of January following the year of payment. 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 wages and salaries paid (i.e. the product of hours worked and the average hourly wage) should be accurately reported.

The data from the Tax Administration also contains other useful information. The firms are classified to different industries according to their main activity. The location is reported but only at the firm- not establishment-level. Also, social security contributions are reported in detail, including the category that determines the rate of National Pension contributions. Exemptions from these contributions due to the experiment are also included in the data.

Figure 2. Timing of firms' participation in the tax experiment.

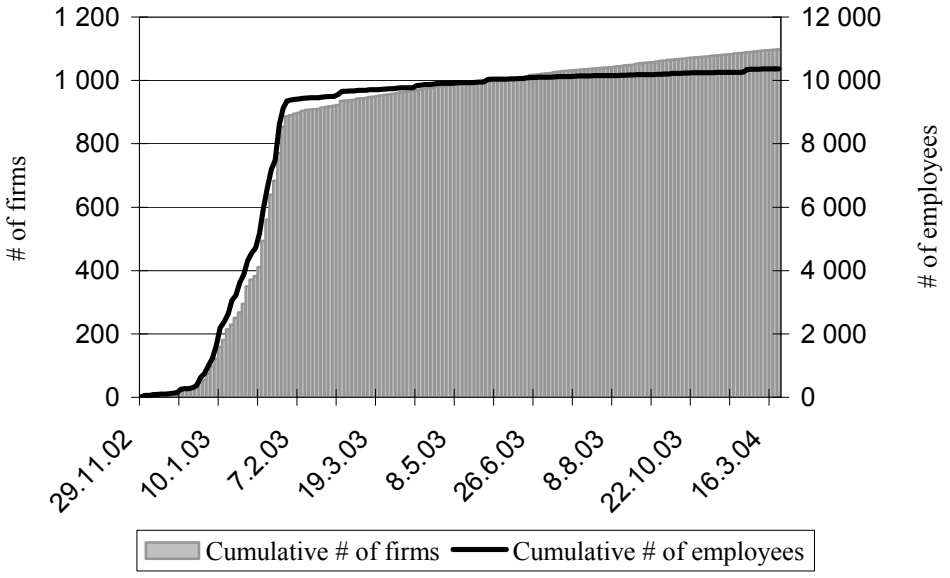


Table 1. Industry distribution of firms and employees in the end of year 2001. Firms that qualify for the matching process.

	Target region			Control region		
	Firms	**Employees, SF**	**Employees, TA**	**Firms**	**Employees, SF**	**Employees, TA**
Industry						
Manufacturing	215	1 031	2 127	653	5 959	10 098
Construction	216	552	1 299	638	2 218	4 938
Trade	525	1 820	4 274	1 229	3 600	8 107
Transport	192	316	827	394	825	2 456
Business services	188	410	1 135	519	1 376	4 116
Other services	144	408	1 224	448	1 237	3 032
All	1 480	4 536	10 886	3 881	15 216	32 747

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

Table 2. Sample means for target and control region firms.

	Target region firms	**Control region firms**	**All**
Employees, SF	3.06	3.92	3.68
Employees, TA	7.36	8.44	8.14
Wage sum, SF	57 760	80 692	74 361
Wage sum, TA	58 233	81 200	74 860
Social security tax contributions	2 560	3 469	3 218
Turnover	465 586	499 826	490 373
New firms, %	0.13	0.14	0.13

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

The main problem in the Tax Administration data is that hours worked or hourly wages are not reported. Taxes are determined according to the annual earnings, and for tax purposes there is no reason to collect data on more detailed level. Unfortunately, estimating the incidence of tax exemption on wages and the labour demand response are impossible based on tax data only. However, the tax data provides a reliable estimate on whether the payroll tax deduction had an impact of total wages. If total wages increased due to the experiment, there must be an effect on either wages or employment.

In order to estimate the incidence of payroll taxes we, therefore, need more detailed information on wages and hours. There is no single database where this information could be gathered for all firms. The best source of data appears to be the Wage Survey of the Employers Federation of the Service Industries (PT). This survey contains individual level data for all the workers in the firms that belong to the Federation. The total number of firms is 9 300, in the target area there are XX member firms. Most of these firms are small; three quarters have less than twenty employees.

The PT Wage Survey contains detailed information on monthly wages and hours worked in October of each year. In addition, there are a number of background variables on the employees including, for example, education, sex, tenure, occupation and industry. Since all employees are included in the database, also changes in employment at the firm-level can be calculated from the data.

Finally, aggregate level employment changes in the target region can be computed from the Labour Force Survey. These numbers are somewhat imprecise since the number of individuals from the target region that end up in the survey sample is rather small.

