LABOR MARKET EFFECTS OF REDUCED LABOR COSTS: EVIDENCE FROM THE SWEDISH 2007 PAYROLL TAX REFORM

Sven-Olov Daunfeldt, Anton Gidehag and Niklas Rudholm

Abstract: One possible way for policymakers to reduce labor costs and stimulate the recruitment of marginalized groups of labor in a highly-unionized economy is to lower payroll taxes. However, the efficiency of this policy instrument has been questioned and previous evaluations have found small employment effects of such reforms. We investigate the labor market effects of a payroll tax cut in Sweden that decreased firms' labor costs in relation to the number of young employees that they had when the reform was implemented. We find that most employers received relatively small labor cost savings due to the reform, but that employers who received a high treatment intensity dose increased their number of employees significantly more than employers who received no, or minor, labor cost savings. We also find that the payroll tax cut increased the wages of incumbent workers, but that the effect is too small to offset the positive employment effect of the reform. In total, we estimate that the payroll tax reform created approximately 16,500 jobs; of which most were within the targeted group of young employees.

Keywords: Payroll tax reform, labor demand, employment, wages

JEL classifications: H25, H32, J23, J32, L20

1. INTRODUCTION

Unemployment rates and individuals neither in employment, education or training increased in many European countries following the financial crisis, and have in many cases remained at high levels – particularly among first-generation immigrants and young adults with low educational attainment (Papademetriou et al., 2010; Bruno et al., 2014). This development is troublesome considering that it might depreciate the human capital of individuals, and because employers might use long unemployment periods as a negative sorting criterion when recruiting personnel (Lockwood, 1991). Long unemployment periods might thus result in persistent high unemployment rates among these groups of individuals (Phelps, 1972; Heckman & Borjas, 1980; Arulampalam et al., 2001).

In highly-unionized economies – such as the Scandinavian welfare states – policymakers have limited influence over minimum wages because they are set in negotiations between the employer organizations and the trade unions. Under such conditions, policymakers often rely on job subsidies for groups of job seekers that have difficulties in entering the labor market (Martin and Grubb, 2001). However, such policies have been criticized because they typically are time-limited and can crowd-out regular jobs (Martin and Grubb, 2001; Kluve, 2006; Nekby, 2008). An alternative way for policymakers to reduce employers' labor costs under such circumstances is to implement payroll tax cuts. The efficiency of such reforms has been questioned, however, since insiders might use their bargaining power to increase their wages at the expense of outsiders' possibility to get hired (Holmlund, 1983; Gruber, 1997).

In Sweden, the center-right government reduced the payroll tax level for 19-25-year-olds by 11.1 percentage points on July 1, 2007. The aim of the reform was to reduce the high youth unemployment rate at that time. An important and generally overlooked aspect of the reform was that the payroll tax cut covered all young workers, and not just those who were recruited after the reform. Employers with many young employees could thus substantially reduce their labor costs due to the payroll tax cut. Since these savings were directly related to the number of young employees at the firm, the firms can be considered to have received different treatment intensities, or doses, of the reform. We use this variation in treatment intensity across firms to investigate the labor markets effects of the Swedish youth payroll tax reform introduced in 2007.

Our approach differs from most of the previous studies that have analyzed the labor market effects of the Swedish payroll tax reform. Some of these studies (e.g., Egebark

and Kaunitz, 2013; 2014, Skedinger, 2014) compare the outcomes for young individuals who were targeted by the reform with the outcome for slightly older individuals, i.e. who were not subject to the reduced payroll tax. Their results indicate relatively small effects on employment, and the youth payroll tax cut is therefore considered costly in terms of foregone tax revenues (Egebark and Kaunitz, 2013). On the other hand, they find no indications that the lack of major employment effects can be explained by significant wage spillovers to incumbent workers (Egebark and Kaunitz; 2013; 2017).

However, the fact that firms decreased their labor costs in relation to their number of young employees at the time of the reform is ignored when comparing the effects on individuals just below and above the age threshold. Firms that received large labor cost savings might have spent them on recruiting older and more experienced individuals. Thus, the overall employment effect could have been different from the effects previously found for the targeted group of young employees.

Only Egebark and Kaunitz (2017) and Saez et al. (2017) have, as far as we know, acknowledged the link between labor cost savings and the number of young individuals employed at the firms at the time of the implementation of the payroll tax reduction. Egebark and Kaunitz (2017) used the 2006 firm-level wage bill for the targeted group, normalized by firm turnover, as a proxy for how sensitive firms' costs are to the payroll tax reduction, while Saez et al. (2017) used the 2006 firm-level share of total wage costs devoted to young employees as their treatment intensity measure. Egebark and Kaunitz (2017) found no indications that a higher treatment intensity was related to improved firm performance in terms of neither profits, labor productivity or investments, while Saez et al. (2017) found that total employment and other firm performance measures increased relatively more among firms with higher treatment intensities.

However, in contrast to Egebark and Kaunitz (2017) and Saez et al., (2017), we rely on an absolute, rather than a relative, treatment intensity measure. We believe this to be important because labor cost savings are not strictly increasing with relative treatment intensity measures. For example, an employer with three out of four employees below the age of 26 will obtain a higher treatment intensity than a firm that has 20 young employees out of 50 employees in total, although the latter firm experiences a substantially larger labor cost reduction following the payroll tax cut.

By exploiting data on the wages of young employees in 2006, we calculate the expected labor cost saving per firm at the time of the payroll tax cut. Moreover, to assign firms into treatment intensity groups, we construct five equally sized quantiles across the

distribution of expected labor cost savings. Thus, our treatment intensity measure is strictly increasing with the wages paid to young employees. A potential concern with our measure might be that the size of labor cost savings and firm size are positively correlated, which induces the risk of differences in firm size biasing the employment effect. It is generally acknowledged that large firms tend to grow more in absolute terms than small firms (Henrekson and Johansson, 2010). Therefore, we rely on a firm-level difference-in-difference-in-differences (DDD) model to estimate the employment effect of the payroll tax reform. In contrast to ordinary difference-in-difference (DiD) estimation, our DDD model eliminates any underlying bias caused by differential trends in employment growth between the treatment and control groups. In addition to estimating how the employment effect varied with treatment intensity, we also evaluate the size of spillover effects to average wages among incumbent workers.

We find that 80 percent of all firms saved less than 60,000 SEK annually ($6,480)[1], suggesting that most firms experienced fairly modest decreases in their labor costs following the youth payroll tax cut in 2007. This might explain why most previous studies have found relatively small employment effects of the payroll tax reform. We do, however, observe a large variation in labor cost savings across firms, and that employers who received a relatively large treatment dose increased their number of employees significantly more than employers who received a low treatment intensity dose.

More specifically, we find that the average firm within the 60-80 % treatment intensity interval of the labor cost savings distribution recruited 0.37 employees more following the youth payroll tax cut, while the average firm within the top 80-100 % interval hired 0.97 more employees. No statistically significant effects of the payroll tax cut on employment are obtained for firms that obtained only minor reductions in labor costs following the payroll tax reform. We also find that the payroll tax cut mainly is associated with an increased number of employees within the targeted age group (19-25 years of age). However, we find some indications that the employers also increased the recruitment of older individuals, implying that the immediate labor cost savings created by the reform had a small but positive impact on employment outside the targeted age group.

[1] As of April 15, 2019, 1 SEK corresponds to approximately 0.108 USD.

We conclude that the 2007 Swedish payroll tax reduction, implemented to decrease youth unemployment, created approximately 16,500 new jobs. The firm-level employment effect is, however, strongly contingent on the size of labor cost savings and therefore mainly determined by the pre-reform age composition of firms' personnel. Furthermore, we find that the labor cost savings are associated with an increase of the average wage within all treatment intensity groups. The wage increase is, however, rather small and do not appear to dominate the extensive-margin employment effect of the reform.

The outline of this paper is as follows. The next section describes the Swedish 2007 payroll tax reform and provides main findings from previous evaluations. Section 3 includes a description of our data, our treatment intensity measure and presents descriptive statistics. The empirical methodology is explained in section 4. Section 5 includes our empirical findings of how the reform affected firm-level employment and wages. Lastly, section 6 summarizes our results and concludes the paper.

2. THE SWEDISH 2007 PAYROLL TAX REFORM

The Swedish payroll tax is entirely paid by the employers and is proportional to the gross wages of the employees. It has increased substantially during the last 50 years, from 11.65 percent in 1970 to 31.42 percent in 2019 (Swedish Tax Authority, 2019). The payroll tax consists of seven different fees, financing various social benefits such as pensions, parental leave and sick leave.

On July 1, 2007, the previous center-right government reduced the payroll tax for individuals who at the start of the year had turned 18, but not yet 25. The aim of the reduction was to decrease the relatively high and growing unemployment rate among young individuals at that time (Swedish Government, 2006). Of the seven different fees that jointly constituted the payroll tax, six fees were halved and the payroll tax was reduced from 32.42 to 21.32 percent for individuals within the targeted age group.[2] The Swedish government argued that the reduction would result in a substantial amount of foregone tax revenues, but that it should be offset by an increased tax collection from labor incomes (Swedish Government, 2006).

The reform was extended on January 1, 2009, by imposing a further reduction of the payroll tax rate to 15.49 percent and by widening the age group to all individuals who had not yet turned 26 by the start of 2009. Thus, the lower age bound was abolished and all individuals born in 1983 or later were targeted by the extension in 2009 (Swedish Government, 2008).

The political left-wing parties, which were in opposition at the time, criticized the reform. They argued that the reform had been inefficient and costly considering the size of foregone tax revenues. Once elected into office in 2014, the left-wing government decided to restore the payroll tax level for young individuals to the initial level of 31.42 percent.[3] The payroll tax reduction for young employees was completely abolished on June 1, 2016.

A number of studies have previously investigated the labor market effects of the youth payroll tax cut in 2007. Egebark and Kaunitz (2013) used a difference-in-difference model to compare the employment outcomes for young individuals who were targeted

[2] To be precise, the payroll tax was reduced from 32.420 to 21.315 percent. Using two decimals, the previous government stated the new payroll tax level to be 21.31, yielding a reduction of 11.11 percentage points (Swedish Government, 2006). However, during the second half of 2007 the reduction was limited to 9.71 percentage points and, thus, the reform was not fully implemented until the start of year 2008.
[3] From year 2009 and onwards, the standard payroll tax level is equal to 31.42 percent.

by the reform with the outcome for slightly older individuals who did not receive any payroll tax reduction. They found that the reform increased employment among young people by 2.7 percentage points, and that the payroll tax cut only had minor effects on their wages. Overall, they estimated the 2007 reform to have created in-between 6,000-10,000 new jobs per year within the targeted age group.

In a closely related paper, Egebark and Kaunitz (2014) investigated the long-term effect of the reform and whether the reduction increased the number of hours worked among those who were already employed. They found no indications that employment increased along the intensive margin, while the employment effect within the target group declined with age along the extensive margin. Hence, the reform appears to have been most beneficial for the youngest employees, i.e. those who were treated the longest time.

Based on these two studies, Egebark and Kaunitz (2013; 2014) concluded that the Swedish payroll tax reduction had been largely unsuccessful in decreasing youth unemployment. They highlighted that the productivity among many young employees might have remained too low in relation to their reduction in labor costs after the reform. Lastly, they also considered the reform to be costly as the foregone payroll tax revenues per created job was estimated to be 1.2 million SEK.

Skedinger (2014) analyzed the effects of the payroll tax reform on employees in the retail industry, which is an industry with a high proportion of young employees, finding only small positive effects of the payroll tax cut on young people's employment. However, the estimated effect was larger for employees who had a wage close to the agreed minimum wage. This indicates that the relatively high entry-wages in the retail industry obstruct entry into the labor market for young individuals.

