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# The Finnish payroll tax cut experiment revisited, or where did the money go?[1]

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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 firm's wage sum in Northern and Eastern Finland. I estimate the effect of the payroll-tax reduction on firms' employment, wage sum and profits, and on workers hourly pay and monthly hours worked, by comparing employment and wage changes before and after the start of the experiment to a control region. According to my results, the reduction in the payroll-taxes did not lead to any clear cut aggregate effects in the target region.

*Keywords: Payroll-tax, Labour demand, Tax incidence*
JEL Classification J18, J23, J38, J58, J65, J68

## 1 Introduction and background

The main objective of this project is by careful empirical analysis to estimate the average effect of a payroll tax cut on employment, wages and firm profitability. This effect is likely to be heterogeneous: different firms according to size, capital intensity, worker turnover, etc. probably experience differing effects. Keeping in mind the restrictions of a regionally narrow experiment, it is possible to some extent probe these differentials. The Finnish payroll tax experiment that started in 2003 was originally limited to duration of three years, but the extension until 2012 makes it likely that firms might make investment decisions and other adjustments targeted for longer time spans.

There is a rather strong consensus of the labour market effects of payroll taxes. A textbook model states that a reduction of 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 has no effect on employment and if the labour supply is fully elastic, then the tax cut results in higher employment. Empirical studies (with micro data and quasi-experimental settings) on this subject include, Gruber (1994) that evaluates the effects of mandated maternity benefits in the US and Anderson and Meyer (1997) and Murphy (2007) that examine the incidence of unemployment insurance taxes. In these cases, the

[1] This research was financed by the Ministry of Employment and the Economy. I am grateful to Roope Uusitalo and Kari Hämäläinen for comments and suggestions that considerably improved the paper.

changes in the payroll tax rates vary between the firms because of a different composition of their labour force or because of the tax rates depend on firm characteristics. Another approach is to examine the effects of regional policies that create different changes in the payroll tax rates across firms that are located in different regions but that are otherwise comparable. Prime examples include Bohm and Lind (1993) who evaluate employment effects of regional wage subsidies in the Northern Sweden, Johansen and Klette (1998) who examine the effects of regional differences in payroll taxes in Norway, 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 the changes in the payroll taxes are partly shifted into wages with little effect on employment[2]. An exception is Crépon and Desplatz (2002) evaluating payroll tax subsidies for low-wage workers. In this French case, the writers 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 observations have dented these theories to some extent[3]. In their book on a closely related subject, minimum wages[4], Card and Krueger (1995) consider the possible mechanisms of how minimum wage changes could affect firms' profits[5]. Lacking suitable micro data for direct measurement, they use the changes in firm's stock market valuation as an indicator of a change in profits. Then the changes in minimum wage legislation (or news of the changes) are used as instruments to identify the minimum wage effect on stock prices and the implied change in profits is calculated. Card and Kruger find tentative evidence that news about a minimum wage hike induces 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

[2] See Bennmarker et al. for a short review of the previous studies.
[3] There is quote attributed to Paul A. Samuelson that "In economics it takes a theory to kill a theory; facts can only dent a theorist's hide."
[4] In the part of the wage distribution where minimum wage rules are binding, the effect of minimum wages can be considered 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.
[5] The focus of the book is on employment effects of minimum wages but there is a chapter on how much profits change. Unlike the payroll tax case, the standard economic theory unambiguously implies that wage floors have a negative impact on employment (Borjas 2004, Brown 1999). Empirical evidence is considerably more mixed; see the comprehensive review by Neumark and Wascher (2007).

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 prises (exceptions are Aaronson 2001 and Aaronson and French 2007). Draca et al. find a significant reduction in profits and a raise 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 the 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 on payroll taxes. In March 2002, the Finnish government agreed on a temporary removal of employer contributions to the National Pension Insurance and the National Health Insurance for firms that operated in the twenty target municipalities in Lapland and the Archipelago. The removal of these contributions lowered the payroll taxes for the eligible firms by 4.1 percentage points, on average. The program was designed as an experiment with a stated aim to evaluate the effect of a cut in the payroll taxes on employment in the target region. The payroll tax exemption was planned for duration of three years, from January 1 2003 to December 31 2005. Already in May 2003, the government had decided to start a regional self-government experiment in Kainuu, eastern Finland, beginning from 2005. That experiment contained a similar provision for lowered payroll taxes as the Lapland experiment and hence enlarged and extended the payroll tax experiment until the end of 2009. The experiment has been further continued until the end of 2012.