5 Results

In this chapter we first try to persuade the reader that the matching procedure indeed does its job in balancing the observables. Then we proceed by presenting the average treatment effects on the treated, i.e. difference in differences estimators for both employment and wage sum changes in the target and comparison regions of the tax experiment, and follow by presenting some further results to clarify and elaborate on the main outcome.

In this chapter we report only results concerning the target and control regions in Northern an Eastern Finland (for the time being).

5.1 Covariate balancing (or match quality)

The variables used in the estimation of the propensity score (the probability of being located in the target area) should on the one hand include all variables that are relevant in determining the probability and that simultaneously affect the response variable(s). On the other hand the large sample properties of the eventual treatment effect estimator deteriorate as the number of (continuous) explanatory variables in the propensity score equation increases. As it is impossible to empirically determine the correct set of variables used, we struck a compromise between the number of variables and the probability of leaving out some influential covariates and used the following explanatory variables in the logit equation: number of employees in 1999, 2000 and 2001, the total earnings of the employees in 1999, 2000 and 2001, the total sales of the firm in 2001 and a three-digit industry code. We made the logit estimation within eight main industries; hence the resulting matches are exact up to that level of industry categorisation. Our argument is that after being selected from very similar regions in the first place, matching industry quite exactly and three year development in both employment and wages should be enough to remove systematic differences relevant to employment growth between matches.

Figure 2 depicts the estimated densities of the probability of firms being located in the tax experiment's target region for the target region firms and comparison region firms. If our focus were on the probability estimation, the results would not be that great. The logit model does indeed a rather poor job of discriminating between firms from the target and control regions. When the purpose is to match firms however, it is encouraging to see that there does not occur any severe common support problem: large fraction of the firms from both regions gets an estimated probability in the range from 0.1 to 0.5. In figures 3 – 6 we have drawn the development of average and aggregate employment and wages in the target region firms, all comparison region firms and the kernel matched comparison region firms. Two conclusions can be drawn from these figures: comparison region firms are on average larger than the target region firms but the evolution of averages and aggregates looks very much alike. The size difference is nicely removed by matching, but we would be hard pressed to say that the changes over time are any more similar in the matched firms than in the group of all comparison region firms.