To our knowledge, only two studies have emphasized that the payroll tax reform resulted in labor cost savings for employers and that these savings were proportional to their number of young employees when the tax cut was implemented. Egebark and Kaunitz (2017) studied the payroll tax reform solely from a firm perspective, exploiting firm-level panel data. Using the 2006 firm-level wage bill for the target group, normalized by total turnover, they constructed a treatment intensity measure. This measure was meant to capture the degree to which firms' overall hiring costs were affected by the reduced payroll tax. Next, they assessed how firm performance outcomes – profits, labor productivity and investments – evolved by treatment

intensity. They found no evidence suggesting that higher treatment intensity was related to improved performance among firms.

Saez et al. (2017) analyzed the reform and its implications from both an individual-level and a firm-level perspective. Tracing different cohorts of individuals over time, they assessed how employment rates and wages within different age groups changed when the payroll tax cut was implemented. For individuals within the targeted age group, they found noticeable increases in employment but that net wages were unaffected, implying that the employment effects were not offset by potential wage spillovers. From a firm perspective, Saez et al. (2017) argued that firms received a cash flow windfall which was conditional on the total wages paid individuals within the targeted age group. More specifically, they used the 2006 wage bill for young individuals as a share of the total wage bill, as a proxy for firms' exposure to the reform. Moreover, using difference-in-difference estimation, they compared the development of various firm-level variables among firms with different treatment intensities. They found total employment and other firm performance measures to have increased relatively more among firms with higher treatment intensities. Hence, Saez et al. (2017) provided evidence that the payroll tax cut increased overall employment and that the increase was not just limited to young individuals.

3. DATA, TREATMENT INTENSITIES AND DESCRIPTIVE STATISTICS

3.1 Data

Our analysis is based on LISA (Longitudinal Integration Database for Health Insurance and Labour Market Studies), a register-based database provided by Statistics Sweden. LISA covers all Swedish residents that are least 16 years old and provides data on, for example, individuals' employment status, educational background and annual earnings.

In LISA, we can observe the yearly employment status of each individual in the month of November. This information is collected from the RAMS (*Regional Arbetsmarknadsstatistik*) register. According to RAMS, an individual is classified as employed if (s)he has a labor income corresponding to at least one work hour during a specific measurement week in November.

Employment and unemployment statuses in LISA are based on two different registers and are measured at different time points in November, which means that a limited a number of individuals will be simultaneously registered as employed and unemployed in the data. We choose to define these employees as unemployed and exclude them from our analysis since they are likely to have weak labor market positions and cannot be concluded to be participating in regular employment. For the same reason, we also exclude individuals that are studying at either a university or university college during the fall semester.

LISA also includes a unique firm identification number, making it possible for us to connect each employee with his/her employer during the month of November. For each employee, we also have access to data on the total gross wage received from his/her primary employer in each year. We utilize this information in combination with the age of the individuals to calculate the gross wages paid to different age groups by each employer. By connecting employees and their employers in that way, we obtain a matched employer-employee dataset of Swedish firms from 2003 to 2008. In total, our dataset contains information on 8,181,226 individuals and 743,808 firms. To ensure

that the empirical analysis is not inflicted by firm outliers, we exclude all firms which have had extreme yearly changes in employment.[4]

3.2 Treatment intensities

The Swedish 2007 payroll tax reform reduced the labor costs for all firms which had young employees at the time of the reform. Following Saez et al. (2017), we consider firms to receive different doses of the payroll tax cut, i.e. to have different treatment intensities that depend on the size of their labor cost savings. Saez et al. (2017) used a relative measure of the treatment intensity (the ratio of youth wage costs to total wage costs) while we rely on the absolute reduction in labor costs as our treatment intensity measure. We find it likely that decision makers in most firms will use the labor cost savings in SEK (perhaps relative to the wage of the potential new employee) in their evaluation of whether to hire or not, rather than the savings as a share of total wages. If this is the case, firms will tend to recruit new employees based on how much they save in absolute terms, and not on their relative cost savings.

Our treatment intensity is thus strictly increasing with the size of labor cost savings. More, specifically the 2006 $Treatment\ intensity_{i,t}$ of firm i at year t can be written:

$$Treatment\ intensity_{i,t=2006} = (0.3242 - 0.2132) \times W\_young_{it=2006}$$

where the figures 0.3242 and 0.2132 represent the payroll tax levels before and after the tax cut, respectively; and W_young_{it} represents the total gross wages (excluding payroll taxes) payed to individuals covered by the tax reform.

Our treatment intensity measure is constructed as follows. First, we calculate the total gross wages paid by each firm to their employees of ages 18-24 in 2006. Since the payroll tax cut was implemented in mid-2007, this measure works as a proxy for expected labor cost savings from mid-2007 to mid-2008. We expect a strong correlation between our estimated labor cost reductions and the actual reductions that occurred at the time of the reform, but without our measure being affected by firms self-selecting into treatment.[5] We then multiply W_young_{it} by 0.111, which is the

[4] More specifically, all firms which have had an annual employment change of more than +/- four standard deviations from the average change, are excluded. This is equivalent to an annual employment change of +/- 114 employees.

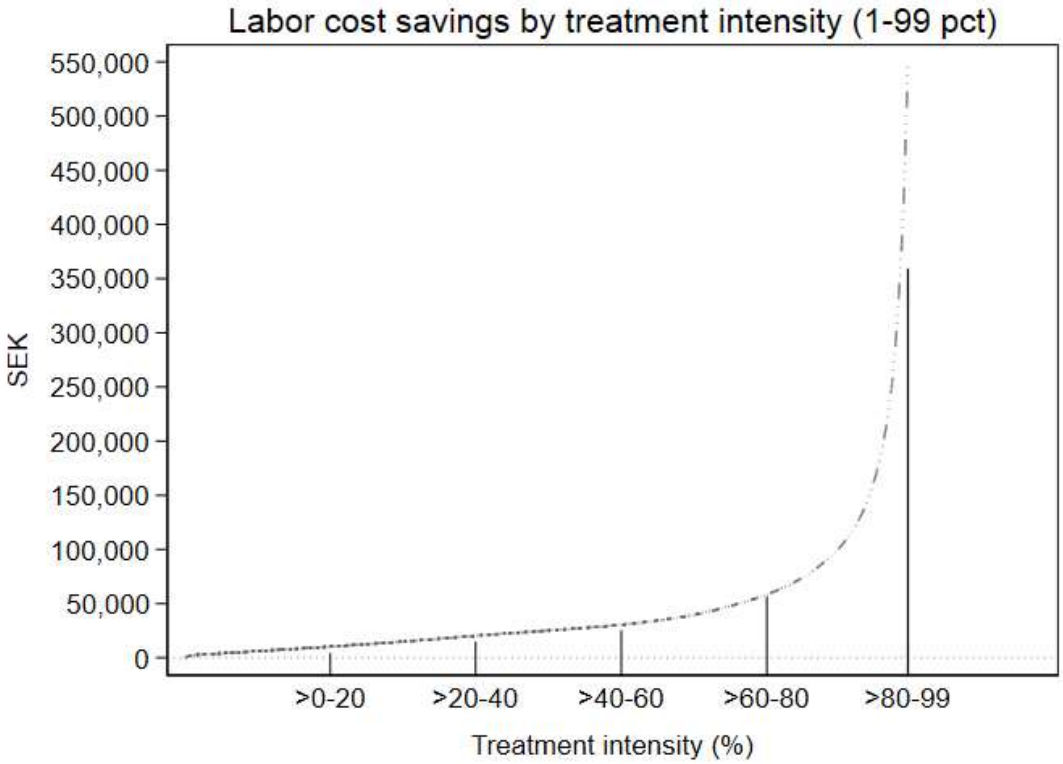
[5] To test this, we plot the distributions of the labor cost savings in 2006 and 2007, respectively. We find the distributions to be very similar, although the 2007 distribution is slightly skewed to the right, implying the employment of, and total wages for, youth to have increased. As such, there is reason to believe that using the cost savings of 2007 would give rise to selection bias as some firms self-select into treatment. See Figure A2 in appendix.

percentage reduction in payroll taxes for these employees once the reform was introduced. This implies that $Treatment\ intensity_{i,t=200}$ measures the size of the one-year labor cost savings that firms receive for young employees in 2006, provided that they remain employed in 2007 at the same wage levels.

Next, we split all firms satisfying $Treatment\ intensity_{i,t=200} > 0$ into five equally sized quantiles based on their rank in the treatment intensity distribution. This is done because we expect the largest treatment effect to be among the firms which receive the largest labor cost savings. The lowest quantile includes the firms with the lowest 20 percent labor cost savings, while the highest quantile includes the firms with labor cost savings in the top >80-100 percent of the treatment intensity distribution. Our control group includes all firms that lacked employees aged 18-24 in 2006, and therefore did not obtain any immediate labor cost savings due to the payroll tax reform.

The relationship between labor cost savings and treatment intensities, with the continuous treatment intensity rate on the x-axis, is presented in Figure 1. Note that of all treated firms, i.e., firms with $Treatment\ intensity_{i,t=200} > 0$, 80 percent saved less than 60,000 SEK (6,480 USD) during the first year after the youth payroll tax cut was implemented. This corresponds to far less than the average annual salary of a full-time employee in Sweden. This means that a vast majority of the employers received a relatively small labor cost savings due to the reform, which partly might explain why previous studies have reported small employment. However, we notice a substantial variation in cost savings within the >80-100 group, with the 99th treatment intensity percentile corresponding to savings of almost 550,000 SEK (59,400 USD).

Figure 1. Labor cost savings by treatment intensity.



Note: Graph shows the expected 2006 labor cost savings measured in SEK on the y-axis. The continuous treatment intensity rate is presented on the x-axis. For visibility reasons, we exclude the upper 100th percentile.

The range of the labor cost savings for each treatment intensity group is presented in Table 1, including the 100th percentile for the >80-100 group. Here, we can notice a maximum one-year labor cost savings which amounts to more than 9 million SEK (972,000 USD.[6]

[6] Further analysis shows that this is for a firm within the retail sector, having 612 young employees and 1475 employees in total during 2006.

Table 1. Labor cost savings by treatment intensity.

Dose group	2006 savings in SEK
>0-20 %	124 – 10,291 *(6,140)*
>20-40 %	10,303 – 20,298 *(14,961)*
>40-60 %	20,310 – 30,466 *(25,178)*
>60-80 %	30,478 – 58,325 *(39,719)*
>80-100 %	58,337 – 9,234,913 *(101,911)*

Note: Measured in price level of 2016. 1 SEK = 0.108 USD. Median cost savings within parentheses.

3.3 Descriptive statistics

Table 2 presents descriptive statistics for our treatment and control groups in year 2006. The average firm size is noticeably larger in the treatment groups than in our control group, where the latter is constituted by firms that do not have young employees prior to the reduction. Furthermore, both the average and median firm size increase with treatment intensity, which is due to the fact that large labor cost savings are associated with a large number of employees. For instance, the average firm in highest dose group has 85 employees, whereas the average firm in the control group has less than three employees.

Moreover, a vast majority of the employees are older than 25. Turning to the share of young employees (18-24-year-olds in 2006), we note that on average in between one fourth and one third of the individuals at the treated firms are about to be covered by the reduced payroll tax. Finally, we note that each dose group contains almost 11,100 firms. In total, we analyze the employment outcomes of approximately 55,000 treated firms and 255,000 control firms.

Table 2. Descriptive statistics for treatment and control groups. Year 2006.