To sum up the current situation and motivate the need to asses if the 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 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 also a common finding from Sweden and Norway. From 1) and 2) and supported by the UK case of minimum wage change it seems likely that changes in payroll tax 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, better data and more years of observations. However, I still do not find 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 the 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 occurred a region specific shock in Kainuu causing the negative effect but I found no reason to believe that the result from Lapland would be the trustworthy one.

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 into 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

In the turn of the millennium there was an ongoing debate over the relative merits of over 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 the National Pension Insurance and the National Health Insurance (see Table 1) for firms that operated in the twenty target municipalities[6]. The program was designed as an experiment with a stated aim to evaluate the effect of a cut in the payroll-taxes on employment in the target region. The 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 euro per year for each firm and the already heavily subsidised industries of agriculture, fishing and transport are 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 in the Finnish Parliament in February 2003 and the experiment started on January 1, 2005. The aim of the self-government experiment is to gain experience of the effects of the regional self-government enforcement on regional development work, basic services, citizen activity, the relationship between the regional and the state central government as well as the municipal and the state local government. The Kainuu experiment

[6] The target region for the original experiment was fourteen municipalities in Lapland and six municipalities on the islands off the South-West coast of Finland.

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 gives the payroll tax cut also to the 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 Belgium, but only a population of 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 most important employer is the local government.

## 2.1 Finnish payroll taxes

Payroll-taxes in Finland consist of employer contributions to the Employees' Pension Scheme, the National Pension Insurance, the National Health Insurance, the Employment Accident Insurance, and the Unemployment Insurance. The tax rates of various components vary across sectors and by firm size and the firms' pension contributions depend on the characteristics of the employees. The components of the payroll tax and their evolution over the fifteen years from 1995 to 2009 are presented in Table 1. The largest component, contributions to employees' pension scheme, has remained stable, while the other components have gradually been lowered after the early 1990's recession.

*Table 1 The Components of the Finnish payroll taxes, percent of the wage sum*

| Date of change | Employees pension scheme | National Pension Insurance + National Health Insurance I | II | III | Accident insurance | Unemployment Insurance Part of wage bill under € 840,940 | Part of wage bill over € 840,940 | Group life insurance | Total 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 percent share. The actual contribution depends on firm size and the characteristics of the employees. Accident insurance cost is also an average.

## 2.2 Target and control regions used in the evaluation

In our evaluation of the beginning of the Lapland experiment, we chose the comparison region from Northern Finland where there are 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, the firms in the Kainuu region and its surroundings to the West and South are eligible for the highest subsidies and Lapland belongs to the second category. There was, however, a special provision for Lapland that granted the firms almost the same investment and other subsidies as the firms in the first category[7]. The subsidy regimens were allocated according to EU level rules, where the main factor was the

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

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 none 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.

