# Do payroll tax cuts raise youth employment?*

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**Abstract**

Many OECD countries today face high levels of youth unemployment and there is an ongoing debate on what policy measures are most efficient to improve young individuals' labor market prospects. This paper examines whether targeted payroll tax reductions are an effective means to raise youth employment. In 2007, the Swedish employer-paid payroll tax was cut on a large scale for young workers, substantially reducing their associated labor costs. We estimate a small impact, both on employment and on wages, implying a labor demand elasticity for young workers at around −0.32. During the recession years 2009–10, the effect appears to have been even smaller; thus, the policy does not seem to have counteracted the surge in youth unemployment during the economic crisis. Furthermore, there is no evidence that the positive effect on employment remained when the individual were no longer eligible to the tax reduction. Since the estimated cost per created job is at more than four times that of directly hiring workers at the average wage we conclude that payroll tax cuts are an inefficient way to boost employment for young individuals.

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# 1 Introduction

High and persistent youth unemployment is a major challenge for many developed economies. In the OECD as a whole, unemployment for individuals below 24 years of age has been twice as high as for those aged 25–64 since the beginning of the 1990's. In addition, young people's employment opportunities have worsened even further in the wake of the 2008 financial crisis. Since labor market difficulties encountered in early working life are known to have lasting consequences, an increasing number of young people risk ending up in long-term unemployment.[1] Consequently, there is a wide and lively debate on what policies should be undertaken to improve young individuals' labor market prospects.

We examine whether targeted payroll tax reductions are an effective means to raise youth employment. Payroll taxes in Sweden are proportional to the employee's gross wage and are paid by the employer. In 2007–09, the tax rate for employers of young workers was reduced on a large scale in two steps. The first reduction, in effect 2007–08, lowered the payroll tax rate with 11 percentage points for employees who at the start of the year had turned 18 but not 25 years of age. In 2009, the reduction was extended to encompass all individuals who at the start of the year had not yet turned 26 years of age; at the same time, the rate was reduced with an additional 6 percentage points for the eligible individuals. Using this variation in payroll tax rates across cohorts, we investigate the causal effect of payroll taxes on youth employment.

We use Difference-in-Differences (DiD) to identify the effects of the payroll tax reductions, pitting individuals in the target group against slightly older individuals who were not subjected. Identification is, however, complicated by the fact that individuals of different ages tend to experience different employment cyclicality, with younger workers displaying larger cyclical variations. We deal with this problem—which essentially

[1] See, e.g., Gregg (2001), Nordström Skans (2004) and Gregg and Tominey (2005) for studies on the so-called scarring effect of early unemployment.

constitutes a threat to the identification assumption of parallel trends—by including a large number of covariates in the DiD model. We estimate the effects both for the entire target group as well as for different subgroups, such as foreign-born and the unemployed. As a special case, we consider treatment-control pairs that are defined at a very small bandwidth around the treatment-defining age threshold; this resembles a regression discontinuity design, but with controlling for pre-reform discontinuity.

We find that lowering payroll taxes for young workers has a small impact on employment. For the whole target group, the relative employment increase was around 2.5 percent in 2007 and 1.8 percent in 2008, whereas for individuals close to the treatment defining cutoff the effect was around 1.3 percent, both in 2007 and in 2008. However, we found no reform effect on hours worked. We find some support for the existence of substitution effects, implying that the reform may have created jobs for one group of individuals at the expense of another. Importantly, the presence of substitution effects also means that the absolute effect on employment is potentially smaller than what our estimates suggest. A striking finding is that there is no additional effect on employment of the 2009 extended reduction; this suggests that even large tax cuts cannot counteract the negative impact of economic slowdowns. Finally, our results show that even though the reform created a relative price wedge that induced employers to hire (or to keep) a young worker, it did not lead to any permanent increase in the likelihood that this individual is employed.

When it comes to explaining the modest impact, we make a number of observations. First, since wages did not adjust much, shifting of the incidence of the tax burden to higher wages cannot explain the small employment effects. Secondly, we show that the tax cut had no impact at all for foreign-born youths, nor for individuals registered as unemployed. We argue that these results (especially the null result for the latter group) can be taken as an indication that labor supply constraints are not the main issue. The question then arises why the demand elasticity of firms is so low. We argue in favor of demand constraints: for the group of uneducated, unexperienced young workers, labor costs are still too high—even with the payroll tax reduction in place.

Our employment and wage estimates in combination imply that the firms' elasticity of demand for young workers in Sweden is at around $-0.32$. Using a different metric: the estimated gross cost per created job for 20–25 year-olds was around SEK 1.2 million (ca. $140,000). Since this corresponds to more than four times the cost of hiring the same number of workers at the average wage for this age group, we conclude that targeted payroll tax reductions are an inefficient way to boost employment for young individuals.

The rest of the paper is organized as follows. Section 2 gives a brief overview of the previous literature. Section 3 presents some of the institutions specific to the Swedish setting. Section 4 describes the data and section 5 the methodology we apply. Section 6 details the results, which are further analyzed in section 7. Section 8 provides a discussion and section 9 concludes.

# 2 Previous literature

Previous evidence on the effects of payroll tax cuts typically concerns general reductions. The basic result for the U.S. is that of extensive shifting of the incidence of the tax onto workers; hence, there are, at most, marginal employment effects (see, e.g., Gruber 1994; Anderson and Meyer 1997, 2000; Murphy 2007).[2] It can, however, be argued that these studies may suffer from endogeneity problems. For example, Anderson and Meyer (1997, 2000) exploit firm, or industry, level variation in unemployment insurance (UI) taxes. Since the UI tax paid by the firm is determined by the firm's lay-off history, and thus is potentially endogenous, it is not clear that the estimates can be interpreted as the causal effect of the UI tax.

More convincing evidence is found in studies that evaluate selective payroll tax re-

[2] Gruber (1997) studies manufacturing firms in Chile and finds that the incidence of payroll taxation is fully on wages, with no effect on employement.

forms. Examples include Bohm and Lind (1993), Bennmarker et al. (2009) and Korkeamäki and Uusitalo (2009) who evaluate reductions targeted towards specific regions in Sweden or Finland. None of these studies find any effects on employment. However, compared to the U.S., the degree of shifting is small. Bennmarker et al. (2009) find that 1 percent reduction in wage costs increased wages by 0.32 percent, whereas in Korkeamäki and Uusitalo (2009) the increase was 0.6 percent.

Besides the above-mentioned literature, there are some studies that focus on workers who display poor labor market outcomes. Kramarz and Philippon (2001) examine the impact of changes in total labor costs on employment of low-wage workers in France between 1990 and 1998. Their results suggest that 1 percent increase of the labor cost leads to 1.5 percent increase in the probability of transiting from employment to non-employment, whereas lower labor costs had no impact on transitions from non-employment to employment. Since payroll tax cuts were offset by rising minimum wages it is difficult, however, to distinguish between the effect of changes in payroll taxes from that of changes in minimum wages. For the case of Finland, Huttunen et al. (2013) study a hiring credit targeted at the employers of older, full-time, low-wage workers. They find no effects on employment or wages of the eligible groups, but a small increase in working hours among those who were already employed. Finally, a study by Cahuc et al. (2014), study the effect of hiring credits in France. They find relativly large effects, but the setup is not quite comparable to ours since the hiring credits applied only to new employments, and were also phased out for high-wage earners.

To the best of our knowledge, the only other study that examines payroll tax reductions explicitly aimed at young workers is Skedinger (2014). Skedinger looks at the same reductions as we do and studies the effects for the Swedish retail industry. He finds small or no effects on job accessions, separations, hours worked and wages. The most important difference between our study and Skedinger's is that he only considers one industry. Thus, he cannot assess the overall employment effect in the economy since he cannot separate new labor market entrants from movements between sectors. In addition, since we are using much more detailed data we are able to study treatment effect heterogeneity with respect to immigration status and unemployment status. Importantly, this allows us to make inferences about what mechanisms might explain our results.

# 3 Institutional background

## 3.1 Youth unemployment in Sweden

Official records show that youth unemployment in Sweden is currently at historically high levels. Unemployment for 15–25 year-olds was roughly at 24 percent in 2013, which is three times higher than overall unemployment (Statistics Sweden, 2014). In 2007 and 2008, which are the years that we mainly focus on in this study, the figueres are somewhat lower, at 20 percent. In 2009–10, when the Swedish economy was fully hit by the financial crisis, youth unemployment increased to 25 percent. It is sometimes argued that these (official) figures exaggerate the problem of youth unemployment in Sweden, primarily because a large number of the unemployed participate in different types of education. Excluding those who study full-time lowers unemployment for 15–25 year-olds to 12 percent in 2013. However, it is not obvious that this adjustment makes sense: since there are no tuition fees is Sweden and students are entitled to allowances, many might chose to study if they cannot find a job (especially if they are not entitled to unemployment benefits).

To provide some further understanding of the problem in the Swedish case we look at two other measures. First, about 10 percent of all 20–24 year-olds were neither employed nor in any education or training in 2013 (i.e., they belong to the so called NEET category). In 2007–08, the corresponding figure was 12 percent, and in 2009–10 roughly 13 percent (Statistics Sweden, 2014). A second measure looks at registrations at the unemployment office. Our data contain yearly information on job search activity, and so we can observe those that are registered as looking for a job. 21 percent of all 20–24 year-olds were registered at the unemployment office at some point during 2007–08, and 8 percent were registered for more than 100 days. In 2009–2010, these figures increased to 24 percent and 12 percent, respectively.

### 3.2 Swedish payroll tax reductions

Swedish payroll taxes are proportional to the employee's wage bill and, in contrast to e.g. the U.S., fully paid by the employer. The tax consists of seven mandatory fees, financing welfare services such as pensions, health and disability insurances, and other social benefits. Up until the beginning of the 1980's the payroll tax rate was the same for all employers in Sweden, but over the last 30 years there have been some exceptions. First, firms in so called regional support areas (RSA) in the northern parts of Sweden were twice subjected to reductions of roughly 10 percentage points in efforts to boost employment in these areas.[3] Secondly, besides these regional reductions, payroll taxes were cut for small firms in all of Sweden between 1997 and 2008.[4]

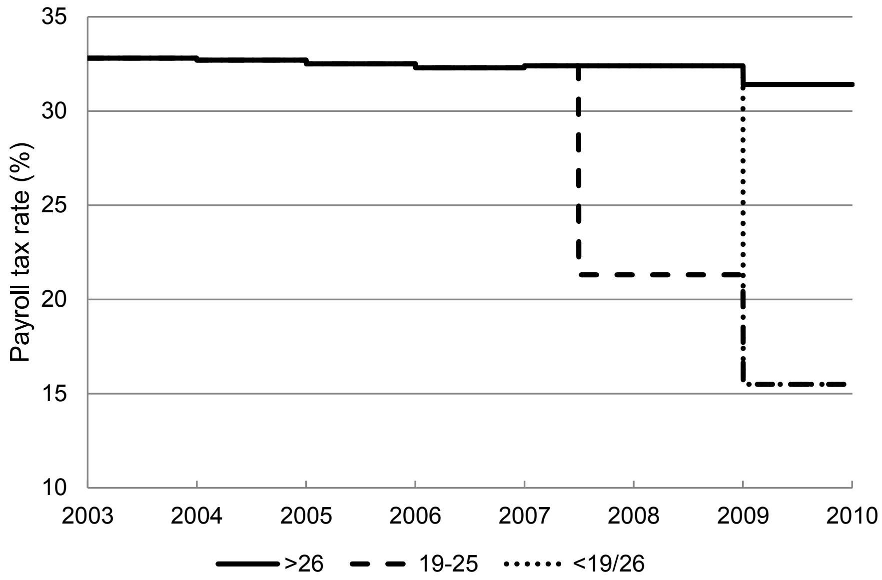
In this paper, we study reductions targeted explicitly at young workers. Figure 1 provides a graphical illustration of the changes in the tax rate. On July 1, 2007, the payroll tax was cut by around 11 percentage points for individuals who at the start of the year had turned 18 but not 25 years of age. Six out of seven mandatory fees were halved, reducing the tax rate from 32.42 to 21.32 percent.[5] On January 1, 2009, the

[3]Neither Bohm and Lind (1993), who study reductions implemented between 1984 and 1999, nor Bennmarker et al. (2009), evaluating reductions introduced in 2002, find any employment effects.