	Mean	Median	Std.dev.	Min	Max	# Firms
Firm size (# Employees)						
Control	2.499	1	6.147	1	481	254,669
Dose >0-20 %	7.917	4	17.368	1	664	11,074
Dose >20-40 %	9.019	5	20.510	1	869	11,070
Dose >40-60 %	10.563	6	23.377	1	817	11,084
Dose >60-80 %	16.812	9	31.775	1	1,093	11,055
Dose >80-100 %	85.040	30	220.940	1	5,132	11,079
# 19-25 yrs old						
Control	0.028	0	0.186	0	9	254,669
Dose >0-20 %	1.127	1	0.937	0	33	11,074
Dose >20-40 %	1.432	1	0.984	0	33	11,070
Dose >40-60 %	1.581	1	1.047	0	19	11,084
Dose >60-80 %	2.595	2	1.671	0	39	11,055
Dose >80-100 %	11.956	6	21.420	0	612	11,079
# >25 yrs old						
Control	2.466	1	6.100	0	479	254,669
Dose >0-20 %	6.525	3	17.144	0	663	11,074
Dose >20-40 %	7.446	3	20.194	0	867	11,070
Dose >40-60 %	8.844	4	23.016	0	810	11,084
Dose >60-80 %	13.952	7	31.272	0	1,086	11,055
Dose >80-100 %	71.941	21	205.378	0	4,939	11,079
Share of young						
Control	0	0	0	0	0	254,669
Dose >0-20 %	0.361	0.25	0.285	0.002	1	11,074
Dose >20-40 %	0.344	0.25	0.270	0.001	1	11,070
Dose >40-60 %	0.301	0.25	0.239	0.002	1	11,084
Dose >60-80 %	0.291	0.25	0.217	0.003	1	11,055
Dose >80-100 %	0.268	0.22	0.187	0.004	1	11,079

Note: Includes all firms surviving years 2006-2008. Outliers (defined as annual employment changes of more than four standard deviations from average change (+/- 114 employees)) are excluded.

4. EMPIRICAL METHODOLOGY

Previous studies evaluating the payroll tax reform have relied upon relative treatment intensity measures, while we measure the treatment intensity in absolute terms. The size of the labor cost savings is thus strictly increasing with treatment intensity, and we assume that it is the labor cost savings created by the reform that incentivizes firms to hire new employees.

However, the amount of labor cost savings and firm size are positively correlated, which means that the average firm size is increasing with treatment intensity. Previous research on firm growth has shown that larger firms grow more in absolute terms than small firms (Henrekson and Johansson, 2010). It is thus likely that firms within the treatment groups will experience a higher employment growth than the control group firms even in absence of a youth payroll tax cut. To correctly identify the treatment effect, we must therefore ensure that the treated and control group firms would have had similar employment growth patterns in absence of the reform, i.e. that the control firms resemble the counterfactual employment outcome of the treated firms.

We rely on a firm-level difference-in-difference-in-differences (DDD) model (Chetty et al, 2009; Gruber, 1994) to achieve this goal. Our DDD model separates the employment effect caused by the payroll tax reform from any other factors that could cause differences in employment growth between the treated and control firms. Essentially, our DDD model compares the employment among treated and control firms before and after treatment at two different time periods. In contrast to a difference-in-difference (DiD) model, the DDD model eliminates bias by deducting potential differences in employment growth trends between the treated- and control firms during the time period 2003-2005. Technically, the DDD model captures the difference between two DiD estimates across the time periods 2003-2005 and 2006-2008, respectively.[7] The underlying DiD for the time period 2003-2005 accounts for differences in employment growth between treatment and control firms in the pre-reform period. By deducting this estimate, we ensure that the estimated employment effects are not affected by non-parallel trends in employment growth between the treated and control firms in the pre-treatment period.[8] Note that if no differences in employment growth exist during 2003-

[7] The reason for limiting our analysis to the time period 2006-2008 is twofold. First, analyzing the employment effects of the further reform extension in 2009 would be difficult since firms could self-select into treatment once the reform was implemented. Second, the likelihood of noise biasing our results increases with the length of the post-treatment period (Mian and Sufi, 2012).
[8] For the 2003-2005 DiD-estimation, we calculate (placebo) treatment intensities using data for 2003.

2005, our DDD model and an ordinary DiD model for the period 2006-2008, would provide identical estimates. For clarification, we decompose the estimates of Figure 2 below into separate DiD estimates over the time periods 2003-2005 and 2006-2008, respectively.[9]

Our DDD model can be expressed as

$$\begin{aligned} Size_{ijt} = \ & \alpha + \beta_1 Time_t + \beta_2 Group_j + \beta_3 Treat_i + \beta_4 (Group_j * Time_t) \\ & + \beta_5 (Treat_i * Time_t) + \beta_6 (Group_j * Treat_i) \\ & + \delta_{DDD} (Group_j * Treat_i * Time_t) + \varepsilon_{ijt} \quad (1) \end{aligned}$$

where i denotes firm, j denotes group (treated or control) and t denotes year. $Time_t$ is a time indicator that is equal to zero for the years 2003 and 2006, and equal to one for the years 2004, 2005, 2007 and 2008. Thus, $Time_t$ is equal to one for the post-treatment years of both the actual reform period and the assumed treatment time period during 2003-2005 used to account for underlying differences in employment trends between treated and control firms. $Group_j$ is a group indicator being equal to zero for the control groups used in the time periods 2003-2005 and 2006-2008, and equal to one for the corresponding treatment groups. Moreover, $Treat_i$ separates all firms used in the time period 2003-2005, i.e. that account for differential employment trends, from firms analyzed in the reform period of 2006-2008 by being equal to zero for the former group and equal to one for the latter group. Our variable of main interest is the interaction term of these three variables, $Group_j * Treat_i * Time_t$, which is equal to one for the treatment group in the actual post-treatment years of 2007-2008. Its population parameter, δ_{DDD}, captures the treatment effect of reduced payroll taxes on employment, net of other factors that could cause differences in employment growth between treated and control firms and can be written as

[9] See Figure A1 in Appendix.

$$
\begin{aligned}
\delta_{DDD} = E\left[Size_{ijt} \middle| Time_t = 1, Group_j = 1, Treat_i = 1\right] \\
- E\left[Size_{ijt} \middle| Time_t = 0, Group_j = 1, Treat_i = 1\right] \\
- \left(E\left[Size_{ijt} \middle| Time_t = 1, Group_j = 0, Treat_i = 1\right]\right. \\
\left. - E\left[Size_{ijt} \middle| Time_t = 0, Group_j = 0, Treat_i = 1\right]\right) \\
- \left(E\left[Size_{ijt} \middle| Time_t = 1, Group_j = 1, Treat_i = 0\right]\right. \\
\left. - E\left[Size_{ijt} \middle| Time_t = 0, Group_j = 1, Treat_i = 0\right]\right) \\
- \left(E\left[Size_{ijt} \middle| Time_t = 1, Group_j = 0, Treat_i = 0\right]\right. \\
\left. - E\left[Size_{ijt} \middle| Time_t = 0, Group_j = 0, Treat_i = 0\right]\right)
\end{aligned}
$$

where the first four lines of the expression represent an ordinary difference-in-difference estimate across the reform period 2006-2008, whereas the last four lines represent an underlying difference-in-difference estimate across 2003-2005 that accounts initial differences in employment growth. Hence, our DDD estimate deducts bias caused by differences in pre-treatment trends and generates the treatment effect of the payroll tax reform.[10] In our context, initial differences in average firm size are likely to be the major concern and we consider δ_{DDD} to be unaffected by such differences.

We estimate our DDD model separately for each treatment intensity group. Hence, the obtained parameter estimates $\hat{\delta}$ will indicate if the employment effect varies with the size of labor cost savings, which is to be expected if it is the cash flow windfall that causes firms to hire.

[10] Note that the firm size differences in the treatment and control groups in the underlying 2003-2005 period and the 2006-2008 period are very similar (compare Table 2 and Table A1). Thus, we expect that employment growth differences that are caused by differences in average firm size will also be practically identical in the time periods 2003-2005 and 2006-2008, respectively, and thus accounted for in the DDD-model.

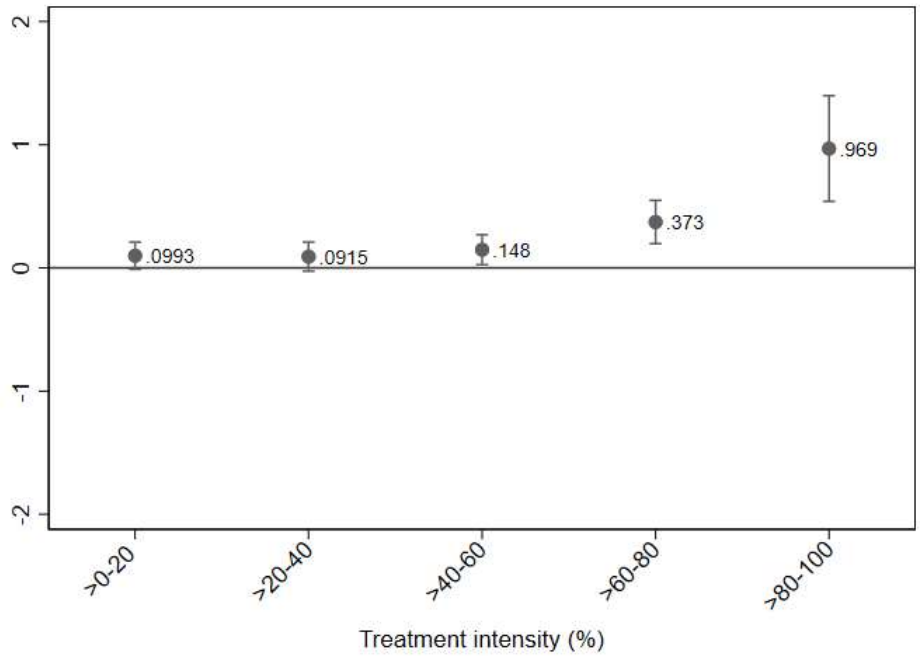
5. FINDINGS

5.1 Effects on number of employees

For our baseline results, we estimate model (1) using OLS within-firm estimation. Consequently, we account for any time-invariant firm-specific effects that might affect our results. In the Appendix, we provide alternative specifications in which we account for heterogeneity across both industries and municipalities (see Table A2-A4). We do also estimate model (1) using firm-level random effects instead of fixed effects. All employment effects are estimated over the 2006-2008 period, meaning that we estimate the employment effects during the first 18 months after the reform introduction.

Figure 2 presents the estimated overall employment effects for all individuals, regardless of age.[11] For each treatment intensity group, we present the estimated employment effect and the 95 percent confidence interval.

Figure 2. Employment effects by treatment intensity. DDD estimation.



Notes. Dependent variable: Firm size (number of employees). Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year, are included. Outliers (defined as annual employment changes of more than four standard deviations from average change (+/- 114 employees)) are excluded. Firm clustered standard errors.
*p<0.1, **p<0.05, ***p<0.01

[11] The corresponding regression tables for our DDD estimations are presented in Table A2 in appendix. Estimates in the figure are represented by the average treatment effect (ATE) estimate in the fifth column of each table.

We find that the labor cost savings are associated with increased employment in the three highest treatment intensity groups, suggesting that firms which received large labor cost savings due to the payroll tax reform increased their number of employees significantly more than firms with small, or no, savings.

From Figure 2, it is also implied that the effect of the youth payroll tax cut on the hiring of new employees is also increasing with firms' labor cost savings. The payroll tax cut is associated with an average increase of 0.15 employees for firms within the >40-60 percent treatment intensity range. The corresponding employment effects for firms within the >60-80 and >80-100 percent ranges amount to 0.37 and 0.97 employees, respectively. In total, considering that each treatment intensity group consists of nearly 11,100 firms, we find that the payroll tax cut for young employees is associated with approximately 16,500 new jobs over the time period 2006-2008.[12]

For reasons of comparison, we do also estimate the employment effects using a traditional difference-in-difference model (DiD). As mentioned, our DDD model estimates the difference between two DiD models across the time periods 2006-2008 and 2003-2005, respectively. Thus, the treated and control firms had different employment trends prior to the payroll tax reform if the estimates using DDD and DiD differ. Conversely, similar 2006-2008 estimates from DDD and DiD would imply parallel pre-treatment trends and, hence, that the underlying DiD for the time period 2003-2005 render insignificant estimates. The DiD estimates can be found in appendix (see Figure A1).

