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Valtion taloudellinen tutkimuskeskus
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Abstract

In this paper I evaluate the effects of a regional experiment that reduced payroll taxes by 3–6 percentage points of the firms' wage sum in northern and eastern Finland. I estimate the effect of the payroll tax reduction on firms' employment levels, wage sum and profits, and on workers' hourly pay and monthly hours worked, by comparing the changes in employment and wages before and after the start of the experiment to a control region. My results indicate that the reduction in payroll taxes did not lead to any unequivocal aggregate effects in the target region.

Key words: payroll-tax, labour demand, tax incidence

JEL classification: J18, J23, J38, J58, J65, J68

Tiivistelmä

Tässä tutkimuksessa arvioidaan Lapin ja Kainuun yritysten alueellisen sosiaaliturvamaksuvapautuksen vaikutusta yritysten työllisyyteen, palkkasummaan ja voittoihin sekä näissä yrityksissä työskentelevien tuntipalkkoihin. Sosiaaliturvamaksut ovat olleet tutkimusperiodilla kolmesta kuuteen prosenttia yritysten palkkasummasta. Tulosten perusteella maksualennus ei aiheuttanut tilastollisesti merkitseviä muutoksia mihinkään tutkituista vastemuuttujista.

Asiasanat: Palkan sivukulut, työvoiman kysyntä, verotuksen kohtaanto

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

Contents

1. Introduction and background

The main objective of this project is to estimate by careful empirical analysis the average effect of a cut in payroll taxes on employment, wages and firm profitability. This effect is likely to be heterogeneous: different firms, depending on their size, capital intensity, worker turnover, etc, will probably experience differing effects. Mindful of the restrictions of a regionally narrow experiment, it is possible to some extent to probe these differentials. The Finnish payroll tax experiment that started in 2003 was originally limited to a three-year period, but its extension until 2012 means that firms may well make investment decisions and other adjustments targeted at longer time spans.

There is a rather strong consensus regarding the labour market effects of payroll taxes. There is a textbook model stating that a reduction in payroll taxes lowers wage costs and hence boosts the demand for labour. Its effect on employment then depends on the incidence of the tax. If the tax cut leads to higher wages that entirely offset the reduction in taxes, the tax cut will have no effect on employment, and if the labour supply is fully elastic, then the tax cut will result in higher employment. Empirical studies (with micro data and quasi-experimental settings) on this subject include Gruber (1994), evaluating the effects of mandated maternity benefits in the US, and Anderson and Meyer (1997) and Murphy (2007), who examine the incidence of unemployment insurance taxes. In these cases, the changes in payroll tax rates vary between firms because of the different composition of their labour forces or because the tax rates depend on firm characteristics. Another approach is to examine the effects of regional policies that create different changes in payroll tax rates between firms that are located in different regions but that are otherwise comparable. Prime examples include Bohm and Lind (1993), who evaluate the employment effects of regional wage subsidies in northern Sweden, Johansen and Klette (1998), who examine the effects of regional differences in payroll taxes in Norway, Bennmarker, Mellander and Öckert (2009) and Korkeamäki and Uusitalo (2009), who evaluate the effects of recent regional wage subsidy schemes in Sweden and Finland. The general finding of these studies is that changes in payroll taxes are partly shifted to wages, with little effect on employment[1]. An exception is Crépon and Desplatz (2002) who evaluate payroll tax subsidies for low-wage workers. In this French case, the authors find significant positive effects on the employment of the affected group of workers.

Textbooks, however, say very little on the effect of payroll taxes on firm profitability. In the neoclassical family of labour market models, the zero profit constraint seems to void the question altogether. In recent years, empirical

[1] See Bennmarker et al. for a short review of previous studies.

observations have dented these theories to some extent[2]. In their book on a closely related subject, minimum wages[3], Card and Krueger (1995) consider the possible mechanisms by which changes in minimum wages could affect firms' profits[4]. Lacking suitable micro data for direct measurement, they use changes in firms' stock market valuations as an indicator of changes in profits. Also, changes in minimum wage legislation (or announced changes) are used as instruments to identify the effect of minimum wages on stock prices, and the implied change in profits is calculated. Card and Kruger find tentative evidence that announced rises in minimum wages induce investors to adjust their valuation of firms downward.

The first study on the direct effect of minimum wages on firm profitability is Draca, Machin and Van Reenen (2008). They use the introduction of a national minimum wage to the UK labour market in 1999 as a quasi-experiment to identify the effect of a rise in minimum wages on profits. The motivation for their study is that in the UK case there was little impact on employment (Machin, Manning and Rahman, 2003 and Stewart, 2004) and also little evidence that firms were able to pass on higher costs to consumers by increasing prices (exceptions here are Aaronson 2001 and Aaronson and French 2007). Draca et al. find a significant reduction in profits and a rise in labour costs owing to the introduction of a national minimum wage scheme, but neither employment nor productivity changed. They also report that in the longer run the labour cost hike did not seem to force the affected firms out of business.

There have not been any abrupt changes in wage schemes in Finland that could be used as instruments to estimate a wage cost effect on firms' profits. There is, however, an ongoing experiment in payroll taxes. In March 2002, the Finnish government agreed on a temporary removal of employer contributions to national pension insurance and national health insurance for firms operating in 20 target municipalities in Lapland and the Turku archipelago. The removal of these contributions lowered payroll taxes for eligible firms by an average of 4.1 percentage points. The programme was designed as an experiment with the stated aim of evaluating the effect of a cut in payroll taxes on employment in the target region. The payroll tax exemption was planned to last for three years, from 1 January 2003 to 31 December 2005. Already in May 2003, the government had decided to start a regional self-government experiment in Kainuu, eastern Finland, begin-

[2] There is a quotation attributed to Paul A. Samuelson that "In economics it takes a theory to kill a theory; facts can only dent a theorist's hide."

[3] In the part of the wage distribution where minimum wage rules are binding, the effect of minimum wages can be considered to be similar to a payroll tax hike. The main difference is that the uneven incidence can cause substitution away from low-wage labour towards both capital and higher-wage labour.

[4] The focus of the book is on the employment effects of minimum wages, but there is a chapter on how much profits change. Unlike the payroll tax case, standard economic theory unambiguously implies that wage floors have a negative impact on employment (Borjas 2005, Brown 1999). Empirical evidence is considerably more mixed; see the comprehensive review by Neumark and Wascher (2007).

ning from 2005. That experiment contained a similar provision for lowered payroll taxes as the Lapland experiment and hence the payroll tax experiment was expanded and extended until the end of 2009. The experiment has since been extended further until the end of 2012.

To sum up the current situation and motivate the need to assess whether payroll tax cuts have had an effect on firm profits, I draw the following conclusions. 1) According to our previous research of the first two years of the payroll tax experiment, the cut in northern Finland did not seem to have any immediate employment effects (Korkeamäki and Uusitalo 2009). This finding is supported by evidence from other Nordic labour markets. 2) There was some indication of rising wages, but not 1:1 with respect to the tax break – this is a finding that has also been made in Sweden and Norway. From 1) and 2) and supported by the UK case of a change in minimum wages, it seems likely that changes in payroll taxes could have an effect on firm profitability. Models of incomplete competition from the IO literature (Aaronson and French 2007) and matching models from the labour market side (e.g. Flinn 2006) can accommodate these profit effects, but their size remains an empirical question.

For this study, I have twice the number of firms in the treatment group compared to the earlier research, richer information on firms and more years of observations. However, I still do not find any effects on employment, wage sum or profits. The wage sum and profits measured in euro terms grew faster in the target region of the experiment, whereas the employment gains were negative, but none of the effects are statistically significant. The additional information available here does make the previous results concerning wages suspect, however – there still is a positive and significant wage effect in Lapland, but in Kainuu the effect is negative and significant. Certainly, there might have been a region-specific shock in Kainuu causing the negative effect, but I found no reason to believe that the result for Lapland was trustworthy.

That there was a tax cut is a fact and it can be observed to have lowered the cost of employment. Other results, however, are either non-existent or drowned in the standard errors. It is unfortunate that researchers were not consulted in the design phase of the experiment. The selection of the target region and the size of the tax cut were mainly driven by political feasibility, not by a focus on facilitating reliable and conclusive research.