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

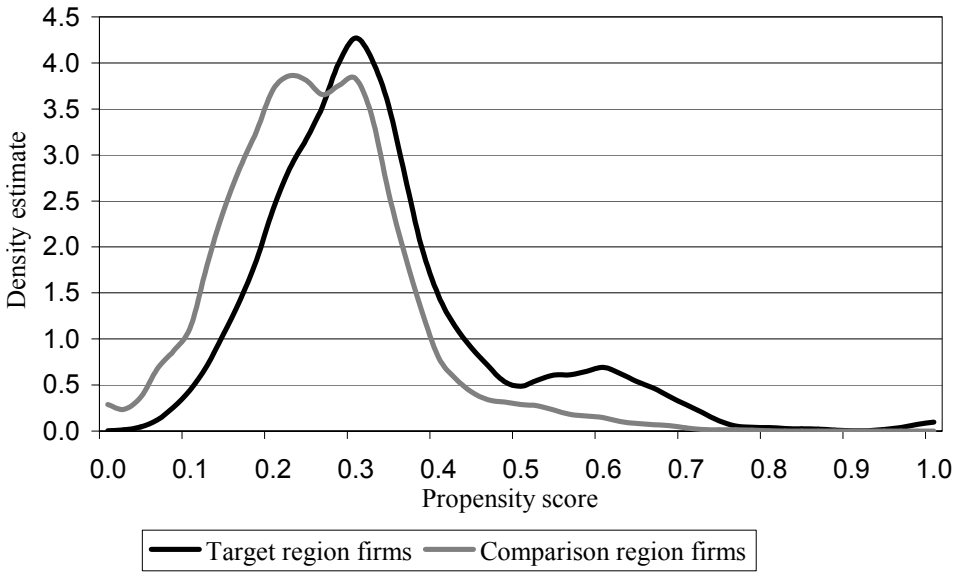
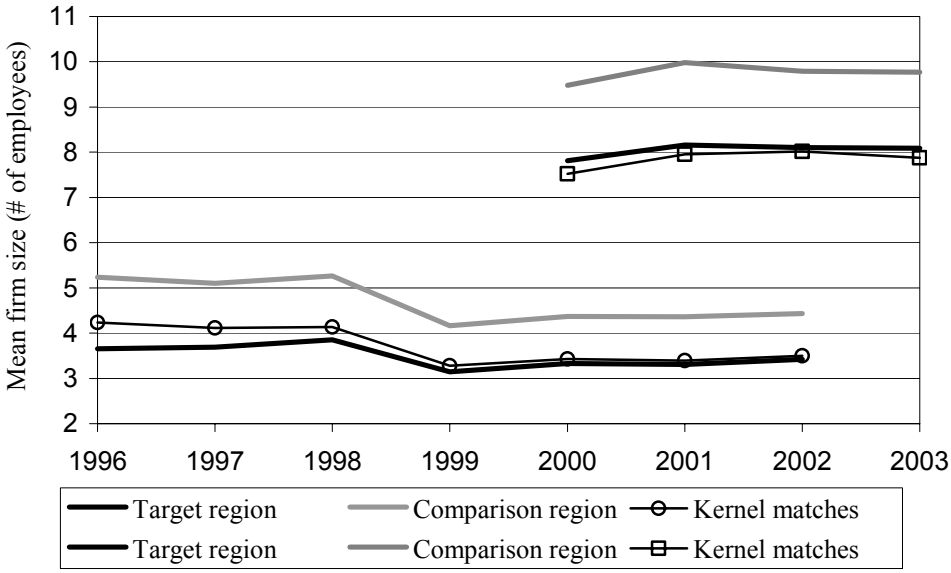
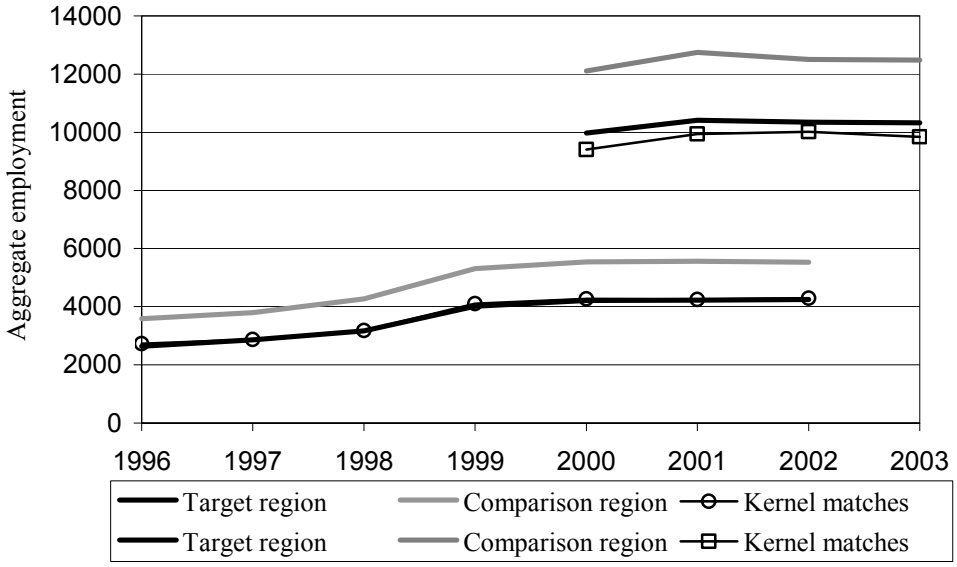


Figure 4. Development of the mean firm size in the target region firms, comparison region and matched comparison region firms. Firms that existed in the end of 2001.



Lower lines (years 1996 – 2002) are Statistics Finland supplied man year estimates, higher lines (years 2000 – 2003) graph Tax Administration supplied numbers that tell the total number of employees on payroll over a one year period.

Figure 5. Development of the aggregate employment in the target region firms, comparison region and matched comparison region firms. Firms that existed in the end of 2001.



Lower lines (years 1996-2002) are Statistics Finland supplied man year estimates, higher lines graph (years 2000 – 2003) Tax Administration supplied numbers that tell the total number of employees on payroll over a one year period.

Figure 6. Development of the mean wage sum in the target region firms, comparison region and matched comparison region firms. Firms that existed in the end of 2001.

