Do payroll tax cuts raise youth employment?*

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Abstract

In 2007, the Swedish employer-paid payroll tax was cut on a large scale for young workers, substantially reducing labor costs for this group. Using Difference-in-Differences paired with exact matching, we estimate a small impact, both on employment and on wages, implying a labor demand elasticity for young workers at around -0.31. Since the tax reduction applied also to existing employments, the cost of the reform was sizable, and the estimated cost per created job is at more than four times that of directly hiring workers at the average wage. Hence, we conclude that payroll tax cuts are an inefficient way to boost employment for young individuals.

Key words: Youth unemployment; Payroll tax; Tax subsidy; Labor costs; Exact matching

JEL classification: H25, H32, J23, J38, J68

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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 of age 25–64 since the beginning of the 1990's. In addition, young peoples' 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 effectively remedy this problem.

This paper examines 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 by including a large number of covariates in the DiD model. In addition, recognizing that the Swedish labor

[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.

market is segmented in geographical regions, we supplement the linear-controls approach with exact matching on local labor markets. We estimate the effects both for the entire target group as well as for different subgroups. 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 (what could be termed *difference-in-discontinuities*).

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 2.7 percent in 2007 and 1.4 percent in 2008. For 25-year-olds, the effect was roughly 1 percent, both in 2007 and in 2008. Furthermore, there is no evidence of any additional effect on employment of the 2009 extended reduction. Since the tax reduction was selective there is a big concern for treatment spillover effects: individuals who were not subjected may have been negatively affected, as they become relatively more costly to hire. As a consequence, the absolute effect on employment is likely to be smaller than what these numbers suggest. In section 5, we discuss these issues at some length.

When it comes to explaining the modest impact, we point at certain observations that help us interpret the results. First, since wages did not adjust, shifting cannot explain the small employment effects. Second, we show that the tax cut had no impact at all for foreign-borns, nor for individuals registred 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. One potential reason may be that, for the group of uneducated, unexperienced young workers, labor costs are still too high—even with the payroll tax reduction in place. We test this hypothesis by focusing on individuals with up to two years of post high school vocational training. For this group, the employment response is considerably larger: the relative increase is now at around 3.8 percent in 2007 and 7 percent in 2008. To some extent, this could be taken as an indication that, in general, labor costs for young workers are still too high in relation to expected productivity.

Our employment and wage estimates in combination imply that the firms' elasticity of demand for young workers in Sweden is at around -0.31. Using a different metric: the estimated gross cost per created job for 19–25 year-olds was SEK 1.0 to 1.6 million ($150,000 to $240,000). This figure corresponds to more than four times the cost of hiring the same number of workers at the average wage. Hence, we draw the conclusion 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] However, since these studies may be argued to suffer from endogeneity problems it is difficult to draw decisive conclusions. 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.

[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.

More convincing evidence is found in studies that evaluate selective payroll tax reforms. 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 a 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 a 1 percent increase of the labor cost leads to a 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. Finally, Huttunen et al. (2013) study a Finnish payroll tax cut 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.

To the best of our knowledge, the only other study that examines payroll tax reductions explicitly aimed at young workers is Skedinger (2013). 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, Skedinger cannot say anything about the effects in the economy as a whole. Further, since we are using much more detailed data, we are able to study treatment effect heterogeneity with respect to immigration status, unemployment status, and education. Importantly, this allows us to make inferences about what mechanisms might explain our results.

3 Institutional setting

3.1 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] Second, besides these regional reductions, payroll taxes were cut for small firms in all of Sweden between 1997 and 2008.[4]