2. The experiment, target and control regions and firms

At the turn of the millennium there was an ongoing debate over the relative merits of across-the-board, low-bureaucracy tax cuts and more targeted measures to promote employment. In March 2002, the Finnish government agreed to a temporary removal of employer contributions to national pension insurance and national health insurance (see Table 1) for firms operating in the 20 target municipalities[5]. The programme was designed as an experiment with the stated aim of evaluating the effect of a cut in payroll taxes on employment in the target region. The tax cut was designed to fit within the European Union *de minimis* regulations that govern firm subsidies. Therefore the maximum tax cut is 30,000 euros per year for each firm and the already heavily subsidised industries of agriculture, fishing and transport were excluded from the experiment. The payroll tax exemption was to continue for three years from January 1 2003 to December 31 2005. In December 2005, the government extended the duration of the experiment to the end of 2009. The original regional tax experiment is exhaustively described in Korkeamäki and Uusitalo (2009).

The act on the regional self-government experiment in Kainuu was passed by the Finnish parliament in February 2003 and the experiment started on 1 January 2005. The aim of the self-government experiment is to gain experience of the effects of regional self-government on regional development work, basic services, citizen activity, the relationship between regional and state central government as well as between municipal and state local government. The Kainuu experiment provides the same payroll tax cut as the Lapland experiment but it is no longer motivated in the law as being an experiment nor is there any specific mention of an evaluation of the tax cut. The Kainuu experiment extends the payroll tax cut to public sector employers and this provision was extended to Lapland from the beginning of 2006.

The Kainuu region has nine municipalities with an area nearly equalling that of Belgium, but a population of only 85,000. The target region in Lapland is even larger in area, with a population of 65,000. Both can be described as sparsely populated, high unemployment regions with little manufacturing or other industrial activity. The share employed in agriculture and forestry is much higher and the average level of education much lower than in the rest of the country. The biggest employer is local government.

[5] The target region for the original experiment was 14 municipalities in Lapland and six municipalities on the islands off the south-west coast of Finland.

2.1 Finnish payroll taxes

Payroll taxes in Finland consist of employer contributions to the employees' pension scheme, national pension insurance, national health insurance, employment accident insurance, and unemployment insurance. The tax rates for the various components vary across sectors and by firm size, and firms' pension contributions depend on the characteristics of their employees. The components of the payroll tax and their evolution over the 15 years from 1995 to 2009 are presented in Table 1. The largest component - contributions to the employees' pension scheme - has remained stable, while the other components have gradually been lowered after the recession in the early 1990s.

Table 1 The components of Finnish payroll taxes, percentage of the wage sum

		National pension insurance + national health insurance				Unemployment insurance			Total	
Date of change	Employees' pension scheme	I	II	III	Accident insurance	Part of wage bill under € 840,940	Part of wage bill over € 840,940	Group life insurance	Low	High
1.1.1995	16.60	4.000	5.600	6.500	1.2	2.00	6.10	0.120	23.920	30.520
1.1.1996	16.80	4.000	5.600	6.500	1.2	1.00	4.00	0.100	23.100	28.600
1.1.1997	16.70	4.000	5.600	6.500	1.4	1.00	4.00	0.090	23.190	28.690
1.1.1998	16.80	4.000	5.600	6.500	1.4	0.90	3.90	0.080	23.180	28.680
1.1.1999	16.80	4.000	5.600	6.500	1.3	0.90	3.85	0.080	23.080	28.530
1.1.2000	16.80	4.000	5.600	6.500	1.2	0.90	3.45	0.090	22.990	28.040
1.7.2000	16.80	3.600	5.600	6.500	1.2	0.90	3.45	0.090	22.590	28.040
1.1.2001	16.60	3.600	5.600	6.500	1.2	0.80	3.10	0.095	22.295	27.495
1.1.2002	16.70	3.600	5.600	6.500	1.1	0.70	2.70	0.095	22.185	27.085
1.3.2002	16.70	2.950	5.150	6.050	1.1	0.70	2.70	0.095	21.535	26.635
1.1.2003	16.80	2.964	5.164	6.064	1.1	0.60	2.45	0.081	21.545	26.495
1.1.2004	16.80	2.964	5.164	6.064	1.1	0.60	2.50	0.080	21.544	26.544
1.1.2005	16.80	2.966	5.166	6.066	1.2	0.70	2.80	0.080	21.746	26.946
1.1.2006	16.70	2.958	5.158	6.058	1.1	0.75	2.95	0.080	21.588	26.888
1.1.2007	16.64	2.951	5.151	6.051	1.1	0.75	2.95	0.080	21.521	26.821
1.1.2008	16.80	2.771	4.971	5.871	1.0	0.70	2.90	0.080	21.351	26.651
1.1.2009	16.80	2.801	5.001	5.901	1.0	0.65	2.70	0.070	21.321	26.471
1.4.2009	16.80	2.000	4.201	5.101	1.0	0.65	2.70	0.070	20.520	25.671

Notes: contribution to employees' pension scheme is the average percentage share. The actual contribution depends on firm size and the characteristics of employees. The cost of accident insurance is also an average.

2.2 Target and control regions used in the evaluation

In our evaluation of the beginning of the Lapland experiment, the comparison region we chose was in northern Finland in an area with municipalities with similar economic and demographic conditions to those in the original target region. However, the core of our comparison region was Kainuu. Therefore, it became

necessary to select a new region to work as a counterfactual for the larger experiment region.

Rather than hand-picking municipalities, I followed Bennmarker et al. and used the national firm subsidy rules to find an area where the operating environment for firms is comparable to the target region. The target region is contained in the two highest subsidy regions for the period 2000–2006 and in the highest category for 2007–2013. In the first period, firms in the Kainuu region and its surroundings to the west and south were eligible for the highest subsidies, with Lapland belonging to the second category. There was, however, a special provision for Lapland that granted firms almost the same investment and other subsidies as for firms in the first category[6]. The subsidy regimes were allocated according to EU rules, where the main factor was the level of NUTS3 region GDP per capita relative to the EU average – regions with less than 75% of the average were eligible for the highest subsidies.

The comparison region is formed of the non-target municipalities of the two highest subsidy regions for the period 2000–2006. I have excluded the largest local administrative centres and university towns (Rovaniemi, Joensuu, Kuopio and Mikkeli) and one highly industrialised region (Kemi-Tornio) as there is nothing comparable in the target region. Figure 1 shows the regions on a map. I decided to drop the target region in the archipelago from this evaluation since it would have been hard to find a credible comparison for this very distinct group of municipalities.

Table 2 highlights some important similarities and differences between the target and comparison regions and contrasts them with the rest of the country. The figures are from 2001, i.e. before the experiment had begun, but the main features are quite persistent through the whole period under evaluation. First, the part of Lapland that received the tax cut and Kainuu are very sparsely populated. The comparison region has more than four times as many inhabitants per square kilometre. However, the rest of Finland is more than five times as densely populated than the comparison region. Second, the population in both the target and control regions is declining and not growing. It is also older and less educated than the rest of the country. Third, the employment rate was markedly lower (and unemployment rate higher) in the target and control regions than in other parts of Finland. The share of municipal employees is particularly high in the Lapland and Kainuu regions and the share in the control region does not quite match that. The employment share of manufacturing is clearly lower in the target region but the shares of other industries are well aligned. I will look at the industry composition more closely when I describe the firms in the target and control regions.

[6] The subsidy scheme is quite complex (details in the Aid to Business Act, 1200/2000). To simplify, the highest share of investment subsidies in category I is 40% and in the northern part (Lapland) of category II it is 34% of the total investment.

