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Do regional payroll tax reductions boost employment?

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Do regional payroll tax reductions boost employment?*

by

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Abstract

Using a Difference-in-Differences approach we evaluate the effects of a 10 percentage points reduction in the payroll tax introduced in 2002 for firms in the northern part of Sweden. We find no employment effects for existing firms and can rule out that a 1 percentage point payroll tax reduction would increase employment with more than 0.2 percent. We do, however, find that tax reductions have significantly positive effects on the average wage bill per employee. These are likely to be driven by higher average wages, but might also be due to more hours worked. As a sensitivity check we investigate if reduced payroll taxes affect the likelihood of firm entry and exit, and find some support for a net firm inflow. Our attempts to assess concomitant effects on employment indicate that payroll tax reductions might yield increases in employment through the start-up of new firms.

Keywords: Payroll tax, Labour demand, Incidence, Firm entry/exit, Difference-in-Differences.
JEL-codes: J23, J38, J58, H22, J68.

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1 Introduction

While the motive of payroll tax reductions is clear, namely to increase employment, the outcome is generally uncertain. Whereas the effect of a payroll tax cut on labour demand will be unambiguously non-negative, it is also likely to induce counteracting supply responses and wage increases, potentially leaving employment (largely) unaffected. However, it is also possible that labour supply may actually increase, thus strengthening the initial demand effect. Accordingly, not even the direction of the employment effect can be determined on theoretical grounds and, of course, much less the magnitude of the change. The employment effect of payroll tax reductions is, thus, an empirical question.

However, empirical assessments are often plagued by limited variation in the payroll tax rates paid by firms or, when there is some degree of differentiation, by difficulties to find comparable firms that meet different payroll taxes. In the literature, the most credible evaluations are based on longitudinal studies of employment changes following the implementation of regionally differentiated tax schemes. Such schemes make it possible to compare employment changes in similar firms located close to another but differing with respect to payroll tax rate (i.e. Difference-in-Differences). Of particular relevance for this paper are three studies of regionally differentiated payroll taxes that have been implemented in the Nordic countries since the mid 1980's. These studies are reviewed in Section 3.

Section 2 describes the institutional features of the 2002 payroll tax reduction considered in our empirical analysis. In Section 4, theoretical issues are considered. Using a wage bargaining model as a starting point, we discuss the wage incidence of a payroll tax cut under various conditions. Section 5 contains a discussion of methodological considerations and the outcome variables that we use. Data issues are discussed in Section 6. Our results are provided in Section 7 and concluding comments in Section 8.

2 Payroll taxes in Sweden

Swedish employers are obliged by law to pay a payroll tax consisting of contributions to pensions, health insurance, and other social benefits. When it was introduced in 1950,

the legal payroll tax rate was relatively low – about 6 percent. However, the tax rate rose sharply in the 1960s and 1970s, and peaked at 39 percent in 1990. Since the mid-1990s, the payroll tax rate has remained rather stable at around 33 percent (Holmlund, 1983; Confederation of Swedish Enterprises, 2006).

In addition to the statutory payroll tax most employers are committed through collective agreements, to pay around 10.4 percent of gross wages to finance job search support, retraining and severance payments when employees are notified about lay-offs.[1]

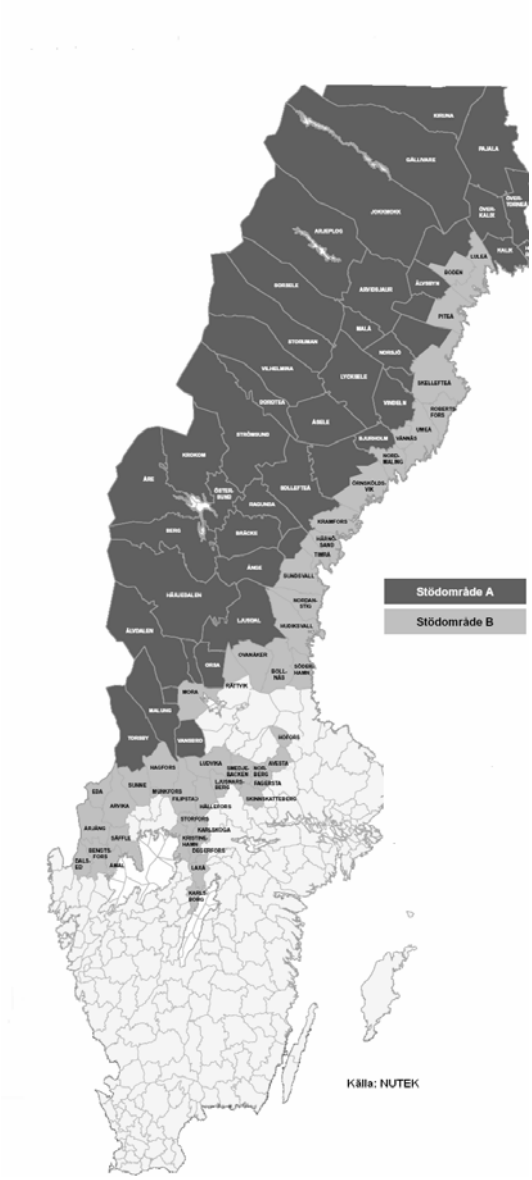
Initially, all firms paid the same legal payroll tax rate. Since the early 1980s, however, firms in the northern part of Sweden have been entitled to different forms of payroll tax cuts. In 1982, firms in four municipalities in the northernmost county (*Norrbotten*), were allowed to cut payroll taxes by 10 percentage points. Starting in 1984, firms in *all* municipalities in Norrbotten could reduce the payroll tax by 10 percentage points. In 1991, the target area was further expanded, to cover the northern half of Sweden with the exception of the coastline. This area is commonly referred to as "Regional Support Area" (RSA) A; see *Figure 1*.[2]

The payroll tax cut in RSA A was reduced from 10 to 8 percentage points in 1997. By the end of 1999 it was abandoned altogether, as it did not to comply with EU regulations (SOU 2000:87; SOU 2005:68).

[1] The information on payroll taxes according to collective agreements comes from Medlingsinstitutet (2008). To compute the payroll taxes rate faced by the average private firm, the payroll taxes paid have been weighted by the share of employees covered by different collective agreements in 2004.

[2] In Sweden two "Regional Support Areas" (RSAs) have been defined, A and B. The aim of the RSAs is to stimulate regional growth in more remote and sparsely populated parts of the country, through investment and employment subsidies. The difference between RSA A and RSA B is simply that subsidies are somewhat more common and more extensive in RSA A than in RSA B.