[4]Firms with up to three employees were allowed a 5 percent reduction for wage sums up to around SEK 750,000 ($90,000) per year. Thus, this cut was relatively small, both in magnitude and comprehension. To the best of our knowledge, this reduction has not been evaluated.

[5]The date July 2007 is first mentioned in a press release from the ministry of Finance in October 2006. This date was confirmed when the new policy was ratified in the parliament on 15 March 2007. The only fee that was left unchanged was the pension fee. Individuals who are self-employed pay *egenavgifter*, roughly equivalent to payroll taxes paid by employers. These fees were also cut with about 10 percentage points, in order to avoid distortionary effects with respect to choice of occupation. Besides the statutory payroll tax, collective-bargaining agreements require most employers to pay around 10 percent of gross wages to finance job search support, retraining and severance payments when employees are laid off. As these fees are not legislated, they were unaffected by the tax reduction.

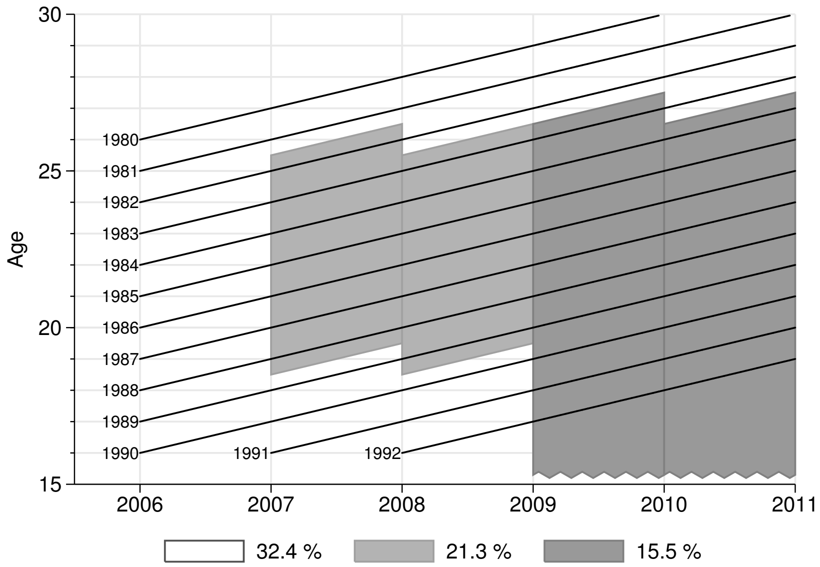
Figure 1: The payroll tax reductions




reform was modified in two ways. First, the tax reduction was extended to encompass all individuals who at the start of the year had not yet turned 26 years of age; i.e., the target group was extended at both ends. Secondly, the magnitude of the payroll tax reduction was increased, resulting in a final tax rate of 15.52 percent for the target group. The payroll tax reductions were automatically implemented via the tax system, i.e., the employers did not have to send in an application to benefit from the lower tax rates. Figure 2 illustrates how different cohorts are subjected to the payroll tax reductions. In 2007, the target group consists of individuals born 1982–88 whereas in 2008 it consits of those born 1983–89. For simplicity, hereafter an age group $a$ denotes all individuals who turn $a$ during the year. Using this terminology, the target group of the 2007 reform is referred to as "individuals aged 19–25", and the target group of the 2009 reform as "individuals aged 26 or below".

The group of 19–25 year-olds comprised around 10 percent of the labor force aged 15–64 in 2007. Thus, the number of people directly affected by the new regime was substantial. Since they applied also to existing employments, the cost of the reform was

Figure 2: Evolution of treatment status across cohorts




sizable. Yearly foregone tax revenues were SEK 9 billion (around \$1.1 billion) in 2007 and SEK 9.9 billion in 2008 (around \$1.2 billion), corresponding to about 1 percent of the fiscal budget in these years. These figures increased substantially when the reductions were extended, resulting in foregone revenues at SEK 17 billion (\$2 billion) in 2009 and SEK 18 billion (\$2.1 billion) in 2010.

### 3.3 Other relevant labor market reforms

With the purpose of increasing employment, both in general and for specific groups, several labor market reforms were introduced in Sweden during 2007. First, temporary subsidies for firms that hire individuals who have been unemployed or have received sickness or disability benefits, *New Start Jobs* (NSJ), were introduced on January 1, 2007. In 2007–08, individuals aged 20–24 could apply for the subsidy after six months of non-employment, whereas those who had turned 25 could apply only after twelve months of non-employment—thus, in contrast to the payroll tax cut, it was the exact age that mattered. In 2009, this cutoff was modified so that those who at the start of the year have turned 20 but not 26 were eligible after six months.[6] Consequently, in 2007–08 the target groups overlapped, and from 2009 onwards they completely coincide. In principle, this raises a concern that the employment estimates of the payroll tax reduction will be contaminated. It turns out, however, that the number of applications for NSJ (available in our data) was comparatively low, at about 0.5 percent of the ages 20–25, and the difference in shares between 20–25 year-olds and 26-year-olds—the potential bias of our estimates—is always below 0.2 percentage points. We thus conclude that this is not a source of concern.

Secondly, income tax deductions were introduced in Sweden on January 1, 2007, with the purpose of increasing labor supply in general. These deductions apply to all workers, regardless of age, but we cannot rule out that there is heterogeneity in labor supply effects with respect to age. If younger workers' labor supply responded differently, we risk misestimating the effect of the payroll tax reductions. Edmark et al. (2012) show that it is difficult to evaluate this deduction scheme due to the lack of unaffected comparison groups; hence, we do not know exactly how different age groups responded. In this paper we assume that the response was similar for individuals close in age.

Finally, a third reform concerns employment protection legislation. Loosening of regulation in 2007 made it easier for employers to use fixed-term contracts. As temporary work is relatively more widespread among young workers, employment (and wages) may have been affected more for younger workers. However, Skedinger (2012) reports that only 1.4 percent of all temporary workers were employed with the new regulations in 2008. The reform, thus, had little impact in practice.

[6] When introduced, the subsidy was equal in size to the payroll tax amount. In 2009, the size of the subsidy increased to twice the payroll tax. The subsidy is given for a period equally long as the earlier non-employment spell and up to 1 year for those aged 20–24 and up to 5 years for those aged 25 or more.

### 3.4 Wage formation in Sweden

Wage setting in Sweden has traditionally been characterized by a high degree of central bargaining. Over the last 10–15 years, there has been a substantial move toward the decentralization of negotiations, but many workers still have centrally agreed wages and this is likely to be more common for young workers.[7] In 2007, between April and July, central agreements covering 75 percent of all workers were renegotiated—i.e., before the implementation of the 2007 reform but after its passing in the parliament in March 2007 (National Mediation Office 2007). New agreements were not made until 2010, one year after the implementation of the new extended reductions.

Another institutional feature specific to the Swedish labor market is the fact that minimum wages are negotiated, not legislated as in most other OECD countries. Collective-bargaining agreements differentiate wages based mainly on age, experience and levels of skill. This means that younger workers are more likely to have wages bound by the minimum wage level.

## 4 Data

The data are collected by Statistics Sweden (SCB) and contain yearly information on employment and demographical characteristics for all individuals living in Sweden at or above 16 years of age in 2001–10 (the Louise and Rams data sets). These data contain, for each individual and year, start and end months as well as total taxable income from each employment source during the year. From this information we can deduce, for each individual and month, total monthly income from paid work. In addition, we use the Structure of Earnings Survey (SES) which contains detailed information on employ-

[7] Union density was at 80 percent in 1990 and 79 percent in 2000, and the share of workers covered by collective-bargaining agreements is even higher. The influence given to the local bargaining parties varies by sector. The private sector, to which most young workers in Sweden belong, has a higher degree of central wage setting than the public sector. See Fredriksson and Topel (2010) for a detailed discussion of the Swedish labor market.

ment characteristics for a subsample of all employees (measured between August and November each year), including data on actual monthly wages, work rate and industry affiliation of workplace. For public sector employers, the total population is surveyed through official registers, while firms in the private sector are sampled using a stratification scheme.[8] This subsample, in addition to being used in the wage analysis, is combined with the income data from the tax registers to create monthly measures of employment for all individuals.

Our employment measure is constructed in the following way. Starting out from the reduced sample of employed workers, for all individuals working at least 25 percent of full-time, we partition the sample in cells defined by all unique combinations of age, gender, three groups of education, firm sector (local/central public, blue-collar/white-collar private), and year. For each cell, we calculate the 10th percentile of actual, full-time equivalent wage; these values are to be used as cutoff values, serving as an income criterion for full-time employment. These monthly cutoff values are matched to the tax register data on all individuals. For each month that an individual's taxable income exceeds the appropriate cutoff value, she is, thus, classified as being full-time employed. Our employment measure uses the quarter of these income cutoffs to arrive at a measure of working *at least* 25 percent of full-time, for a particular month.[9]

It should be noted that our employment measure is likely to be misleading when comparing specific months within a given year: the income cutoffs used for deducing

[8]The stratification is based on six firm size classes and 54 industry groups, giving a total of 324 strata. Stratification weights are supplied with the data and used for table 1 and in the analysis of wages.