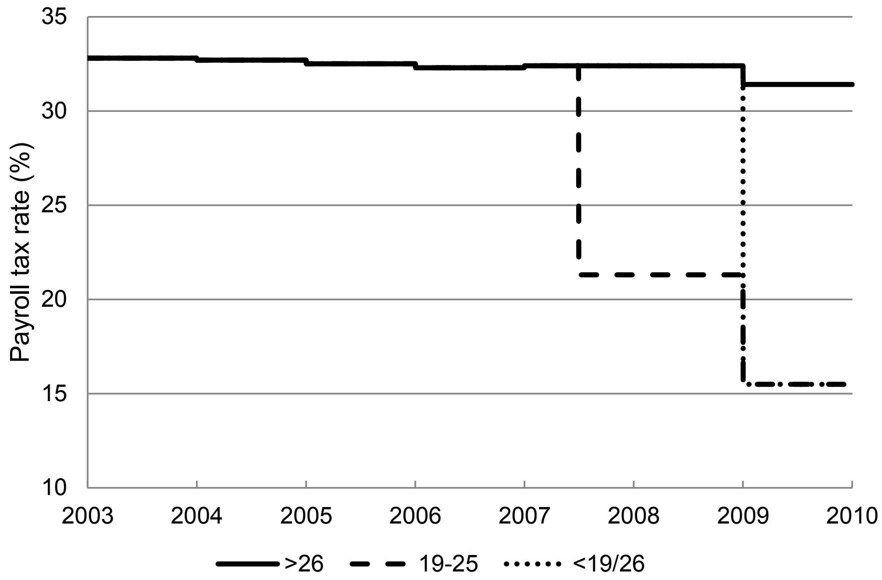
In this paper, we study reductions targeted explicitly at young workers. Figure 1 provides a graphical illustration of the changes in the tax rate. On July 1, 2007, the payroll tax was cut by around 11 percentage points for individuals who at the start of the year had turned 18 but not 25 years of age. 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 reform was

[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 ($115,000) 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



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. Second, the payroll tax reduction was increased, down to 15.52 percent. In 2007 and 2008, the eligible individuals are those born 1982–88 and 1983–89, respectively. For simplicity, hereafter an age group a denotes all individuals who turn a during the year. With 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. 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). Since they applied also to existing employments, the cost of the reform was sizable. The yearly gross cost was SEK 9 billion (around $1.4 billion) in 2007 and SEK 9.9 billion in 2008 (around $1.5 billion), corresponding to about 1 percent of the fiscal budget in these years. This figure increased substantially when the reductions were extended, resulting in gross costs at SEK 17 billion ($2.6 billion) in 2009 and SEK 18 billion ($2.8 billion) in 2010.

3.2 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 19–26, and the difference in shares between the target group as a whole and 26-year-olds—the potential bias of our estimates—is around 0.1 percentage points. We can thus conclude that this is not a source of concern.

Second, 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

[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 5 years.

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.

3.3 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

[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.

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). The employment 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 have access to detailed information on employment characteristics for a subsample of all employees (measured between August and November each year), containing data on actual monthly wages, work rate, industry affiliation of workplace, etc. 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 also 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.

[8]The stratification is based on six firm size classes and 54 industry groups, giving a total of 324 strata. Stratification weights are included with the data and used in all wage analyses.

Our employment measure uses the quarter of these income cutoffs to arrive at a measure of working *at least* 25 percent of full-time.[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 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

[9]In practice, the procedure is slightly more complicated: as cells with less than ten 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 a 20th percentile instead of the 10th percentile when defining full-time employement; although this, by definition, lowers all employment levels, it 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. While the 10 percent work rate gives essentially the same results, the 50 percent work rate produces somewhat smaller treatment effects. Using an outcome measure of full-time employment is not viable, since the difference in cyclicality between age groups then becomes too strong.

[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.

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, conditional on working at least a quarter of full-time. As expected, both (full-time equivalent) montly 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.

Finally, we take a look at the evolution of employment and wages over time. Figure 2 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 19–25 year olds in 2008. Second, within the target group, workers at age 21–24 seem to have gained the most. This suggests that there exists an employment effect, and that the effect is decreasing 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 3, we depict the corresponding distributional change in wages. As seen, there is no clear-cut evidence of larger wage growth for younger workers.

5 Identification

5.1 Modelling the counterfactual outcome

We use two different estimation techniques in parallel, both relying on the Difference-in-Differences (DiD) framework.[11] In its simplest form, DiD uses the evolution of the