Figure 1 The regional support areas (RSAs) A and B

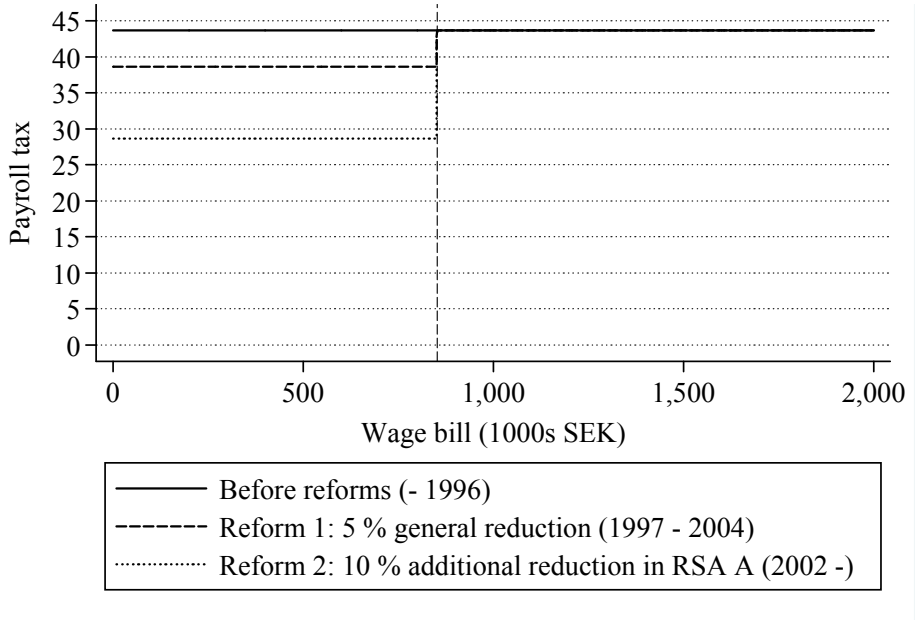


A new regional payroll tax cut for firms in RSA A was introduced in 2002. Again, the reduction was set to 10 percentage points, but this time it was restricted to annual gross wage bills up to SEK 852 000. This limit corresponds roughly to three employees with average earnings in the manufacturing sector. To comply with EU regulations, the payroll tax reduction was also restricted to private sector employers, not active in the agriculture, fishing or transport industries. Further, the reduction applied only to workers below the age of 65, the stipulated age of retirement.

The regional payroll tax reduction was put on top of an already existing general cut of 5 percentage points for all firms in the country, introduced in 1997.[3] The same wage bill ceiling applied to both the general and the additional regional payroll tax reduction.[4]

The 2002 reform implied that firms in RSA A could cut their payroll taxes by 15 percentage points up to the wage bill ceiling, whereas firms outside RSA A could only cut their taxes by 5 percentage points up to the ceiling. The statutory payroll tax rate in 2002 was 32.82 percent. To this, the average payments determined through collective agreements amounted to about 10.4 percentage points on average. Thus, the additional payroll tax reduction for RSA A firms below the wage bill ceiling was 7.3 percent [0.10 / (1.4322 - 0.05)]. To illustrate how the payroll tax rate varies over time and by region for firms of different sizes, *Figure 2a* and *Figure 2b* depicts the marginal and average payroll tax by the gross wage bill, respectively. Both the general reduction and the RSA A reduction are shown in the diagrams.[5]

Figure 2a Marginal payroll taxes: variations over time and by region

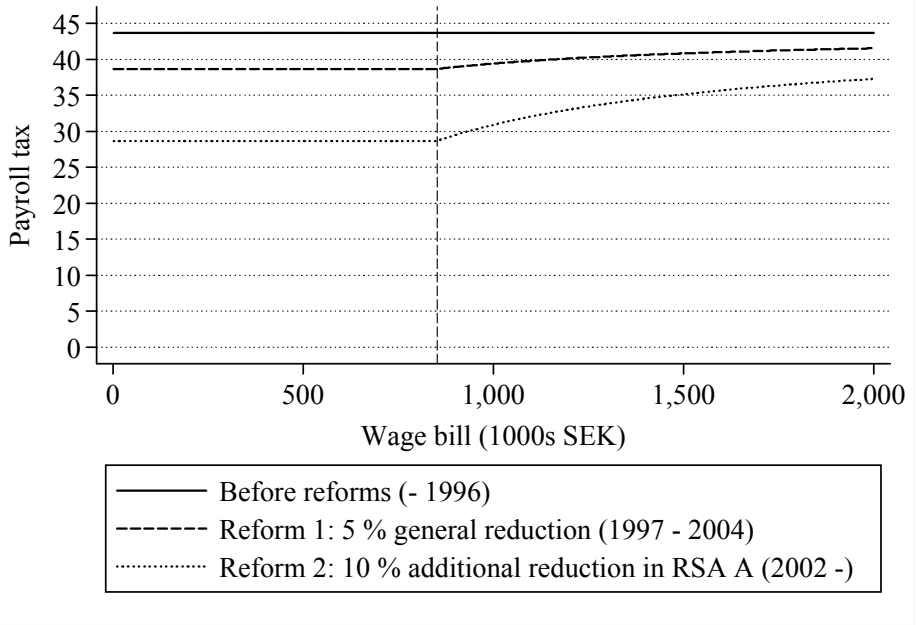


[3] The general payroll tax reduction was unchanged in magnitude until 2007 when it was reduced from 5 percentage points to 2.5 percentage points. In 2008, the general payroll tax cut was abolished altogether.

[4] In 2005, all firms received capital tax cuts, which were financed by lowering the wage bill ceiling for the general payroll tax cut from SEK 852 000 to SEK 741 600.

[5] The figures are schematic in the sense that the payroll costs determined through collective agreements do not constitute a constant share of the wage bill, but vary over time, around the average rate of 10.4 percent.

Figure 2b Average payroll taxes: variations over time and by region



For firms with wage bills not exceeding SEK 852 000, the marginal and the average payroll tax rates are equal. For firms with wage bills over the ceiling, the marginal tax rate reduction is zero while the average rate (bound from below by 28.22 percent) is monotonically increasing towards the total payroll tax rate of 43.22 percent. It is evident from *Figure 2b* that this increase is quite rapid; the payroll tax reduction becomes successively more thinly spread out over the wage bill. In a firm with a wage bill of SEK 2 500 000 – corresponding to at most ten employees – the reduction will only amount to 5 percentage points.

It should be noticed that profiles of the marginal and average tax rates pertaining to the general reduction are qualitatively the same as the corresponding profiles associated with the RSA A reduction. This means that if firms in RSA A are representative of firms in other parts of Sweden, too, then inferences about the effects of payroll tax reductions in RSA A should in a qualitative sense be valid with respect to the general reduction, as well. Thus, even though the effects of the general tax reduction cannot be evaluated by themselves, qualitative conclusions about them can be drawn by analogy with the effects estimated for the RSA A reduction.

3 Previous studies of payroll tax cuts in the Nordic countries

For the sake of brevity, we focus here on a few studies that are of particular relevance to our analysis, namely studies investigating the effects of regionally differentiated payroll taxes in the Nordic countries.[6] What makes the Nordic countries especially interesting in this context is, of course, that they are quite similar and share many institutional features. We will consider studies of Swedish, Norwegian, and Finnish experiences, respectively.