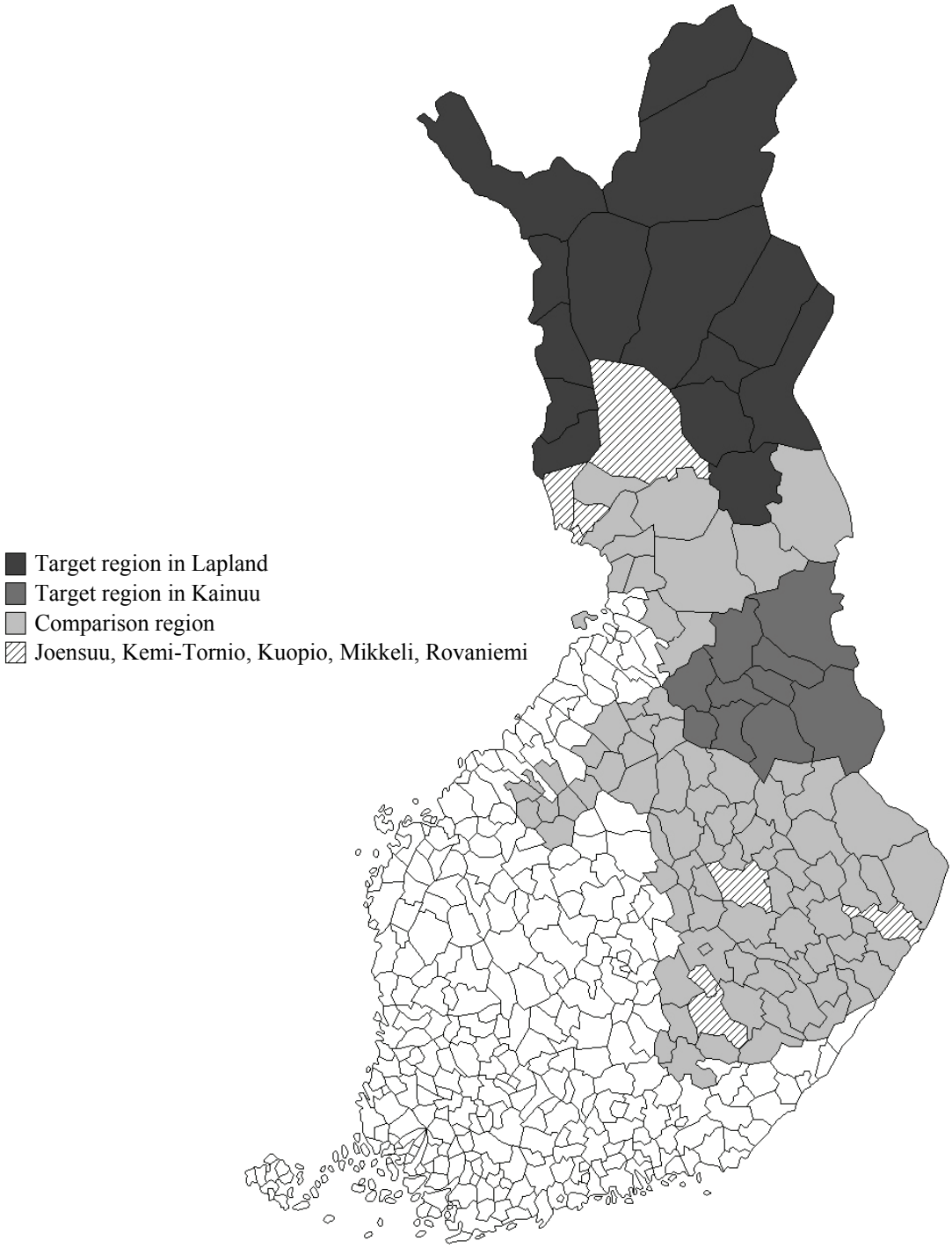
Last, if we consider the public finance situation in the region that received the tax cut, we can see that the target and control regions are very alike: both are heavily dependent on state grants to finance their public sector.

Table 2 Target and control regions in 2001

	Target	Control	Rest of Finland
Population			
Total population	153,452	522,418	4,500,946
Population density [1)]	1.56	6.54	35.94
Population growth, % / a	-1.80	-0.98	0.48
Percentage pensioners	26.96	27.78	21.01
Dependency ratio	1.94	1.85	1.28
Secondary education, % [2)]	38.15	37.60	35.68
University level education, %	16.72	15.40	25.14
Employment			
Employment rate, %	52.33	55.98	65.70
Unemployment rate, %	21.05	16.30	11.17
Municipal employees, %	21.98	18.43	13.47
Agriculture, forestry and fishing, %	10.02	13.97	3.61
Manufacturing, %	20.75	25.84	27.00
Trade, %	9.56	9.16	12.33
Municipal finance			
State grants, € / person	1,591	1,399	593
Tax revenue, € / person	2,134	2,007	2,807

Notes: 1) inhabitants / km^2, 2) Persons aged 15 or over with a degree from an upper secondary school, vocational or professional education institution, or a university. Source: ALTIKA regional statistics database by Statistics Finland.

Figure 1 Target and control regions



Notes: Lapland and Kainuu form the target region of the tax cut. Joensuu, Kemi-Tornio, Kuopio, Mikkeli and Rovaniemi are removed from the comparison region.

2.3 Target and control firms

Here I take a short look at the firm population in the tax cut's target and control regions. The first observation is that the firms are small – none of the firms that have all of their establishments situated in the combined area of the target and control regions has more than 600 employees. Furthermore, none of the firms in the target region has over 300 employees. This leads me to make one common support-type restriction for the comparison group: I drop a few large firms from the control group, as it is unclear if they are comparable to any firms in the target area. Other restrictions have to do with EU regulations on firm subsidies (firms in agriculture, fisheries and transport are not eligible for the payroll tax cut) and the technical properties of the firm and establishment data. I use only observations for firms that can be reliably linked between different registers and to all of their establishments for each year they occur in the datasets. In addition, I require that the information from all sources on the key variables is consistent[7].

The main firm-level response variables in this study are employment, wage sum and operating profit. Almost all the other variables, e.g. various attributes of the firms' workforces and financial position, are more or less endogenous and hence cannot be used as explanatory variables. Were this a matching exercise, however, these and other pre-experiment firm characteristics would be used to first match and then to assess the quality of the matches. Therefore, I gauge the validity of the quasi-experimental setting in a similar manner by comparing the target area and control area firm populations. The only control variables in the regressions I run are industry and firm age group dummies. Even though the difference-in-differences set-up should remove time-constant firm-specific (and hence region-specific) differences in levels, dissimilarities in industry growth trends should be taken into account if there are differences in the industry distributions between the target and control region firm populations. I report these distributions in Table 3. In addition to the distributions, I also calculated a normalised difference for each industry share: Imbens and Wooldridge (2009) consider this a good measure to evaluate whether the regression methods are well suited to estimate the treatment effects. Imbens and Rubin (forthcoming) argue that normalised differences exceeding one quarter in absolute value would probably indicate problems. I also calculated a *t*-statistic for each variable. If this is a reasonable thing to do for a set of inter-related dummies might be questioned, but in the case of the industry distributions, it is not of importance if they differ in a statistically significant manner. The main point is to show that the distributions are similar enough that after controlling for industry the comparison between regions is internally valid.

[7] Observations are dropped if there is conflicting information on the same variable from different sources. For example, if according to Financial Statements data a firm has three establishments but not all of those are given in the Business Register, or if there are large discrepancies in total wages or turnover from different sources, the observation is removed.

Table 3 Industry distribution of target and control region firms in 2001

	Target	Control	Normalised difference	*t*-statistic
Mining and quarrying	0.011	0.020	-0.054	-4.39
Food, beverages and tobacco	0.021	0.021	0.002	0.12
Clothes, etc.	0.009	0.010	-0.005	-0.40
Wood, paper, etc.	0.042	0.040	0.006	0.45
Petro-chemical, etc.	0.002	0.004	-0.022	-1.83
Non-metallic mineral products	0.007	0.008	-0.008	-0.65
All metal industries, except ↙	0.037	0.063	-0.085	-6.83
Electronic and optical products	0.005	0.005	0.000	0.03
Water and electricity supply	0.011	0.011	-0.002	-0.14
Construction	0.147	0.170	-0.044	-3.41
Trade of gasoline, repair & trade of motor vehicles	0.044	0.054	-0.031	-2.38
Wholesale and retail trade	0.180	0.175	0.008	0.62
Accommodation and restaurants	0.103	0.071	0.079	5.73
Information and communication	0.126	0.100	0.060	4.38
Finance and banking	0.001	0.001	0.007	0.53
Business services	0.129	0.128	0.001	0.07
Other services	0.125	0.116	0.020	1.46

Notes: Normalised difference is the difference in sample means scaled by the root of the sum of the sample variances, i.e. $\Delta_x = \frac{\bar{X}_T - \bar{X}_C}{\sqrt{S_T^2 + S_C^2}}$ and $t = \frac{\bar{X}_T - \bar{X}_C}{\sqrt{S_T^2/N_T + S_C^2/N_C}}$. Subscript *T* refers to the target group and *C* to the control group.

According to Table 3 there are some statistically significant differences in the industry dummies but the standardised differences are well under the aforementioned 0.25 in absolute value. Table 4 reports the pre-experiment values of the dependent variables (and turnover). Here it might be argued that the *t*-statistic is the more interesting measure. If, indeed, there are significant differences (in differences) in the main outcomes immediately before the experiment, a possible point of concern is whether controlling for firm fixed effects is enough to make causal inferences on the effects of the tax cut valid. In Table 4 there are no statistically significant differences at the one per cent risk level. The target region firms are somewhat smaller and their growth two years prior to the start of the experiment in Lapland was a little slower than in the control region. Even if this difference is not significant, this might call for the use of firm-specific slopes in the regressions.