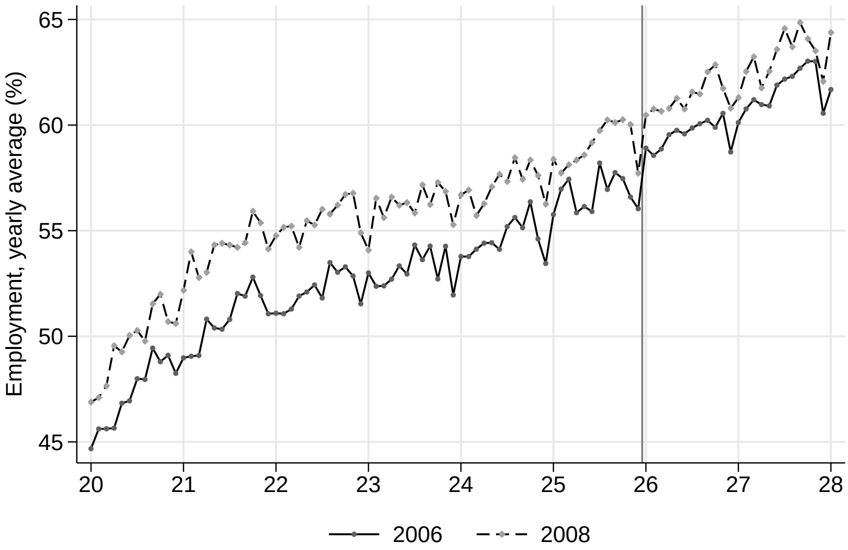
[11] While using a regression discontinuity design on the 25–26 age threshold might, *prima facie*, appear attractive, it is clear from figure 2 that such a strategy is not viable. There are systematic discontinuities at each cohort boundary in 2006, before the tax reduction was implemented. This pattern has its main cause in the fact that it is year of birth that determines when a child starts school in Sweden (see

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: Employed subsample				
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

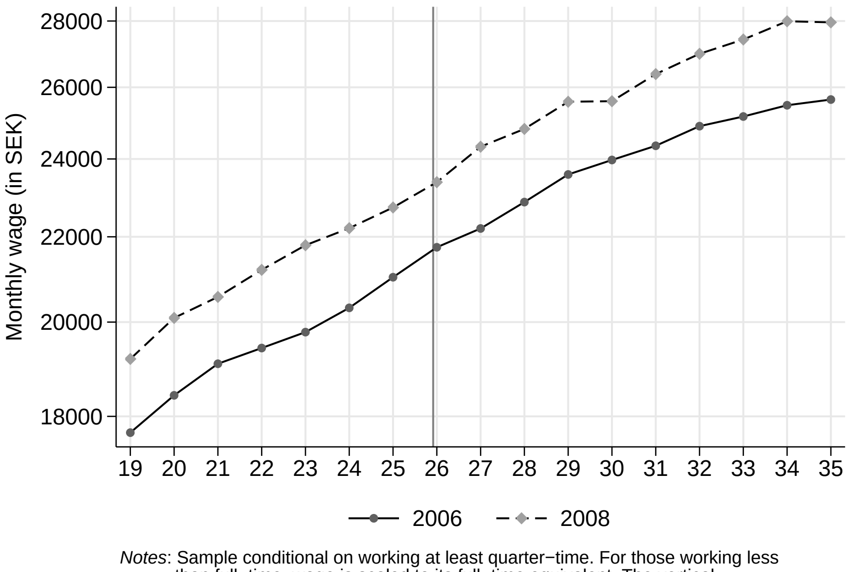
Notes: The employment measure is constructed as described in section 4. For the employed subsample, the sum of stratification weights indicates population size.

Figure 2: 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.

Figure 3: 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.

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

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

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 α. Figure 4 below 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 use two different approaches in tandem. The first method consists of supplementing the basic DiD model with a large number of covariates. The estimated specification is

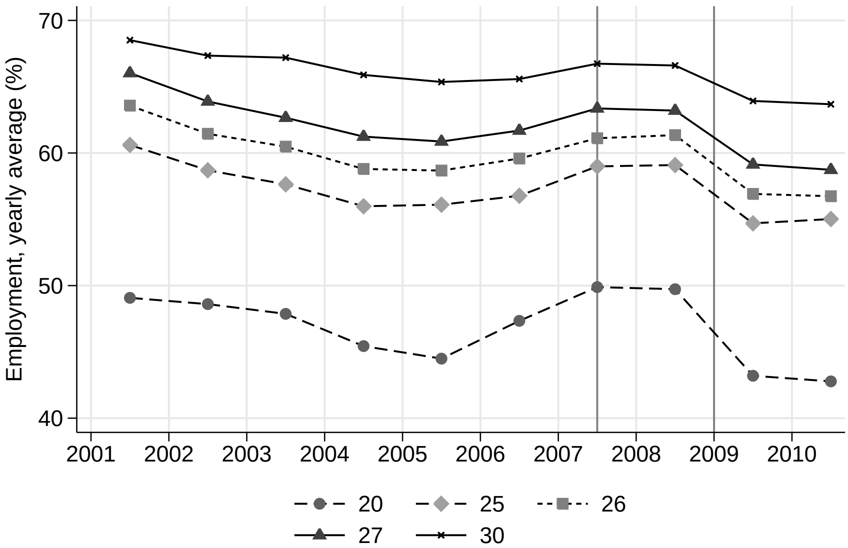
$$y_{i,t} = \delta_t \cdot D(i,t) + \mathbf{x}_{i,t}'\boldsymbol{\beta} + \varepsilon_{i,t}, \tag{2}$$

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, δ_t is the DiD estimate for year t, and $\mathbf{x}_{i,t}$ is a vector of control

Fredriksson and Öckert 2005). With a DiD design, we assume that these cohort discontinuities are constant over time, for each age pair.