The Swedish example concerns an evaluation of the payroll tax reduction by 10 percentage points, that was implemented in Norrbotten in 1984 (Bohm and Lind, 1993). The 1984 payroll tax cut reform is similar to the 2002 reform, in that the tax reductions are identical and the geographical areas overlap (Norrbotten is part of the Region Support Area A targeted in 2002). However, there are some important differences. The 1984 payroll tax reduction was not limited with respect to wage costs – the reduced rate was applicable to the firm's entire wage bill. Moreover, the cut in the payroll tax rate was accompanied by a marginal employment subsidy, amounting to around 10 % of the average yearly earnings of a manufacturing worker in 1984. The subsidy extended over a three-year period, per full-time equivalent net increase in the firm's workforce.

Bohm and Lind (1993) used a Difference-in-Differences approach to assess the employment effects of the 1984 regional payroll tax cut. In particular, they compared the changes in employment between manufacturing firms in Norrland and in a nearby county. The analysis was conducted both for all firms in the two counties and for matched pairs of firms in the counties. In the matching approach, firms in the two counties were matched by firm age, firm size, and industry in the pre-reform period. In neither of the analyses could they find any evidence of statistically significant employment effects.

[6] As it happens, studies investing the effects of regionally differentiated payroll tax reductions outside the Nordic countries are hard to come by. Two interesting exceptions are Anderson and Meyer (1997) and Murphy's (2007) both of which employ US data, making use of variation across states in unemployment insurance payroll taxes. Further, Gruber (1997) estimates the effects of payroll tax reductions on employment and wages in Chile.

The Norwegian study (Johansen and Klette, 1998), concerns the manufacturing sector over the years 1983 – 1993. In this period, Norwegian payroll taxes differed by region – from around 17 percent down to 2 percent – and the rate structure changed over time. Johansen and Klette (op cit) estimated wage effects, but not employment effects, of changes in the payroll tax rate. To this end, they employed a Difference-in-Differences approach, were the change in hourly wages for firms that received lower (or higher) payroll taxes were compared to the change in wages for firms that were unaffected. They also allowed for different wage trends across industries. The estimates indicated that a 1 percentage reduction in the labour costs led to an increase in the hourly wages by 0.4 percent (evaluated at the average payroll tax rate of 12.5 percent in 1993).

In Finland, a regional payroll tax exemption experiment started in 2003.[7] The payroll tax was reduced by, on average, 4.1 percentage points in 14 municipalities in northern Finland and 6 municipalities along the western coastline. As the average rate before the reduction was 23.86 percent, this amounted to an average decrease in labour costs of around 3.3 percent. As in the Swedish 2002 reform, the payroll tax cut was restricted to gross wage bills up to a ceiling. However, in Finland the ceiling was set roughly 8 times higher than the Swedish ceiling, thus allowing most firms to reduce the payroll tax for the all its' employees. In practise, most of the firms applying for the tax reduction were very small; the median firm had four employees. In total, some 2 300 firms with about 17 000 employees participated during the first year.

Korkeamäki and Uusitalo (2008) evaluated the employment and wage effects of the Finnish experiment. They used a Difference-in-Differences strategy where control firms were selected by means of a two-stage procedure. In the first step, regions similar to the target regions in terms of unemployment, industrial structure and workforce characteristics were selected. In a second step, firms in the target and comparison regions were matched by industry, the number of employees, the gross wage bill and the total sales of the firm, etc.[8]

In the evaluation, no significant employment effects could be found, neither on average or when the effects were allowed to vary by wages paid. Unfortunately, the wage

[7] The experiment is planned to continue to the end of 2009.

effects could only be analysed for a smaller subsample of large firms in the manufacturing and in the service sector. In the service sector a 1 percentage reduction in the labour costs was estimated to increase wages by 0.6 percent, while no clear-cut results emerged for the manufacturing sector.

To sum up: in the two studies analyzing employment responses to payroll tax cuts, no significant effects could be established. Wage changes were also investigated in two of the three studies considered. In both cases, lower payroll taxes led to higher wages, amounting to between 40 percent and 60 percent of the reduction in the labour costs.

4 Theoretical issues

To simplify the discussion of the effects of a payroll tax reduction on employment and wages we will make a number of simplifying assumptions. These assumptions are spelled out in Section 4.1. Section 4.2 considers likely effects on wages from a reduction in payroll taxes, as well as employment effects.

4.1 Simplifying assumptions

Discussions about employment and wage effects are complicated by the fact that, in general, wages and employment are simultaneously determined, making it difficult to discuss the two separately. There are plausible analytical frameworks which model wages and employment as sequentially determined, however. We will assume that such a model is appropriate in the present context. Specifically, we make the following assumption:

A.1 Wages are determined according to the "right to manage" version of the wage bargaining model proposed by Nickell and Andrews (1983). This means that the firm and the union bargain first over the wage, whereupon the firm determines employment, contingent upon the (real) wage.

This assumption does not appear to be very restrictive in the Swedish context. Sweden has a long tradition of strong unions which definitely have considerable bargaining

[8] All matching variables were measured during the three years preceding the experiment.

power, although this power has been reduced during the last decades. Moreover, the firm's "right to manage" has never been seriously disputed.

A.2 Firms are assumed to be price takers in the output market.

For a small open economy like Sweden, where (most) firms compete in the world market, this assumption is quite natural. Still, it does not apply universally, of course; specialized domestic services being an example to the contrary. Also, most of the small firms targeted by the payroll tax reductions considered here probably do not compete on the world market. To take this into account, we discuss (in the end of Section 4.2) how relaxing A.2 affects our conclusions.

A.3 The return on capital is assumed to be internationally determined.

Like A.2, this is a very natural assumption in a small open economy context.

A.4 Labour demand is not infinitely elastic, i.e. the labour demand curve is not horizontal.

In principle, given the assumption A.1, this assumption follows automatically; a horizontal demand curve can only arise in the context of perfect competition on the labour market.

A.5 The labour supply curve is not vertical.

While the concept of labour supply is not well defined in the context of a union bargaining model, this assumption merely ascertains that the union has to balance the objectives of maximizing the wages of its employed members against the objective of finding jobs for its unemployed members.

4.2 Effects on wages and general employment effects

According to assumption A.1, firms and unions bargain about the (nominal) wage according the "right to manage" wage bargaining framework. The real wage will then depend on the union's relative bargaining power, the level of unemployment, the unemployment benefit replacement ratio, and the properties of the firm's production technology, as manifested in labour productivity.

Momentarily, a decrease in the payroll tax will lower labour costs. Given that firms were in equilibrium before the change, the reduction will imply that the firm's labour costs are reduced below the workers' marginal productivity values. Under assumptions A.2 - A.5 this will increase labour demand and decrease the demand for capital. How-

ever, noting that it has now become cheaper for the firm to keep a given workforce, the union will demand wage increases.

For the outcomes in terms of wages and employment, there are three crucial issues. The first is whether the payroll tax reduction is anticipated or known beforehand. The second is the frequency of wage negotiations. The third issue is the union's relative bargaining power.