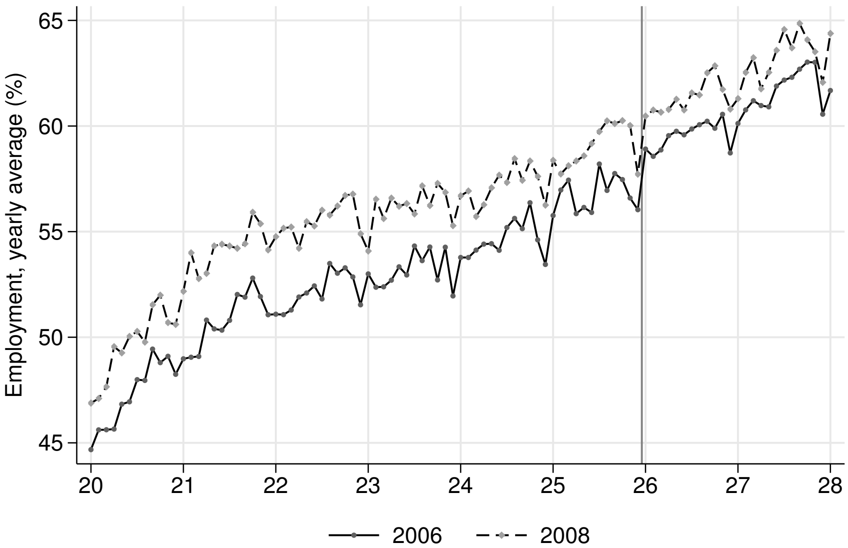
[9]In practice, the procedure is slightly more complicated: as cells with ten or fewer individuals (about two percent of all cells) cannot be used (otherwise we would overestimate the 10th percentile), the cutoffs for these cells are instead estimated. We predict the (log of) wage cutoffs using the other cells in a linear regression, controlling for all interactions of female-age-year, and female-age-year-education. In other words, we impute the wage cutoffs for the small cells through linear interpolation. When an individual has multiple income sources for a particular month, the largest income source is used for sector matching. We have tested using the 20th percentile instead of the 10th percentile when defining full-time employement; although raising the cutoff point, by definition, lowers all employment *levels*, the dynamics are essentially the same and, thus, this does not significantly change our results. Further, we have experimented with using different work rate conditions for the outcome variable, such as 10 or 50 percent of full-time employment. Again, the results are not much affected (see section 6).

employment status are computed on a yearly basis, while wages tend to rise continuously over time. Moreover, information on employment spells are only available separately for each year. This means that, e.g., for an employment stretching from December 2007 to April 2008 we have the exact income for December, but a 4-month average for January to April. We therefore use an annual measure of employment, taking the average of monthly employment status for each year.[10] Note that this method, in conjunction with our estimation method, handles most forms of remaining measurement errors. Only an error that evolves differently over time for different age groups, and that is uncorrelated to all control variables, would result in a bias in our DiD estimates.

Table 1 shows summary statistics divided by age, both for the full population (panel A) and for the smaller subsample (panel B). The table highlights some of the large differences in background characteristics across ages. For example, only 8.6 percent of the 20-year-olds have some form of education above high school, whereas among 27 year-olds this figure is 44.6 percent. Moreover, while foreign-born constitute 12.4 percent of the 20-year-olds, the same figure for 27-year-olds is 18.3 percent. These differences are unlikely to be stable over time since they depend on, e.g., the state of the economy, demographical changes and fluctuations in immigration. Panel B characterizes the subsample of employed individuals from the Structure of Earnings Survey, conditional on working at least a quarter of full-time. This is the sample we use for the regression on wages and hours worked. As expected, both (full-time equivalent) monthly wage and the work rate tend to increase in age. Older workers are also increasingly tenured, public-sector employed, higher educated and foreign-born. By comparing the two panels, we can deduce that, e.g., those with low education, women and foreign-born have lower employment than other groups. Table 2 presents the corresponding figures for

[10]Our measure differs from the official ILO definition of employment, according to which an individual is considered to be employed if working at least one hour per week (ILO 1983). For our purpose, this is too lax a restriction; we are interested in employments that actually have an economic impact for an individual. We have also tried using the employment measure from Statistics Sweden (constructed to emulate the ILO definition), as an additional robustness check. Although this measure is more noisy, the results are broadly in line with those obtained using our own measure.

Figure 3: Employment rates by age, 2006 and 2008




*Notes*: Employment is defined as working at least quarter–time. The vertical line indicates the age cutoff for the 2007 reform.

unemployed and foreign-born... [For both groups: markedly lower levels of employment, less educated ]

Finally, we take a look at the evolution of employment and wages over time. Figure 3 gives the age distribution of employment before and after the 2007 payroll tax reduction. There are two things to notice in the figure. First, there is a relative employment increase for 20–25 year-olds in 2008. Second, within the target group, workers at age 21–22 seem to have gained the most. This suggests that the reduction did have an impact on employment, and that this impact decreases in age. However, we know that, in general, younger workers perform better in economic expansions, so the relative increase in employment may simply be a result of the growing Swedish economy in 2006–08. This problem is further discussed in the next section. In figure 4, we depict the corresponding distributional change in wages. As seen, there is no clear-cut evidence of larger wage growth for younger workers.

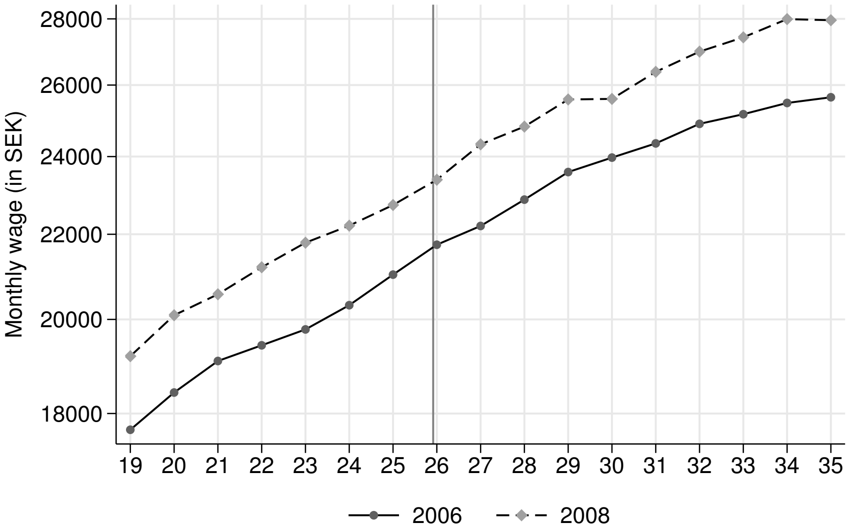
Table 1: Summary statistics, year 2006 (percentages)

| | Age cohort, 2006 | | | | |
|---|---|---|---|---|---|
| | 20 | 23 | 25 | 27 | 30 |
| | *Panel A: Full sample* | | | | |
| Employed, quarter-time | 47.3 | 53.2 | 56.8 | 61.7 | 65.6 |
| Employed, full-time | 15.7 | 25.0 | 31.0 | 37.8 | 40.7 |
| Educ. below high school | 14.4 | 12.5 | 11.8 | 13.1 | 8.2 |
| Educ. high school | 77.0 | 53.5 | 46.2 | 42.3 | 46.1 |
| Educ. above high school | 8.6 | 34.1 | 42.0 | 44.6 | 45.7 |
| Female | 48.7 | 48.8 | 49.1 | 49.0 | 49.0 |
| Foreign-born | 12.4 | 16.6 | 17.7 | 18.3 | 19.0 |
| N | 112,618 | 105,303 | 108,174 | 110,202 | 112,582 |
| | *Panel B: Structure of Earnings Survey* | | | | |
| Wage, full-time eq. (SEK) | 18,428 | 19,776 | 21,028 | 22,205 | 23,972 |
| Work rate (mean %) | 86.3 | 90.1 | 92.7 | 93.7 | 93.7 |
| Tenured | 60.3 | 67.3 | 69.8 | 75.2 | 80.1 |
| Public sector | 15.1 | 20.4 | 23.3 | 25.8 | 26.9 |
| Educ. below high school | 8.1 | 10.8 | 6.4 | 9.5 | 4.5 |
| Educ. high school | 83.8 | 58.6 | 50.4 | 44.4 | 48.7 |
| Educ. above high school | 8.1 | 30.6 | 43.2 | 46.1 | 46.8 |
| Female | 44.4 | 45.7 | 45.1 | 45.6 | 44.7 |
| Foreign-born | 8.2 | 10.2 | 10.8 | 11.1 | 11.6 |
| Sum of weights | 46,150 | 48,740 | 61,664 | 64,875 | 75,815 |
| N | 22,621 | 27,393 | 35,836 | 38,834 | 46,073 |

*Notes*: The Structure of Earnings Survey sample is conditioned on working at least quarter-time. Note that the sum of stratification weights indicates population size.

Table 2: Summary statistics for unemployed and foreign-born, year 2006 (percentages) – TO BE ADDED!

Figure 4: Average wage by age, 2006 and 2008 (log scale)




*Notes*: Sample conditional on working at least quarter−time. For those working less than full−time, wage is scaled to its full−time equivalent. The vertical line indicates the age cutoff for the 2007 reform.

# 5 Identification

## 5.1 Modelling the counterfactual outcome

We rely on the Difference-in-Differences (DiD) framework to estimate the effects of the payroll tax cuts. While, *prima facie*, using a regression discontinuity design on the 25–26 age threshold might appear attractive, it is clear from figure 3 that such a strategy is not viable. There are systematic discontinuities at each cohort boundary in 2006, before the tax reduction was implemented.[11]

In its simplest form, DiD uses the evolution of the control group over time as a measure of how the treatment group would have evolved, had the intervention not taken place. This results in the identifying assumption

[11] This pattern has its main cause in the fact that it is year of birth that determines when a child starts school in Sweden; see Fredriksson and Öckert (2014). With a DiD design, we assume that these cohort discontinuities are constant over time, for each age pair.

$$E\left[y_{i,t}^0 \mid \text{Tr}=1\right] = E\left[y_{i,t}^0 \mid \text{Tr}=0\right] + \alpha, \tag{1}$$

where $y_{i,t}^0$ is the no-treatment outcome for individual $i$ at time $t$. In other words, the counterfactual outcome of the treatment group is identical to the actual outcome of the control group, except for a constant $\alpha$. Figure 5 demonstrates that, in the present context, this is too strong an assumption. Inspecting the evolution of employment in the period before the reform (2001–06), it is clear that individuals of different ages differ in the degree of employment cyclicality, with younger workers tending to display larger cyclical variations.[12] As 2007 coincided with an economic expansion, comparing, say, 20-year-olds to 26-year-olds would result in an upward-biased reform estimate: even in absence of a reform, a relative employment increase for 20-year-olds would have been expected solely due to this group's higher employment cyclicality. In addition to this systematic age heterogeneity, there are idiosyncratic differences between cohorts (e.g., due to temporary waves of immigration).