[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 4: Employment trends for different age groups



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

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 immigration into Sweden.

The previous technique models the covariates in equation (2) in a linear functional form. That is, we require the identifying assumption

$$E\left[y_{i,t}^{0} \mid \mathbf{X} = \mathbf{x}_{i,t}\right] = \mathbf{x}_{i,t}'\boldsymbol{\beta} \tag{3}$$

to hold for individuals of both the treatment and control groups. However, there are likely to be structural differences between the metropolitan and the rural parts of Sweden due to, e.g., differences in industrial structure and demographics. Since, additionally, the size and composition of cohorts vary over time across Sweden (e.g., through immigration), linear controls may be inadequate. For this reason, we also use DiD with an exact-matching approach. Specifically, treatment-control group pairs are matched with respect to local labor markets, and for each matched pair the (standard) DiD estimator is used to obtain the average treatment effect of the treated (ATT).[13] The identifying assumption required is similar to assumption (3), except that we now condition on local labor markets, L:

$$E\left[y_{i,t}^{0} \mid \mathbf{X} = \mathbf{x}_{i,t}, L = l\right] = \mathbf{x}_{i,t}' \boldsymbol{\beta}_l \tag{4}$$

The overall ATT is obtained by averaging the ATT_l:s for all subgroups $L = l$, weighted by the distribution of L in the treatment group. Formally, for each time period, t,

$$\mathrm{ATT}_t = \sum_{l \in L} \mathrm{ATT}_{l,t} \cdot \frac{\#\{i : \mathrm{Tr} = 1, T = t, L = l\}}{\#\{i : \mathrm{Tr} = 1, T = t\}}$$

where ATT_l is the DiD estimate for the local labor market l. Most of our covariates that are intended to control for important compositional variations over time capture labor-market specific circumstances, such as integration of immigrants in the labor market. The rationale for matching on local labor markets is that the dynamics of these circumstances may differ across Sweden. Since local labor markets cover entire commuting areas, they should be less sensitive to endogeneity concerns than had we used a finer regional division.[14]

To summarize, the difference between our two methods lie in how the counterfactual outcome of the treatment group is modelled, as formalized by equations (3) and (4).

[13] The concept of local labor market is conceived of by Statistics Sweden. Based on workers' commuting patterns, Sweden is divided into 75 broad commuting areas. Thus, commuting mostly takes place within, but not between, separate local labor markets. We have also experimented with matching on whether foreignborn, and on gender, but this gives very similar results. Evidently, our linear controls are sufficient for these dimensions.

[14] Blundell et al. (2004) use propensity score matching to evaluate the employment effects of a mandatory job search program in the U.K. The fact that we match on a single, discrete variable, in combination with a large number of observations, allows us to use exact matching. Compared to propensity score matching, exact matching is less dependent on functional assumptions.

By approaching the identification problem from two slightly different angles we should obtain a more robust overall picture. In particular, if the two methods produce similar estimates, we consider the results credible.

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 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.

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 25-year-olds, and for the whole target group, 19–25 year-olds.

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–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.