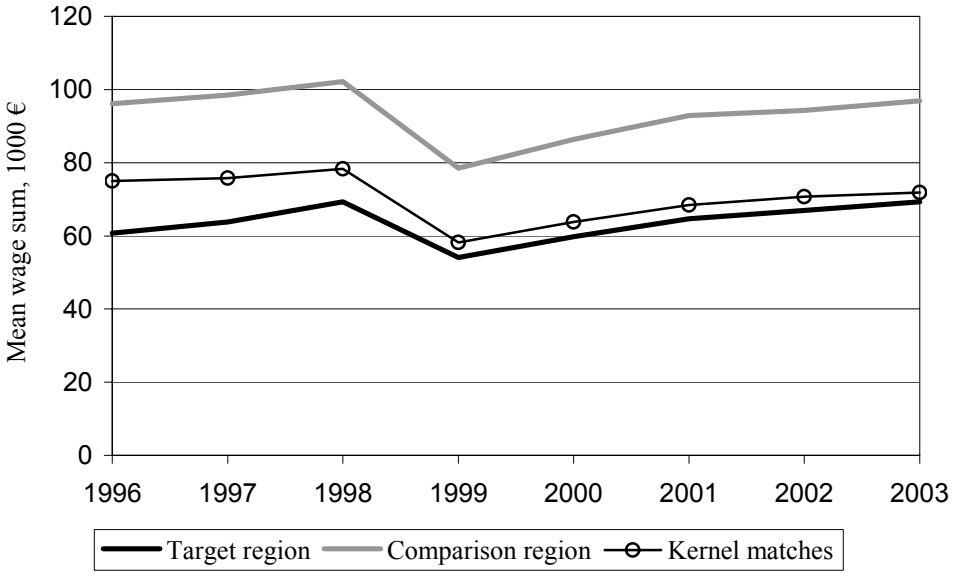
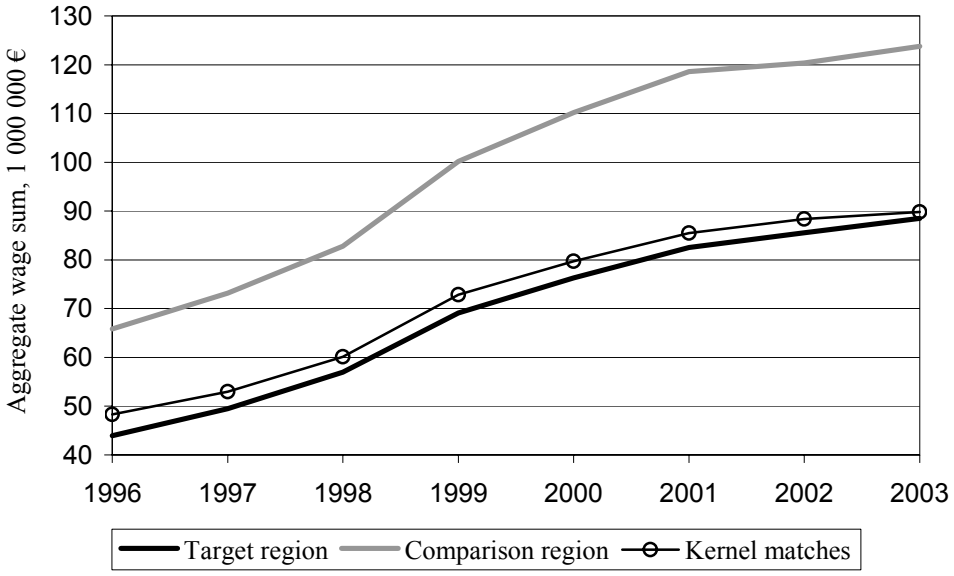


Figure 7. Development of the aggregate wage sum in the target region firms, comparison region and matched comparison region firms. Firms that existed in the end of 2001.



A more analytic way of examining how successful the matching process was, is to draw some measures to gauge the difference between the firms in target and control regions before and after matching. We have used the one supplied by a Stata procedure by Leuven and Sianesi (2003), i.e. a measure of bias in variables before and after matching. The bias is calculated as 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). It should be noted that we have used two measures for employment on the years they were available to us, the Statistics Finland supplied man year estimate and the total number of persons on a firm's payroll during the observed year acquired from the Tax Administration. The number of hours worked would have been the ideal measure for our purposes, but unfortunately that information is not collected for a majority of small firms.

The results on covariate balancing are reported in table 3. The differences between the firms in the target and control groups are quite small to begin with and matching removes most of the existing bias. The matching variables are used as levels, but our focus will be on employment and wage sum changes. Therefore we also calculated measures of differences in our response variables for the period 2000 – 2001 to see that they do not exhibit divergent behaviour. These results are reported in the lower section of table 3 and again it is evident that the differences in growth are small between the

regions and are further balanced through matching. We do not report match quality with respect to industry since the industry distributions are virtually identical down to the three digit level.

Table 3. Covariate balancing in propensity score matching (kernel matches).