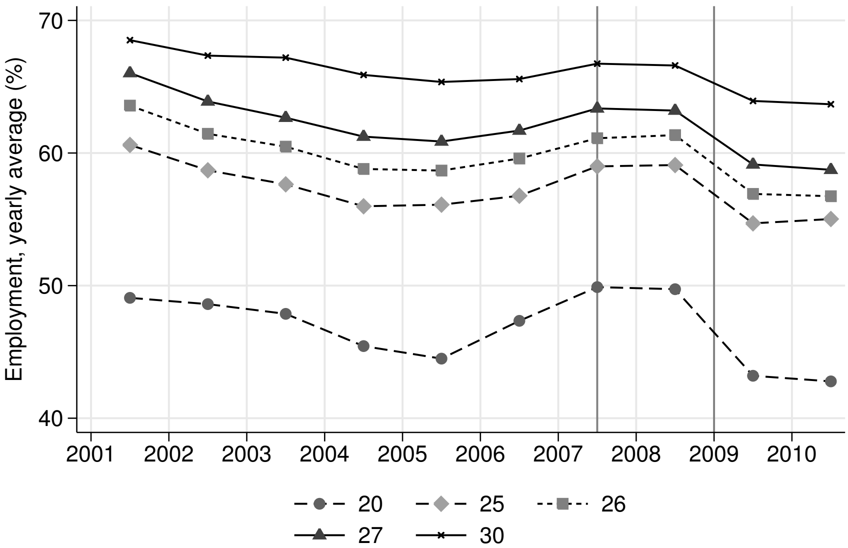
In order to model the counterfactual outcome of the treatment group we supplement the basic DiD model with a large number of covariates. The estimated specification is

$$y_{i,t} = \delta_t \cdot D(i,t) + \mathbf{x}_{i,t}'\boldsymbol{\beta} + \varepsilon_{i,t}, \tag{2}$$

where $y_{i,t} \in [0,1]$ is average employment status in year $t$, $D(i,t)$ is a treatment indicator for individual $i$ in year $t$, $\delta_t$ is the DiD estimate for year $t$, and $\mathbf{x}_{i,t}$ is a vector of control variables, capturing a multitude of factors that may influence the probability of being employed. These include dummy variables for year, age, county of birth (including indicator for foreign-born), gender, geography, and whether the parents immigrated into Sweden. For foreign-borns, we also control for country of birth and years since

[12]This heterogeneity is caused by, among other things, differences in labor market attachment, educational attainment and social situation. See Hoynes et al. (2012) for an extensive treatment of employment cyclicality for the U.S. labor market.

Figure 5: Employment trends for different age groups




*Notes*: Employment is defined as working at least quarter–time. The two vertical lines indicate the reform years.

immigration into Sweden.[13]

### 5.2 Absolute versus relative effects

An implication of the DiD identifying assumption of parallel trends is that the control group must not be affected by the intervention. If such treatment spillovers exist, we will not measure the difference between the reform outcome and the counterfactual outcome, but the difference to the control group deviation from its counterfactual outcome. In other words, we obtain a measure of the *relative* rather than the absolute effect of the reform. In the present case, there are strong reasons to suspect that the tax reduction had an indirect impact also on individuals not in the target group. The treatment spillover takes the form of substitution and scale effects. As a way of illustration, consider individuals at 25–26 years of age. The 2007 payroll tax reduction increases the cost of

[13]By using covariates we assume that the impact of demographic factors are homogeneous across different parts of Sweden. We have tried relaxing this assumption by combining DiD with exact matching on local labor markets. The results of this exercise, which can be obtained from the authors at request, are very similar to the figures reported in this study.

26-year-old labor relative to 25-year-old labor. If firms consider 25-year-olds and 26-year-olds as substitute inputs they will, all else equal (i.e. holding output constant), lower demand for the latter group of workers, resulting in a negative substitution effect for 26-year-old labor. The magnitude of the negative substitution effect on non-treated individuals should depend on their similarity to individuals in the target group. Hence, the effect should decrease in age.

The scale effect tends to work in the opposite direction to the substitution effect. A factor input price drop results in a downward shift of the firms' cost functions, potentially causing them to expand output. Similar to income effects in consumer theory, the sign of the scale effect can be either positive or negative, but for normal factor inputs, demand is increasing in output. If employers prefer older, more experienced, workers, the scale effect increases in age. Nonetheless, this scale effect asymmetry, if it exists, is likely to be small, especially if we use treatment-control pairs that are close in age. Hence, the substitution effect bias is, arguably, the bigger problem.

To clarify these mechanisms, consider a two-period setup, and decompose the change in the outcome variable into a counterfactual (temporal) component ($\tau$) and a treatment effect component ($\xi$), for each age group:

$$\Delta y_a = \tau_a + \xi_a. \tag{3}$$

The standard DiD estimate, for treatment group TG and control group CG, is then

$$\mathrm{DD}_{\mathrm{TG/CG}} = \Delta y_{\mathrm{TG}} - \Delta y_{\mathrm{CG}} = (\tau_{\mathrm{TG},t} - \tau_{\mathrm{CG}}) + (\xi_{\mathrm{TG}} - \xi_{\mathrm{CG}})\,.$$

Consistent estimation—defined as $\mathrm{DD}_{\mathrm{TG}} = \xi_{\mathrm{TG}}$—thus requires two assumptions: parallel counterfactual trends, $\tau_{\mathrm{TG}} = \tau_{\mathrm{CG}}$, and the absence of treatment spillovers in the control group, $\xi_{\mathrm{CG}} = 0$. From the above discussion, it is clear that the latter may not hold.

Using the decomposition in 3, we can distinguish between a couple of different concepts. The causal effect on the treatment group, $\xi_{\mathrm{TG}}$, is denoted the *absolute effect* of the reform. However, the DiD estimator will be biased by the treatment spillover effect, $\xi_{\mathrm{CG}}$, thus giving only the *relative treatment effect*: $\mathrm{DD}_{\mathrm{TG/CG}} = \xi_{\mathrm{TG}} - \xi_{\mathrm{CG}}$ (assuming parallel counterfactual trends, $\tau_{\mathrm{TG}} = \tau_{\mathrm{CG}}$). The difference between these, $-\xi_{\mathrm{CG}}$, we denote the *control group bias*. (Thus, a negative treatment spillover results in a positive control group bias on the treatment estimate.) The total absolute treatment effect of the treated, *the gross economy-wide treatment effect*, is given by $\sum_{a\in[19,25]} \xi_a$. Finally, taking into acocunt all treatment spillovers, the, *net economy-wide treatment effect* is given by $\sum_{a\in[19,25]} \xi_a + \sum_{a\notin[19,25]} \xi_a$.

As it turns out, we cannot estimate any absolute treatment effects in this study. To be sure, we can think of control groups where treatment spillovers should be negligible (if not strictly zero), but for these groups the parallel trends assumption ($\tau_{\mathrm{TG}} = \tau_{\mathrm{CG}}$) cannot be validated (as evidenced by non-parallel pre-treatment trends in those regressions).[14] Consequently, we are limited to estimating relative effects. However, by holding the control group constant across specifications, we can net out the control group bias when comparing treatment effects across different age groups. For example, comparing the treatment effect for 24-year-olds against that of 25-year-olds when using 26-year-olds as the control group (again assuming parallel counterfactual trends),

$$\mathrm{DD}_{24/26} - \mathrm{DD}_{25/26} = (\xi_{24} - \xi_{26}) - (\xi_{25} - \xi_{26}) = \xi_{24} - \xi_{25}.$$

We are, however, always limited to relative estimates—in one way or the other.

[14]For example, a tax cut for youths arguably has negligible treatment spillover on, say, 50-year-old highly specialized medical doctors. However, for the latter to function as a control group we must assume that their employment rate fluctuations over the business cycle are identical to the employment rate cyclicality of 20–25 year-olds; an assumption which is neither credible *a priori*, nor supported by historical employment rate fluctuations.

## 5.3 Choice of comparison groups

The previous discussion suggests that there is an element of trade-off involved when choosing comparison groups: decreasing the age interval around the cutoff should get us closer to estimating a causal, albeit relative, treatment effect, but the estimate is unlikely to be generalizable to the target group as a whole. With this in mind, we evaluate the effects of the payroll tax reduction both for age-groups close to the cutoff and for 20–25 year-olds. The reason for excluding 19-year-olds is that they turn out to be substantially different in terms of employment cyclicality, thus invalidating the use of DiD. Most likely, this is explained by the fact the majority of 19-year-olds are in their final year of high school for the first half of the year.[15]

The parallel trends assumption is, by definition, not testable since it concerns counterfactual outcomes. A common convention is to consider the evolution of the treatment and control groups prior to the intervention, thus getting an indication on whether the assumption is likely to hold. (Or rather, when it is not likely to hold.) While this procedure does not guarantee unbiased estimates, as is clear from the above discussion of treatment spillover effects, we consider parallel pre-treatment trends a minimal condition. This constrains us to use control group individuals close to the treatment cutoff, mainly 26-year-olds. As discussed above, these individuals are probably negatively affected by the reform and, thus, we interpret the estimations as upper bounds of the employment effect for the target group. As a special case, we consider individuals within a small bandwidth just around the treatment cutoff, comparing 25-year-olds born in January–

[15] A different approach would be to follow cohorts, rather than age groups, over time. This, however, would require a somewhat different identifying assumption. When comparing age groups, we assume that, for a specific age, any employment differences in cohorts over time can captured by our control variables. This is a reasonable assumption, since cohort differences are mainly due to demographic factors on which we have data. For the pre-treatment years, this is born out in the data and manifested as parallel pre-treatment trends (see Results section). The corresponding assumption for comparing cohorts over time is that employment differences between ages can be captured by control variables. This is less reasonable, since age differences in employment depend not only on education, but also on experience, psychological maturity, etc., which are far more elusive than demographic factors. In line with this reasoning, we find that Difference-in-Differences regressions on fixed cohorts tend to display non-parallel pre-treatment trends.

March with 26-year-olds born in October–December. This specification has elements of a regression discontinuity design, but with controlling for the pre-reform discontinuity. While heterogeneous cyclicality should no longer be an issue, with comparison group so close in age, this comes at a cost: similar to RD designs in general, the estimates risk being only locally valid.

In theory, we should expect stronger treatment effects for younger workers since the remaining available treatment years (the treatment dose) is decreasing in age. Estimating effects for individuals close to the cutoff may, for this reason, underestimate the average treatment effect on the treated. Additionally, since the treatment and control groups are defined in terms of age groups they are each year redefined in terms of cohorts. Consequently, an estimate based on single age groups is more sensitive to cohort heterogeneity, showing up as year shocks. In contrast, when using a treatment group of multiple ages, this heterogeneity is averaged out.[16] Another way of dealing with this issue is to estimate pooled treatment effects for two years at a time, e.g., the 2007–08 effect. Such an approach averages out cohort offsets, but at a loss in temporal resolution. We have chosen to use the more transparent yearly estimates when presenting the main results. In the cost-benefit analysis, however, we utilize the pooled estimates in order to get more robust measures. (As cohorts are roughly of the same size, the joint estimate will be close to the average of its corresponding yearly components.)

### 5.4 Repeated treatment and the 2009 extension

A difficulty with our method of evaluation is that, with time, it gets increasingly difficult to find individuals who have not been previously subjected to the payroll tax reduction. This makes it hard to identify the reform effect for the later years in our sample. Essen-

[16]Insofar as this cohort heterogeneity consists of compositional differences in dimensions that we observe, our control variables should take care of the problem. However, a *constant* offset for, say, the cohort of 25-year-olds in 2007 would bias the estimate of the reform effect. Cohort heterogeneity in the control group remains a potential problem since we, in most cases, cannot extend the age-interval upwards.

tially, the problem of lagged treatment exists whenever employment spells extend from one year to the next. Figure 2 in section 3 illustrates how different cohorts are subjected to the payroll tax reductions. In 2007, the target group consists of individuals born 1982–88. Their natural control group consists of individuals that are slightly older, i.e., those born 1981. In 2008, individuals born 1983–89 are in the target group, and those born 1982 constitute the control group. Arguably, the employment estimate for 2007 is best identified since there is no earlier intervention, for any age group. Already in 2008, the control group may be affected by earlier treatment. For example, comparing 25-year-olds to 26 year-olds implies that our control group in 2008 (those born 1982) was in the target group the year before. One way to handle this is to use 27-year-olds instead of 26-year-olds as control, when possible.