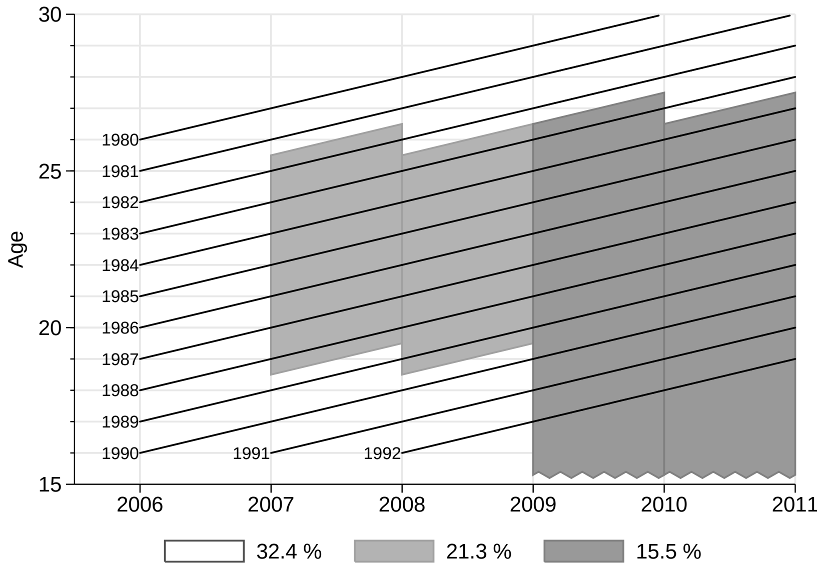
In theory, we should expect stronger treatment effects for younger workers since the remaining available treatment years (treatment dose) is decreasing in age. Estimating effects for individuals close to the cutoff may, for this reason, underestimate the average treatment effect on the treated. Additionally, 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.[15] 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. (When 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. Essentially, the problem of lagged treatment exists whenever employment spells extend from one year to the next. Figur 5 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

[15] 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.

Figure 5: Evolution of treatment status across cohorts



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, and our treatment group (those born 1983) was treated the previous year. One way to handle this is to use 27-year-olds instead of 26-year-olds as control, when possible.

Figure 5 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 25. We are thus restricted to studying the effects of the 2009 extension only for 25-year-olds and 26-year-olds. The former age group transitions from 2007 treatment to 2009 treatment, while the latter 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 apparent from figure 5.

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 25-year-olds, the 2009 estimate will measure the impact of an extended reduction in the wake of the financial crisis. For 26-year-olds, we, correspondingly, get the effect of introducing a payroll tax reduction in the wake of 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.[16]

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 so that the payroll tax reductions 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.

As young workers' wages are predominantly centrally negotiated, there is less concern for regional differences across Sweden and we, therefore, refrain from matching on local labor markets (the matched DiD results are similar to those reported in the paper).

[16]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.

Finally, it is important to stress that we only study the immediate impact on wages. If wage adjustments appear in the longer run, we will underestimate the long-term general equilibrium consequences of the payroll tax cuts.

6 Results

6.1 Main findings

Table 2 presents the main results for the 2007 reduction. The outcome variable is yearly average employment status, ranging from zero to one, and for each treatment-control pair we report the estimates from both DiD methods side by side. 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.[17]

The first four columns study the effect at the treatment cutoff. Starting with the smallest bandwidth, we compare the three oldest birthmonth cohorts (born in January–March) of the 25-year-olds with the three youngest birthmonth cohorts (born in October–December) of the 26-year-olds. Arguably, in these specifications the issue of heterogenous cyclicality should be minor, implying that any relative difference that we observe is most likely caused by the reform. The DiD with linear controls shows a statistically significant, albeit small, positive employment effect, both in 2007 and in 2008. For the matched DiD, point estimates are almost identical but precison is lower.[18] From the local estimation

[17]Another method 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. Naturally, these estimates should be taken with a critical view: as the null hypothesis is that of parallel trends, violations of the identifying assumption may be too difficult to discover. Conversely, we are also likely to have some instances of false positives (specifically, in roughly one regression out of twenty when using the conventional 5-percent significance level).

[18]A potential reason for the lack of significance is that bootstrapping overestimates standard errors when the number of observations are too small. But it is also possible that the OLS cluster-adjusted standard errors are underestimated—note that the latter rely on stronger assumptions than the standard errors obtained by bootstrapping. Be that as it may, the two methods conform (in terms of statistical significance) as the sample size grows.

Table 2: Employment effects of the 2007 reduction, main results

	Local		25 vs. 26		19–25 vs. 26	
	Linear	Matched	Linear	Matched	Linear	Matched
DD 2005	0.001	0.001	0.003	0.002	−0.002	−0.003’
	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
DD 2006	−0.001	0.000	−0.001	−0.001	0.003	0.002
	(0.004)	(0.004)	(0.001)	(0.002)	(0.003)	(0.002)
DD 2007	0.006*	0.006	0.008***	0.008***	0.014***	0.013***
	(0.003)	(0.005)	(0.002)	(0.002)	(0.003)	(0.001)
DD 2008	0.007*	0.007’	0.005*	0.005*	0.007*	0.007***
	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)	(0.002)
R^2	0.09	0.11	0.09	0.10	0.10	0.11
N	419,153	419,153	1,735,836	1,735,836	6,902,252	6,902,252
$\overline{y}_{\text{TG}}$	0.59	0.59	0.59	0.59	0.52	0.52