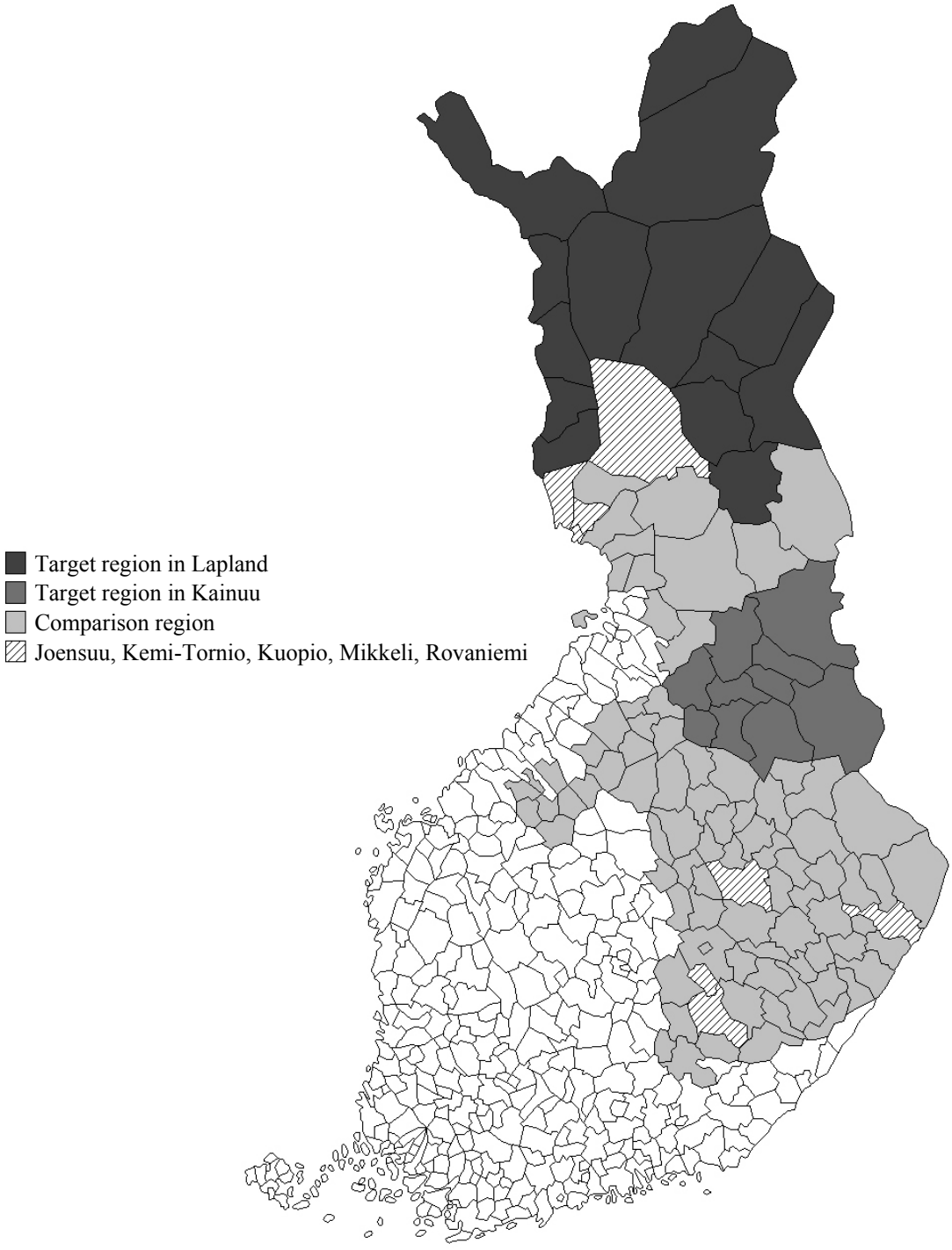
Table 2 highlights some important similarities and differences between the target and comparison regions and contrasts them with the rest of the country. The figures are from 2001, i.e. before the experiment had begun, but the main features are quite persistent through the whole period under evaluation. First, the part of Lapland that received the tax cut and Kainuu are very sparsely populated. The comparison region has more than four times more inhabitants per square kilometre. However, the rest of Finland is more than five times more densely populated still than the comparison region. Second, the population in both target and control regions is diminishing instead of 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 Lapland and Kainuu regions and the share in the control region does not quite match that. The employment share of manufacturing is clearly on a lower level in the target region but the shares of other industries are rather nicely aligned. I will look at the industry composition more closely when I describe the firms of the target and control regions. 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 much 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 |
| Percent 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 who have a degree from a senior secondary school, vocational or professional education institution, or from 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 its establishments situated in the combined area of 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 are owing to the EU-regulations of firm subsidies (firms in industries agriculture, fishing and transport are not eligible for the payroll tax cut) and the technical properties of the firm and establishment data. I use only observations on firms that can be reliably linked over different registers and to all of their establishments each year they occur in the datasets. In addition, I require that information from all sources on key variables is consistent[8].

The main firm level response variables in this study are employment, wage sum and operating profit. Almost all other variables, e.g. different attributes of the firms' work force 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 to gauge the validity of the quasi-experimental setting in a same 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 wise 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 distributions, I also calculated a normalised difference for each industry share: Imbens and Wooldridge (2009) consider this a good measure to evaluate whether 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

[8] 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 found from the Business Register, or if there are large discrepancies in wage sum or turnover from different sources, the observation is removed.

is to show that the distributions are similar enough that after controlling for industry the comparison between regions is internally valid.