Table 4 Pre-reform comparison of key variables for target and control region firms

	Mean target	Mean control	Normalised difference	t-value	N obs. target	N obs. control
Employment [1]						
2001	3.40	3.70	-0.022	-1.56	2,933	8,851
2000	3.40	3.69	-0.022	-1.54	2,894	8,620
1999	3.41	3.65	-0.019	-1.31	2,707	8,117
Employment growth [2]						
2000–2001	0.01	0.00	0.002	0.13	2,665	8,006
1999–2000	0.04	0.13	-0.028	-1.78	2,597	7,810
1999–2001	0.05	0.12	-0.019	-1.22	2,453	7,469
Wage sum, €						
2001	76,756	79,493	-0.007	-0.42	3,029	9,076
2000	69,862	76,164	-0.019	-1.29	2,894	8,620
1999	66,878	72,526	-0.018	-1.19	2,707	8,117
Wage sum growth						
2000–2001	8,652	4,500	0.018	0.96	2,761	8,231
1999–2000	4,096	5,857	-0.023	-1.38	2,597	7,810
1999–2001	13,459	9,824	0.014	0.74	2,540	7,664
Turnover						
2001	466,197	497,373	-0.009	-0.59	2,933	8,851
2000	464,644	474,579	-0.003	-0.16	2,894	8,620
1999	433,121	457,062	-0.007	-0.44	2,707	8,117
Turnover growth						
2000–2001	2,671	25,464	-0.024	-1.39	2,665	8,006
1999–2000	32,366	32,945	-0.001	-0.04	2,597	7,810
1999–2001	41,601	58,119	-0.018	-1.13	2,453	7,469
Operating profit						
2001	47,418	43,745	0.007	0.41	2,933	8,851
2000	57,472	42,665	0.015	0.83	2,894	8,620
1999	45,208	40,667	0.011	0.61	2,707	8,117
Operating profit growth						
2000–2001	-10,349	957	-0.021	-1.08	2,665	8,006
1999–2000	13,560	3,201	0.016	0.80	2,597	7,810
1999–2001	4,396	4,901	-0.003	-0.15	2,453	7,469

Notes: 1) Employment as in the Financial Statements data. 2) Measured in levels. All other growth variables are also in levels, not percentages. For the definition of the normalised difference and *t*-statistic, see Table 3.

The numbers in Table 4 are in levels but a reproduction of the table in logs yields qualitatively similar numbers, only with smaller (less significant) differences between the groups. A more rigorous way to look into the validity of the target-control grouping is to estimate the treatment effect model with dummy experiments for the pre-treatment years. This is done in the robustness checks section of the results chapter.

3. Data sets

The primary data sources are the company panel of Statistics Finland's Finnish Linked Employer Employee Data (FLEED), Business Register and the Structure of Earnings data. The FLEED company panel is compiled from the Financial Statements data and the information content is harmonised over the years. The company panel covers almost all active firms in Finland. The Business Register contains basic information on all establishments and firms.

The information on financial statements and balance sheets in the firm data come mainly from the tax authorities and are checked for consistency by Statistics Finland. The employment measure, the number of employees on a firm's payroll over the calendar year, which we used in our previous study, also came from the tax register. In this study, I use an alternative measure, the number of employees in the firm in the last week of the year. This is calculated from the FLEED employee panel[8] and was not previously available for the relevant years. I consider the cross section information on employment a more reliable measure of a firm's average annual employment than the tax register number. The Business Register data is used mainly to identify firms that reside entirely in either the target or comparison region of this study, i.e. that all establishments of a given firm are in the same area. That enables me to keep the multi-establishment firms in the data. There are only a few of those but as they are large firms, it is potentially important to keep them in the data instead of dropping them altogether. In principle, the firms with establishments both in the experiment and control regions would be very interesting cases but there are very few of those in the data and they are dropped from the sample.

The Structure of Earnings data come from Statistics Finland's data on wages and salaries, which is compiled by combining data collected by employer organisations from their members with those from Statistics Finland's own wage and salary survey. The Confederation of Finnish Industries (EK) collects comprehensive wage data from all of its member firms in October of each year. The data consists of complete payroll information, excluding top management and owners of the firms. EK member firms cover ~70% of Finnish GDP and have ~950,000 employees. The number of employees in EK member firms represents approximately half of entire private sector employment (~1.8 million in 2009). The Statistics Finland wage survey is sample-based and stratified by size category and industry classification. Wage and salary data on employees are collected from October. Only firms with five or more employees are sampled and the sample covers ~10% of workers in unorganised firms. The Structure of Earnings

[8] The employee panel includes the total working age population in Finland. The firm panel also has an employment measure: average full-time equivalent yearly labour force. However, that number is imputed for most of the small firms and hence is not applicable in this study.

wage data covers all organised employers and is *representative* of unorganised employers. However, while the firm level data consists of near-complete firm populations, the Structure of Earnings data is much more limited in scope. The wage data covers ~5% of the target region firms and ~20% of the target region employees and the samples are by no means random. There is also quite a lot of yearly variation in the number of wage records per firm. Compared to the firms' personnel as calculated from the FLEED worker panel, or what is stated in the firm register, it seems that for some years much of the personnel of some firms is missing. Therefore, the validity of the wage data is not as good as the firm data. On the other hand, the quality of the information on wages should be very good, and much better than a proxy calculated from the firm data. As long as the method of selection into the dataset does not vary between the regions (it should not), comparisons should be possible. Information on hours is less accurately measured. It is calculated as 4.345 times the regular weekly hours plus overtime. The reporting of overtime varies, and for employees with a monthly salary (two thirds of the wage data) it might be a more error-prone measure than for the workers paid by the hour.

The datasets are available for research in the research laboratory of Statistics Finland.

4. Identification

The starting point for estimating the effect of the payroll tax reduction on firm- (or individual-) level responses y_i is a regression

$$y_{it} = c_i + \lambda_t + \tau w_{it} + \mathbf{x}_{it}\boldsymbol{\gamma} + u_{it},\ t = 1,...,T, \tag{1}$$

where λ_t are year effects, w_{it} indexes the treatment[9], $\mathbf{x}_{it}$ are the firm-level control variables, c_i is the firm fixed effect and u_{it} are the idiosyncratic errors[10]. Estimation by FE or first differencing to remove c_i is standard if the treatment is uncorrelated with u_{it}. Removing firm fixed effects would also remove any systematic differences between the treatment and control groups. While focusing on the changes differences away pre-existing dissimilarities 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 the response variables. In particular, differing industrial structures may lead to different timing of the business cycle in the control and the target regions. It is easy to add region- or industry-specific time trends or their interactions to (1). It is also possible to account for differing trends for each firm:

$$y_{it} = c_i + g_i t + \lambda_t + \tau w_{it} + \mathbf{x}_{it}\boldsymbol{\gamma} + u_{it},\ t = 1,...,T. \tag{2}$$

Equation (2), a random linear trend model, is a special case of a correlated random coefficient model, which can be consistently estimated for $T \geq 3$ by first differencing

$$\Delta y_{it} = g_i + \eta_t + \tau \Delta w_{it} + \Delta \mathbf{x}_{it}\boldsymbol{\gamma} + \Delta u_{it},\ t = 2,...,T, \text{ where } \eta_t = \lambda_t - \lambda_{t-1} \tag{3}$$

and then running a fixed effects regression – or by differencing for a second time (Wooldridge 2005).

If assignment to the treatment and comparison groups is a random draw or an unconfounded natural experiment, (3) estimated with standard regression methods will yield unbiased estimates and inference. Donald and Lang (2007), Bertrand, Duflo and Mullainathan (2004), and Hansen (2007a, 2007b) consider a case with unobserved group effects that introduce dependencies in the error terms between firms within groups (Donald and Lang) or over observations of the same units over time (Bertrand & al.), and how these could be dealt with in a setting where both the number of groups and observed time periods becomes large (Han-

[9] w_{it} is a payroll tax cut indicator that is one if firm i gets the tax cut at time t and zero otherwise.

[10] See Imbens and Wooldridge (2009), section 5, for a review of programme evaluation methods under unconfoundedness and section 6 for the selection in the unobservables case.

sen). Not accounting for these group-wise or temporally correlated errors still gives the correct treatment effect estimate but invalidates inference.

In the Finnish tax cut case the number of groups is two or, at a stretch, three. Hence, the cluster sample methodology of Donald and Lang is not applicable. With two clusters, the cluster effect cannot be estimated and inference on the treatment effect estimator is impossible. I argue that in the Finnish case the test and control firms, although situated in geographically distinct areas, are actually in the same region as defined by firm subsidy rules. Therefore, it is unlikely that there would be a group effect large enough to swamp the sampling variance in sample means for treatment and control firms. Indeed, the identification (rather than the correct inference) of the tax cut effect hinges on the experiment being uncorrelated with other shocks in the target or control regions. On the other hand, it is likely that observations on the same firm are correlated over time. Therefore, I use one of the methods advocated by Bertrand et al. (2004) to take this type of error correlation into account.