If the tax reduction is known long beforehand or if wage negotiations occur often, then wage increases might possibly offset the payroll tax cut immediately, leaving total labour costs unaffected, provided that the union's bargaining power is sufficiently high. In this case there will be no direct effects on employment. Possibly, there will be second order employment effects, to the extent that the enhanced incomes resulting from the wage increases raise demand in general and, hence, employment. However, such a second order effect presupposes that the increases in nominal incomes also yield increases in *real* incomes.

If, on the other hand, the payroll tax reduction is not anticipated and the time distance between wage negotiations is large enough, then the payroll tax cut is likely to increase employment in the short run. When, subsequently, the firm and the union meet to bargain, wages will rise. If the firm's bargaining power is large enough to prevent wages from rising beyond the workers' marginal productivity and impinge on the internationally given return to capital, employment will stay at the new, higher, level reached by the initial increase.

In both of these examples, the stronger the firm's relative bargaining power, the smaller will be the wage increases and, hence, the larger the potential for enhanced employment. It should be noted, though, that the firm might alternatively choose to increase its profit margin. Whether the firm decides to expand production or raise profits also depends on its relative bargaining power. The higher its bargaining power the more likely is the firms to add to its workforce. Of course, the firm can also choose booth alternatives simultaneously by employing some additional workers, without exploiting the full recruitment potential created by the tax cut.

Regarding the assumptions made in Section 4.1, it should be noted that relaxing A.2, i.e. allowing for imperfect competition in the output market, will increase the likelihood

of positive employment effects. The reason is twofold. First, it is hard to imagine that firms lower their output price in response to the payroll tax cut unless they are convinced that after wages are renegotiated they will be at least as well off as before the payroll tax reduction. A necessary (but not sufficient) condition for this to happen is that wage increases do not offset the decrease in labour costs resulting from the payroll tax cut. Accordingly, the relative price of labour will fall, inducing an increase in labour demand. Secondly, the aim of lowering of the output price is to enable a higher level of production. This will lead the firm to demand more of all factors of production and thus, in particular, increase its demand for labour.

To sum up: a payroll tax reduction is most likely to be followed by wage increases. The less anticipated the tax cut and the less frequent the occurrence of wage negotiations the smaller will be the extent to which the decrease in labour costs is immediately offset by wage increases and, hence, the larger the potential short-run employment effects. The magnitude of the realized employment effects will, in addition, be increasing in the firm's relative bargaining power. The likelihood of positive employment effects will be further increased if the firm is not a price taker in the output market.

5 Empirical considerations

We are interested in estimating the effects of payroll taxes on the gross wage bill, number of employees and average wage bill per employee. We will also study the effects on firm entry, firm exit and net firm inflow. Assume that the relation between payroll taxes and different outcomes for firm j, in industry k, located in region r at time t can be given by the following function:

$$Y_{jkrt} = \beta\tau_{jrt} + \gamma_{kt} + \lambda_j + \varepsilon_{jkrt}, \tag{5}$$

where Y_{jkrt} is one of the outcomes just mentioned, τ_{jrt} is the average payroll tax rate, γ_{kt} is an industry-specific time fixed effect, λ_j is a firm fixed effect, and ε_{jkrt} is an error term.[9]

In general, estimating the effects of payroll taxes might be problematic for two reasons. First, there might be limited or no variation in the payroll taxes paid by firms. In most countries, all firms meet the same payroll tax rate, which makes empirical evaluations more or less impossible. Second, firms that do meet different payroll tax rates, might differ also in other important respects. For instance, changes in payroll taxes over time might coincide with other macro economic changes, and firms in different regions might have different outcome potentials.

The 1997 general payroll tax cut reform and the 2002 additional tax cut reform introduced yet another empirical problem. By imposing a ceiling on the wage bill for maximum pay roll tax reduction, the tax rate became a function of the gross wage bill. In particular, smaller firms met lower payroll taxes than larger firms, introducing the problem of reverse causality. Not only could the payroll tax rate affect the gross wage bill (through employment, hours worked and/or wages), but a change in the wage bill also fed back directly to a change in the payroll tax rate.

This paper attempts to estimate the effects of payroll taxes by exploiting the 2002 payroll tax reform for firms in RSA A. The idea is to compare the change in outcomes for firms in RSA A – before and after the reform – to the change in outcomes for firms outside RSA A (Difference-in-Differences). One complication, however, is that not all firms in a given region pay the payroll tax rate as suggested by the reform (imperfect compliance). First, the payroll tax cut was only given to firms that filed an application to the tax authorities. Due to imperfect information, however, not all eligible firms did. Second, firms could move to RSA A to receive the lower payroll tax rate. Thus, there might be (endogenous) variation in the payroll tax rate even within a region at a given point in time.

[9] Since firms rarely change industry or region, the firm fixed effect also accounts for constant differences between industries in different regions. In the estimations, the firm fixed effects are handled by first differencing the data.

To isolate the variation in payroll taxes induced by the 2002 reform, we will use the interaction between time and region as an instrument for the average payroll tax:

$$\tau_{jkrt} = \phi D_R \times D_T + \delta_{kt} + \varpi_{jk} + \eta_{jkrt}, \tag{6}$$

where D_R is a dummy variable for firms in RSA A in 2001 and D_T is a dummy variable for the post-reform period. The interaction between time and region, $D_R \times D_T$, is our instrument. Time effects and firm fixed effects are given by δ_{kt} and ω_{jk}, respectively, while η_{jkrt} denotes the error term.

The identifying assumption for using the interaction between time and region as an instrument for the average payroll tax, is that firms inside and outside RSA A would have experienced the same changes in outcomes in absence of the reform. In other words, there should be no difference in underlying trends for firms in different regions. We will attempt to make this assumption more plausible in two ways. First, we will compare firms in RSA A to firms operating in nearby regions. In particular, we will use firms in RSA B as a comparison group (see *Figure 1*). We believe that firms in these regions face similar external conditions. In particular, they share the same local labour markets and compete on the same local product markets. Thus, labour supply or product demand shocks should affect firms in both regions equally. Further, like firms in RSA A, firms in RSA B are eligible for regional subsidies. Second, we will compare firms operating in the same industry. It is quite possible that outcomes evolve differently by industry. Therefore, we control for both industry specific region fixed effects (which are captured by the firm fixed effects) and industry specific time fixed effects. Thus, we estimate Difference-in-Differences models at the industry level, by comparing the change in outcomes for firms in RSA A with the change in outcomes for firms in the same industry in RSA B.[10]

[10] Note that region and industry are defined in the pre-reform period. Thus, the IV-estimates are not biased by firms moving between regions or by firms changing industries.

6 Data

To assess the effects of the 2002 payroll tax cut reform, we exploit yearly firm-level data for the 2001-2004 period from Statistics Sweden (SCB). The data include central variables such as the gross wage bill, number of employees, and average payroll taxes paid. All firms can also be linked to a municipality and an industry[11], which makes it possible to identify the firms that were affected by the reform. A few firms with work places both within and outside RSA A were excluded, since it was unclear how they were affected by the reform.