Estimating the 2006-2008 employment effects using DiD yields substantially larger point estimates than using DDD. For instance, the estimated employment effect within the >80-100 range amounts to 1.38 individuals, which should be compared to the corresponding estimate of 0.97 in Figure 2 above. Furthermore, the confidence intervals are typically narrower using DiD, yielding higher statistical significance. Since the DiD estimates are considerably larger than the DDD estimates, it implies that the treated firms have had a higher employment growth than the control firms also in the pre-reform years of 2003-2005. Evaluating the DiD estimates for the 2003-2005 period reveals that this is the case (lower part of Figure A1). This finding further motivates our choice to rely on DDD to accurately estimate the employment effects.

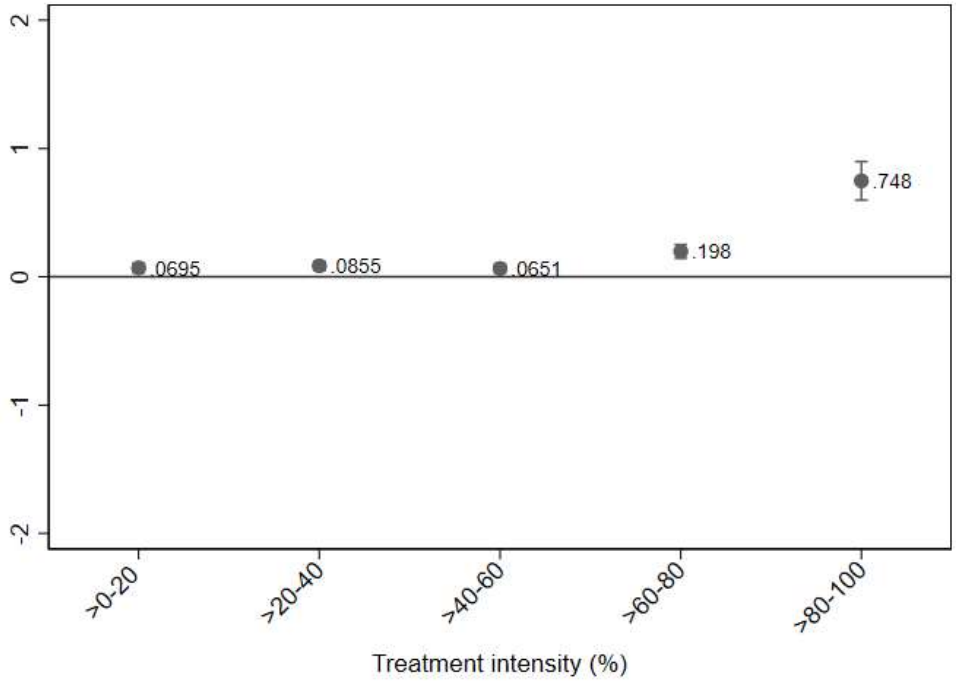
[12] This number is derived by relating the statistically significant point estimates to the corresponding number of firms, i.e., 0.148*11,084+0.373*11,055+0.969*11,079=16,499 jobs.

Next, we investigate the effect of the payroll tax reform on the recruitment of employees within the targeted age group (19-25-year-olds) and compare it with the effect on the hiring of older individuals (above 25 years of age). This is of importance because the treated firms, on the one hand, might recruit more young employees after the reform since the payroll tax for young individuals was reduced by 11 percentage points. On the other hand, employers could also use their labor cost savings to recruit more experienced and senior employees.[13]

The estimated employment effects for 19-25-year-olds are presented in Figure 3. In contrast to the previous findings regarding the total employment effect, our DDD estimates suggest that firms within all treatment intensity groups increased their number of young employees. Albeit the estimates for the first three treatment intensity groups are very small, the employment effect is generally increasing with the size of labor cost savings. For firms in the highest treatment intensity group, we find the payroll tax cut to have increased the employment of individuals within the targeted age group by, on average, 0.75 employees per firm. Relating the point estimates of Figure 3 to the corresponding number of firms, we conclude that approximately 12,900 of the 16,500 jobs in total were created for young individuals, targeted by the reform.

[13] I.e., the reform has both substitution and income effects for the affected firms. The substitution effect incentivizes the employment of young individuals as they become relatively less costly to hire. The income effect is created due to the labor cost savings and could promote employment of individuals outside the targeted age group.

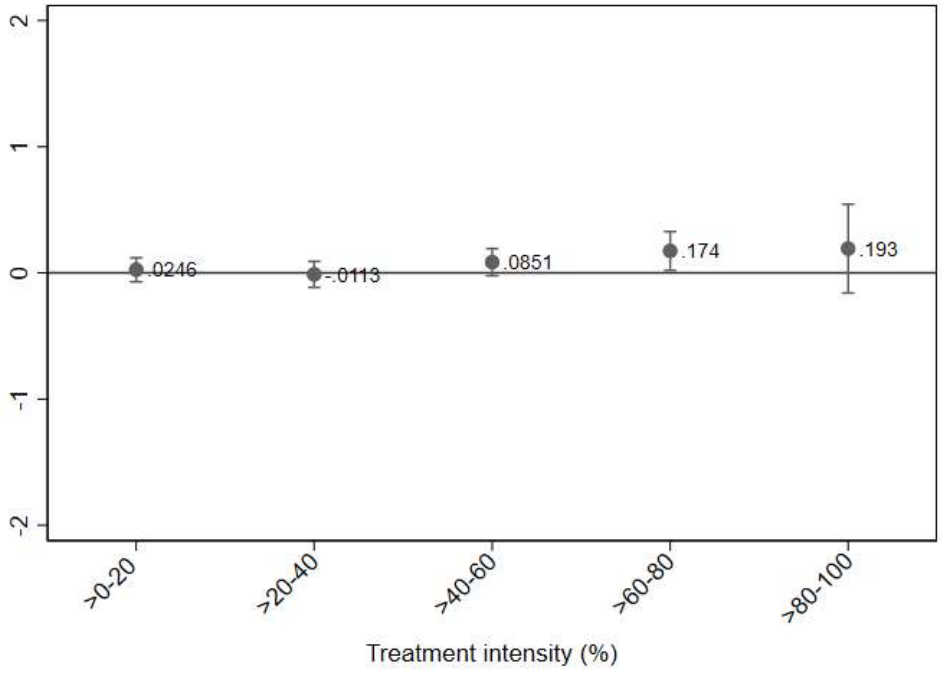
Figure 3. Employment effect among 19-25-year-olds by treatment intensity. DDD estimation.



Notes. Dependent variable: Number of young employees aged 19-25. Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year, are included. Outliers (defined as annual employment changes of more than four standard deviations from average change (+/- 114 employees)) are excluded. Firm clustered standard errors. *p<0.1, **p<0.05, ***p<0.01

The estimated employment effects for individuals who were at least 26 years old at the time of the reform, i.e. who were not directly targeted by the tax cut, are presented in Figure 4. The DDD estimates are generally statistically insignificant. An exception is found for the second highest treatment intensity group, in which the payroll tax reform, on average, increased the employment of older individuals by 0.17 employees per firm. Note also that the point estimate for the largest treatment intensity group is positive and large in magnitude, but not statistically significant at the conventional 5% significance level.

Figure 4. Employment effect among older individuals by treatment intensity. DDD estimation.



Notes. Dependent variable: Number of employees above the age of 25. Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year, are included. Outliers (defined as annual employment changes of more than four standard deviations from average change (+/- 114 employees) are excluded Firm clustered standard errors. *p<0.1, **p<0.05, ***p<0.01

To summarize, we find that the labor cost savings associated with the payroll tax reform had employment-promoting effects and, especially, among firms with the largest savings. The positive employment effects are mainly explained by an increased recruitment of the reform's target group, i.e. 19-25-year-olds. We do however also find some indications of an increased employment outside the targeted age group.

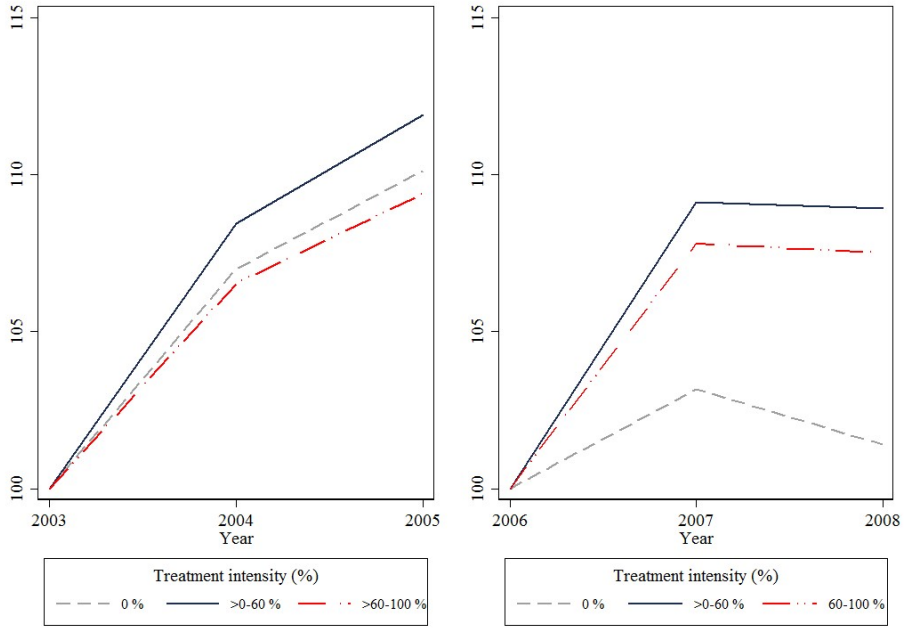
5.2 Effects on wages

From the standard model of labor demand and supply, we should expect at least part of the payroll tax cut to generate wage spillovers. The wage effect will, however, depend on the elasticities of labor demand and supply. Furthermore, the relative bargaining power of employers and trade unions will determine whether, and to which extent, reduced labor costs are translated into wage increases.

We analyze how the average wages changed post-reform within the different treatment intensity groups. To ensure that our estimates are unaffected by firms' employment decisions, we limit our analysis to only include average wages among individuals that

were working at the same firm during the time periods 2003-2005 and 2006-2008, respectively. For instance, since the payroll tax cut especially increased the employment of young individuals – with wages typically below average – our estimates could be underestimated if we would include newly recruited individuals in the analysis. By only studying the average wages of individuals that were working for the same employer pre- and post-reform, we are able to assess if the labor cost savings generated wage spillovers for incumbent employees.

Figure 5. Relative changes in average gross wage per employee. Individuals staying at the same firm.



Notes: Percentage change in average wages for job non-switchers. Base years: 2003 and 2006. Average wages are adjusted for inflation, measured in the price level of year 2016. Outliers (defined as extreme annual changes in employment and/or wages) are excluded. Employment outliers are defined as annual employment changes of more than four standard deviations from average change (+/- 114 employees). Wage outliers are defined as annual changes in average gross wage per employee of more than three standard deviations from the average change.