Figure 2 also shows why it is difficult to evaluate the 2009 extension. Since 26-year-olds are included in the redefined target group, the youngest age group that can be used as a control group is now 27-year-olds, and they are not comparable—in terms of parallel pre-treatment trends—to any age group below 24. We are thus restricted to studying the effects of the 2009 extension only for 24–26 year-olds. Those 24–25 years of age transition from 2007 treatment to 2009 treatment, while 26 year-olds transitions from no treatment straight to 2009 treatment. Note, however, that for the 2009 extension we can only study individuals who have been previously treated, as is apparent from figure 2.

In addition to these issues, the fact that the global financial crisis had its largest impact on Swedish employment in 2009–10—disproportionally affecting employment for younger workers—makes identification for these years even more difficult. When considering the 24–25 year-olds, the 2009 estimate will measure the impact of an extended reduction in the wake of the financial crisis. Correspondingly, for 26-year-olds we get the effect of introducing a payroll tax reduction in an economic depression. Hence, both of these specifications could be seen as testing how the payroll tax reduction fare when labor market conditions worsen.

### 5.5 Estimating wage effects

The impact on employment depends on how much of the tax cut is shifted onto workers in the form of higher wages. In the long run, wages may adjust to counteract the effect of a payroll tax change. In the extreme case of full shifting, the payroll tax decrease will be fully cancelled out by wage increases, resulting in unchanged net labor costs for employers and, consequently, no employment effects. In the present case, with targeted reductions and a target group that has little attachment to the labor market, it is difficult, *ex ante*, to predict whether shifting will occur.[17]

Wage effects can appear through two channels: individual bargaining and union bargaining. In the latter case, there is a possibility that unions seek to make sure that all workers benefit; if so, the payroll tax reductions would have resulted in general shifting. This gives rise to a problem similar to when estimating employment effects: the $\boldsymbol{\delta}$ in equation 2 captures only the relative wage effect. However, the primary question we are interested in is not whether shifting occurred *per se*; rather, our focus is on whether relative wage increases around the cutoff can explain (the lack of) relative changes in employment.

Finally, it is important to stress that we only study the immediate impact on wages. If wage adjustments appear in the longer run, the long-term employment effects of the payroll tax cuts will be lower than what our estimates suggest.

[17]Some guidance may be found in Kolm (1998), who considers a two-sector (general equilibrium) model where market competitiveness differs between sectors, and where a general payroll tax cut would be fully shifted to workers. In this model unemployment can be reduced by taxing the less competitive sector relatively more.

# 6 Results

## 6.1 Main findings

Table 3 presents the main results for the 2007 reduction. The outcome variable is yearly average employment status, ranging from zero to one. All treatment effects are relative to the reference period 2001–04. The first two rows show whether the comparison groups move in parallel prior to the 2007 reform: significant pre-treatment effects for 2005 or 2006 would indicate that the control group is invalid.[18]

Table 3: Employment effects of the 2007 reduction, main results

| | Local | 20–25 vs. 26 | 24–25 vs. 26 | 24,25 vs. 27 |
|---|---|---|---|---|
| DD 2005 | 0.001 | −0.003’ | 0.001 | −0.003 |
| | (0.003) | (0.001) | (0.003) | (0.002) |
| DD 2006 | 0.000 | 0.002 | −0.000 | −0.000 |
| | (0.004) | (0.002) | (0.001) | (0.002) |
| DD 2007 | 0.008** | 0.014*** | 0.008** | 0.005*** |
| | (0.003) | (0.003) | (0.003) | (0.002) |
| DD 2008 | 0.008* | 0.010*** | 0.006*** | 0.008*** |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| $R^2$ | 0.12 | 0.10 | 0.11 | 0.12 |
| N | 419,153 | 6,015,905 | 2,588,746 | 2,606,207 |
| $\overline{y}_{\mathrm{TG}}$ | 0.63 | 0.58 | 0.61 | 0.61 |

*** $p < 0.1\%$, ** $p < 1\%$, * $p < 5\%$, ’ $p < 10\%$

*Notes*: Outcome is average employment status during the year (ranging from 0 to 1), $\overline{y}_{TG}$ denotes treatment group average employment in the treatment period. All treatment effects are relative to the reference period 2001–04. Fixed effects included for year and demographic characteristics. Standard errors are cluster-robust w.r.t. local labor markets. ‘Local’ compares 25-year-olds born in Jan-Mar to 26-year-olds born in Oct-Dec. The gray line indicates reform implementation.

The first column studies the effect at the treatment cutoff, comparing the three oldest birthmonth cohorts (born in January–March) of the 25-year-olds to the three

[18] Another method to check whether the DiD assumption is credible, sometimes used in the literature, is to run separate placebo regressions for selected years. Our method is, arguably, less arbitrary since we calculate pre-reform effects routinely for every specification used.

youngest birthmonth cohorts (born in October–December) of the 26-year-olds. This bears some resemblance to a regression discontinuity design, but with controlling for prereform discontinuity. (To reiterate, a clean RD design is not possible due to pre-reform discontinuities between cohorts.) There is a statistically significant, albeit small, positive employment effect, both in 2007 and in 2008, representing a shift in relative employment trends around the cutoff. This is most likely caused by the reform since the point estimates for both pre-treatment years are insignificant and close to zero. From the local estimation we conclude that the lower payroll taxes increased the employment rate with roughly 0.8 percentage points, corresponding to a rise in employment of around 1.3 percent.[19] Column 2 looks at the whole target group except 19-year-olds. The treatment effect is now substantially larger: for 2007, the point estimate corresponds to a rise in employment of roughly 2.5 percent, while for 2008 the increase is at around 1.8 percent. The larger effect for younger individuals is consistent with a treatment dose explanation, as younger individuals have longer expected exposure to the reduced payroll tax. However, the larger effect may also depend on labor force composition. For example, if low-skilled jobs are affected more by lower payroll taxes and younger individuals to a larger extent are low-skilled, we would expect the treatment effect to decrease in age, even without age differentials in treatment dose. As in column 1, the insignificant (at standard levels) pre-treatment estimates support a causal interpretation of the employment increase.[20]

We cannot rule out that the 2008 estimates in columns 1–2 are downward biased due to treatment in the previous year (those 26 years of age in 2008 were treated in 2007). As mentioned in section 5, one way to handle this issue is to use 27-year-olds instead of 26-year-olds as the control group. Unfortunately, due to significant pre-treatment effects, we cannot include individuals older than 26 years in the control group when studying the

[19] The percentage increase is relative to the counterfactual outcome. It is, thus, obtained as $\beta/(\overline{y}_{\mathrm{TG}}-\beta)$.

[20] The 2005 estimate is significant at the 10%-level. There are no significant pre-treatment effects when considering slightly smaller age-intervals, as seen in columns 2–5 of table 4.

whole target group. What we can do, however, is to look at 24–25 year-olds and alternate between using 26-year-olds and 27-year-olds as the group of comparison. Columns 3–4 of table 3 presents the result of this exercise: the 2008 effect is slightly larger when using 27-year-olds. On the surface, this suggests that specifications using 26-year-olds as the control group suffer from biased estimates for years later than 2007. Note, however, that the difference between the 2008 point estimates is small and their confidence intervals overlap; thus, we cannot formally decide whether this is a real problem.

The strategy of alternating the control group can be used also to get an idea of the magnitude of the substition effect. Since the negative substitution effect on non-treated individuals should decrease in age, the relative treatment effect estimated by DiD (being the difference of the positive treatment effect on the treated and the negative spillover effect on the control group—see section 5) should decrease with an older control group, at least for the 2007 estimate. (Remember that according to the previous paragraph, the 2008 estimate is potentially downward biased when using 26-year-olds as the control group.) We find that by changing the control group from 26-year-olds to 27-year-olds, the treatment effect does drop somewhat for 2007, but not for 2008. While, again, being *prima facie* evidence for our working hypothesis, the difference is small and not statistically significant. Thus, we cannot make inferences about the possible existence of substitution effects—albeit intuitively plausible.

Next, we examine age heterogeneity in more detail. Since we cannot in general compare single age groups to any age groups above the cutoff (due to non-parallel pre-treatment trends), we proceed by expanding the treatment group step by step.[21] As shown in table 4, there are statistically significant treatment effects irrespective of how we define the treatment group. In columns 1–5 the magnitude of the effect grows smoothly as we gradually include younger individuals; this is what would be expected considering that the period of remaining treatment is decreasing in age. Another reason for why

[21]

the treatment effect should decrease in age is that as employment rate increase in age, the unemployed are a increasingly selected group of low-productivity workers. Note that for 20-year-olds, the effect appears to decrease again. A possible explanation is that labor force participation is lower for the youngest individuals, which means that a large number of 20-year-olds are not, in practice, eligible for the payroll tax reduction. Another interpretation is that not even the substantially higher treatment dose can compensate for their lower expected productivity.

Table 4: Employment effects of the 2007 reduction: Age heterogeneity

| | 25 vs. 26 | 24–25 vs. 26 | 23–25 vs. 26 | 22–25 vs. 26 | 21–25 vs. 26 | 20–25 vs. 26 |
|---|---|---|---|---|---|---|
| DD 2005 | 0.002 | 0.001 | 0.000 | 0.001 | 0.000 | −0.003’ |
| | (0.002) | (0.003) | (0.002) | (0.002) | (0.001) | (0.001) |
| DD 2006 | −0.002’ | −0.000 | 0.001 | 0.003 | 0.003 | 0.002 |
| | (0.001) | (0.001) | (0.002) | (0.002) | (0.002) | (0.002) |
| DD 2007 | 0.006** | 0.008** | 0.011*** | 0.014*** | 0.015*** | 0.014*** |
| | (0.002) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| DD 2008 | 0.004* | 0.006*** | 0.009*** | 0.013*** | 0.013*** | 0.010*** |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.003) | (0.003) |
| $R^2$ | 0.11 | 0.11 | 0.11 | 0.10 | 0.10 | 0.10 |
| N | 1,735,836 | 2,588,746 | 3,438,874 | 4,291,748 | 5,148,083 | 6,015,905 |
| $\overline{y}_{\mathrm{TG}}$ | 0.63 | 0.61 | 0.60 | 0.60 | 0.59 | 0.58 |

*** $p < 0.1\%$, ** $p < 1\%$, * $p < 5\%$, ’ $p < 10\%$

*Notes*: See notes for table 3.