*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 target group and *C* to 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 right before the experiment, it might be of concern whether controlling for firm fixed effects is enough to make causal inference on the effects of the tax cut valid. In Table 4 there are no statistically significant differences in one percent risk level. The target region firms are somewhat smaller and their growth two years prior to the start of the experiment in Lapland has been a tad 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 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 the Statistics Finland's Finnish Linked Employer Employee Data (FLEED), Business Register and the Structure of Earnings data. FLEED company panel is compiled from the Financial Statements data and the information content is harmonised over years. The company panel covers almost all functioning firms in Finland. 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, number of employees on a firm's payroll over the calendar year, which we used in the previous study, also came from the tax register. In this study, I use an alternative measure, the number of employees in the firm during the last week of the year. That is calculated from the FLEED employee panel[9] and was not previously available for the relevant years. I consider the cross section information on employment a more reliable measure of 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 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 having establishments both in 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.

Structure of Earnings data derive from Statistics Finland's data on wages and salaries, which are formed by combining data collected by employer organisations from their members with those from Statistics Finland's own wage and salary inquiries. The Confederation of Finnish Industries (EK) collects in October, each year, comprehensive wage data on all of its member firms. The data consists of complete payroll information excluding top management and owners of the firms. The EK member firms cover ~70% of the Finnish GDP and have ~950.000 employees. The number of employees in EK member firms is approximately one-half of the whole private sector employment (~1.8 million in 2009). The Statistics Finland wage inquiry is sample-based, stratified on 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 the workers in unorganised firms. The Structure of Earnings wage data

[9] 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 usable in this study.

covers all organised employers and is *representative* of the unorganised employers. However, while the firm level data consists of almost 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 calculated from the FLEED worker panel, or what is stated in the firm register, it seems that for some years a large number of 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, much better than some proxy calculated from the firm data. As far as the selection into the data does not vary between the regions (it should not), the comparisons should be possible. Information on hours is less accurately measured. It is calculated as 4.345 times the regular weekly hours plus overtime. 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 data sets are available for research in the research laboratory of Statistics Finland.

# 4 Identification

The starting point to 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 $\lambda_t$ are year effects, $w_{it}$ indexes the treatment[10], $\mathbf{x}_{it}$ are the firm level control variables, $c_i$ is the firm fixed effect and $u_{it}$ are the idiosyncratic errors[11]. Estimation by FE or first differencing to remove $c_i$ is standard if treatment is uncorrelated with $u_{it}$. Removing firm fixed effects would also remove any systematic differences between 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:

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

[10] $w_{it}$ is a payroll tax cut indicator that is one if firm $i$ gets the tax cut at time $t$ and zero otherwise.
[11] See Imbens and Wooldridge (2009), section 5, for a review of program evaluation methods under unconfoundedness and section 6 for the selection on unobservables case.

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

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

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

If the assignment to 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 the case when there exists unobserved group effects that introduce dependencies in error terms between firms within groups (Donald and Lang) or over observations of the same units over time (Bertrand & al.) and how these might be dealt with in a setting where both the number of groups and observed time periods becomes large (Hansen). 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, with 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, albeit residing in geographically distinct areas, are actually on the same region defined by the firm subsidy rules. Therefore, it is unlikely that a group effect large enough to swamp the sampling variance in sample means for treatment and control firms would exist. Indeed, the identification (instead of correct inference) of the tax cut effect hinges on the experiment being uncorrelated with other shocks onto the target or control regions. On the other hand, it is rather 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 practise I first estimate (1) and (2) *without* the tax cut indicators. Then I aggregate[12] the error terms over *target region* firms to 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

[12] 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.

2001–2004 and one over 2005–2006 in Kainuu. These are my preferred estimates reported in the next section.

I also ran the 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 get significant coefficients in wrong years. I comment on these and 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. Main reason for doing this is to avoid using years too far from the tax change and thereby avoid mixing the tax cut effect with other possible regionally occurring shocks. Another reason for dropping the year 2007 is a 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.

# 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*[13] *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 union of firms in the highest quartile of either measure) are removed*,

3) *firms where the part of payroll taxes to be removed is well below (≤ 25,000) the limit of the maximum deduction, 30,000 euro 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.