The main outcomes of interest are the natural logarithms of the gross wage bill, the number of employees and the average wage bill per employee, respectively. The gross wage bill is an encompassing measure of the labour demand responses to payroll tax reductions. It is a function of the number of employees, the average number hours worked per employee and the average hourly wage. To trace the source of the effect we will decompose the wage bill (B) into the number of employees (N) and the average wage bill per employee (B/N):

$$\ln B = \ln N + \ln(B/N). \tag{7}$$

From (7) it is clear that a zero wage bill effect does not imply zero effects on employment and wage bill per employee. We thus conduct separate estimations where the outcome variables are the log of employment and the log of wage bill per employee.

Ideally, we would like to be able to decompose the wage bill into hourly wages and hours worked, because that would enable us to estimate the price and quantity effects of the payroll tax reduction. Unfortunately, we lack this information. We thus have to be content with the regressions using number of employees and wage bill per employee as dependent variables. This means, e.g., that we cannot exclude the possibility of positive employment effects even if we find no effects on the number of employees. There might still be positive effects on the number of hours worked. Conversely, a positive effect on

[11] We use the most detailed (5-digit) level of industry information to define the industry specific time effects. In total, firms in 435 different industries are studied.

the number of employees may be compensated by a reduction in the number of hours worked per employee, leaving unaffected the total number of hours worked. Likewise, a positive effect on the wage bill per employee does not necessarily imply that the payroll tax reduction induced a wage increase. The higher remuneration per worker may be due to longer working hours.

The main analysis is restricted to firms existing in both the pre-reform period (2001), and the post-reform period of interest (2002, 2003 or 2004). It is, however, quite possible that the 2002 payroll tax reform affected firm entries and firm exits as well. This is a problem not well recognized in the empirical literature on payroll taxes. In this study, we will study the effects of payroll taxes on firm flows in a sensitivity analysis. One problem associated with this analysis is that the population of firms "at risk" of being established is not well defined. Therefore, we will follow the literature on job creation and destruction (see, e.g., Davis and Haltiwanger, 1992), and define the population at risk as firms existing in the pre-reform period and/or in the post-reform period of interest. For this sample we will analyse the effect of payroll taxes on the probability of firm entry and the probability of firm exit. We will also study the effect on net firm inflows (the probability that a firm enters the market less the probability that a firm exits).

As noted above, our basic identifying strategy is to assess the effects of the 2002 payroll tax reduction on different outcomes, using a Difference-in-Differences estimator. *Table 1* shows the means of the central variables used in the analysis and illustrates the logic of the Difference-in-Differences approach. In the first column we show data for firms operating in RSA A. On average, the payroll tax rate fell by 6.2 percentage points for these firms; see Panel A.[12] At the same time the wage bill increased by 15.8 log points; cf. Panel B).

The second column of *Table 1* shows data for firms operating in RSA B. For them, the payroll tax rate paid was more or less unaffected (0.5 percentage points), while the wage bill increased by 14.6 log points. Thus, the gross wage bill increased in all firms,

[12] The average fall in the actual payroll tax rate is thus around 3 percentage points smaller than the maximum possible reduction introduced by the regional differentiation of the tax rate. Partly, this is due to some firms not filing applications at the tax authorities to receive the tax reductions they are entitled to (cf. Section 5). The main reason, however, is the ceiling on the wage bill which makes the average reduction for large firms well below 10 percentage points; cf. Section 2.

but slightly more in firms that received a reduction in the payroll tax (RSA A) than in other firms (RSA B).

If we calculate the ratio of the differences of two changes (1.2 log points divided by -6.7 percentage points) we obtain the IV-estimate of the payroll tax rate on the wage bill. As shown at the bottom of panel B, a one percentage point increase in the payroll tax rate reduces the gross wage bill by 0.18 percent. However, the estimate is not significantly different from zero.

Panel C provides the corresponding estimate of the effect of payroll tax changes on employment. A one percentage point increase leads to an increase in employment by 0.06 percent. That is to say, the estimated effect is negligible. Moreover, the standard error of this estimate, 0.0015, is not very large, either, indicating that it is highly likely that the "true" effect is very close to zero, too.

In the remainder of the paper we investigate whether these result hold up to a more formal regression analysis.

Table 1 Means of payroll taxes, wage bills and employment by region and time

	RSA A	RSA B	Difference
	Panel A: Payroll tax rate		
Year 2002-04	33.1696	40.0942	-6.9246***
	(0.1025)	(0.0322)	(0.1074)
Year 2001	39.3590	39.5685	-0.2095***
	(0.0157)	(0.0260)	(0.0304)
Difference	-6.1894***	0.5257***	-6.7151***
	(0.0964)	(0.0175)	(0.0974)
	Panel B: ln(gross wage bill)		
Year 2002-04	12.8797	13.0383	-0.1586***
	(0.0192)	(0.0313)	(0.0367)
Year 2001	12.7212	12.8921	-0.1709***
	(0.0193)	(0.0330)	(0.0382)
Difference	0.1584***	0.1462***	0.0123
	(0.0092)	(0.0082)	(0.0123)
	IV-estimate of payroll taxes on the wage bill		
	-0.0018		
	(0.0018)		
	Panel C: ln(employment)		
Year 2002-04	1.1657	1.3070	-0.1413***
	(0.0136)	(0.0257)	(0.0291)
Year 2001	1.0978	1.2348	-0.1370***
	(0.0133)	(0.0273)	(0.0304)
Difference	0.0679***	0.0722***	-0.0042
	(0.0070)	(0.0073)	(0.0101)
	IV-estimate of payroll taxes on employment		
	0.0006		
	(0.0015)		

Notes: The observations are weighted by the number of firms in different industries in RSA A in 2001. Standard errors adjusted for clustering (municipality) are in parantheses. ***/**/*=the estimate is significantly different from zero at the 1/5/10 per cent level of confidence, respectively.

7 Results

7.1 Comparing alternative estimators

The first column of *Table 2* presents the naïve estimate of the effect of payroll taxes on the gross wage bill using OLS for the 2002-04 period. It shows that payroll taxes are positively associated with the gross wage bill. However, this estimate is plagued by the problems of omitted variables and reverse causality discussed in Section 5.

One way to address these problems is to add firm fixed effects, which is done in column 2. This leads to a substantially lower estimate, but the association between the payroll tax rate and the gross wage bill is still positive. Note, however, that this estimate exploits variation in payroll taxes stemming both from moving along a given payroll tax schedule and from shifts between different payroll tax schedules induced by the 2002 reform for firms in RSA A.[13] Since expanding (contracting) firms can meet a higher (lower) payroll tax rate in a given tax rate schedule, the problem of reverse causality might still bias the estimates in the fixed effects specification.