We have described in section 5 that the estimated Difference-in-Differences effects are to be interpreted as *relative* effects: each estimate is a combination of the treatment effect of the treatment group and potential spillover effects for the control group. If the spillover effect is negative—as is the case with control group substitution—the DiD will overestimate the treatment effect. However, holding the control group constant, as in table 4, means that the control group bias is held constant as well. Consequently, we can make relative comparisons between treatment groups with the control group bias netted out. We can thus state that the *absolute* treatment effect is 0.6–0.8 percentage points higher for 20–25 year-olds than for 25-year-olds.[22] Going further, we can use the constancy of the control group in yet another way. Since all DiD estimates include the same substitution effect bias, we can take the smallest of these estimates as an upper bound for the substitution effect bias (assuming that no age group in the target group has a negative absolute treatment effect). Consequently, we can use the 25–26 estimates in column 1 as an upper bound for the negative substitution effect for the 26-year-olds.. This implies that the absolute employment increase for 20–25 year-olds is *at least* 0.8 percentage points in 2007 and 0.6 percentage points in 2008.

In 2009, the Swedish labor market was hit by the financial crisis (see figure 5, page 18). By considering the 2009–10 time period we can thus examine whether reduced payroll taxes counteract the negative effects of an economic downturn. In 2009 the payroll tax reform was modified in two ways. First, 26-year-olds were now also subjected to reduced payroll taxes. Second, the tax reduction for the target group was extended by an additional five percentage points. Table 5 shows yearly treatment effects for three different age group compositions up until 2010. As 26-year-olds are part of the target group from 2009 and onwards, we have switched to using 27-year-olds as the control group.

Column 1 returns to 24–25 year-olds from table 3, who transition from 2007 treatment to 2009 treatment (to reiterate, these are the youngest age groups that we can consider when using 27-year-olds as control group). In contrast to the clear pattern of the first two treatment years 2007–08, when the economy was expanding, the recession years 2009–10 display a somewhat jumpy pattern. The point estimate for 2009 is small and not significant, whereas in 2010, when employment levels no longer fell dramatically, the

[22] Using the terminology of section 5—where $\delta_g$ is the DiD estimate and $\xi_g$ is the causal effect of the reform, for group $g$—the result of comparing 20–25 year-olds against 25-year-olds (assuming parallel counterfactual trends) is

$$\delta_{20\text{–}25/\mathrm{CG}} - \delta_{25/\mathrm{CG}} = (\xi_{20\text{–}25} - \xi_{\mathrm{CG}}) - (\xi_{25} - \xi_{\mathrm{CG}}) = \xi_{20\text{–}25} - \xi_{25}.$$

Table 5: Employment effects of the 2009 extension

| | 24–25 vs. 27 | 26 vs. 27 |
|---|---|---|
| DD 2005 | −0.003 | −0.004** |
| | (0.002) | (0.002) |
| DD 2006 | −0.000 | 0.000 |
| | (0.002) | (0.002) |
| DD 2007 | 0.005*** | −0.003 |
| | (0.002) | (0.002) |
| DD 2008 | 0.008*** | 0.002 |
| | (0.002) | (0.001) |
| DD 2009 | 0.002 | −0.003* |
| | (0.002) | (0.001) |
| DD 2010 | 0.009*** | 0.001 |
| | (0.002) | (0.002) |
| $R^2$ | 0.12 | 0.14 |
| N | 3,305,579 | 2,224,418 |
| $\overline{y}_{\mathrm{TG}}$ | 0.59 | 0.63 |

*** $p < 0.1\%$, ** $p < 1\%$, * $p < 5\%$, ' $p < 10\%$

*Notes*: See notes for table 3.

effect is again significant and similar in magnitude to 2007–08. Next, in column 2 of table 5, we study 26-year-olds—the age group that were subjected to reduced payroll taxes for the first time in 2009. Strikingly, for this age group there is no apparent effect of the lower payroll taxes in any of the recession years 2009–10 (there is even a small negative effect in 2009).[23] It seems reasonable to interpret the results in table 5 as evidence against any additional employment effect of the 2009 extended reduction; if anything, the effect is even lower than in the preceding years. This finding is important as it suggests that (substantial) payroll tax cuts are even less effective in economic downturns.[24]

[23] While there is a significant pre-treatment effect in 2005, all of 2006–08 are free from pre-treatment effects.

[24] It is, however, also possible that the null result for 2009 signals that the identifying assumption for our estimation strategy fails for 2009–10, since this period was characterized by a historically large downturn. The argument is as follows: The DiD strategy is to assume that the treatment group counterfactual trend can be obtained by extrapolating the control group trend. In the present study, we validate this assumption by statistically testing for trend deviation in the pre-treatment years 2005–06. But it is

Column 2 points to another interesting finding. From a welfare perspective, it is important to understand how lasting the effect is for an individual who is no longer eligible for the lower payroll tax, but who was previously treated. While 26-year-olds are not part of the treatment group until 2009, we may nonetheless expect an impact in 2008 if the treatment effect for the 25-year-old individuals in 2007 persists over to the next year (when they are 26 years old, and thus no longer treated). Comparing the 2007 estimate for 25-year-olds in column 3 to the 2008 estimate for 26-year-olds in column 2 shows that the treatment effect vanishes quickly when an individual transitions from treatment to no treatment. Ultimately, this shows that even though the reform created a relative price wedge that induced employers to hire, or to keep, a young worker, it did not lead to any permanent increase in the likelihood that this individual is employed.

Our employment measure uses the quarter of the income cutoffs to arrive at a measure of working *at least* 25 percent of full-time (for a particular month). We have also tried alternative definitions of employment. The stricter definition of working at least 50 percent of full-time produces somewhat smaller treatment effects, while, on the other hand, relaxing the employment restriction to 10 percent of full-time does not change the estimates. The latter suggests that it is not the case that we fail to account for part of the employment effect by choosing too strict an employment definition. Using an outcome measure of full-time employment, or less than 10 percent of full-time, is not viable since we then face significant pre-treatment effects.[25] In summary, there seem to have been positive, but small, employment effects of the 2007 payroll tax reduction.

conceivable that while the identifying assumption holds for the years up to 2008, the great recession in 2009 was associated with even greater differences in employment cyclicality across ages. This would be the case if the downturn had asymmetrical employment effects across ages. If so, the DiD estimates for 2009–2010 should be interpreted with caution. We can, however, establish that if the additional reduction had an impact in 2009, this impact was not large enough to counteract the effects of the economic downturn.

[25] In addition, we have used a measure constructed by Statistics Sweden (SCB), emulating the ILO definition of working at least one hour per week; although the SCB measure is to blunt to study the full age group 20–25, for the age groups 25 and the local estimate this gives similar estimates as those reported above. These results, as well those obtained with the other employment definition discussed in this paragraph, are available from the authors upon request.

This holds irrespective of whether we study a small interval around the treatment cutoff, or examine the whole target group of 20–25 year-olds. For the 2009 extended reduction, there is no evidence of any additional effect.

### 6.2 Treatment effect heterogeneity

We next turn to the subsample of young foreign-borns, in columns 1–2 of table 6. This group, which constituted about 15 percent of the age group 20–25 in 2007–08, is characterized by weak attachment to the Swedish labor market. Their employment rate is about 20 percentage points lower than for the whole population of young workers, as reported in the bottom rows of tables 3 and 6. Strikingly, there is no evidence that the payroll tax cut had any impact at all for this group. It should be noted that the lack of treatment effects is not the result of noisy estimates due to a smaller number of observations.[26]

In theory, an explanation for the small general employment effects could be labor supply constraints. A significant share of 20–25 year-olds are taking part in higher education, and it is, thus, perhaps not reasonable to expect a strong employment response for this group. We examine this hypothesis by studying previously unemployed 25–26 year-olds—defined here as those individuals who were registered unemployed at the unemployment office for at least 100 days during the previous year. (In 2007, this group amounted to around 38 percent of all 25–26 year-old registered, and around 9 percent of the full cohorts.) For this group, labor supply constraints should be less of a problem: by definition, registered unemployed are not taking part in education, and the fact that these individuals are attending the unemployment office at least signals a willingness to take a job. As column 3 of table 6 shows, there is no indication that the effect for unemployed 25-year-olds were larger than in the general case, albeit the estimates are

[26]Since the sample of foreign-born is far from homogenous, we have also used finer subdivisions of region of birth, as well as disregarding newly arrived immigrants. Eastern Europeans is the only group for which we find a positive effect; the magnitude is similar to that of Swedish-born. These results are available from the authors upon request.

Table 6: Employment effects for foreign-born and previously unemployed

| | FOREIGN-BORN | | UNEMPLOYED |
|---|---|---|---|
| | 25 vs. 26 | 20–25 vs. 26 | 25 vs. 26 |
| DD 2005 | 0.002 | −0.001 | −0.000 |
| | (0.004) | (0.003) | (0.005) |
| DD 2006 | −0.002 | −0.001 | −0.005 |
| | (0.003) | (0.003) | (0.005) |
| DD 2007 | 0.003 | 0.005’ | 0.007 |
| | (0.003) | (0.003) | (0.007) |
| DD 2008 | −0.006’ | −0.002 | 0.002 |
| | (0.004) | (0.004) | (0.007) |
| $R^2$ | 0.19 | 0.18 | 0.04 |
| N | 291,125 | 890,911 | 153,931 |
| $\overline{y}_{\text{TG}}$ | 0.39 | 0.35 | 0.45 |

*** $p < 0.1\%$, ** $p < 1\%$, * $p < 5\%$, ’ $p < 10\%$

*Notes*: Control variables include region of birth, year since immigration into Sweden, among others. ‘Unemployed’ is defined as having been registered at the unemployement office at least 100 days during the previous year. See also notes for table 3.

somewhat noisy. While these results do not completely rule out the labor supply story, they indicate that labor demand is the more important factor.

### 6.3 Intensive margin

It is conceivable that employers reacted to the decrease in labor costs not only by increased hiring, but by increasing hours for their existing labor stock. To explore this channel we turn to the Structure of Earnings Study, which contains information on hours worked for all private sector employees in the sample, as well as for those employed in local government sectors (municipality and county).[27] We also look specifically at the subsample of previously employed, so as to avoid the potential composition problem arising from the employment increase.

[27]For central government employees, information on hours worked is not available. This should, however, only be a minor problem since less than two percent of 20–25 year-old employees work in the central government sector.