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

These groupings are defined 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 by other strategic actions of large firms[14]. Group 3 is probably the most interesting as in this group hiring an extra employee becomes cheaper owing to the tax cut and the restriction does not severely cut the number of observations (see Table 5 for number of observations).

Some of the response variables are not defined for all observations: logarithms do not exist for non-positive values, relative changes[15] 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 only those where all 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 report the number of observations on each response type for employment for the aforementioned groupings in Table 5. Numbers for other response variables follow closely these and are not reported.

The most important difference between the measures (levels, logs, differences, differences in logs, differences in relative changes) is that for 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.

[14] E.g., one large electronic components supplier in Lapland shifted its entire operation to China, resulting in a large employment effect, surely not tied to the experiment.
[15] I use the definition introduced e.g. in Davis, Haltiwanger and Schuh, 1996, see notes under Table 5.

*Table 5 Number of observations on 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 the employment of the firms. Employment is measured as the number of workers employed at the end of the year.

Treatment indicator's coefficients 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-diff's 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 a largest "effect" for group 3, but standard errors are far too large to warrant any conclusions of 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 Tax cut effect on firms' employment*

| | | | | | 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 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 € until 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 in 4-digit level (338 classes in data) to account for industry specific trends.

Findings from our previous study[16] 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-diff estimator (2,427 euro) for group three without firm specific trends. The differences in relative changes estimators that were positive for employment are here also positive but smaller.

[16] 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 euro, was a diff-in-diff five nearest neighbours matching estimator for Lapland for years 2003 and 2004. Curiously enough, here the diff-in-diff estimator with firm specific slopes for Lapland (2003–2006) and Kainuu (2005–2006) is a very close hit, 1,720 euro.

*Table 7 Tax cut effect 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 the 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 show rather often (23% of the observations) negative values, the taking of logs and calculating relative changes is not very meaningful. Therefore, I took the proportional measures of profits relative to wage sum. Operating profit relative to wage sum does not measure directly the effect of the tax cut on profits but gives an indication if 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 Tax cut effect 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. 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 occur on both treatment and control area*.

The main reason to forming group 5 is to reduce noise. On the other hand, it could be argued that wage changes occur often in conjunction with a change of occupation or employer and therefore these 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 the target (or control) area was completely lacking and thus one could consider these parts of the data uncomparable to the other region, even after controlling for industry.

*Table 9* *Number of observations on response variable hourly wage 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 |
| Diff's, diff's 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 |
| Diff's, diff's 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 |
| Diff's, diff's 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 the Table 10) are mostly negative and also statistically significant for group 6, the 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 six 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 Tax cut effect on hourly wage*

| **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 5% risk level, standard errors in (parenthesis). Columns marked 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 € until 2002, 4) firms fulfilling conditions 2 and 3, 5) observations were worker stayed in the same firm and occupation from $t$–1 to $t$, 6) firms on 4-digit industry common support, i.e. e. in industries that occur on treatment and control area.

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

Wage sum could rise more than employment without changes in hourly wages if the hours worked increased. 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 four when the measure is diff-in-diff's without employee specific slopes. Most of the estimates measuring proportional changes (differences in logs, differences in relative changes) are negative.

*Table 11 Tax cut effect 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 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 surprising negative effect on hourly wages. I did some robustness to put these results under scrutiny, but first I try to see whether the data allow any certain identification of the experiment.

### 5.3.1 Where did the money go?

After finding no clear effects on quite a few measures of firms' performance, the question arises whether the experiment is too small to register in the data at all or if the data is of too poor quality for the purpose. Compliance from the part of the firms is not a problem. Taking part into the experiment requires the firms only to notify the local tax office that the firm is not going to pay the first 30,000 euro 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 firms that have employees in the target area have filed a starting declaration.

The most disaggregated measure in the data that contains the abolished part of payroll taxes (National Pension Insurance and National Health Insurance) is called "other labour costs". They are the labour costs directly related to wage sum, excluding pension contributions. The abolished part is a little over a 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 the other labour costs. If it were not visible at all, it would raise a serious concern of data quality. Results from these regressions are presented in Table 12.