The empirical strategy in this paper is to exploit the exogenous variation in payroll taxes induced by the 2002 payroll tax cut reform. In particular, we compare the change in outcomes for firms in RSA A to the change in outcomes for firms outside RSA A. As noted above, however, some firms did not comply with the reform, i.e. they did not file for the payroll tax cut or they moved to another area. Therefore we will use the difference in trends between firms inside and outside of RSA A as an instrument for the payroll tax rate. Thus, we will relate the (reduced form) Difference-in-Differences estimate for the outcome to the corresponding (first stage) Difference-in-Differences estimate for the payroll tax rate.

[13] There is also variation in the payroll tax rate stemming from the probability to file an application for a payroll tax cut, as well as from firm reallocation between RSA A and RSA B.

Table 2 OLS and IV estimates of the effect of payroll taxes on the log wage bill, 2001-04

	OLS	FE	Reduced Forms		IV
Dependent variable	ln(Wage bill)	ln(Wage bill)	Payroll tax	ln(Wage bill)	ln(Wage bill)
Payroll tax	0.1686***	0.0038***	.	.	-0.0021
	(0.0170)	(0.0014)	.	.	(0.0014)
RSA A × Post reform	.	.	-6.7445***	0.0145	.
	.	.	(0.0943)	(0.0092)	.
RSA A	0.6965***	.	.	.	.
	(0.0871)	.	.	.	.
Post reform	0.2204***	0.0541***	0.1946***	0.0417***	0.0421***
	(0.0722)	(0.0043)	(0.0541)	(0.0046)	(0.0045)
Firm fixed effects		X	X	X	X
n	81,269	81,269	81,269	81,269	81,269

Notes: All models include an intercept and industry fixed effects interacted with the post reform period. The sample is restricted to firms in RSA A or in RSA B. Standard errors adjusted for clustering on municipality are in parantheses. ***/**/*=the estimate is significantly different from zero at the 1/5/10 per cent level of confidence, respectively.

The instrumental variable estimate relies on the assumptions that firms in RSA A did indeed face lower payroll taxes as a result of the reform (i.e. that there is a first stage), and that firms inside and outside of RSA A would have had the same change in outcomes had there not been a reform. The third column of *Table 2* shows the effect of the 2002 payroll tax cut reform on the average payroll tax rate paid in the 2002-2004 period. There is a strong first-stage relation between the difference in trends for firms inside and outside of RSA A, were firms in RSA A on average receives a payroll tax cut of about 6.74 percentage points in the 2002-2004 period.[14] Thus, the reform reduced the payroll tax rate for firms in RSA A substantially, even though the change for the average firm was somewhat lesser than the 10 percentage reduction available for small firms.[15]

The fourth column in *Table 2* shows the reduced form effect of the change in the log wage bill for firms in RSA A, compared to firms in RSA B. This tells us how the 2002 payroll tax cut reform changed the wage bill. It turns out that the gross wage bills have

[14] The instrument is extremely strong. The F-statistic is 5121 and the partial R^2 is 0.61.

[15] As noted above, there are a number of reasons for why the first stage effect of the payroll tax cut reform is not exactly 10 percentage points. First, only firms with wage bills below the ceiling received the full 10 average payroll tax cut. The larger the firm, the smaller the average payroll tax reduction. Second, not all firms filed an application to receive the payroll tax cut. Third, older employees (above 65 years of age) did not qualify for the payroll tax cut. Four, some firms moved in or out of RSA A.

grown more rapidly for firms in RSA A than for firms in RSA B, even though the difference is not statistically significantly different from zero. On average, firms in RSA A increased their wage bill with 1.45 percent more than firms in RSA B over the 2002-2004 period.

The last column of *Table 2* presents the IV-estimate of the effect of the payroll tax rates on the log wage bill. The IV-estimator relates the Difference-in-Differences estimate for the wage bill to the corresponding Difference-in-Differences estimate for the payroll tax rate, by simply dividing the (reduced form) outcome effect by the (first stage) payroll tax effect. The result show that a one percentage increase in the payroll tax rate leads to 0.21 percent lower wage bill, although the effect is not statistically different from zero. Note, however, that the IV-estimate is significantly lower than the corresponding fixed effects estimate in column two.[16]

7.2 Sample restrictions and choice of comparison region

In *Table 3* we let the IV-estimate be subject to different model specification checks. Column 1 in *Table 3* simply reproduces the last column in *Table 2*. In the next two columns the sample is divided into firms above and below the wage bill ceiling in 2001, respectively. Firms in RSA A with wage bills exceeding SEK 852 000 SEK, did not receive any marginal payroll tax cut. Instead, the 2002 reform acted as an income subsidy, changing only the firms' *average* payroll tax. From a theoretical point of view, one would expect the effect for these firms to be small. Since only about one quarter of the firms had wage bills above the ceiling in 2001, restricting the analysis to larger firms leads to even less precise estimates, however. Interestingly enough, the estimate for firms below the wage bill ceiling, reported in column 3, does not loose in precision as the sample size falls. The estimate is now significantly different from zero, but does not differ from the estimate for all firms (column 1). Since firms of different sizes might compete with each other, we will keep the sample intact in the remaining analysis.

[16] The IV-estimate in the last column of Table 2 and the IV estimate in the bottom of Panel B in Table 1 differ in precision. This is probably due to the fact that the full model in Table 2 accounts for firm fixed effects and industry specific time effects.

Table 3 IV estimates of the effect of payroll taxes on the log wage bill, 2001-04, different sample restrictions and comparison regions

	(1)	(2)	(3)	(4)	(5)
Payroll tax	-0.0021	0.0038	-0.0028**	-0.0031**	-0.0029
	(0.0014)	(0.0028)	(0.0014)	(0.0014)	(0.0020)
Wage bill restrictions:					
- Wage bill ≤ 852,000			X		
- Wage bill > 852,000		X			
Study region:					
- RSA A	X	X	X	X	X
- Excluding remote areas					X
Comparison region:					
- RSA B	X	X	X	X	X
- Excluding border areas				X	
n	81,269	21,100	60,169	70,642	68,692

Notes: All models include an firm fixed effects, time fixed effects and industry fixed effects interacted with the post reform period. The wage bill restrictions refers to the pre-reform year (2001). Standard errors adjusted for clustering on municipality are in parantheses. ***/**/*=the estimate is significantly different from zero at the 1/5/10 per cent level of confidence, respectively.

In columns 4 and 5 we investigate how the choice of comparison area affects our estimates. As noted above, we choose to compare firms in RSA A to firms in RSA B, since these firms are likely to share the same local labour market and compete on the same local product market. Thus, shocks to these local markets are likely to hit all firms in RSA A and RSA B equally. One shortcoming with this choice of comparison group, however, is that firms in RSA B located close to RSA A might loose market shares to firms in RSA A. Since only firms in RSA A receive the payroll tax cut, they are probably able to lower their product prices more than firms in RSA B. It is, thus, possible that firms in RSA A might expand at the expense of lower employment for firms in RSA B. This would tend to overstate the effect of the payroll tax reduction.