		Mean			
	Sample	**Treated**	**Control**	**Bias, %**	**Reduction in bias, %**
Variables used in matching, in log's					
Employment 2001, SF	Unmatched	0.640	0.740	-9.9	
	Matched	0.651	0.669	-1.8	82.2
Employment 2000, SF	Unmatched	0.648	0.751	-10.2	
	Matched	0.661	0.678	-1.7	83.2
Employment 1999, SF	Unmatched	0.609	0.709	-9.9	
	Matched	0.621	0.638	-1.7	82.7
Employment 2001, TA	Unmatched	1.280	1.481	-15.4	
	Matched	1.291	1.358	-5.1	67.2
Employment 2000, TA	Unmatched	1.160	1.334	-11.8	
	Matched	1.180	1.210	-2.0	82.7
Wage sum 2001, TA	Unmatched	9.438	9.787	-13.9	
	Matched	9.463	9.625	-6.4	53.7
Wage sum 2000, TA	Unmatched	9.119	9.373	-8.3	
	Matched	9.165	9.252	-2.8	65.5
Wage sum 1999, SF	Unmatched	8.978	9.241	-8.4	
	Matched	9.014	9.118	-3.3	60.4
Turnover 2001	Unmatched	11.327	11.095	7.2	
	Matched	11.373	11.282	2.8	61.0
Response variables for 2000 – 2001 (not used in matching)					
Employment change, # of employees	Unmatched	0.663	0.841	-2.5	
	Matched	0.644	0.773	-1.8	27.3
Employment change, %	Unmatched	0.188	0.207	-2.4	
	Matched	0.182	0.181	0.2	91.1
Wage sum change, €	Unmatched	6 660.1	9 676.2	-6.0	
	Matched	6 729.5	6 562.8	0.3	94.5
Wage sum change, %	Unmatched	0.245	0.275	-3.7	
	Matched	0.242	0.245	-0.3	91.0

SF = Employment figure supplied by Statistics Finland, estimated man-years.
TA = Employment figure supplied by Tax Administration, # of employees on payroll over a year.

In addition to kernel matching (Epanechnikov kernel with a bandwidth of 0.05) we also employed the nearest neighbour and five nearest neighbours matching methods, with

the same bandwidth on the propensity score metric. Covariate balancing results do not have qualitative differences; kernel matching seems to perform better than the single nearest neighbour method and equally well with the five nearest neighbours method, see appendix 1, appendix table 2.

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

Our response variables to measure employment *growth* in the target and control regions are the change in the number of employees on firm's payroll over a one year period, relative change in employment[6], change in the firm's total payroll and relative change in the total payroll[7]. We prefer the changes in the levels variables owing to the fact that they tell directly whether the employment grew faster in the target region firms or not. Percent changes on the other hand tell, if the target region firms on average grew faster or not. (epäselvästi sanottu!) The matching estimator for determining the difference between treatment and control region firms is hence a *difference-in-differences estimator*, i.e. we measure the difference in the growth of the target region firms and their matched control region firms. This differencing should remove some of the unobservable heterogeneity between target region firms and their matches.

We calculated the employment and wage sum changes for three time periods, 2001 – 2002 to work as a control case[8], 2002 – 2003 as the main effect, and 2001 – 2003 for the sake of getting more robust results. Our main findings are reported in table 4. The table contains six rows for each response variable that tell the outcome for each time period under investigation. We report the responses for the treated and control regions as a whole (row *unmatched*) and separately for the subset of matched firms (*ATT-weights*). The average treatment effect on the treated is then calculated as a difference between the treated and control region outcomes.

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[6] Measured in "flow percents", or as in Davis et al. (1996), where growth rate $g_t = \frac{EMP_t - EMP_{t-1}}{\frac{1}{2}\left(EMP_t + EMP_{t-1}\right)}$.
This measure is additive and accommodates both births and closures of firms. It is also bounded on the closed interval [-2, 2].

[7] Measured in the same fashion as the relative change in employment.

[8] Payroll tax cut in the target region begun from the beginning of 2003. Therefore there should not be any noticeable effect on the previous period, apart from some small anticipatory effect.

*Table 4. Average treatment effects on the treated, propensity score estimates. Effects significant at the 95 % confidence level are marked with an asterisk *.* [9]

		Treated	Controls	Treatment effect on the treated
Employment change, # of employees				
2001 – 2002	Unmatched	-0.060	-0.188	0.129
	ATT-weights	-0.080	0.065	-0.145
2002 – 2003	Unmatched	-0.015	-0.023	0.008
	ATT-weights	-0.010	-0.141	0.131
2001 – 2003	Unmatched	-0.074	-0.211	0.137
	ATT-weights	-0.090	-0.075	-0.014
Employment change, %				
2001 – 2002	Unmatched	0.055	-0.005	0.060
	ATT-weights	0.046	0.016	0.029
2002 – 2003	Unmatched	-0.200	-0.251	0.051
	ATT-weights	-0.198	-0.258	0.060*
2001 – 2003	Unmatched	-0.134	-0.244	0.110
	ATT-weights	-0.141	-0.231	0.090*
Wage sum change, €				
2001 – 2002	Unmatched	2 347	1 402	945
	ATT-weights	2 314	2 275	39
2002 – 2003	Unmatched	2 314	2 665	-351
	ATT-weights	2 351	1 157	1 193
2001 – 2003	Unmatched	4 661	4 067	594
	ATT-weights	4 665	3 432	1 233
Wage sum change, %				
2001 – 2002	Unmatched	0.084	0.039	0.045
	ATT-weights	0.075	0.050	0.025
2002 – 2003	Unmatched	-0.156	-0.227	0.071
	ATT-weights	-0.152	-0.239	0.087*
2001 – 2003	Unmatched	-0.071	-0.173	0.101
	ATT-weights	-0.077	-0.172	0.095*