Figure 5 plots the relative change in average wages during the pre-reform period 2003-2005 and the reform period 2006-2008. For visibility reasons, we split the treated firms into two treatment intensity groups. The first group includes treated firms within the >0-60 range, i.e. the three lower treatment intensity groups, while the second group includes firms within the >60-100 range (the two upper treatment intensity groups). In the pre-reform years 2003-2005, we find the wage trends to be fairly similar between the (placebo) treatment and control groups. The groups have

experienced relative increases of the average wage, ranging between approximately 8 to 12 percent. Interestingly, the average wage for the treated firms – especially within the >60-100 range - increases by more during 2006-2007 than 2003-2004, whereas the opposite holds for the control firms. Hence, these findings could imply that part of the labor cost savings were shifted into wage increases for individuals that were already employed at the time of the reform. However, the average wage developments do not suggest a clear link between the treatment intensity and wage growth, as firms within the lower >0-60 % group experienced larger wage increases over the time period 2006-2008 than firms within the upper >60-100 % group.

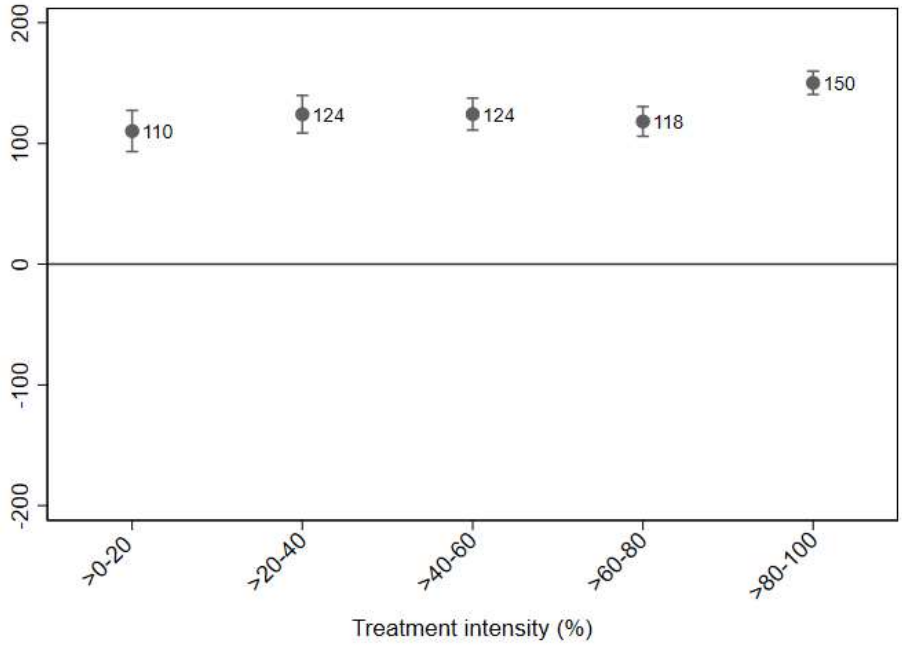
In Figure 6, we estimate the effect on average wages among incumbent employees using our DDD model.[14] The point estimates suggest statistically significant increases of the average wage across all treatment intensity groups. On average, firms within the lowest and highest treatment intensity groups have increased the average wage per employee by 11,000 SEK (1,188 USD) and 15,000 SEK (1,620 USD), respectively across the time period 2006-2008 (relative to the control group and the pre-reform period 2003-2005). Relating these point estimates to average wages in 2006, we find that the treated firms have had average wage increases above their control group counterparts of roughly 4-5 percent during the period 2006-2008.

Hence, in contrast to the estimated employment effects of the payroll tax reform, we do not find a strong and positive link between the average wage increase and the size of labor cost savings. A potential explanation might be that firms within the lower treatment intensity groups did not obtain large enough labor cost savings to increase employment along the extensive margin (additional recruitment), but instead increased employment along the intensive margin (number of working hours) and that we thus find higher reported annual wages per employee in these groups also.[15]

We also do separately analyze the average wages among individuals above the age of 25. The estimates suggest statistically significant wage increases also among individuals who were not directly targeted.[16] Our results are in line with Saez et. al (2017) finding of rent sharing, i.e. that the reform caused average wage increases also among non-targeted individuals.

[14] Complete regression tables are available upon request.
[15] Unfortunately, we lack data on the number of work hours and, thus, we cannot examine intensive margin effects further.
[16] These results are available upon request.

Figure 6. Effect on average wages among staying employees, by treatment intensity. DDD estimation.



Notes: Dependent variable: Average wage per employee (measured in 100SEK). Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year, are included. Outliers (defined as extreme annual changes in employment and/or wages) are excluded. Employment outliers are defined as annual employment changes of more than four standard deviations from average change (+/- 114 employees). Wage outliers are defined as annual changes in average gross wage per employee of more than three standard deviations from the average change. Firm clustered standard errors. *p<0.1, **p<0.05, ***p<0.01

6. SUMMARY AND DISCUSSION

In the Scandinavian welfare states, minimum wages are set in negotiations between the employers' organizations and the trade unions. The politicians have very limited opportunities to influence the minimum wages within this institutional context. Under such circumstances, payroll taxes represent another opportunity for politicians to influence the labor costs of employers and thus their labor demand. However, several studies have argued that payroll tax cuts are inefficient because insiders will use their bargaining power to increase their wages at the expense of outsiders' opportunities to get employed.

We have investigated the efficiency of a payroll tax reform in Sweden that lowered payroll taxes for employees aged 19-25 years by 11 percentage points. The reform was designed so that employers received a reduced payroll tax for all individuals aged 19-25 years who were employed when the reform was implemented. The employers thus received reduced labor costs, which were dependent on their number of young employees when the payroll tax cut was implemented, and they could use their labor cost savings to employ both young individuals within the target group of the reform and more senior employees.

To study the total employment effect of the reform, we have explicitly considered that it was the employers who received a treatment; not the employees. We have also taken into account that there was a variation in treatment intensity between the firms, depending on how many young employees they had employed at the time of the reform. Employers who had many young employees received a large reduction in labor costs, i.e., a high treatment intensity, while employers who had few young employees received a smaller reduction in their labor costs.

We use the variation in the treatment intensity across firms to evaluate the effects of the payroll tax reform on both the number of employees and average wages among incumbent workers. Our empirical analysis is based on matched employer-employee data from Statistics Sweden covering all residents in Sweden that are at least 16 years old. To account for the fact that firms with a high treatment intensity are larger on average and tend to grow more in absolute terms than small firms, a difference-in-difference-in-differences (DDD) model was estimated. Unlike an ordinary difference-in-difference (DiD) model, our DDD model was able to account for any factor that could have caused the trends in the outcome variable to differ between the treated and control firms prior to the reform.

Our results showed that most employers received a relatively small reduction in labor costs when the youth payroll tax cut was implemented in 2007. However, employers who received a relatively large reduction in labor costs increased their employment significantly more than employers who received marginal reductions or no reduction at all. We also found that the reform mainly increased the employment of 19-25-year-olds, who were the targeted age group. In total, we calculate that the 2007 payroll tax reform created approximately 16,500 jobs over the period 2006-2008. Approximately 12,900 of these jobs were created for young individuals, belonging to the reform's target group. This is a considerably larger job creation than in Egebark and Kaunitz (2013), who estimated that 6-10,000 jobs per year were created within the target group and who, moreover, considered it likely that these numbers overestimated the net job creation. Saez et al. (2017) estimated that total employment increased by 2.8-6.5 percent, depending on firms' treatment intensity.

Our study differs in two important respects from the studies mentioned above. First, in contrast to Egebark and Kaunitz (2013), we considered the employers rather than the employees to be subject to treatment. Furthermore, we have argued that it was primarily the labor cost savings in absolute – not in relative - terms that determined the firm-level employment and, thus, we calculated an absolute treatment intensity measure unlike Saez et al. (2017). Hence, payroll tax cuts which reduce the labor costs for employers can be an effective way of facilitating the labor market introduction for groups of workers who have difficulties in getting a regular employment.

One concern when implementing payroll tax reforms is that insiders can take advantage of the labor cost reductions by increasing their wages at the expense of outsiders' possibilities to get employed. We have therefore also investigated the effect of the youth payroll tax reform on the gross wages of incumbent workers. Our results showed that employers who received labor cost reductions increased the gross wages of incumbent workers more than employers who did not receive any labor cost reductions. Moreover, we found that the wage increases were not limited to young individuals directly targeted by the reform, which is in line with Saez et al. (2017) finding of rent sharing. The wage increases were however not large enough to offset increased employment along the extensive margin. Unfortunately, the data does not allow us to investigate if the wage effect is due to higher actual wages or more working hours, i.e., if the employers used the labor cost reductions to let the incumbents work more hours. We believe that more research is needed on how payroll tax reforms

influence the labor incomes of those already employed and to which extent it hinders outsiders' possibility to enter employment.

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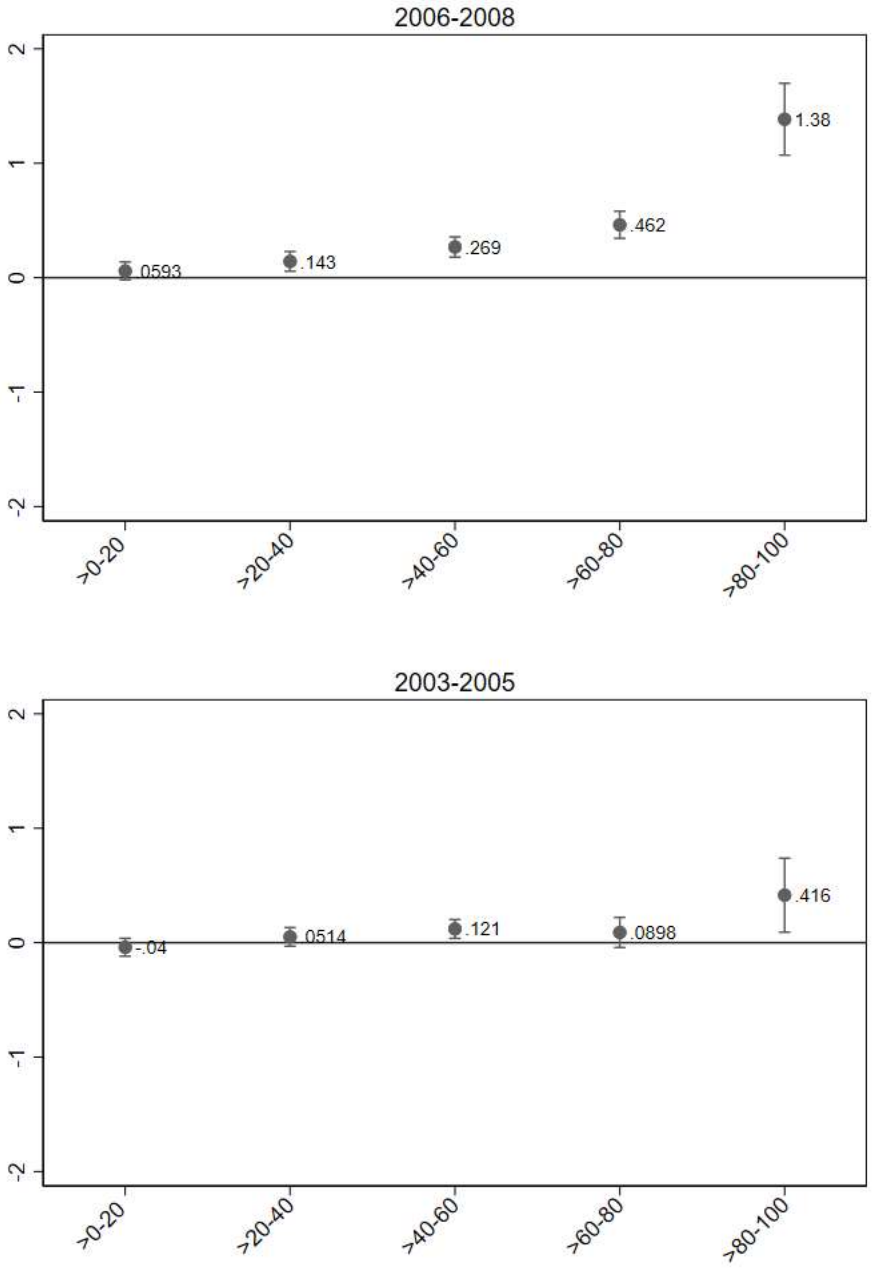
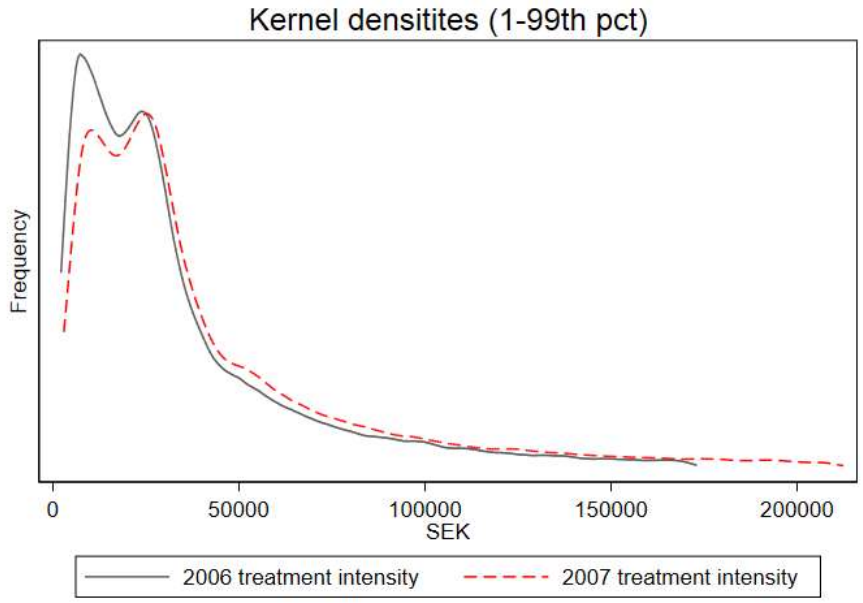
APPENDIX