Table 7: Effects on hours (subsample of employed)

| | All workers | | | Prev. empl. | | |
|---|---|---|---|---|---|---|
| | Local | 20–25 vs. 26 | 24–25 vs. 27 | Local | 20–25 vs. 26 | 24–25 vs. 27 |
| DD 2005 | −1.1 | −1.1 | −0.6 | −2.3 | −0.1 | −1.5 |
| | (2.4) | (0.8) | (0.8) | (2.9) | (1.4) | (1.1) |
| DD 2006 | −3.2’ | 0.6 | −0.5 | −0.9 | 1.0 | −1.4 |
| | (1.8) | (0.9) | (1.0) | (2.7) | (1.2) | (1.0) |
| DD 2007 | 0.8 | 0.1 | −0.3 | 3.6 | 0.9 | −0.5 |
| | (2.8) | (1.0) | (1.1) | (3.2) | (0.7) | (1.3) |
| DD 2008 | −3.3* | 0.6 | 0.0 | −3.1 | 0.9 | −1.4 |
| | (1.4) | (1.4) | (1.1) | (2.0) | (1.6) | (1.3) |
| DD 2009 | | | −0.6 | | | −2.2’ |
| | | | (1.2) | | | (1.3) |
| DD 2010 | | | −1.0 | | | −3.0* |
| | | | (1.1) | | | (1.4) |
| $R^2$ | 0.09 | 0.11 | 0.09 | 0.08 | 0.10 | 0.08 |
| N | 131,886 | 1,651,544 | 1,013,310 | 95,677 | 1,125,795 | 742,526 |
| $\sum w_i$ | 235,405 | 3,066,697 | 1,821,843 | 165,775 | 2,024,320 | 1,302,088 |
| $\overline{y}_{\text{TG}}$ | 120.6 | 108.3 | 115.5 | 122.6 | 112.1 | 118.2 |

*** $p < 0.1\%$, ** $p < 1\%$, * $p < 5\%$, ’ $p < 10\%$

*Notes*: Outcome is the monthly total of hours worked, $\overline{y}_{TG}$ denotes treatment group average outcome in the treatment period. All treatment effects are relative to the reference period 2001–04. Fixed effects included for year and demographic characteristics. Standard errors are cluster-robust w.r.t. local labor markets.

Table 7 shows the effect on hours worked for the main age groups studied above, for the original payroll tax cut as well as for the 2009 extension. The results suggest that there were no effects on the intensive margin as a result of the payroll tax cut. (The local estimation for all employees appears unstable, with tendencies of pre-treatment effects.) For 24–25 year-olds there is a significant negative effect in 2010, but since this coincided with the great downturn we should probably not interpret this as a reform effect. For example, this might be a composition effect if firms, as a response to weaker demand, cut down on hours more for younger, on average less productive, workers.

## 6.4 Wage effects

We next examine whether part of the payroll tax cut was passed on to employees as higher wages. The outcome measure is now the log of monthly full-time equivalent wage, and we condition the sample on those employed at least quarter-time (in symmetry with our main employment definition used above). Table 8 gives the impacts of both the 2007 initial cut and the 2009 extension.[28] Starting with the 2007 reduction, there is no effect around the cut-off; the point estimates for 25-year-olds are small in economic terms, and insignificant. For 20–25 year-olds there is, however, a small relative wage increase, slightly above one percent both in 2007 and in 2008. This could indicate that some of the younger workers of the target group have taken home a small fraction of the tax cut given to employers. Notably, the wage increase shows up already in 2007. Comparing 24–25 year-olds to 27-year-olds allows us to study the evolution of wages into the 2009 extension. Since there is no additional wage effect in 2009–10 we conclude that wages did not adjust more in the somewhat longer run.

Understanding these wage effects requires making a few observations. To start with, there is evidence that the unions and the employer organizations agreed on letting minimum wages increase faster than general wages after 2007 (National Mediation Office 2007). Thus, we are potentially picking up negotiated minimum wage increases. It is an open question, however, whether these increases were the result of the reform or part of a long-term trend. (As mentioned in section 3, wages were renegotiated at the central level just after the passing of the 2007 reduction in the parliament, but before the reduction was implemented.) What speaks against the minimum wage increase explanation is the evidence of wage effects even for such age groups (24–25) that typically have wages strictly above the minimum wage level.[29] This would suggest that shifting instead works

[28] For each of the two age groups that we consider, we have tested for heterogeneity with respect to private or public sector, and for blue collar or white collar workers. The results for these subgroups are similar to the general cases.

[29] Forslund et al. (2012) report that young workers' wages in the private sector are often higher than the negotiated minimum wages, even for workers as young as 19 years old.

Table 8: Effects on wages (subsample of at least quarter-time employed)

| | All workers | | | Prev. empl. | | |
|---|---|---|---|---|---|---|
| | Local | 20–25 vs. 26 | 24–25 vs. 27 | Local | 20–25 vs. 26 | 24–25 vs. 27 |
| DD 2005 | −0.017 | 0.004 | 0.000 | −0.003 | 0.007’ | −0.001 |
| | (0.013) | (0.003) | (0.005) | (0.010) | (0.004) | (0.005) |
| DD 2006 | −0.011 | 0.006 | −0.003 | −0.007 | 0.008’ | −0.004 |
| | (0.013) | (0.004) | (0.003) | (0.013) | (0.004) | (0.004) |
| DD 2007 | −0.007 | 0.012*** | 0.009* | −0.008 | 0.010** | 0.011* |
| | (0.007) | (0.002) | (0.004) | (0.008) | (0.004) | (0.005) |
| DD 2008 | 0.003 | 0.013** | 0.014* | 0.009 | 0.013* | 0.017*** |
| | (0.007) | (0.005) | (0.006) | (0.010) | (0.006) | (0.004) |
| DD 2009 | | | 0.011*** | | | 0.012** |
| | | | (0.003) | | | (0.004) |
| DD 2010 | | | 0.009** | | | 0.005 |
| | | | (0.003) | | | (0.005) |
| $R^2$ | 0.21 | 0.25 | 0.27 | 0.21 | 0.25 | 0.27 |
| N | 128,383 | 1,485,391 | 981,757 | 93,921 | 1,027,557 | 726,355 |
| $\sum w_i$ | 223,666 | 2,758,227 | 1,727,823 | 159,404 | 1,849,043 | 1,249,271 |
| $\overline{wage}_{\text{TG}}$ | 21,790 | 20,543 | 22,029 | 21,987 | 20,912 | 22,311 |

*** $p < 0.1\%$, ** $p < 1\%$, * $p < 5\%$, ’ $p < 10\%$

*Notes*: Outcome is the log of monthly full-time equivalent wage (truncated below to 0), $\overline{y}_{TG}$ denotes treatment group average outcome in the treatment period, in non-log form. All treatment effects are relative to the reference period 2001–04. Fixed effects included for year and demographic characteristics. Standard errors are cluster-robust w.r.t. local labor markets.

through individual wage bargaining. Such an impact, if it exists, is likely to be more immediate than union-negotiated wage increases. With this being said, we conclude that given the small size of the wage increase, shifting cannot by itself explain the modest employment effects we have found.

# 7 Cost-benefit analysis

In the following, we present some further metrics for evaluating the payroll tax reduction in 2007–08. When calculating demand elasticity and cost per job, we have, for increased efficiency, reestimated the models, using pooled treatment effect estimates for 2007–08 (omitting pre-treatment interactions). It is important to stress, however, that these derived measures are likely to be overly optimistic. First, the substitution effect bias causes us to overestimate the treatment effect and, consequently, to overestimate the demand elasticity and underestimate the cost per job. Second, it is by no means clear that the target group employment increase reflects a *net* increase of jobs in the economy. Rather, a part of this increase may be at the expense of older workers in the labor force. Although this will not affect the elasticity estimate—which is defined as being with respect to *young* labor—it will further bias the measure of cost per job, as job losses for older workers are not taken into account. This is discussed further in section 8.

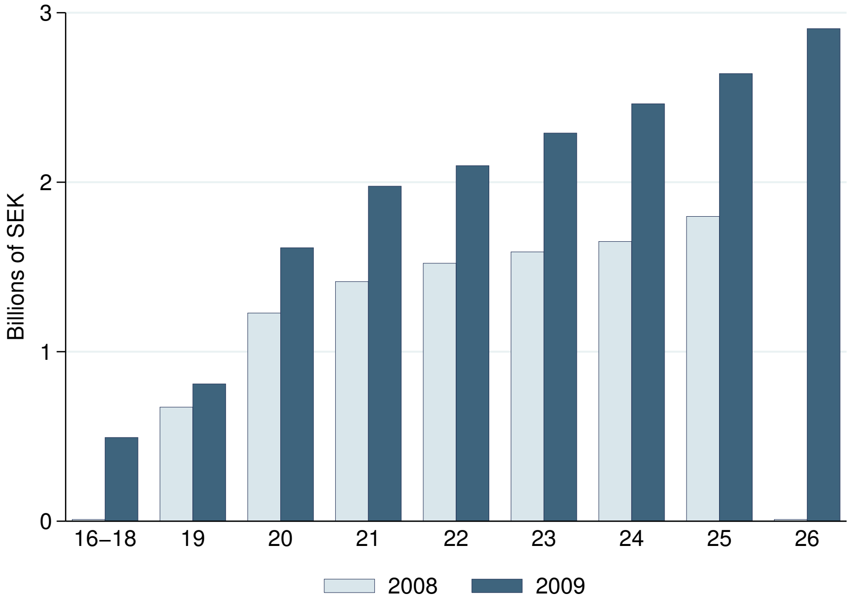
## 7.1 Elasticities

We can combine employment and wage estimates to obtain the elasticity of demand for young workers with respect to labor costs. For 20–25 year-olds, an estimate of the joint 2007–08 employment increase is 2.1 percent, and the corresponding wage increase is 1.2 percent. Hence, we arrive at a labor demand elasticity at about −0.32.[30] Although this number may appear small, previous literature typically finds no employment effects of targeted payroll tax reductions. For example, employment was unaffected by regional reductions in the Nordic countries, and by reductions targeted at the employers of older, full-time, low-wage workers in Finland (see Bohm and Lind 1993; Bennmarker et al. 2009; Korkeamäki and Uusitalo 2009; Huttunen et al. 2013).

[30]Note that the employment effect is estimated in absolute numbers while the wage estimate is in log form. In addition to wage level and payroll tax, labor cost also includes a union negotiated fee at around 10 percent. Thus, labor demand elasticity is obtained as

$$\epsilon = \frac{\beta_{\text{empl}}/(\overline{empl}_{\text{TG}} - \beta_{\text{empl}})}{(e^{\beta_{\text{wage}}} - 1) - 0.111/(1 + 0.3242 + 0.10)}.$$

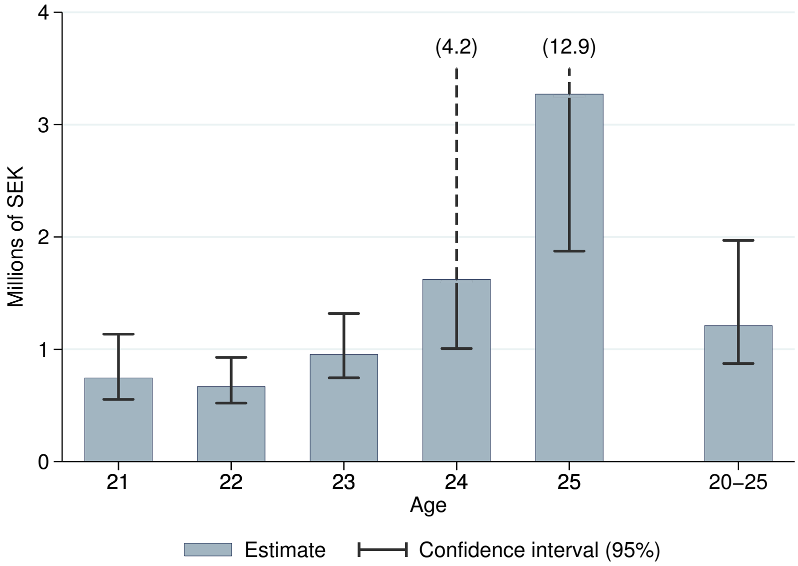
Figure 6: Gross cost per age group, 2008 and 2009




## 7.2 How much money was spent on each job?

The gross cost of the payroll tax reductions—the sum of foregone payroll taxes, disregarding potentially increased revenues due to, e.g., higher profits—can be straightforwardly calculated since total taxable income is available to us in the tax registers. (Skedinger 2014 provides indications that part of the payroll tax reduction did in fact end up as firm profits.) Figure 6 shows the gross cost broken down by age for the years 2008 and 2009, thus demonstrating the effect of the 2009 extension. The figure illustrates that incomes are markedly higher for the older individuals of the target group, as they both have higher average wages and work more hours. As a consequence, the cost of the reductions increases in age. The figure also shows that the cost increased dramatically in 2009, by simultaneously increasing the size of the reduction and targeting a larger age group. The total gross cost increased from SEK 9.9 billion ($1.2 billion) in 2008 to 17 billion ($2.1 billion) in 2009. These high numbers reflect the fact that all employments were subsidized, not only new ones.