[9] These are (our favoured) kernel estimates, estimated using Epanechnicov kernel with a bandwidth of 0.05 on propensity score metric. Confidence intervals are bootstrapped with 500 replications.

Results in table 4 are quite what we expected. 95 % of the firms (80 % of the workers) belong to a payroll tax bracket where the reduction in taxes is ~3 % of the wage sum. Therefore the employment effect should not be a large one, if distinguishable at all. Both measures of treatment effect for the period 2002 – 2003 are positive. The average change in the number of employees was 0.131 workers larger in the target region firms than in their matched counterparts in the comparison region and the average wage sum grew 1 193 euros more in the target region. Neither of these differences is statistically significant, both being smaller than their standard deviations. The differences in other periods, when measured in number of workers or euros, are insignificant as well. The wage sum growth behaves quite nicely tough, the difference being almost zero in the 2001 – 2002 period and positive in the other two periods.

Differences in the proportional growth of the number of employees and wage sum tell a slightly different story. Treatment effects for the period 2001 – 2002 are insignificant as they should be owing to the fact that the experiment started in 2003. The average proportional growth in the target region was however, clearly faster in both number of employees and wage sum, the difference being 6 to 9.5 percentage points depending on variable and time period. These differences are also statistically significant at the 95 % confidence level. The results may seem slightly contradictory, i.e. why did the proportional growth display a statistically significant average treatment effect on the target region firms but average employment or wage sum did not grow. The explanation to this is that it was the small firms that contributed to the proportional growth measure but as they represent only a quite small share of the total employment and wage sum, they did not have a measurable impact on the totals. Another feature that at first sight seems suspect, is the fact that on average the firms did not grow, but actually shrank quite noticeably (in percentage terms) over the periods 2002 – 2003 and 2001 – 2003. This is explained by the fact a considerable fraction (14 %) of the firms that were in the sample in 2001 ceased to exist towards the end of 2003. Every firm closure contributes a negative growth percent to the average. This makes the proportional growth distribution to lean to the left as it is impossible for new firms to enter the matched (or unmatched) sample.

When we dropped the firms that did not pay any wages in 2003 (our indicator for a firm closure) and re-estimated the average treatment effects, we did not find any statistically significant treatment effects on any of the response variables. These results are reported in the appendix 1, appendix table 4. Furthermore, the firms in the sub samples where closures were eliminated did on average show positive growth in levels and proportions. It seems then that the tax experiment has had little effect otherwise but it may have saved some firms, mostly small ones, from exiting. To examine if this indeed is the case, we estimated average treatment effects where the response variable is an indicator

for firm closure. The results in table 5 show that in the control period 2001 – 2002 the exit frequency of firms was identical in the treatment and comparison regions. The results also clearly indicate that the firms in the target region exited some three percent less frequently during the tax experiment than their matched counterparts in the control region. This three percent differential is also statistically significant at the 95 % confidence level[10].

*Table 5. Average treatment effects on the treated, propensity score estimates. Effects significant at the 95 % confidence level are marked with an asterisk *.* [11]

		Treated	Controls	Treatment effect on the treated
Share of firm closures, measured with # of employees				
2001 – 2002	Unmatched	0.017	0.023	-0.006
	ATT-weights	0.018	0.023	-0.005
2002 – 2003	Unmatched	0.117	0.151	-0.033
	ATT-weights	0.118	0.153	-0.036*
2001 – 2003	Unmatched	0.117	0.151	-0.033
	ATT-weights	0.118	0.153	-0.036*
Share of firm closures, measured with wage sum				
2001 – 2002	Unmatched	0.016	0.017	0.000
	ATT-weights	0.017	0.016	0.001
2002 – 2003	Unmatched	0.114	0.140	-0.026
	ATT-weights	0.114	0.143	-0.029*
2001 – 2003	Unmatched	0.114	0.140	-0.026
	ATT-weights	0.114	0.143	-0.029*

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[10] Appendix table 5 contains the propensity score based estimates and standard errors with nearest neighbour, five nearest neighbours, and kernel method. The covariate balancing estimates point to the same direction, but are of smaller magnitude and not statistically significant.