In practice I first estimate (1) and (2) *without* the tax cut indicators. Then I aggregate[11] the error terms over the *target region* firms into pre- and post-treatment values and regress these on the treatment indicator, as in Bertrand et al. (2004). The treatment indicator is zero for 2001–2002 and one for 2003–2006 in Lapland and the indicator is zero over 2001–2004 and one over 2005–2006 in Kainuu. These are my preferred estimates reported in the next section.

I also ran regressions (1) and (2) directly, using policy change indicators for each year (2001,…, 2006) separately to better understand the timing of the effects and to see if experiments defined in this way obtain significant coefficients in wrong years. I comment on these and the other robustness checks in the next section. One direct observation is that estimating (1) is not sufficient: there appear to be trends in some of the response variables. Therefore, I report only results where trends are accounted for.

The original dataset has information on all relevant variables for the period 1999–2007. In the regression analysis, I use data on 2001–2006, i.e. from two years prior to the start of the experiment in Lapland until the experiment has run for two years in the Kainuu region. The main reason for doing this is to avoid using years too far from the tax change and thereby avoid mixing up the tax cut effect with other possible regionally occurring shocks. Another reason for dropping the year 2007 is the start of yet another regional employment subsidy scheme, where the experiment area partly overlaps with both the Kainuu region and the comparison region used in this study.

[11] When the response variable is in levels, the aggregation is done by taking the mean of pre and post experiment residuals. In the case of first differenced responses, the aggregate is the sum of pre- and post-experiment residuals to capture the aggregate growth in the variables.

5. Results

The impact of the payroll tax cut is explored in this section. To account for the potentially heterogeneous effects of the tax cut I consider the results for four groups:

1) *all* firms *that existed*[12] *in 2001,* i.e. two years before the experiment started and before there was any common knowledge of the experiment,

2) *a group of firms where the most capital-intensive firms and the firms with the highest turnover per employee ratio (the firms in the highest quartile of either measure) are removed*,

3) *firms where the part of payroll taxes to be deducted is well below (≤ 25,000) the maximum deduction limit, 30,000 euros a year, before the experiment starts (2001 and 2002) and hence face a lowered marginal labour cost* and

4) *the intersection of groups 2 and 3*.

Group 1 is the base group and groups 2–4 are formed from it according to the above criteria.

These groupings are designed to focus on groups of firms intuitively the most sensitive to changes in labour costs and to ascertain that the possible effect of the tax cut is not drowned out by other strategic actions by large firms[13]. Group 3 is probably the most interesting as in this group the tax cut makes hiring an extra employee cheaper and the restriction does not severely reduce the number of observations (see Table 5 for number of observations).

Some of the response variables are not defined for all observations: there are no logarithms for non-positive values, relative changes[14] are not defined for two consecutive missing values (for two consecutive zeros I set the relative change to zero) and for differences one needs two consecutive non-missing observations. I choose not to limit the observations to those where all the responses exist and therefore, in addition to the groupings, the number of observations differs across the response variables. This decision does not affect the estimates much but helps to tighten the confidence intervals by making use of all available information. I

[12] I define existence as having positive turnover and wage sum.

[13]For example, one large electronic components supplier in Lapland shifted its entire operation to China, resulting in a large employment effect, certainly not related to the experiment.

[14] I use the definition introduced e.g. in Davis, Haltiwanger and Schuh, 1996, see notes under Table 5.

report the number of observations on each response type for employment for the aforementioned groupings in Table 5. The numbers for the other response variables follow these closely and are not reported.

The most important difference between the measures (levels, logs, differences, differences in logs, differences in relative changes) is that for the levels, differences and differences in relative changes I have added an observation for firm exits. Otherwise, the last change in e.g. employment from a positive value to zero would be omitted. For logs or changes in logs, this is not possible.

Table 5 *Number of observations on the response variable Employment in firm groups 1–4 and number of firms.*

	Number of observations				Number of firms			
Group	**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
Levels								
Target	15,137	8,036	14,829	7,977	2,934	1,602	2,879	1,592
Control	45,483	26,576	44,451	26,262	8,807	5,257	8,617	5,200
Logs								
Target	12,518	6,605	12,214	6,549	2,706	1,456	2,651	1,446
Control	37,949	22,225	36,943	21,922	8,165	4,832	7,975	4,775
First differences and differences in relative changes[#]								
Target	14,953	7,922	14,646	7,863	2,934	1,602	2,879	1,592
Control	44,904	26,197	43,877	25,885	8,801	5,252	8,611	5,195
Differences in logs								
Target	11,750	6,226	11,447	6,170	2,566	1,375	2,511	1,365
Control	35,710	20,972	34,710	20,672	7,787	4,603	7,598	4,546

#) Relative changes calculated as $P = \frac{X_t - X_{t-1}}{\frac{1}{2}(X_t + X_{t-1})}$, i.e. $P \in [-2, 2]$. $P = 0$ when X is zero for periods $t - 1$ and t.

5.1 Effect on employment, wage sum and profits

I start by looking at employment in the firms. Employment is measured as the number of workers employed at the end of the year.

The coefficients of the treatment indicator from regressions where the dependent variable is employment are reported in Table 6. The first four *columns* contain estimates from regressions without firm-specific slopes, but there are controls for region and industry trends. Columns five to eight are from regressions with firm-specific slopes.

The estimates in Table 6 show no statistically significant effects on employment. All estimates where the unit of measurement is employees (row 1) are negative, meaning that the aggregate effect for the target region was also negative. The estimates are positive when the response is measured in differences-in-differences in log employment (without firm-specific trends) and diff-in-diffs in percentage terms (with and without firm-specific trends). The differences in growth estimators in the lowest row are actually not that small and show the largest "effect" for group 3, but the standard errors are far too large to warrant any conclusions regarding positive effects. While the differences in logs and differences in relative changes measure the same thing, the results differ owing to the exclusion of zero employment observations from the logs.

Table 6 Effect of tax cut on employment in firms

					Firm fixed effects			
Group	**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
First differences	-0.019	-0.039	0.034	-0.071	-0.077	-0.049	-0.045	-0.081
	(0.107)	(0.105)	(0.078)	(0.101)	(0.096)	(0.101)	(0.075)	(0.098)
Differences in logs	0.0017	0.0149	0.0058	0.0140	-0.0141	-0.0017	-0.0102	0.0000
	(0.0178)	(0.0224)	(0.0178)	(0.0226)	(0.0166)	(0.0209)	(0.0166)	(0.0208)
Differences in relative changes#	0.0338	0.0142	0.0369	0.0122	0.0127	0.0066	0.0152	0.0070
	(0.0297)	(0.0386)	(0.0302)	(0.0388)	(0.0273)	(0.0353)	(0.0277)	(0.0354)

Notes: Coefficients in **bold** are significant at 5% risk level, standard errors in (parenthesis). Columns marked as follows: 1) all firms functional in 2001, 2) firms where turnover / employee ratio and capital intensity are in the highest third are dropped, 3) firms with potential payroll tax cut ≤ 25,000 € up till 2002, 4) firms fulfilling conditions 3 and 4.

#) Relative changes are calculated as $P = \frac{X_t - X_{t-1}}{\frac{1}{2}(X_t + X_{t-1})}$, i.e. $P \in [-2, 2]$. $P = 0$ when X is zero for periods $t-1$, t. All regressions have controls for year effects and firm age. In all regressions without firm fixed effects, industry is controlled at the 4-digit level (338 classes in data) to account for industry-specific trends.

Findings from our previous study[15] and the findings from other recent studies of northern Sweden and Norway showed that a payroll tax cut is likely to push wages up. In Table 7 I report the results from regressions on firms' yearly wage sum. None of the estimates is statistically significantly non-zero. The aggregate effect on the target area wage sum is positive (group 1, row 1). The estimate coming closest to being significant is the diff-in-diffs estimator (2,427 euro) for

[15] We did not find any statistically significant effects on wage sum but some indication that wage rates had risen in service industries. Our earlier estimate for the wage effect, 1,728 euros, was a diff-in-diffs five nearest neighbours matching estimator for Lapland for the years 2003 and 2004. Curiously enough, here the diff-in-diffs estimator with firm-specific slopes for Lapland (2003–2006) and Kainuu (2005–2006) is a very close hit: 1,720 euros.

group three without firm-specific trends. The differences in relative changes estimators that were positive for employment are also positive here, but are smaller.