Figure A1. Employment effects by treatment intensity. DiD estimation.



Notes: Dependent variable: Firm size (no. employees). Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year, are included. Outliers (defined as annual employment changes of more than four standard deviations from average change (+/- 114 employees)) are excluded. Firm clustered standard errors. *p<0.1, **p<0.05, ***p<0.01

Figure A2. Labor cost savings in 2006 and 2007.



Notes: Figure shows the firm-level distribution of estimated and actual labor cost savings in years 2006 and 2007, respectively. Measured in the price-level of 2016. Including the 1-99th percentile of each distribution.

Table A1. Firm size in 2003, by treatment and control groups for time period 2003-2005.

	Mean	Median	Std.dev.	Min	Max	# Firms
Firm size (# Employees)						
Control	2.618	1	6.030	1	761	234,380
Dose >0-20 %	7.747	4	17.432	1	1,061	9,876
Dose >20-40 %	8.621	5	19.464	1	1,035	9,833
Dose >40-60 %	10.772	6	19.049	1	516	9,871
Dose >60-80 %	17.669	10	31.300	1	768	9,865
Dose >80-100 %	90.622	32	228.402	1	4,733	9,862
# 19-25 yrs old						
Control	0.036	0	0.212	0	9	234,380
Dose >0-20 %	1.095	1	0.827	0	14	9,876
Dose >20-40 %	1.403	1	0.909	0	22	9,833
Dose >40-60 %	1.510	1	0.929	0	15	9,871
Dose >60-80 %	2.540	2	1.627	0	34	9,865
Dose >80-100 %	11.644	6	22.174	1	707	9,862
# >25 yrs old						
Control	2.581	1	5.974	0	756	234,380
Dose >0-20 %	6.373	3	17.234	0	1,058	9,876
Dose >20-40 %	7.063	3	19.193	0	1,032	9,833
Dose >40-60 %	9.134	4	18.733	0	509	9,871
Dose >60-80 %	14.867	7	30.872	0	764	9,865
Dose >80-100 %	77.763	23	211.933	0	4,607	9,862

Note: Includes firms surviving 2003-2005. *Outliers (defined as annual employment changes of more than four standard deviations from average change (+/- 114 employees)) are excluded.*

Table A2. 2006-2008 employment effect by treatment intensity. DDD regressions

Specification:	**1**	**2**	**3**	**4**	**5**	**6**
Tr. intensity:	*0-20*	*0-20*	*0-20*	*0-20*	*0-20*	*0-20*
Time	0.161***	0.161***	0.152***	0.156***	0.161***	0.161***
	(0.0196)	(0.0183)	(0.00434)	(0.00435)	(0.00407)	(0.00407)
Group	5.420***	5.420***	4.934***	4.929***	0.609***	0.904***
	(0.0794)	(0.523)	(0.183)	(0.182)	(0.0753)	(0.0726)
Treat	-0.110***	-0.110***	-0.0758***	-0.0707***	0.0770***	0.0721***
	(0.0222)	(0.0257)	(0.0148)	(0.0148)	(0.00616)	(0.00610)
Group*Time	-0.0400	-0.0400	-0.0452	-0.0480	-0.0400	-0.0400
	(0.0972)	(0.0543)	(0.0399)	(0.0399)	(0.0397)	(0.0397)
Time*Treat	0.0270	0.0270	0.0323***	0.0299***	0.0270***	0.0270***
	(0.0272)	(0.0207)	(0.00578)	(0.00579)	(0.00546)	(0.00546)
Group*Treat	0.372***	0.372	0.312	0.279	0.359***	0.334***
	(0.110)	(0.264)	(0.242)	(0.242)	(0.0891)	(0.0866)
ATE	0.0993	0.0993	0.107*	0.106*	0.0993*	0.0993*
	(0.134)	(0.0788)	(0.0568)	(0.0568)	(0.0563)	(0.0563)
Constant	2.692***	2.692***	1.022***	3.429***	2.792***	2.857***
	(0.0160)	(0.122)	(0.0144)	(0.774)	(0.00488)	(0.0143)
Observations	1,396,224	1,396,224	1,396,224	1,396,224	1,396,224	1,396,224
R-squared	0.022	0.022	0.133	0.138	0.007	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Specification:	**1**	**2**	**3**	**4**	**5**	**6**
Tr. intensity:	*20-40*	*20-40*	*20-40*	*20-40*	*20-40*	*20-40*
Time	0.161***	0.161***	0.152***	0.156***	0.161***	0.161***
	(0.0204)	(0.0183)	(0.00438)	(0.00439)	(0.00407)	(0.00407)
Group	6.158***	6.158***	5.513***	5.489***	0.480***	0.839***
	(0.0818)	(0.494)	(0.155)	(0.155)	(0.0773)	(0.0742)
Treat	-0.110***	-0.110***	-0.0795***	-0.0740***	0.0762***	0.0715***
	(0.0231)	(0.0257)	(0.0148)	(0.0148)	(0.00620)	(0.00614)
Group*Time	0.0514	0.0514	0.0316	0.0269	0.0514	0.0514
	(0.100)	(0.0716)	(0.0422)	(0.0422)	(0.0415)	(0.0415)
Time*Treat	0.0270	0.0270	0.0321***	0.0297***	0.0270***	0.0270***
	(0.0283)	(0.0207)	(0.00587)	(0.00587)	(0.00546)	(0.00546)
Group*Treat	0.544***	0.544	0.605***	0.588**	0.669***	0.624***
	(0.113)	(0.342)	(0.230)	(0.229)	(0.0999)	(0.0972)
ATE	0.0915	0.0915	0.117*	0.121**	0.0915	0.0915
	(0.138)	(0.103)	(0.0611)	(0.0611)	(0.0604)	(0.0604)
Constant	2.692***	2.692***	1.011***	3.954***	2.831***	2.903***
	(0.0166)	(0.122)	(0.0148)	(0.624)	(0.00511)	(0.0150)
Observations	1,397,784	1,397,784	1,397,784	1,397,784	1,397,784	1,397,784
R-squared	0.028	0.028	0.192	0.196	0.008	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Specification:	**1**	**2**	**3**	**4**	**5**	**6**
Tr. intensity:	*40-60*	*40-60*	*40-60*	*40-60*	*40-60*	*40-60*
Time	0.161***	0.161***	0.152***	0.156***	0.161***	0.161***
	(0.0208)	(0.0183)	(0.00439)	(0.00441)	(0.00407)	(0.00407)
Group	8.227***	8.227***	7.427***	7.404***	0.500***	0.951***
	(0.0828)	(0.488)	(0.181)	(0.180)	(0.0824)	(0.0801)
Treat	-0.110***	-0.110***	-0.0790***	-0.0727***	0.0794***	0.0746***
	(0.0236)	(0.0257)	(0.0149)	(0.0149)	(0.00619)	(0.00613)
Group*Time	0.121	0.121*	0.124***	0.118***	0.121***	0.121***
	(0.101)	(0.0659)	(0.0436)	(0.0436)	(0.0422)	(0.0422)
Time*Treat	0.0270	0.0270	0.0337***	0.0313***	0.0270***	0.0270***
	(0.0289)	(0.0207)	(0.00591)	(0.00592)	(0.00546)	(0.00546)
Group*Treat	-0.0224	-0.0224	0.0957	0.0941	0.723***	0.706***
	(0.114)	(0.371)	(0.264)	(0.263)	(0.0956)	(0.0937)
ATE	0.148	0.148	0.143**	0.149**	0.148**	0.148**
	(0.140)	(0.0942)	(0.0635)	(0.0635)	(0.0619)	(0.0619)
Constant	2.692***	2.692***	1.000***	2.391**	2.908***	2.976***
	(0.0170)	(0.122)	(0.0155)	(1.102)	(0.00520)	(0.0152)
Observations	1,398,978	1,398,978	1,398,978	1,398,978	1,398,978	1,398,978
R-squared	0.044	0.044	0.163	0.167	0.009	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Specification:	**1**	**2**	**3**	**4**	**5**	**6**
Tr. intensity:	*60-80*	*60-80*	*60-80*	*60-80*	*60-80*	*60-80*
Time	0.161***	0.161***	0.151***	0.155***	0.161***	0.161***
	(0.0244)	(0.0183)	(0.00453)	(0.00455)	(0.00407)	(0.00407)
Group	15.13***	15.13***	14.03***	13.95***	1.203***	2.276***
	(0.0970)	(0.940)	(0.294)	(0.294)	(0.149)	(0.144)
Treat	-0.110***	-0.110***	-0.0829***	-0.0775***	0.0797***	0.0751***
	(0.0277)	(0.0257)	(0.0150)	(0.0150)	(0.00626)	(0.00620)
Group*Time	0.0898	0.0898	0.0841	0.0800	0.0898	0.0898
	(0.119)	(0.123)	(0.0677)	(0.0676)	(0.0671)	(0.0671)
Time*Treat	0.0270	0.0270	0.0342***	0.0315***	0.0270***	0.0270***
	(0.0339)	(0.0207)	(0.00611)	(0.00612)	(0.00546)	(0.00546)
Group*Treat	-0.685***	-0.685	-0.541	-0.529	1.208***	1.168***
	(0.133)	(0.482)	(0.381)	(0.381)	(0.142)	(0.140)
ATE	0.373**	0.373**	0.381***	0.386***	0.373***	0.373***
	(0.163)	(0.171)	(0.0910)	(0.0909)	(0.0896)	(0.0896)
Constant	2.692***	2.692***	1.005***	3.697***	3.151***	3.222***
	(0.0199)	(0.122)	(0.0153)	(0.825)	(0.00671)	(0.0181)
Observations	1,399,332	1,399,332	1,399,332	1,399,332	1,399,332	1,399,332
R-squared	0.098	0.098	0.215	0.219	0.013	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Notes. Dependent variable: Firm size (no. employees). Treatment period: 2006-2008. Underlying time period: 2003-2005. Only surviving firms with at least one employee per year, are included. Point estimates in figures are in the fifth column. DDD estimation.
*p<0.1, **p<0.05, ***p<0.01

Cont. – Table A2. 2006-2008 employment effect by treatment intensity. DDD regressions.