*Table 12 Tax cut effect 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 5% risk level, standard errors in (parenthesis). Columns marked 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 € until 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 in 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-diff estimates are approximately 600 euro, 15%, or 2.3 percent of the wage sum. What is notable, however, is that in groups two (and four) the effects in share of wage sum are very small or non-existent. A more careful inspection of the one year treatment effect regressions shows that the there might be a problem with the comparability of treatment and comparison regions after all: many response variables for 2006 of the target region firms, especially in Lapland, indicate that some other factors on top of 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 (like in Kainuu), instead of four. Results of this exercise are in Table 13. Now almost all effects on the other labour costs become larger and more statistically significant – especially the results for groups two and four 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 the treatment status from 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-diff's 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 operation instead of exiting. The yearly number and share of exits of firms that existed in 2001 are reported Table 14. The definition of exit is based on information in the Statistics Finland's firm and establishment registers: a real exit occurs during a year if the firm code and all establishments that were 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 on 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 with a Cox proportional hazards model where I controlled for treatment area (Lapland and Kainuu separately), industry and firm's age group. The coefficient of interest is the tax cut indicator. I used first the same dataset as in 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 tax cut effects on firm exits are similar and they are not statistically significant in either case. I report the results only for 2001–2006 period (see Table 15).

*Table 15 Tax cut effect 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 5% risk level, standard errors in (parenthesis). Columns marked 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 € until 2002, 4) firms fulfilling conditions 3 and 4.

The use of a stock sample, i.e. firms that exist on 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-lived firms is not a similar issue.

### 5.3.3 Other subgroups

The results were derived for subgroups that I considered sensible but that were somewhat arbitrary. Here I comment the results for a few more groupings, omitting the

tables for the sake of brevity. First alternative are the firms that existed throughout the entire observation period 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 limits the exiting firms outside the sample. On one hand, the 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 the total employment of 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 the ones 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 ran the firm level regressions also 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 the hourly pay is calculated from a monthly wage and information on actual hours worked. If the information on hours were for some reason less reliably recorded than the wage for this group, it could lead to 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 the hourly paid and the monthly hours effects are larger for the 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 use the limit of employing a maximum of ten employees on any of the pre-experiment years, I capture 90% of the firms. They employ almost exactly one-half of the total employment in target and control area firms. Results for this group are very close to group 3 results.

Last subgroup I considered was the large firms, defined as being above the maximum payroll tax cut for both years 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. As a reassuring finding amidst the non-existent effects, the

differences in differences estimate with firm specific slopes for the reduction in the other labour costs is 23,483 euro for the basic pre-post estimate and 28,539 euro when the treatment is limited to 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 setup, 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 already before the experiment started it gives reason to suspect that something else 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 on 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 originating from a downward trend in treatment area wage sums compared to the control area. The estimates for 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-diff's type estimates are the largest 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 percent. In Lapland, however, there is a positive effect of 1.7 percent in 2003 – after a negative and significant effect of 3.4 percent in 2002. 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 on 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 would be 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 percent of the wage sum, up to the maximum reduction of 30,000 euro. It would therefore appear reasonable to use the actual reduction percent 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 euro 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 Lapland and Kainuu regions, keeping the comparison region constant. Keeping in mind the results on the "other labour costs" regression that the year 2006 in Lapland might be a problem, I also estimated 2-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 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 reported already in the one-year effects section; there is a negative and significant effect in Kainuu and 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 having an employment relationship to 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, it 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 the lowest level in the first quarter, then raises 5–9 percent for the middle quarters, after which it drops in the last quarter close to the first quarter value. The pattern across regions is stable and similar and does not give rise to a concern that seasonality would mask 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 estimates a little, although none change their sign. Dropping 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 from this study are that the payroll tax cut did not have a statistically significant effect on the total target area employment, wage sum, profits or wages. 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 the profits grew the most. Alternatively, if we consider the point estimates of percent changes, employment and wage sum did grow 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 was some uncertainty in the case of small and 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 findings from this and other similar experiments in Nordic Countries, the National Pension Insurance payments have been gradually lowered over the 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 were 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 the lack of those facts.

The Finnish payroll tax experiment is a rare example where a tax change is made in an experimental setting with the stated aim to facilitate 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 of how the 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 the future experiments to see if they are likely to yield accurate and reliable results.

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