[11] These are (our favoured) kernel estimates, estimated using Epanechnicov kernel with a bandwidth of 0.05 on propensity score metric. Confidence intervals are bootstrapped with 500 replications.

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Appendix

1 Covariate balancing and average treatment effect estimates with different matching methods

Appendix Table 1. Number of observations used with different matching methods.

	Treatment Region	Control region
Number of firms used in propensity score estimation	1 513	3 994
Number of firms used in calculating		
Nearest neighbour estimates	1 250	922
5-nearest neighbours estimates	1 250	2 503
Kernel estimates	1 250	3 240

Appendix Table 2. Covariate balancing with different propensity score matching methods.

	Bias before matching	Bias after matching (reduction, %)		
		1 nearest neighbour	5 nearest neighbours	Kernel
Variables used in matching, in log's				
Employment 2001, SF	-9.9	0.8 (91.4)	1.1 (89.0)	-1.8 (82.2)
Employment 2000, SF	-10.2	1.0 (89.8)	1.1 (89.2)	-1.7 (83.2)
Employment 1999, SF	-9.9	-0.3 (96.7)	0.5 (94.9)	-1.7 (82.7)
Employment 2001, TA	-15.4	-7.2 (53.3)	-2.9 (81.4)	-5.1 (67.2)
Employment 2000, TA	-11.8	-4.2 (64.5)	1.8 (84.8)	-2.0 (82.7)
Wage sum 2001, TA	-13.9	-7.1 (48.6)	-3.7 (73.5)	-6.4 (53.7)
Wage sum 2000, TA	-8.3	-5.9 (28.0)	0.1 (98.4)	-2.8 (65.5)
Wage sum 1999, SF	-8.4	-5.9 (29.6)	-1.2 (85.2)	-3.3 (60.4)
Turnover 2001	7.2	3.8 (46.9)	2.4 (66.7)	2.8 (61.0)
Response variables for 2000 – 2001 (not used in matching)				
Employment change, # of employees	-2.1	-1.5 (29.1)	-1.1 (47.5)	-1.6 (22.8)
Employment change, %	-2.1	-0.4 (79.0)	-0.6 (69.1)	-1.5 (29.5)
Wage sum change, €	-3.9	-1.0 (74.4)	0.1 (97.3)	0.1 (96.5)
Wage sum change, %	-2.2	4.1 (-86.6)	0.7 (69.8)	0.1 (97.2)

Appendix Table 3. Average treatment effect on the treated estimators with different matching methods. Standard errors in (parenthesis).

	Matching method				
	Propensity score, 1-nn	Propensity score, 5-nn	Propensity score, kernel	Covariate, 1-nn	Covariate, 5-nn
Employment change, # of employees					
2001 – 2002	-0.218	-0.092	-0.145	0.030	-0.042
	(0.2878)	(0.2445)	(0.2121)	(0.1682)	(0.1400)
2002 – 2003	0.130	0.103	0.131	-0.050	0.140
	(0.3261)	(0.2646)	(0.2393)	(0.2718)	(0.2053)
2001 – 2003	-0.087	0.011	-0.014	-0.021	0.098
	(0.4126)	(0.3317)	(0.2810)	(0.3014)	(0.2426)
Employment change, %					
2001 – 2002	0.027	0.024	0.029	0.001	-0.022
	(0.0319)	(0.0268)	(0.0246)	(0.0219)	(0.0178)
2002 – 2003	0.059	0.060	0.060	0.033	0.083
	(0.0453)	(0.0353)	(0.0325)	(0.0344)	(0.0264)
2001 – 2003	0.084	0.086	0.090	0.029	0.101
	(0.0494)	(0.0387)	(0.0340)	(0.0352)	(0.0269)
Wage sum change, €					
2001 – 2002	-502.8	-151.0	39.361	-253.9	436.7
	(1 632.2)	(1 430.0)	(1 306.0)	(1 254.0)	(971.7)
2002 – 2003	1 905.6	705.0	1 194.0	359.4	53.62
	(1 561.8)	(1 309.0)	(1 271.9)	(2 392.2)	(1 374.8)
2001 – 2003	1 402.8	554.0	1 233.4	105.5	490.3
	(2 618.3)	(2 192.4)	(1 931.8)	(2 745.8)	(1 788.1)
Wage sum change, %					
2001 – 2002	0.004	0.016	0.025	-0.012	0.003
	(0.0357)	(0.0294)	(0.0265)	(0.0241)	(0.0198)
2002 – 2003	0.101	0.089	0.087	0.039	0.055
	(0.0462)	(0.0359)	(0.0318)	(0.0365)	(0.0292)
2001 – 2003	0.085	0.082	0.095	0.026	0.052
	(0.0512)	(0.0401)	(0.0352)	(0.0374)	(0.0302)

Standard errors for propensity score method are bootstrapped, normal approximated estimates from 500 replications; standard errors for covariate matching estimators are heteroskedasticity corrected as in Abadie et al. (2001).