Specification:	**1**	**2**	**3**	**4**	**5**	**6**
Tr. intensity:	*80-100*	*80-100*	*80-100*	*80-100*	*80-100*	*80-100*
Time	0.161	0.161***	0.144***	0.146***	0.161***	0.161***
	(0.125)	(0.0183)	(0.00839)	(0.00851)	(0.00407)	(0.00407)
Group	88.38***	88.38***	71.39***	71.26***	5.477***	8.659***
	(0.496)	(15.91)	(1.494)	(1.495)	(0.588)	(0.576)
Treat	-0.110	-0.110***	-0.202***	-0.194***	0.0689***	0.0659***
	(0.142)	(0.0257)	(0.0347)	(0.0350)	(0.00661)	(0.00659)
Group*Time	0.416	0.416	0.154	0.156	0.416**	0.416**
	(0.608)	(1.607)	(0.275)	(0.276)	(0.165)	(0.165)
Time*Treat	0.0270	0.0270	0.0345***	0.0329**	0.0270***	0.0270***
	(0.174)	(0.0207)	(0.0129)	(0.0130)	(0.00546)	(0.00546)
Group*Treat	-4.936***	-4.936*	-1.885*	-1.859*	4.739***	4.700***
	(0.682)	(2.975)	(1.051)	(1.051)	(0.381)	(0.380)
ATE	0.969	0.969	1.500***	1.514***	0.969***	0.969***
	(0.835)	(1.474)	(0.372)	(0.372)	(0.219)	(0.219)
Constant	2.692***	2.692***	1.031***	4.569***	5.990***	5.333***
	(0.102)	(0.122)	(0.0437)	(0.467)	(0.0226)	(0.0773)
Observations	1,400,028	1,400,028	1,400,028	1,400,028	1,400,028	1,400,028
R-squared	0.121	0.121	0.315	0.316	0.028	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Notes. Dependent variable: Firm size (no. employees). Treatment period: 2006-2008. Underlying time period: 2003-2005. Only surviving firms with at least one employee per year, are included. Point estimates in figures are represented by the ATE estimate in the fifth column. DDD estimation.
*p<0.1, **p<0.05, ***p<0.01

Table A3. 2006-2008 employment effect for 19-25-year-olds by treatment intensity. DDD regressions.

Specification:	1	2	3	4	5	6
Tr. intensity:	*0-20*	*0-20*	*0-20*	*0-20*	*0-20*	*0-20*
Time	0.0567***	0.0567***	0.0563***	0.0564***	0.0567***	0.0567***
	(0.00126)	(0.00467)	(0.000878)	(0.000879)	(0.000881)	(0.000881)
Group	1.063***	1.063***	1.018***	1.018***	0.551***	0.877***
	(0.00510)	(0.0220)	(0.00852)	(0.00851)	(0.0131)	(0.00868)
Treat	-0.00842***	-0.00842***	-0.00747***	-0.00724***	0.00862***	0.00204***
	(0.00142)	(0.000901)	(0.000607)	(0.000608)	(0.000844)	(0.000668)
Group*Time	-0.169***	-0.169***	-0.168***	-0.168***	-0.169***	-0.169***
	(0.00625)	(0.0166)	(0.0136)	(0.0136)	(0.0136)	(0.0136)
Time*Treat	0.0179***	0.0179***	0.0182***	0.0182***	0.0179***	0.0179***
	(0.00174)	(0.00258)	(0.00123)	(0.00123)	(0.00123)	(0.00123)
Group*Treat	0.0419***	0.0419**	0.0396***	0.0382***	0.111***	0.0586***
	(0.00704)	(0.0182)	(0.0128)	(0.0127)	(0.0172)	(0.0121)
ATE	0.0695***	0.0695***	0.0688***	0.0689***	0.0695***	0.0695***
	(0.00862)	(0.0196)	(0.0191)	(0.0191)	(0.0191)	(0.0191)
Constant	0.0376***	0.0376***	-0.0285***	0.196	0.0483***	0.0506***
	(0.00103)	(0.00215)	(0.00101)	(0.124)	(0.000736)	(0.000575)
Observations	1,396,224	1,396,224	1,396,224	1,396,224	1,396,224	1,396,224
R-squared	0.151	0.151	0.167	0.168	0.031	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Specification:	1	2	3	4	5	6
Tr. intensity:	*20-40*	*20-40*	*20-40*	*20-40*	*20-40*	*20-40*
Time	0.0567***	0.0567***	0.0563***	0.0564***	0.0567***	0.0567***
	(0.00130)	(0.00467)	(0.000878)	(0.000879)	(0.000881)	(0.000881)
Group	1.371***	1.371***	1.324***	1.322***	0.754***	1.148***
	(0.00522)	(0.0269)	(0.00911)	(0.00911)	(0.0129)	(0.00910)
Treat	-0.00842***	-0.00842***	-0.00753***	-0.00728***	0.00835***	0.00193***
	(0.00147)	(0.000901)	(0.000608)	(0.000610)	(0.000849)	(0.000676)
Group*Time	-0.217***	-0.217***	-0.218***	-0.218***	-0.217***	-0.217***
	(0.00640)	(0.0139)	(0.0124)	(0.0124)	(0.0124)	(0.0124)
Time*Treat	0.0179***	0.0179***	0.0181***	0.0180***	0.0179***	0.0179***
	(0.00181)	(0.00258)	(0.00123)	(0.00123)	(0.00123)	(0.00123)
Group*Treat	0.0363***	0.0363**	0.0367***	0.0363***	0.0737***	0.0276**
	(0.00719)	(0.0156)	(0.0131)	(0.0131)	(0.0180)	(0.0125)
ATE	0.0855***	0.0855***	0.0860***	0.0861***	0.0855***	0.0855***
	(0.00881)	(0.0231)	(0.0191)	(0.0191)	(0.0191)	(0.0191)
Constant	0.0376***	0.0376***	-0.0296***	0.0786	0.0541***	0.0530***
	(0.00106)	(0.00215)	(0.00107)	(0.116)	(0.000740)	(0.000593)
Observations	1,397,784	1,397,784	1,397,784	1,397,784	1,397,784	1,397,784
R-squared	0.216	0.216	0.234	0.235	0.042	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Specification:	1	2	3	4	5	6
Tr. intensity:	*40-60*	*40-60*	*40-60*	*40-60*	*40-60*	*40-60*
Time	0.0567***	0.0567***	0.0563***	0.0564***	0.0567***	0.0567***
	(0.00128)	(0.00467)	(0.000878)	(0.000879)	(0.000881)	(0.000881)
Group	1.474***	1.474***	1.430***	1.429***	0.801***	1.238***
	(0.00510)	(0.0408)	(0.00933)	(0.00933)	(0.0128)	(0.00941)
Treat	-0.00842***	-0.00842***	-0.00740***	-0.00710***	0.00862***	0.00192***
	(0.00145)	(0.000901)	(0.000609)	(0.000611)	(0.000852)	(0.000670)
Group*Time	-0.140***	-0.140***	-0.140***	-0.140***	-0.140***	-0.140***
	(0.00625)	(0.0166)	(0.0117)	(0.0117)	(0.0117)	(0.0117)
Time*Treat	0.0179***	0.0179***	0.0182***	0.0181***	0.0179***	0.0179***
	(0.00178)	(0.00258)	(0.00123)	(0.00123)	(0.00123)	(0.00123)
Group*Treat	0.0748***	0.0748***	0.0740***	0.0738***	-0.00761	0.0267**
	(0.00703)	(0.0147)	(0.0134)	(0.0134)	(0.0181)	(0.0132)
ATE	0.0651***	0.0651***	0.0646***	0.0648***	0.0651***	0.0651***
	(0.00861)	(0.0221)	(0.0182)	(0.0182)	(0.0182)	(0.0182)
Constant	0.0376***	0.0376***	-0.0306***	-0.0391	0.0595***	0.0539***
	(0.00105)	(0.00215)	(0.00116)	(0.120)	(0.000771)	(0.000584)
Observations	1,398,978	1,398,978	1,398,978	1,398,978	1,398,978	1,398,978
R-squared	0.269	0.269	0.284	0.285	0.044	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Specification:	1	2	3	4	5	6
Tr. intensity:	*60-80*	*60-80*	*60-80*	*60-80*	*60-80*	*60-80*
Time	0.0567***	0.0567***	0.0562***	0.0564***	0.0567***	0.0567***
	(0.00160)	(0.00467)	(0.000878)	(0.000879)	(0.000881)	(0.000881)
Group	2.498***	2.498***	2.433***	2.430***	1.172***	2.071***
	(0.00637)	(0.0972)	(0.0159)	(0.0159)	(0.0234)	(0.0167)
Treat	-0.00842***	-0.00842***	-0.00782***	-0.00759***	0.00873***	0.00310***
	(0.00182)	(0.000901)	(0.000627)	(0.000629)	(0.000862)	(0.000703)
Group*Time	-0.261***	-0.261***	-0.261***	-0.261***	-0.261***	-0.261***
	(0.00780)	(0.0303)	(0.0197)	(0.0197)	(0.0197)	(0.0197)
Time*Treat	0.0179***	0.0179***	0.0182***	0.0182***	0.0179***	0.0179***
	(0.00223)	(0.00258)	(0.00123)	(0.00123)	(0.00123)	(0.00123)
Group*Treat	0.0647***	0.0647***	0.0663***	0.0663***	0.0756***	0.0651***
	(0.00876)	(0.0207)	(0.0215)	(0.0215)	(0.0282)	(0.0213)
ATE	0.198***	0.198***	0.197***	0.197***	0.198***	0.198***
	(0.0107)	(0.0301)	(0.0274)	(0.0274)	(0.0274)	(0.0274)
Constant	0.0376***	0.0376***	-0.0316***	0.286	0.0857***	0.0627***
	(0.00131)	(0.00215)	(0.00122)	(0.178)	(0.000999)	(0.000693)
Observations	1,399,332	1,399,332	1,399,332	1,399,332	1,399,332	1,399,332
R-squared	0.399	0.399	0.412	0.413	0.046	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Notes. Dependent variable: Number of 19-25-year-olds. Treatment period: 2006-2008. Only surviving firms with at least one employee per year, are included. Point estimates in figures are represented by the ATE estimate in the fifth column. DDD estimation.
*p<0.1, **p<0.05, ***p<0.01

Cont. – Table A3. 2006-2008 employment effect for 19-25-year-olds by treatment intensity. DDD regressions.