Table 7 Effect of tax cut on firms' wage sum

					Firm fixed effects			
Group	**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
First differences	2,732	56	2,427	-345	1,720	-29	1,425	-414
	(2,268)	(1,660)	(1,272)	(1,508)	(2,038)	(1,527)	(1,122)	(1,309)
Differences in logs	0.0134	0.0140	0.0117	0.0117	0.0014	-0.0007	0.0009	0.0001
	(0.0144)	(0.0175)	(0.0145)	(0.0175)	(0.0135)	(0.0165)	(0.0136)	(0.0166)
Differences in relative changes[#]	0.0306	0.0087	0.0300	0.0086	-0.0007	-0.0214	-0.0002	-0.0192
	(0.0202)	(0.0284)	(0.0205)	(0.0286)	(0.0173)	(0.0245)	(0.0175)	(0.0244)

See Table 6 for explanatory notes.

Compared to the wage and employment effects, the detection of profit effects is made even harder by the fact that profits is a quantity containing far more idiosyncratic and time series variation than the wage sum or employment. The measure for profits I use is operating profit. Due to changes in accounting practises, this is the only profit measure in the data that is consistent over time. As operating profits quite often show negative values (23% of the observations), the taking of logs and calculating relative changes is not very meaningful. Therefore, I took proportional measures of profits relative to the wage sum. Operating profit relative to the wage sum does not directly measure the effect of the tax cut on profits but gives an indication of whether the tax cut had an effect over and above the effect on wages. The estimation results in Table 8 show, again, no statistically significant coefficients.

Table 8 Effect of the tax cut on firms' operating profit

					Firm fixed effects			
Group	**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
First differences	8,951	1,703	1,283	-297	9,811	2,077	395	27
	(19,823)	(1,921)	(1,792)	(1,436)	(20,353)	(1,895)	(1,856)	(1,377)
Difference in the share of wage sum	0.0265	-0.0140	0.0265	-0.0147	0.0223	0.0053	0.0232	0.0048
	(0.0297)	(0.0276)	(0.0298)	(0.0278)	(0.0277)	(0.0286)	(0.0279)	(0.0288)

See Table 6 for explanatory notes.

5.2 Effect on wages and hours, individual wage records

The number of individual wage and hours observations is decent (see Table 9) but the number of firms is small compared to the total number of firms. I have maintained the same grouping (1–4) as in the previous subsection. The added groups are

5) *observations where a worker stayed in the same firm and occupation from t–1 to t* and

6) *firms on 4-digit industry common support, i.e. in industries that are found in both the treatment and control areas*.

The main reason for forming group 5 is to reduce noise. On the other hand, it could be argued that wage changes often occur in conjunction with a change of occupation or employer and therefore such movers should be kept in the data. This grouping might also be used to account for different mobility patterns across the regions but such differences do not exist. I generated group 6 to account for the fact that in the wage data there are a few firms operating in industries that were completely lacking in the target (or control) area and thus one could consider these parts of the data incomparable to the other region, even after controlling for industry.

Table 9 *Number of observations on the response variable hourly wages in worker-firm groups 1–6. Also a number of individual workers and distinct firms in the wage data.*

Group	1	2	3	4	5	6
N observations						
Levels & logs						
Target	9,556	2,110	4,271	1,256	4,473	8,270
Control	38,016	9,916	14,379	5,539	19,910	23,547
Diffs, diffs in logs, Differences in relative changes						
Target	4,951	988	2,118	598	4,095	4,142
Control	21,356	4,796	7,157	2,444	18,746	12,897
N individuals						
Levels & logs						
Target	4,004	965	1,973	632	1,944	3,595
Control	14,846	4,636	6,584	2,874	8,022	9,411
Diffs, diffs in logs, Differences in relative changes						
Target	2,013	451	946	287	1,813	1,731
Control	8,242	2,058	3,069	1,110	7,675	5,131
N firms						
Levels & logs						
Target	179	49	151	46	132	158
Control	561	211	450	180	424	412
Diffs, diffs in logs, Differences in relative changes						
Target	135	38	109	35	127	122
Control	412	149	322	123	405	311

The evidence from the wage regressions to back up the earlier result of the tax cut being channelled to higher wages is scant. I present the results in Table 10. The differences-in-differences estimates without worker fixed effects (left half of Table 10) are mostly negative and also statistically significant for group 6, workers in the industry common support, indicating a wage drop of 26 cents per hour, or 1.6 percent. When worker-specific slopes are added, all estimates become smaller in absolute value but those for group 6 retain their statistical significance. Here the standard errors are also tight enough to show that wage changes cannot have accommodated any large changes in the firms' wage sum.

Table 10 Effect of tax cut on hourly wages

Group						Worker fixed effects					
1	**2**	**3**	**4**	**5**	**6**	**1**	**2**	**3**	**4**	**5**	**6**
First differences											
-0.14	-0.03	-0.03	0.35	**-0.19**	**-0.26**	-0.11	-0.23	-0.05	-0.15	-0.10	**-0.21**
(0.09)	(0.35)	(0.17)	(0.59)	(0.09)	(0.11)	(0.08)	(0.19)	(0.10)	(0.30)	(0.07)	(0.09)
Differences in logs											
-0.0049	-0.0146	-0.0125	0.0010	-0.0072	**-0.0162**	-0.0020	-0.0089	-0.0002	0.0005	-0.0021	**-0.0117**
(0.0059)	(0.0159)	(0.0090)	(0.0205)	(0.0060)	(0.0069)	(0.0046)	(0.0101)	(0.0060)	(0.0147)	(0.0043)	(0.0053)
Differences in relative changes											
-0.0053	-0.0146	-0.0124	0.0003	-0.0074	**-0.0159**	-0.0020	-0.0085	0.0001	0.0012	-0.0020	**-0.0115**
(0.0058)	(0.0154)	(0.0087)	(0.0196)	(0.0058)	(0.0067)	(0.0045)	(0.0099)	(0.0058)	(0.0143)	(0.0042)	(0.0052)

Notes: Coefficients in **bold** are significant at the 5% risk level, standard errors in (parenthesis). Columns marked as follows: 1) all firms functional in 2001 (positive employment and wage sum), 2) firms where turnover / employee ratio and capital intensity are in the highest third are dropped, 3) firms with potential payroll tax cut ≤ 25,000 € up till 2002, 4) firms fulfilling conditions 2 and 3, 5) observations where worker stayed in the same firm and occupation from t–1 to t, 6) firms on 4-digit industry common support, i.e. in industries that are found in the treatment and control areas.

All regressions have controls for year effects. Industry is controlled up to 16 classes. The individual controls are gender, education level, age and age squared, tenure and indicators for firm or occupation change. Occupation is controlled at the 3-digit level (91 classes in the data).

The wage sum could rise more than employment without changes in hourly wages if the hours worked increase. The Structure of earnings data has information on monthly hours but how accurately it measures the actual hours worked varies across industries and depends on which collective agreement is followed. The regression results where the dependent variable is "monthly hours worked" are presented in Table 11. The only statistically significant estimates are found for group 4 when the measure is diff-in-diffs without employee-specific slopes. Most of the estimates measuring proportional changes (differences in logs, differences in relative changes) are negative.

Table 11 Effect of tax cut on monthly hours[*]

Group						Worker fixed effects					
1	**2**	**3**	**4**	**5**	**6**	**1**	**2**	**3**	**4**	**5**	**6**
First differences											
0.51	2.74	1.07	**5.40**	0.50	0.02	0.28	0.59	0.63	2.29	0.34	-0.13
(0.72)	(1.67)	(1.23)	(2.30)	(0.69)	(0.82)	(0.50)	(0.97)	(0.79)	(1.56)	(0.45)	(0.57)
Differences in logs											
-0.0002	-0.0050	-0.0047	-0.0023	-0.0011	-0.0026	-0.0002	-0.0012	0.0002	0.0002	-0.0007	-0.0009
(0.0019)	(0.0061)	(0.0026)	(0.0075)	(0.0019)	(0.0021)	(0.0012)	(0.0034)	(0.0016)	(0.0048)	(0.0011)	(0.0014)
Differences in relative changes[#]											
-0.0003	-0.0049	-0.0046	-0.0024	-0.0011	-0.0026	-0.0002	-0.0010	0.0002	0.0004	-0.0007	-0.0008
(0.0019)	(0.0059)	(0.0026)	(0.0072)	(0.0019)	(0.0021)	(0.0012)	(0.0033)	(0.0015)	(0.0047)	(0.0011)	(0.0013)

Notes: Coefficients in **bold** are significant at the 5% risk level, standard errors in (parenthesis). See Table 10 for description of the grouping.

* Hours are calculated as 4.345 times the regular weekly hours + overtime.