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

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 (Linear) or obtained by bootstrapping with 250 replications (Matched). ‘Local’ compares 25-year-olds born in Jan-Mar to 26-year-olds born in Oct-Dec.

we conclude that the lower payroll taxes increased the employment rate with roughly 0.6 percentage points in 2007 and 0.7 percentage points in 2008; this corresponds to a rise in employment of around 1 percent in 2007, and 1.3 percent in 2008.[19] When we use a somewhat larger bandwidth, including complete cohorts of 25–26 year-olds, both methods show significant effects. As before, the size of the employment increase is relatively modest, at 1.3 percent in 2007 and 0.8 percent in 2008.

In the last two columns we examine the whole target group. The treatment effect is larger than for 25-year-olds: for 2007, point estimates correspond to a rise in employment of roughly 2.7 percent, while for 2008 the increase is at around 1.4 percent. Note that, since we use the same control group, the substitution effect bias is held constant when going from 25-year-olds to 19–25 year-olds. Consequently, we can deduce that

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

the increase in treatment effect represents an absolute increase. The larger effect for younger individuals is consistent with treatment dose effects: younger individuals have longer expected exposure to the reduced payroll tax. However, this difference 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.

Both for 25-year-olds and for the whole target group the treatment effect is smaller in 2008 than in 2007. As discussed in section 5, if the treatment effect for one year persists to the following year, using 26-year-olds as the control group will bias the 2008 estimate downwards. In table A.1 in the appendix, we address this issue by comparing 25-year-olds to 27-year-olds. While the 2007 estimate is essentially the same, the 2008 estimate is now much larger. In fact, when using 27-year-olds as the control group, the effect *increases* over time. Since we know that the control group has not been previously treated, this result is consistent with the possibility of a lagged treatment effect for the treatment group. It is, however, important to stress that these results may, instead, be due to cohort heterogeneity; changing the control group age is equivalent to changing the control group cohort for each year. By pooling 26–27 year-olds in the control group, we should to some extent net out cohort heterogeneity. Indeed, this gives almost identical employment effects in 2007 and 2008, at roughly 1.3 percent. Unfortunately, in most of our specifications we cannot include individuals older than 26 years old in the control group. Thus, in what follows we use a control group of 26-year-olds. (As an example, the last two columns of table A.1 show that pitting 19–25 year-olds against 26–27 year-olds results in non-parallel pre-treatment trends, and so we cannot test whether the 2008 effect for 19–25 year-olds is downward biased as well.)

In 2009, the payroll tax was further reduced for young workers, the results of which are presented in table 3. Columns 1–2 demonstrate that the extended reductions did not further boost employment for 25-year-olds in 2009–10; the estimates seem to be roughly similar to those of the initial 2007 reform. (As 26-year-olds are part of the target group from 2009 and onwards, we have switched to using 27-year-olds as the frame of reference.) In columns 3–4 we study 26-year-olds, who were subjected to reduced payroll taxes for 2009 onwards, but not before. While there are significant estimates in 2008—possibly due to a lagged treatment effect—there is no evidence of an employment effect of the 2009 extension. However, as we discuss in section 5, there are two reasons for why we should be cautious in interpreting these results: all examined individuals were previously treated, and the 2009 extension coincided with a severe slump in the Swedish economy, and so it is not possible to isolate the effect of the extension from asymmetrical employment effects of the downturn (any symmetrical effect would be handled by the methods applied). We can, however, establish that if the additional reduction had an impact on 25 year-olds, this impact was not large enough to counteract the effects of the economic downturn.

Because of treatment spillovers, the estimates presented above are likely to be upward-biased measures of the absolute employment increase. However, since for any specific year, the reported treatment effect estimate is the sum of the treatment effect for the treatment group and the negative substitution effect for the control group, we can get an idea of the magnitude of this bias by comparing different specifications. In particular, we can use the 25–26 estimates in columns 3–4 of table 2 as an upper bound for the negative substitution effect for the 26-year-olds, and hence, as an upper bound for the substitution effect bias affecting estimations for the entire target group. Consequently, the absolute employment increases for the age group 19–25 are *at least* around 0.6 and 0.2 percentage points in 2007 and 2008, respectively. Leaving control group substitution aside, it is an open question whether the estimated employment increase in the target group occurred at the cost of substitution with even older individuals in the labor market. That is, even when taking the control group substitution bias into account, the remaining employment effect is still likely to overestimate the net employment effect in