Figure 7: Estimated cost per new job of the 2007 reduction




We can also deduce the total number of new jobs created each year by the payroll tax reduction. For 20–25 year-olds, a 95 percent confidence interval gives an estimate of 5,000 to 11,300 new jobs (with a point estimate of 8,200). In combination with the gross cost, we now get an estimate of the gross cost per created job; figure 7 shows this cost for 20–25 year-olds and for separate age groups (excluding 20-year-olds where we cannot properly identify the treatment effect). For the entire target group, the gross cost for each job is SEK 0.9–2.0 million (ca. \$110,000–\$240,000)., with a point estimate at SEK 1.2 million (ca. \$140,000). Notably, the latter is more than four times the hiring cost for an average-paying job in this age group.[31] Since the gross cost increases in age and, additionally, the number of new jobs decreases in age, it is not surprising that the cost per job soar as we move closer to the treatment age cutoff. For 25-year olds, the point estimate of the cost per job amounts to SEK 3.3 million (ca. 390,000)—exceeding ten times the average hiring cost for 25-year-olds working at least quarter-time.

[31] When calculating the hiring cost, we take the average income of those employed at least quarter-time, adding the cost for payroll taxes and the union-negotiated fee of (around) ten percent—in total 42.42 percent.

Finally, we note that these numbers apply only for the first tax reduction. In 2009, the payroll tax reduction was both increased in magnitude and extended to encompass all individuals under 26. Although we have no useful employment estimates for this period, we know that the gross cost almost doubled in 2009. Thus, if our results are indicative also for the employment response of the 2009 extension, the cost per job is likely to be significantly higher for this period than for the 2007 original tax reduction.

# 8 Discussion

The previous sections have painted a picture of the 2007 and 2009 payroll tax cuts as being largely unsuccessful—the impact on youth employment was small, and the cost per created job, in terms of foregone tax revenues, was high. This may seem puzzling at first glance: wages should be rigid in the short run, so we might at least have expected a temporary employment boost. Indeed, the wage regressions demonstrate that there were no extensive wage adaptations that could explain the meager impact on employment. This raises the question of why employers do not increase their hiring of young workers, despite the latter now being significantly less expensive. In discussing potential answers to this question, we will consider labor supply constraints and labor demand constraints, in that order.

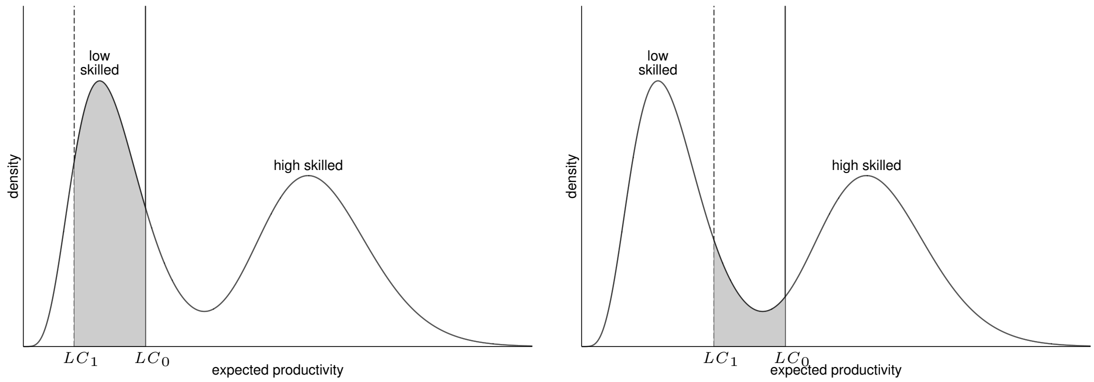
It is, in principle, possible that the lack of employment response is caused by low labor supply. There are many alternatives to employment for young individuals in Sweden. Many are taking part in higher education, others spend a couple of years after high school travelling the world. It is also possible that some of those who are formally applying for a job are actually quite satisfied with the comfortable life of receiving unemployment benefits while living with their parents, thus stifling the willingness to work. These speculations are, to some extent, tested in our regressions for the subsample of previously unemployed 25-year-olds. By restricting the sample to those registered at the unemployment office, we disregard both the unemployed students and the globe trotters. While the fundamental issue of weak economic incentives remain, we should diminish its importance by studying 25-year-olds—for individuals at this age there is a strong social stigma both of being unemployed and of living with one's parents (thus the economic incentives kick in stronger as well). The null effect for unemployed indicates that labor supply is not the main problem. We thus conclude that the weak employment response is more likely to be a consequence of low demand elasticity.

Turning to labor demand, we discuss a number of alternative interpretations. First, it is unlikely that employers were unaware of the new rules since the reform was covered rather extensively in the media, both when it was ratified and later on. (The payroll tax reductions were also criticized by the political oppositon in Sweden and, therefore, rather intensely debated.) It is also unlikely that employers were reluctant to take any action in the short run because they were uncertain about how persistent the new rules would be. The reform was implemented shortly after the 2006 elections, meaning that employers should have anticipated the new rules to be in place for at least one length of office, which is four years in Sweden. To be sure, the extension of the payroll tax reductions in 2009 should clearly signal that this was not a temporary policy, but even here, we find small, or no effects.

Another possible explanation is linked to short-term capital rigidity. Since increasing output may require long-run capital investment, the scale effects are not allowed to work to its full extent in the short run. Thus, if firms were capacity constrained when the payroll taxes were cut, they could not immediately make the capital investments to accomodate more labor. The fact that the 2007 reduction was implemented in a booming economy speaks for this explanation. But this story is, at the very most, plausible only for the very short run—if this were true, we would see increasing effects at least at the end of the period under study. Furthermore, Skedinger (2014) finds small effects also in the Swedish retail industry, where firms should be less capacity constrained. Indeed, for this industry it is during a boom that employers should be most willing to hire young workers, also in the short run.

A third possible explanation for the lack of large employment effects is that the wage cost for the typical young worker is too high in relation to her productivity, even after the tax cut. That is, the labor cost reduction does not compensate for the risk premium of hiring a young, untrained, and unexperienced worker. This corresponds to a situation where, for many firms, factor demand for young labor is at a corner solution, at zero demand. In such a scenario, any cost-reducing measure that does not push labor costs below the hiring threshold will have zero effect on the firm's labor demand—i.e., the demand elasticity will be locally zero. This idea can be made more clear by considering a stylized model of labor demand: Assume that a worker is hired by the representative firm if her expected productivity exceeds the minimum cost of employment (associated with the *de facto* minimum wage), and consider the effect of a payroll tax cut, which lowers this cost. Figure 8 shows two stylized situations. In the left-hand figure, a large pool of workers have expected productivity just below the initial minimum cost of hiring, $LC_0$. Thus, the reduction makes many of these workers employable. In the right-hand figure, on the other hand, the initial minimum labor cost is substantially above the productivity of most low-skilled workers, so that the new level $LC_1$ is still too high for most low-skilled workers. Note that in both of these cases, the minimum labor cost is binding for a large share of the labor force. Nonetheless, the tax reduction considered has wildly different employment effects in the two settings, as this is determined by the density of the productivity distribution in the interval $[LC_1, LC_0]$. We believe that the Swedish labor market bears resemblance to the right-hand part of figure 8. This case is made stronger if we return to the results for previously unemployed and foreign-born. For both of these groups we would expect productivity to be lower (table 2, page 15, indicates that they have both lower education levels and weaker attachment to the labor market). Consistent with the explanation above, we find that the cut in payroll taxes

Figure 8: Depending on the initial minimum employment cost ($LC_0$), in relation to the distribution of (expected) productivity in the labor force, a labor cost reduction may give either a large (left) or a small (right) effect on low-skilled employment.




resulted in at most weak effects for these groups, if at all.

It is important to stress that the estimates reported in this study may not reflect net effects on the labor market as a whole. In section 5 we describe how treatment spillover to the control group induces a substitution effect bias in all of our estimates. But negative substitution is likely to affect also older workers in the economy—if they are similar to the target group in terms of labor market characteristics. Thus, the larger employment increase for 20–25 year-olds, compared to 25-year-olds, can be the result of increased substitution with older workers. In other words, while we do find an *absolute* employment increase for the target group, this may not reflect a *net* increase in the economy as a whole. The share of the employment increase that is associated with a net creation of jobs corresponds to the relative share of the scale effect (as defined in section 5), which, unfortunately, we cannot quantify. However, it should be noted that if factor inputs are close to perfect substitutes (e.g., low-skilled labor at different ages), there may be large substitution effects even though the scale effect is small.[32] As a

[32]Indeed, in 2013 an employer of low-skilled labor stated explicitly that they only hire workers who are subjected to the lower payroll tax. This prompted the Swedish Trade Union Confederation to sue the employer on behalf of a 35-year-old worker (Svenska Dagbladet 2013). At the time of writing, the case has not yet been settled.

consequence, it is likely that our estimates grossly overestimate the number of new jobs created: partly because the estimates overestimate the actual employment increase (due to control group treatment spillover), partly because the actual employment increase may have been at the expense of older workers in the economy. Correspondingly, the estimated cost per job, reported in the previous section, is bound to underestimate the true cost.

# 9 Conclusion

This paper studies whether large-scale payroll tax reductions for employers of young workers is an effective means to raise youth employment. In 2007–08, payroll taxes in Sweden were cut with 11 percentage points for employers of workers at 19–25 years of age. We estimate the short-run effect of this substantial tax cut to be, at most, an employment increase of around 2.7 percent. We find no employment effect of an extension of the original reductions, implemented in 2009. Shifting of the tax cut onto workers in the form of higher wages cannot explain the modest employment effect: the size of the wage adjustments in the wake of the reform is small, at roughly one percent.

The employment and wage estimates in combination imply that the short-run elasticity of demand for young workers in Sweden is at around $-0.32$. Using a different metric, the estimated cost per created job for 20–25 year-olds is at between three and seven times the cost of directly hiring workers at the average wage.

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