5.3 Robustness checks

The payroll tax experiment did not, on average, have a *statistically significant* effect on employment, firms' wage sum or operating profit at a regional level. It might have had a surprisingly negative effect on hourly wages. I did some robustness checks to scrutinise these results, but first I try to see whether the data allows any precise identification of the experiment.

5.3.1 Where did the money go?

After finding no clear effects having examined a number of measures of firms' performance, the question arises whether the experiment is too small to register in the data at all or if the quality of the data is too poor for the purpose. Compliance on the part of the firms is not a problem. Taking part in the experiment only requires firms to notify their local tax office that the firm is not going to pay the first 30,000 euros of the combined national pension insurance and national health insurance and to report the deducted amount at the end of the calendar year. According to the data from the tax authorities, practically all the firms with employees in the target area filed a starting declaration.

The most disaggregated measure in the data containing the waived part of payroll taxes (national pension insurance and national health insurance) is called "other labour costs". These labour costs are directly related to the wage sum, excluding pension contributions. The waived part is a little over half of this "other labour costs" entry. The other half consists mainly of accident, unemployment and

group life insurance payments (see Table 1). The exact amount of the reduced payroll taxes for each firm would be available from the tax records, but in order to see how reliable the firm register data is, I estimated the effect of the payroll tax reduction on other labour costs. If it were not visible at all, it would raise a serious concern about data quality. The results of these regressions are presented in Table 12.

Table 12 Effect of tax cut on firms' "other labour costs"

					Firm fixed effects			
Group	**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
First differences	-527	**-588**	**-453**	**-480**	**-711**	**-619**	**-525**	**-490**
	(341)	(187)	(159)	(168)	(307)	(176)	(146)	(159)
Differences in logs	**-0.1519**	-0.0996	**-0.1506**	-0.1002	**-0.1503**	**-0.0957**	**-0.1476**	**-0.0931**
	(0.0363)	(0.0521)	(0.0370)	(0.0524)	(0.0327)	(0.0457)	(0.0332)	(0.0460)
Differences in relative changes[#]	**-0.1477**	**-0.1560**	**-0.1456**	**-0.1547**	**-0.1611**	**-0.1719**	**-0.1576**	**-0.1678**
	(0.0333)	(0.0485)	(0.0339)	(0.0488)	(0.0308)	(0.0445)	(0.0313)	(0.0446)
Differences in share of wage sum	**-0.0232**	-0.0006	**-0.0232**	-0.0004	**-0.0227**	-0.0002	**-0.0228**	0.0000
	(0.0035)	(0.0045)	(0.0035)	(0.0045)	(0.0029)	(0.0043)	(0.0030)	(0.0043)

Notes: Coefficients in **bold** are significant at the 5% risk level, standard errors in (parenthesis). Columns marked as follows: 1) all firms functional in 2001, 2) firms where turnover / employee ratio and capital intensity are in the highest third are dropped, 3) firms with potential payroll tax cut ≤ 25,000 € up till 2002, 4) firms fulfilling conditions 3 and 4.

#) Relative changes are calculated as $P = \frac{X_t - X_{t-1}}{\frac{1}{2}(X_t + X_{t-1})}$, i.e. $P \in [-2, 2]$. $P = 0$ when X is zero for periods $t-1$, t. All regressions have controls for year effects and firm age. In all regressions without firm fixed effects industry is controlled at the 4-digit level (338 classes in data) to account for industry-specific trends.

The "other labour cost" regressions show clear reductions, but the reductions are smaller than what one would expect. The diff-in-diffs estimates are approximately 600 euro, or 2.3% of the wage sum. What is notable, however, is that in groups 2 (and 4) the effects in terms of share of the wage sum are very small or non-existent. A more careful inspection of the one-year treatment effect regressions shows that there might be a problem with the comparability of the treatment and comparison regions after all: many response variables for the target region firms for 2006, especially in Lapland, indicate that some other factors besides the tax experiment are probably driving the differences between the regions. Hence I re-estimated the models for differences so that the treatment in Lapland lasted only for two years (as in Kainuu), instead of four. The results of this exercise are given in Table 13. Now almost all the effects on other labour

costs become larger and more statistically significant – in particular the results for groups 2 and 4 change.

Table 13 Tax cut effect on firms' "other labour costs", 2-period treatment

					Firm fixed effects			
Group	**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
First differences	**-1,264**	**-870**	**-1,033**	**-783**	**-1,311**	**-867**	**-1,044**	**-783**
	(343)	(195)	(172)	(176)	(297)	(180)	(160)	(163)
Differences in logs	**-0.3783**	**-0.2568**	**-0.3770**	**-0.2543**	**-0.3573**	**-0.2172**	**-0.3554**	**-0.2133**
	(0.0393)	(0.0543)	(0.0401)	(0.0547)	(0.0369)	(0.0498)	(0.0376)	(0.0501)
Differences in relative changes[#]	**-0.3340**	**-0.2826**	**-0.3291**	**-0.2790**	**-0.3354**	**-0.2833**	**-0.3301**	**-0.2779**
	(0.0353)	(0.0497)	(0.0359)	(0.0500)	(0.0330)	(0.0462)	(0.0335)	(0.0464)
Differences in share of wage sum	**-0.0205**	**-0.0118**	**-0.0202**	**-0.0117**	**-0.0193**	**-0.0112**	**-0.0192**	**-0.0112**
	(0.0033)	(0.0045)	(0.0034)	(0.0045)	(0.0030)	(0.0042)	(0.0030)	(0.0043)

Notes: see Table 12 for explanatory notes.

However, dropping treatment status from the years 2005 and 2006 in Lapland does not change the results for any other responses – none of the firm-level responses becomes statistically significant and the possible negative effect on wages remains the same.

5.3.2 The role of firm exits

The proportional diff-in-diffs estimator for employment is larger than the changes in log's estimator (Table 6). As the changes in log's estimator omits the effect of firm exits, the result gives an indirect indication that the tax break might have helped some firms to continue operating rather than exiting. The yearly number and share of exits of firms that existed in 2001 are given in Table 14. The definition of exit is based on information in Statistics Finland's firm and establishment registers: a real exit occurs during a year if the firm code and all establishments linked to it at the end of the previous year disappear from the register.

Table 14 Share and number of firm exits in target and control regions*

	Lapland		Kainuu		Control region	
	Share of exits	# of exits	Share of exits	# of exits	Share of exits	# of exits
2002	0.0267	39	0.0357	51	0.0301	261
2003	0.0273	38	0.0407	55	0.0249	204
2004	0.0248	33	0.0317	40	0.0238	186
2005	0.0252	32	0.0319	38	0.0257	191
2006	0.0284	34	0.0264	30	0.0274	194

* Exits are defined based on firm and establishment registers. An exit has occurred if the firm identifier and all establishments linked to it disappear from the register in the year following the potential exit. The shaded area in the table indicates the tax experiment.

I estimated the effect of the tax experiment on exits using a Cox proportional hazards model where I controlled for the treatment area (Lapland and Kainuu separately), industry and the firm's age group. The coefficient of interest is the tax cut indicator. I used first the same dataset as in the regressions on employment. Then I estimated the model for an extended time period where I included all firms functional in 1999 in order to have more pre-experiment years for Lapland. The effects of the tax cut on firm exits are similar and they are not statistically significant in either case. I report the results only for the 2001–2006 period (see Table 15).

Table 15 Effect of tax cut on firms' exit probability. Odds ratios from proportional hazards model.

Group			
1	2	3	4
0.9193	1.0382	0.9090	1.0253
(0.1097)	(0.1486)	(0.1093)	(0.1469)

Notes: Odds ratios in **bold** are significant at the 5% risk level, standard errors in (parenthesis). Columns marked as follows: 1) all firms functional in 2001, 2) firms where turnover / employee ratio and capital intensity are in the highest third are dropped, 3) firms with potential payroll tax cut ≤ 25,000 € up till 2002, 4) firms fulfilling conditions 3 and 4.

The use of a stock sample, i.e. firms that exist in a certain time period, would be problematic if my interest was in the duration dependence of firm survival. Here the focus is on the effect of the tax cut on the existing firm population and hence the oversampling of long-standing firms would be a different issue.

5.3.3 Other subgroups

The results were derived for subgroups that I considered sensible but that were somewhat arbitrary. Here I comment on the results for a number of groupings, omitting the tables for the sake of brevity. The first group comprises firms that

existed throughout the entire observation period of 2001–2006. This is a potentially more stable group of firms, and a group that was exposed to the experiment for the longest time span. This kind of constraint also excludes exiting firms from the sample. On the one hand, exits most definitely belong to the data as a vital part of firm dynamics. On the other hand, exits of large firms could have a substantial effect on total employment in the region and it could be argued that relocating a manufacturing firm to China or Estonia is not directly related to small changes in payroll taxes. Therefore excluding these events would be justified. These results, however, are very close to those obtained for group 1. If one were to look very carefully, the drop in the other labour cost variable is largest and the rise in the wage sum biggest in this group, but the differences are small.