In column 4 we have replaced RSA B by a region that is located close to RSA A but that does not share its borders. Firms in this area are less likely to compete on the same local markets as firms in RSA A and, thus, should not be directly affected by the payroll tax cut reform. On the other hand, since the latter firms probably act on other markets than the firms in RSA A, they might also be less comparable. The estimated effect of payroll taxes is actually larger when using this more remote comparison area, even though the estimates in column one and four are not significantly different from each

other. Thus, our result here does not lend support to the hypothesis that comparing firms in RSA A with firms in RSA B would tend overstate the effects due to unfair competition. Quite to the contrary, the use of more distant comparison regions seems to lead to higher (point) estimates, indicating that the corresponding firms might be less comparable to firms in RSA A. Thus, we feel confident in keep using RSA B as our main comparison area.

A related concern is that firms in RSA A located far away from the border might not be entirely comparable to firms in RSA B. Therefore, the last column of *Table 3* excludes firms in the far north-western part of RSA A. This leads to a point estimate that is somewhat larger in magnitude than the estimate in column one. However, due to the sample in column five being smaller, the standard errors also increases, making the estimated effects with and without the most distant areas of RSA A not significantly different from one another. Thus, we think it is reasonable to keep all firms in RSA A in the analysis.

7.3 Effects on employment and wage bill per employee

As noted in Section 6, eq. (7), the wage bill effect can be decomposed into effects on the number of employees and average wage bill per employee. The corresponding estimates are reported in *Table 4*.

Table 4 IV estimates of the effect of payroll taxes on different outcomes, 2001-04

Dependent variable	ln(Wage bill)	ln(Employment)	ln(Wage bill / employment)
Post-reform year			
2002	-0.0003	0.0010	-0.0013
n = 42,984	(0.0014)	(0.0010)	(0.0011)
2003	-0.0019	-0.0003	-0.0016
n = 39,380	(0.0016)	(0.0011)	(0.0011)
2004	-0.0042**	-0.0003	-0.0040***
n = 36,062	(0.0020)	(0.0015)	(0.0012)
2001-04	-0.0021	0.0001	-0.0023***
n = 81,269	(0.0014)	(0.0010)	(0.0008)

Notes: Each estimate comes from a separate regression. All models include firm fixed effects, time fixed effects and industry fixed effects interacted with the post reform period. The sample is restricted to firms in RSA A or in RSA B in the pre-reform year (2001). Standard errors adjusted for clustering on municipality are in parantheses. ***/**/*=the estimate is significantly different from zero at the 1/5/10 per cent level of confidence, respectively.

In addition to the average outcome over the period 2001-04, *Table 4* also provides separate estimates for the years covered by this period. It can be seen that the 2004 wage bill effect differs from the effects in 2002 and 2003 by being much larger and also statistically significant. Indeed, the fact that the estimate of the average outcome is almost statistically significant derives almost entirely from the 2004 effect. This pattern is even more marked in the last column of the table, displaying the effects on the wage bill per employee. These results are consistent with the fact that major wages negotiations were finalized in Sweden during the year 2004.[17]

The effect on the wage bill per employee, as measured by the average estimate for 2002-2004, says that a 1 percentage point reduction in the payroll tax rate increased wages by 0.23 percent. Accordingly, of the 10 percentage point reduction in payroll taxes induced by the regional differentiation the incidence on wages was 1/4.

To compare our results by those obtained in the Norwegian study by Jakobsen and Klette (1978), cited in Section 3, we have to convert our estimate to the impact of a 1 percent reduction in labour costs (wages + payroll taxes). Since total payroll taxes were 38.22 (43.22 - 5) when the regional differentiation was introduced, a 1 percent reduction in the labour costs corresponds to 1.3822 times a reduction of 1 percentage point in the payroll tax rate. Accordingly, a 1 percent reduction in labour costs (per employee) increases wages (per employee) by $1.3822 \times 0.23 \approx 0.32$ percent, which is in the same ball park as the estimates reported by Jakobsen and Klette (op cit).

Our finding that there are no significant effects on employment is also in accordance with the previous literature. As noted in Section 3, no significant employment effects could be established in connection with regionally differentiated payroll taxes in the north of Sweden in the 1980's [Bohm and Lind (1993)] and in Finland in the beginning of the second millennium [Korkeamäki and Uusitalo (2008)].

7.4 Effects on firm entry and firm exit

So far, the analysis has been restricted to firms existing both before and after the 2002 reform. It is quite possible, however, that a payroll tax rate reduction can affect firm

[17] The 2004 negotiations were some of the most extensive during the second millennium, with respect to coverage of the national labour market (Medlingsinstitutet, 2005).

entry and firm exit. In fact, one of the objectives of the reform was to stimulate the growth of new firms. *Table 5* shows the effects of payroll taxes on the probability of firm entry, the probability of firm exit and the net effect of firm entry and firm exit (net firm inflow). The first column reports positive and significant effects on firm entry from payroll tax reductions for all of the outcome years considered. The average estimate of -0.0011 implies that a 1 percentage point increase in the payroll tax rate reduces the probability of firm entry by 0.11 percentage points. And, by symmetry, a 1 percentage point reduction in the payroll tax rate increases the probability of firm entry by 0.11 percentage points.[18]

Table 5 IV estimates of the effect of payroll taxes on firm flows, 2001-04

Dependent variable	Firm entry	Firm exit	Net firm inflow
Post-reform year			
2002	-0.0010*	-0.0006	-0.0004
n = 52,440	(0.0005)	(0.0007)	(0.0008)
2003	-0.0010*	-0.0001	-0.0009
n = 52,978	(0.0006)	(0.0008)	(0.0009)
2004	-0.0014***	-0.0001	-0.0013
n = 53,326	(0.0005)	(0.0008)	(0.0010)
2001-04	-0.0011**	-0.0003	-0.0009
n = 110,484	(0.0005)	(0.0007)	(0.0008)

Notes: Each estimate comes from a separate regression. All models include firm fixed effects, time fixed effects and industry fixed effects interacted with the post reform period. The sample is restricted to firms in RSA A or in RSA B, that were active in the pre-reform year (2001) and/or in the given post-reform year. Standard errors adjusted for clustering on municipality are in parantheses. ***/**/*=the estimate is significantly different from zero at the 1/5/10 per cent level of confidence, respectively.

The second column of *Table 5*, report the effects on firm exits. Contrary to the firm entry effects, however, we do not find any statistically significant effect of payroll taxes on firm exits. Since both the firm entry and the firm exit effects are measured with uncertainty, the net effect of the two effects is not significantly different from zero. Thus, even though we find some support for an increase in the number of entering firms, we can not say that a decrease in the payroll taxes affects the net of firm entry

[18] In the 2002-04 period, on average 5.13 percent of the firm population in RSA A were new entrants. Thus, the firm

and firm exit. Given the insignificant net effects, it appears that the impacts of payroll tax changes on changes in the firm population are not very important. Nevertheless, if entering firms on average are larger than exiting firms, payroll taxes might still affect employment through firm entry and exit. This possibility is investigated in the next subsection.