Specification:	**1**	**2**	**3**	**4**	**5**	**6**
Tr. intensity:	*80-100*	*80-100*	*80-100*	*80-100*	*80-100*	*80-100*
Time	0.0567***	0.0567***	0.0559***	0.0561***	0.0567***	0.0567***
	(0.0121)	(0.00467)	(0.00102)	(0.00103)	(0.000881)	(0.000881)
Group	11.60***	11.60***	10.38***	10.36***	2.479***	4.991***
	(0.0480)	(1.061)	(0.191)	(0.191)	(0.122)	(0.116)
Treat	-0.00842	-0.00842***	-0.0192***	-0.0184***	0.00621***	0.00392***
	(0.0138)	(0.000901)	(0.00241)	(0.00246)	(0.000988)	(0.000971)
Group*Time	-0.667***	-0.667***	-0.697***	-0.697***	-0.667***	-0.667***
	(0.0588)	(0.137)	(0.0556)	(0.0556)	(0.0541)	(0.0541)
Time*Treat	0.0179	0.0179***	0.0176***	0.0174***	0.0179***	0.0179***
	(0.0169)	(0.00258)	(0.00146)	(0.00147)	(0.00123)	(0.00123)
Group*Treat	0.360***	0.360	0.512***	0.514***	1.098***	1.071***
	(0.0660)	(0.240)	(0.137)	(0.137)	(0.117)	(0.115)
ATE	0.748***	0.748***	0.788***	0.789***	0.748***	0.748***
	(0.0808)	(0.130)	(0.0788)	(0.0788)	(0.0767)	(0.0767)
Constant	0.0376***	0.0376***	-0.0338***	0.143	0.410***	0.255***
	(0.00991)	(0.00215)	(0.00351)	(0.117)	(0.00425)	(0.00601)
Observations	1,400,028	1,400,028	1,400,028	1,400,028	1,400,028	1,400,028
R-squared	0.210	0.210	0.287	0.287	0.030	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Notes. Dependent variable: Number of 19-25-year-olds. Treatment period: 2006-2008. Only surviving firms with at least one employee per year, are included. Point estimates in figures are represented by the ATE estimate in the fifth column. DDD estimation.
*p<0.1, **p<0.05, ***p<0.01

Table A4. 2006-2008 employment effect for older individuals by treatment intensity. DDD regressions.

Specification:	**1**	**2**	**3**	**4**	**5**	**6**	**Specification:**	**1**	**2**	**3**	**4**	**5**	**6**
Tr. intensity:	*0-20*	*0-20*	*0-20*	*0-20*	*0-20*	*0-20*	**Tr. intensity:**	*20-40*	*20-40*	*20-40*	*20-40*	*20-40*	*20-40*
Time	0.0914***	0.0914***	0.0825***	0.0862***	0.0914***	0.0914***	Time	0.0914***	0.0914***	0.0825***	0.0863***	0.0914***	0.0914***
	(0.0192)	(0.0164)	(0.00407)	(0.00408)	(0.00377)	(0.00377)		(0.0200)	(0.0164)	(0.00412)	(0.00413)	(0.00377)	(0.00377)
Group	4.078***	4.078***	3.648***	3.644***	-0.108	0.124*	Group	4.630***	4.630***	4.045***	4.022***	-0.302***	-0.0195
	(0.0778)	(0.535)	(0.181)	(0.180)	(0.0720)	(0.0697)		(0.0802)	(0.506)	(0.152)	(0.152)	(0.0743)	(0.0717)
Treat	-0.107***	-0.107***	-0.0737***	-0.0689***	0.0610***	0.0571***	Treat	-0.107***	-0.107***	-0.0772***	-0.0721***	0.0608***	0.0569***
	(0.0217)	(0.0254)	(0.0147)	(0.0147)	(0.00596)	(0.00592)		(0.0226)	(0.0254)	(0.0147)	(0.0147)	(0.00600)	(0.00596)
Group*Time	0.263***	0.263***	0.257***	0.254***	0.263***	0.263***	Group*Time	0.286***	0.286***	0.266***	0.262***	0.286***	0.286***
	(0.0953)	(0.0525)	(0.0348)	(0.0348)	(0.0346)	(0.0346)		(0.0982)	(0.0694)	(0.0375)	(0.0375)	(0.0366)	(0.0366)
Time*Treat	0.0133	0.0133	0.0183***	0.0160***	0.0133***	0.0133***	Time*Treat	0.0133	0.0133	0.0182***	0.0159***	0.0133***	0.0133***
	(0.0266)	(0.0191)	(0.00541)	(0.00542)	(0.00508)	(0.00508)		(0.0277)	(0.0191)	(0.00550)	(0.00551)	(0.00508)	(0.00508)
Group*Treat	0.345***	0.345	0.287	0.256	0.254***	0.238***	Group*Treat	0.528***	0.528	0.588***	0.571**	0.600***	0.566***
	(0.107)	(0.258)	(0.239)	(0.239)	(0.0841)	(0.0822)		(0.110)	(0.338)	(0.225)	(0.225)	(0.0946)	(0.0924)
ATE	0.0246	0.0246	0.0327	0.0318	0.0246	0.0246	ATE	-0.0113	-0.0113	0.0139	0.0173	-0.0113	-0.0113
	(0.132)	(0.0727)	(0.0491)	(0.0491)	(0.0486)	(0.0486)		(0.135)	(0.0960)	(0.0536)	(0.0536)	(0.0526)	(0.0526)
Constant	2.654***	2.654***	1.057***	3.189***	2.741***	2.790***	Constant	2.654***	2.654***	1.048***	3.895***	2.773***	2.829***
	(0.0156)	(0.120)	(0.0142)	(0.879)	(0.00467)	(0.0141)		(0.0163)	(0.120)	(0.0145)	(0.574)	(0.00490)	(0.0148)
Observations	1,396,224	1,396,224	1,396,224	1,396,224	1,396,224	1,396,224	Observations	1,397,784	1,397,784	1,397,784	1,397,784	1,397,784	1,397,784
R-squared	0.015	0.015	0.127	0.132	0.003	.	R-squared	0.018	0.018	0.186	0.191	0.003	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes	Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No	Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No	Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes	Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No	Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No	Municip FE	No	No	No	Yes	No	No
Specification:	**1**	**2**	**3**	**4**	**5**	**6**	**Specification:**	**1**	**2**	**3**	**4**	**5**	**6**
Tr. intensity:	*40-60*	*40-60*	*40-60*	*40-60*	*40-60*	*40-60*	**Tr. intensity:**	*60-80*	*60-80*	*60-80*	*60-80*	*60-80*	*60-80*
Time	0.0914***	0.0914***	0.0821***	0.0859***	0.0914***	0.0914***	Time	0.0914***	0.0914***	0.0810***	0.0850***	0.0914***	0.0914***
	(0.0204)	(0.0164)	(0.00413)	(0.00414)	(0.00377)	(0.00377)		(0.0239)	(0.0164)	(0.00428)	(0.00429)	(0.00377)	(0.00377)
Group	6.626***	6.626***	5.878***	5.857***	-0.309***	0.0551	Group	12.37***	12.37***	11.35***	11.28***	-0.0120	0.831***
	(0.0812)	(0.492)	(0.178)	(0.177)	(0.0790)	(0.0771)		(0.0950)	(0.959)	(0.290)	(0.290)	(0.141)	(0.137)
Treat	-0.107***	-0.107***	-0.0770***	-0.0710***	0.0636***	0.0597***	Treat	-0.107***	-0.107***	-0.0805***	-0.0753***	0.0637***	0.0601***
	(0.0231)	(0.0254)	(0.0147)	(0.0147)	(0.00599)	(0.00594)		(0.0271)	(0.0254)	(0.0148)	(0.0149)	(0.00605)	(0.00601)
Group*Time	0.264***	0.264***	0.267***	0.261***	0.264***	0.264***	Group*Time	0.369***	0.369***	0.363***	0.359***	0.369***	0.369***
	(0.0994)	(0.0625)	(0.0394)	(0.0394)	(0.0379)	(0.0379)		(0.116)	(0.115)	(0.0604)	(0.0603)	(0.0599)	(0.0599)
Time*Treat	0.0133	0.0133	0.0198***	0.0174***	0.0133***	0.0133***	Time*Treat	0.0133	0.0133	0.0202***	0.0176***	0.0133***	0.0133***
	(0.0283)	(0.0191)	(0.00555)	(0.00556)	(0.00508)	(0.00508)		(0.0332)	(0.0191)	(0.00575)	(0.00577)	(0.00508)	(0.00508)
Group*Treat	-0.104	-0.104	0.0156	0.0141	0.714***	0.696***	Group*Treat	-0.746***	-0.746	-0.605	-0.593	1.126***	1.090***
	(0.112)	(0.364)	(0.259)	(0.259)	(0.0894)	(0.0880)		(0.131)	(0.480)	(0.375)	(0.375)	(0.132)	(0.131)
ATE	0.0851	0.0851	0.0806	0.0863	0.0851	0.0851	ATE	0.174	0.174	0.183**	0.188**	0.174**	0.174**
	(0.137)	(0.0869)	(0.0565)	(0.0564)	(0.0547)	(0.0547)		(0.160)	(0.159)	(0.0800)	(0.0799)	(0.0786)	(0.0786)
Constant	2.654***	2.654***	1.038***	2.470**	2.845***	2.899***	Constant	2.654***	2.654***	1.045***	3.444***	3.057***	3.114***
	(0.0167)	(0.120)	(0.0151)	(1.009)	(0.00496)	(0.0149)		(0.0195)	(0.120)	(0.0148)	(0.927)	(0.00637)	(0.0178)
Observations	1,398,978	1,398,978	1,398,978	1,398,978	1,398,978	1,398,978	Observations	1,399,332	1,399,332	1,399,332	1,399,332	1,399,332	1,399,332
R-squared	0.031	0.031	0.152	0.157	0.004	.	R-squared	0.071	0.071	0.194	0.198	0.006	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes	Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No	Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No	Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes	Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No	Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No	Municip FE	No	No	No	Yes	No	No

Notes. Dependent variable: Number of employees above the age of 25. Treatment period: 2006-2008. Only surviving firms with at least one employee per year, are included. Point estimates in figures are represented by the ATE estimate in the fifth column. DDD estimation.
*p<0.1, **p<0.05, ***p<0.01

Cont. – Table A4. 2006-2008 employment effect for older individuals by treatment intensity. DDD regressions.

Specification:	**1**	**2**	**3**	**4**	**5**	**6**
Tr. intensity:	*80-100*	*80-100*	*80-100*	*80-100*	*80-100*	*80-100*
Time	0.0914	0.0914***	0.0747***	0.0763***	0.0914***	0.0914***
	(0.117)	(0.0164)	(0.00792)	(0.00803)	(0.00377)	(0.00377)
Group	75.58***	75.58***	59.95***	59.82***	2.819***	5.106***
	(0.462)	(14.92)	(1.375)	(1.376)	(0.523)	(0.512)
Treat	-0.107	-0.107***	-0.187***	-0.181***	0.0555***	0.0533***
	(0.132)	(0.0254)	(0.0329)	(0.0331)	(0.00632)	(0.00630)
Group*Time	1.178**	1.178	0.951***	0.953***	1.178***	1.178***
	(0.566)	(1.491)	(0.248)	(0.248)	(0.138)	(0.138)
Time*Treat	0.0133	0.0133	0.0212*	0.0198	0.0133***	0.0133***
	(0.162)	(0.0191)	(0.0122)	(0.0123)	(0.00508)	(0.00508)
Group*Treat	-5.233***	-5.233*	-2.344**	-2.320**	3.680***	3.651***
	(0.635)	(2.778)	(0.963)	(0.963)	(0.314)	(0.313)
ATE	0.193	0.193	0.679**	0.692**	0.193	0.193
	(0.777)	(1.376)	(0.334)	(0.334)	(0.179)	(0.179)
Constant	2.654***	2.654***	1.073***	4.431***	5.536***	4.970***
	(0.0953)	(0.120)	(0.0406)	(0.461)	(0.0202)	(0.0720)
Observations	1,400,028	1,400,028	1,400,028	1,400,028	1,400,028	1,400,028
R-squared	0.104	0.104	0.305	0.306	0.020	.
Firm clustered s.e	No	No	Yes	Yes	Yes	Yes
Industry clustered s.e	No	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	No
Firm RE	No	No	No	No	No	Yes
Industry FE	No	No	Yes	Yes	No	No
Municip FE	No	No	No	Yes	No	No

Notes. Dependent variable: Number of employees above the age of 25. Treatment period: 2006-2008. Only surviving firms with at least one employee per year, are included. Point estimates in figures are represented by the ATE estimate in the fifth column. DDD estimation.
*p<0.1, **p<0.05, ***p<0.01