To account for the fact that the wage regressions were run on a small subgroup of the firm data, I also ran the firm-level regressions for the same subgroup of firm-year observations that occur in the wage data. The possibly negative wage effect is indeed mirrored in these results. Most of the coefficients in the wage sum regressions are negative but do not differ significantly from zero. The estimates of changes in operating profit are much larger than for the complete firm data. Owing to the small sample size, none of the effects is statistically significant.

For two thirds of the workers in the wage data, hourly pay is calculated from monthly wages and information on actual hours worked. If the information on hours were for some reason less reliably recorded than wages for this group, it could lead to a blurring of the results. I ran separate regressions for workers with hourly wage and monthly salary and the results do not reveal any large differences. The wage effects (still mostly insignificant) are, however, consistently more negative for hourly paid workers and the monthly hours effects are larger for salary earners. The difference between the coefficients is insignificant.

The small firms (group 3) were defined in terms of their pre-experiment payroll tax payments. Another way of defining small firms is to use a clear personnel limit. If I apply a limit of a maximum of 10 employees on any of the pre-experiment years, I capture 90% of the firms. They account for almost exactly half of the total employment in the target and control area firms. The results for this group are very close to the group 3 results.

The last subgroup I considered was the large firms, defined as being above the maximum payroll tax cut for both 2001 and 2002. This is a small group and does not weigh too much in the firm level regressions. None of the treatment effects is statistically significant for them alone. One reassuring finding amidst the non-existent effects was that the differences-in-differences estimate with firm-specific slopes for the reduction in other labour costs is 23,483 euros for the basic pre-post estimate and 28,539 euros when the treatment is limited to the two post-experiment years in Lapland. This gives an indication that the data correctly captures the maximum tax cut for this group of firms.

5.3.4 Yearly (placebo) experiments

In my regression set-up, the last stage regression is run on observations of one pre- and one post-experiment observation per firm. As there are several pre- and post- experiment years in the original data, it makes it possible to estimate a yearly effect separately for each year. If statistically significant differences appear even before the experiment started, this could indicate that something other than the payroll tax experiment is driving the results.

Employment. None of the yearly effects is statistically significant. There also does not seem to be any pattern to these effects, neither in the aggregates nor if I look at Lapland and Kainuu separately.

Wage sum. A negative effect on the *level* of the wage sum can be seen to originate from a downward trend in treatment area wage sums compared to the control area. The estimates for the first observation years are positive and the estimates decline year by year. None of the one-year estimates for first differenced responses is statistically significant.

Operating profit. None of the one-year effects is statistically significant. The diff-in-diffs-type estimates are greatest and positive for the start year of the experiment (2003 in Lapland and 2005 in Kainuu), giving some weak evidence that the firms might have pocketed the savings.

Hourly wage. The significant and negative effect obtained for the industry common support group (group 6 in Table 10) originates from the year the experiment started in Kainuu (2005), where there was a clear overall wage drop of three and a half per cent. In Lapland, however, there is a positive effect of 1.7 per cent in 2003 – after a negative and significant effect of 3.4 per cent in 2002. The other one-year effects are not statistically significant. That the negative effect occurs only in Kainuu and that there are statistically significant effects in "wrong" years implies that the observed negative effect has probably more to do with Kainuu-specific firm dynamics than with the payroll tax experiment.

The overall conclusion from the yearly effect exercise is that a) in most cases the standard errors are large compared to the pre-post regressions and b) the results for profits and wage sum give (very weak) support to the previous findings in the sense that the timing of the effects seems correct and c) the negative trend in the level of the wage sum implies that the firm- or region-specific trends are important and the fixed effects regressions on levels and logs are mis-specified.

5.3.5 Heterogeneous treatment

The size of the tax cut depends on the payroll tax class of the firm and could vary from three to six per cent of the wage sum, up to the maximum reduction of

30,000 euros. It would therefore appear reasonable to use the actual reduction percentage as the treatment, not just a dummy for being in the target region. Almost all of the small firms (groups 2–4) belong to the lowest payroll tax category, however, and even though I can classify the larger firms quite accurately into the right categories, there are bound to be some errors. That makes the usefulness of differentiated treatment level assignment less useful than it first appears. The actual percentage reduction in payroll taxes is also endogenous for the large firms, where the tax cut is topped at 30,000 euros, since it depends on the observed wage sum. I ran the regressions with the percentage reduction as the treatment but the results were almost identical with those obtained with a treatment indicator.

5.3.6 Lapland and Kainuu as separate experiments

I estimated models separately for the Lapland and Kainuu regions, keeping the comparison region constant. Keeping in mind the results for the "other labour costs" regression indicating that the year 2006 might be a problem in Lapland, I also estimated two-year treatment effects for Lapland.

The differences between the results concerning employment are small. The estimates for employment effects are close to each other for Lapland and Kainuu and none is statistically significant. The effect on the wage sum is larger in Kainuu but again not statistically significant, whereas the estimates for changes in operating profit are larger in Lapland, still without being significantly non-zero. The hourly wage regressions reflect the results already reported in the one-year effects section; there is a negative and significant effect in Kainuu and a positive and significant effect in Lapland that cancel each other out, yielding no overall effect. This is the largest difference in results compared to the Korkeamäki and Uusitalo paper.

5.3.7 Seasonality of employment and the data

The employment measure for a firm used in this study is the number of workers with an employment relationship with it at the end of the year. The wage sum and profit measures are for the accounting period, which is most often the calendar year. The wage records are for the October of each year. This time pattern could hide some effects in wages and employment. Spring and summer are the high seasons for tourism in Kainuu and Lapland and one could argue that a temporary reduction in labour costs would have the largest effect on short-term work contracts during the high season. Unfortunately, municipality-level monthly employment figures are not available. The only source would be the Labour force survey, but there the sample size in the treated region is not large enough for this purpose. Quarterly employment figures are available to a decent approximation of the target and control areas, however, and they show that employment is at its lowest in the first quarter, then rises 5–9 per cent for the middle quarters, after

which it drops in the last quarter to close to the first quarter value. The pattern across regions is stable and similar and does not give rise to any concerns that seasonality masks some effects from the experiment.

5.3.8 Selecting the observation period

Dropping years from the beginning or the end of the current observation period of 2001–2006 has little effect on the results. The aforementioned trouble with 2006 in Lapland is to some extent present for Kainuu as well. Dropping that year lowers all the estimates a little, although none change their sign. Dropping the year 2001 has the opposite effect, i.e. the effects become somewhat larger but all remain well under the limit of becoming statistically significant.

6. Discussion and some conclusions

The main results of this study are that the payroll tax cut did not have a statistically significant effect on total employment, the wage sum, profits or wages in the target area. Most of the estimates are positive but unfortunately the standard errors are so wide that they could accommodate values indicating full shifting of the tax cut to either the wage sum, profits or, indeed, to employment. If we look at the euro-valued point estimates, the conclusion would be that the wage sum in the target region firms rose, employment did not and profits grew the most. Alternatively, if we consider the point estimates of the percentage changes, employment and the wage sum did grow by an equal amount and profits did not react. The only unambiguous finding is that the tax cut can be found from the Financial Statements data, although even there there was some uncertainty in the case of small and the least capital-intensive firms.

The effect on hourly wages found in Korkeamäki and Uusitalo (2009) is not found here for the combined target region of Kainuu and Lapland. The effect is still found for Lapland – but the estimates for Kainuu would imply a negative wage effect. The results also show one statistically significant change in a non-experiment year and hence do not warrant any strong conclusions.

Irrespective of the findings from this and other similar experiments in the Nordic countries, national pension insurance payments have been gradually lowered over recent years. From the beginning of 2010 they were abolished altogether, on the grounds that it would be beneficial for employment. There was some debate if this was the most effective way to help firms generate jobs, but empirical facts had a rather small role in the discussion. This is partly due to a lack of such facts.

The Finnish payroll tax experiment is a rare example where a tax change is made in an experimental setting with the stated aim of facilitating economic research. Hence it is important to evaluate it, even if the results tell rather little about the effects on employment. This is also an opportunity to gather information on the experiment itself to learn more about how possible future experiments should be designed and implemented to the greatest scientific advantage. Based on my results I argue that it is still important to continue experimenting – it is also important to pre-evaluate future experiments to see if they are likely to yield accurate and reliable results.

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