7.5 Employment effects accounting for firm entry and exit

Extending the analysis to also include firms that either entered or left the market during the period of study, we have to consider the problem of how to handle zero-valued observations. Up to now, the outcome variables have been expressed in logarithmic form. However, the logarithm of zero is not well defined. This problem can be dealt with in many alternative ways, some of which are illustrated in *Table 6*. The first column just replicates our estimates of the employment effects when excluding firms with zero-value observations.

Our first approach is to assign a small positive number to the zero observations. Two examples of this approach are provided in the second and third columns of *Table 6*, where firms with zero employment are assigned employment numbers equal to 0.50 and 0.25, respectively. We find some support for a positive employment effect from reduced payroll taxes under both of these specifications. In particular, the 2002-04 specification where zero employment is set to 0.5 is significantly different from zero at the 10 percent level of confidence.

entry effect of -0.0011 corresponds to a decrease in the probability of firms entering the market by about 2 percent.

Table 6 IV estimates of the effect of payroll taxes on employment, 2001-04, accounting for employment effects from firm entries and firm exits

Dependent variable	ln(Employment) 0 is excluded	ln(Employment) 0 is set to 0.50	ln(Employment) 0 is set to 0.25	Job creation/ destruction
Post-reform year				
2002	0.0011	-0.0014	-0.0016	-0.0001
n = 52,440	(0.0010)	(0.0014)	(0.0018)	(0.0019)
2003	-0.0008	-0.0034**	-0.0041*	-0.0019
n = 52,978	(0.0011)	(0.0016)	(0.0022)	(0.0021)
2004	-0.0010	-0.0029	-0.0037	-0.0023
n = 53,326	(0.0016)	(0.0019)	(0.0025)	(0.0022)
2001-04	-0.0002	-0.0026*	-0.0032	-0.0015
n = 110,484	(0.0010)	(0.0015)	(0.0020)	(0.0019)

Notes: Each estimate comes from a separate regression. All models include firm fixed effects, time fixed effects and industry fixed effects interacted with the post reform period. The sample is restricted to firms in RSA A or in RSA B, that were active in the pre-reform year (2001) and/or in the given post-reform year. Standard errors adjusted for clustering on municipality are in parantheses. ***/**/*=the estimate is significantly different from zero at the 1/5/10 per cent level of confidence, respectively.

Apparently, the non-linearity close to zero of the logarithmic transformation is important. Moreover, the value chosen to substitute for zero observations matters. Neither of these findings is very comforting – functional form and numeric precision are not solid foundations for the identification of effects.

In the last column of *Table 6* we consider yet another approach to deal with the zero-valued observation. Following Davis and Haltiwanger (1992) we define the observed change (in the level of) employment relative to the arithmetic mean of the two points. By so doing, we avoid zero observations and obtain a relative change that is bounded by the closed interval [-2, 2].[19] Although the estimates in the last column are of about the same magnitude as the earlier ones, they are measured with lower precision. Thus, none of them are significantly different from zero.

The Davis and Haltiwanger approach has the advantage that it has been developed specifically to analyze firm entry and exit. Moreover, it leaves less room for manipulation than the procedure of assigning arbitrary (positive) values to the zero observations. Still, the transformation used has one feature that isn't very appealing: by defining the change relative to the mean of the pre- and post-reform levels it (partly) "conditions" on the outcome.

[19] Formally, the job creation/destruction rate is given by: $(e_t-e_{t-1})/((e_t+e_{t-1})/2)$, where e_t is employment in at time t.

To sum up, accounting for employment effects arising from firm entry/exit, might potentially be of importance. Using different approaches to deal with zero employment, we find point estimates corresponding to larger positive effects of payroll tax cuts than when we restrict the analysis to existing firms. On the other hand, only in a few cases are the estimates significantly different from zero (and then mostly at the lower level of confidence).

8 Concluding comments

To relate our results to the previous literature, it is useful to make a distinction between two populations of firms that we consider in our empirical analysis. The first population consists of firms that existed in 2001, i.e. before the introduction of the regionally differentiated payroll tax in 2002, and in at least one of the years 2002, 2003 and 2004 – "existing" firms for short. The second population contains the existing firms and, in addition, firms that either were established or closed down during the 2001-2004 period – "all" firms, for short.

With respect to the existing firms, our results are very much in line with the earlier literature. Thus, we do not find that the regionally differentiated payroll tax has had any significant effects on employment. In this context, it is noticeable that the reform we are investigating involved a decrease in the payroll tax rate that in terms of percentage points reduction was more than twice as large as the reduction in the recent Finnish experiment evaluated by Korkeamäki and Uusitalo (2008), Thus, the reduction that we consider can hardly be dismissed as too small to care about, especially as the eligible firms receive it unconditionally.[20]

We do find significant estimates of positive wage effects; the wage incidence is on average 1/4 of the payroll tax reduction. This result is also in line with previous empirical research. Our results are consistent with the interpretation that in the wage bargaining process the employers and the unions treat the cost reduction as a windfall gain which they split among themselves. That the employers are able to reap 75 percent of

[20] Given this result it seems reasonable to conclude that the general reduction of the Swedish payroll tax rate by 5 percentage points, introduced in 1997, has not had any positive employment effects for existing firms, either.

the gain indicates that their bargaining power markedly exceeds that of the unions. The relatively modest wage incidence might also be due to the fact that firms in RSA A only employ a small fraction of workers in the country. Thus, they probably have little weight in the industry level wage negotiations. A nation-wide payroll tax refom might have quite different wage effects.

Another qualification is in place here. Due to data constraints, we have been confined to analyzing the effects on the number of employees and on the wage bill per employee, instead of measuring employment in terms of hours work and wages in terms of hourly or monthly pay. Thus, e.g., we cannot rule out the possibility that while leaving the number of employees unaffected the tax cut may have affected employment in terms average hours per employee.

With respect to the estimations where we allow for entry and exit of firms – our "all" firms analysis – we haven't found any results in the previous literature. This is most likely due to the problems associated with the definitions of entry and exit, as well as the corresponding outcome variables. Our estimations reflect this uncertainty; we have tried different alternatives and still have not taken a stand regarding preferable specifications.

Estimating, first, linear probability models of entry and exit, we find that, throughout, payroll tax reductions have positive and significant effects on firm entry. On average, a 1 percentage point in the payroll tax rate increases the probability of entry by 0.11 percentage points. In contrast, the point estimates of the impact on firm exit are very unstable with respect to both magnitude and sign, and never significant. The point estimate of the net effect is negative but never significantly different from zero.

To investigate whether employment might be affected through firm entry/exit, we have tried several alternative employment regressions were all firms are included that existed in the pre-reform year and/or in the post-reform years considered. In so doing, we have to account for zero-valued observations. Unfortunately, the estimates are sensitive to the method chosen to handle the zero observations. We tentatively conclude, however, that it appears that the positive effects of a payroll tax reduction on firm entry might yield positive effects on employment, too. In any case, we believe that by extending the effect analysis from the impacts on already existing firms to firms

flowing in and out of the firm population we have opened up for further interesting research.

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