Table 3: Employment effects of the 2009 extension

	25 vs. 27		26 vs. 27	
	Linear	Matched	Linear	Matched
DD 2005	0.002	0.002	−0.001	−0.001
	(0.002)	(0.002)	(0.002)	(0.002)
DD 2006	0.001	0.001	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
DD 2007	0.007***	0.007***	−0.001	−0.001
	(0.002)	(0.002)	(0.002)	(0.002)
DD 2008	0.010***	0.010***	0.005***	0.005**
	(0.002)	(0.002)	(0.002)	(0.002)
DD 2009	0.005***	0.005*	0.001	0.000
	(0.001)	(0.002)	(0.001)	(0.002)
DD 2010	0.009***	0.009***	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
R^2	0.10	0.11	0.11	0.11
N	2,214,808	2,214,808	2,224,418	2,224,418
$\overline{y}_{\text{TG}}$	0.57	0.57	0.59	0.59

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

Notes: See notes for table 2.

the economy. This issue is further discussed in section 8.

In summary, there seem to have been positive, but small, employment effects of the 2007 payroll tax reduction. This holds irrespective of whether we study a small interval around the treatment cutoff, or examine the whole target group of 19–25 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 immigrants, in table 4. This group, which constituted about 15 percent of the age group 19–25 in 2007–08, is characterized by weak attachment to the Swedish labor market. The employment rate for this group is 20 percentage points lower than for the whole population of young workers, as reported in the bottom rows of tables 2 and 4. Strikingly, there is no evidence that the payroll tax cut had any impact at all for this group. The lack of treatment effects is not the result of noisy estimates due to a smaller number of observations, but are precisely estimated. The only significant effect is a negative one for the most narrow comparison, where we are more likely to pick up cohort heterogeneity, e.g. due to yearly shocks of immigration. Finally, since the sample of foreign-born is far from homogenous, we have also used finer subdivisions of region of birth. This does not change the outcome: young immigrants seem to have gained nothing from the reduction in payroll taxes.

Table 4: Employment effects for foreign-born

	25 vs. 26		19–25 vs. 26	
	Linear	Matched	Linear	Matched
DD 2005	0.001	0.002	−0.004	−0.004
	(0.004)	(0.005)	(0.002)	(0.004)
DD 2006	−0.002	−0.002	−0.001	−0.002
	(0.003)	(0.005)	(0.003)	(0.003)
DD 2007	0.004	0.004	0.004	0.004
	(0.004)	(0.005)	(0.003)	(0.004)
DD 2008	−0.008*	−0.007	−0.005	−0.003
	(0.004)	(0.005)	(0.004)	(0.004)
R^2	0.18	0.20	0.17	0.18
N	291,125	291,125	990,850	990,850
$\overline{y}_{\mathrm{TG}}$	0.37	0.37	0.32	0.32

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

Notes: Control variables include region of birth, year since immigration into Sweden, among others. See also notes for table 2.

In theory, an explanation for the small general employment effects could be labor supply constraints. For the age group 19–25, many are taking part in higher education, and it is 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 unemployed, 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 columns 1–2 of table 5 shows, there is little indication that unemployed 25-year-olds were helped at all. While these results do not completely rule out the labor supply story, they indicate that labor demand is the more important factor.

Table 5: Employment effects for unemployed and for those with vocational training, 25-year-olds vs. 26-year-olds

	Prev. unemployed		Vocational	
	Linear	Matched	Linear	Matched
DD 2005	−0.000	−0.002	−0.004	0.003
	(0.006)	(0.007)	(0.009)	(0.013)
DD 2006	−0.005	−0.009	−0.003	0.009
	(0.006)	(0.007)	(0.014)	(0.014)
DD 2007	0.004	−0.001	0.024*	0.024’
	(0.008)	(0.007)	(0.010)	(0.014)
DD 2008	−0.003	−0.004	0.044**	0.048***
	(0.009)	(0.007)	(0.014)	(0.012)
R^2	0.03	0.07	0.08	0.13
N	153,931	153,931	37,963	37,963
$\overline{y}_{\text{TG}}$	0.43	0.43	0.66	0.66

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

Notes: Previously unemployed defined as having been registered at the unemployement office at least 100 days during the previous year. Vocational is conditioned on having 0.5–2 years of post high-school education. See also notes for table 2.