Appendix Table 4. Average treatment effect on the treated estimators with different matching methods. Standard errors in (parenthesis). ***Firms that had zero employment or wage sum on year 2003 excluded.***

	Matching method				
	Propensity score, 1-nn	Propensity score, 5-nn	Propensity score, kernel	Covariate, 1-nn	Covariate, 5-nn
Employment change, # of employees					
2001 – 2002	-0.406	-0.208	-0.211	-0.022	-0.032
	(0.3425)	(0.2707)	(0.2334)	(0.1818)	(0.1496)
2002 – 2003	0.075	0.050	0.001	0.205	0.017
	(0.3274)	(0.2791)	(0.2781)	(0.2782)	(0.2201)
2001 – 2003	-0.331	-0.158	-0.209	0.183	-0.015
	(0.4341)	(0.3482)	(0.3396)	(0.3130)	(0.2586)
Employment change, %					
2001 – 2002	0.010	0.017	0.019	0.009	0.016
	(0.0382)	(0.0309)	(0.0277)	(0.0232)	(0.0191)
2002 – 2003	-0.019	-0.015	-0.012	0.009	0.004
	(0.0311)	(0.0263)	(0.0230)	(0.0231)	(0.0192)
2001 – 2003	-0.039	-0.003	0.001	0.020	0.019
	(0.0367)	(0.0323)	(0.0280)	(0.0236)	(0.0197)
Wage sum change, €					
2001 – 2002	-346.5	259.3	-50.0	156.5	732.7
	(2 031.1)	(1 660.9)	(1 498.6)	(1 341.2)	(1023.2)
2002 – 2003	1 985.9	1 275.5	338.6	299.9	-1010.4
	(1 993.8)	(1 541.4)	(1 619.9)	(2 352.3)	(1 409.9)
2001 – 2003	1 639.4	1 534.8	288.6	456.4	-272.7
	(3 153.2)	(2 480.8)	(2 408.6)	(2 760.0)	(1 855.9)
Wage sum change, %					
2001 – 2002	0.019	0.015	0.016	0.008	0.007
	(0.0379)	(0.0307)	(0.0273)	(0.0233)	(0.0193)
2002 – 2003	0.032	0.034	0.020	0.031	0.023
	(0.0349)	(0.0286)	(0.0252)	(0.0251)	(0.0211)
2001 – 2003	0.037	0.030	0.017	0.031	0.017
	(0.0421)	(0.0340)	(0.0308)	(0.0272)	(0.0227)

Standard errors for propensity score method are bootstrapped, normal approximated estimates from 500 replications; standard errors for covariate matching estimators are heteroskedasticity corrected as in Abadie et al. (2001).

Appendix Table 5. Average treatment effect on the treated estimators with different matching methods. Standard errors in (parenthesis).

	Matching method				
	Propensity score, 1-nn	Propensity score, 5-nn	Propensity score, kernel	Covariate, 1-nn	Covariate, 5-nn
Share of firm closures, measured with # of employees					
2001 – 2002	-0.000	-0.004	-0.005	0.005	-0.000
	(0.0071)	(0.0056)	(0.0049)	(0.0054)	(0.0043)
2002 – 2003	-0.027	-0.038	-0.036	-0.012	-0.021
	(0.0182)	(0.0142)	(0.0125	(0.0137)	(0.0107)
2001 – 2003	-0.027	-0.038	-0.036	-0.012	-0.021
	(0.0182)	(0.0142)	(0.0125)	(0.0137)	(0.0107)
Share of firm closures, measured with wage sum					
2001 – 2002	0.004	0.000	0.001	0.005	0.003
	(0.0143)	(0.0051)	(0.0044)	(0.0048)	(0.0039)
2002 – 2003	-0.021	-0.030	-0.029	-0.008	-0.014
	(0.0175)	(0.0135)	(0.0123)	(0.0133	(0.0104
2001 – 2003	-0.021	-0.030	-0.029	-0.011	-0.002
	(0.0175)	(0.0135)	(0.0123)	(0.0135)	(0.0108)

Standard errors for propensity score method are bootstrapped, normal approximated estimates from 500 replications; standard errors for covariate matching estimators are heteroskedasticity corrected as in Abadie et al. (2001).