If we take the above results as evidence against a supply side explanation, the question arises why the demand elasticity of firms is so low. One potential reason may be that, for the group of uneducated, unexperienced young workers, labor costs are still too high—even with the tax reduction in place. If this reasoning holds, we would expect the effect to be bigger for those individuals who do have relevant education or experience. This hypothesis is tested in columns 3–4 of table 5. Considering 25–26 year-olds with up to two years of post high school vocational training, the employment response is considerably larger than for the same age groups without conditioning: the relative increase is now at around 3.8 percent in 2007 and 7 percent in 2008. While we do expect a stronger employment response for this small subgroup, it is likely that there is also a high degree of substitution bias at play: workers at age 25 and 26, additionally restricted to a well-defined education level, should be close to perfect substitutes. In such an environment, even a small difference in labor cost could result in large substitution effects

We conclude that the small employment response of the payroll tax cut does not seem to be explained by lack of labor supply. It appears more likely that, in general, labor costs for young workers are still too high in relation to expected productivity.

6.3 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 for those employed at least quarter-time (in symmetry with our main employment definition used above). Table 6 gives the impacts of both the 2007 initial cut and the 2009 extension. 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 the target group as a whole 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 for 19–25 year-olds shows up already in 2007.[20]

Next, we compare 25-year-olds not to 26-year-olds, but to 27-year-olds, studying the evolution of wages into the 2009 extension. With this control group, we do find wage

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

Table 6: Wage effects of the 2007 reduction and the 2009 extension

	2007 Reform		2009 Reform	
	25 vs. 26	19–25 vs. 26	25 vs. 27	26 vs. 27
DD 2005	−0.006	0.005’	−0.001	0.005
	(0.004)	(0.003)	(0.006)	(0.004)
DD 2006	−0.005	0.006	−0.006*	−0.001
	(0.004)	(0.004)	(0.003)	(0.004)
DD 2007	0.004	0.012***	0.007	0.003
	(0.003)	(0.003)	(0.004)	(0.004)
DD 2008	0.004	0.013*	0.010**	0.006
	(0.005)	(0.005)	(0.003)	(0.004)
DD 2009			0.007*	0.011**
			(0.003)	(0.004)
DD 2010			0.007’	0.004
			(0.004)	(0.003)
R^2	0.21	0.27	0.27	0.27
N	537,619	1,615,138	701,024	739,208
$\overline{y}_{\mathrm{TG}}$	21,672.71	20,286.88	22,282.25	22,879.42

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

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.

effects in 2008, but also pre-treatment effects in 2006. As with employment, cohort heterogeneity may be the reason for the difference compared to using a control group of 26-year-olds. More importantly, there is no additional wage effect for 25-year-olds of the 2009 extended reduction, nor do wages seem to adjust more in the longer run for 25-year-olds. Finally, in the last column, we examine the effect on relative wages of going from no treatment to 2009 treatment, comparing 26-year-olds to 27-year-olds. The wage effect for the 26-year-olds coincides with their switch of treatment status in 2009, but is not apparent in 2010.

Understanding these wage effects requires making a few observations. To start with, there are indications 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, however, an open question 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. Hence, since both the unions and the employer organizations were aware of the forthcoming tax reduction there is, in principle, a possibility that the wage response came *before* the actual implementation.) What speaks against the minimum wage increase explanation is the evidence of wage effects even for age groups that typically have wages strictly above the minimum wage level.[21] Another potential explanation is that shifting works through individual wage bargaining. Such an impact, if it exists, is likely to be more immediate than union-negotiated wage increases. Having said this, 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-benfit analysis

In the following, we present some further metrics for evaluating the payroll tax reduction, with emphasis on 2007–08 where we have the most credible identification. When calculating demand elasticity and cost per job, we choose to reestimate the models, using pooled treatment effect estimates for 2007–08. Averaging effects over two years has the effect of reducing cohort heterogeneity in the control group, thus producing more robust estimates on which to base our derived measures. 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

[21] 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.

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 the employment and wage estimates to get estimates of the elasticity of demand for young workers with respect to labor costs. For the target group as a whole, the 2007–08 employment increase is 2 percent, and the 2007–08 wage increase is 1.2 percent. Hence, we arrive at a labor demand elasticity at about −0.31. The corresponding figure for 25-year-olds is −0.14.[22] Although these numbers may appear small, previous literature typically finds no employment effects of targeted payroll tax reductions. In particular, 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).

7.2 How much money was spent on each job?

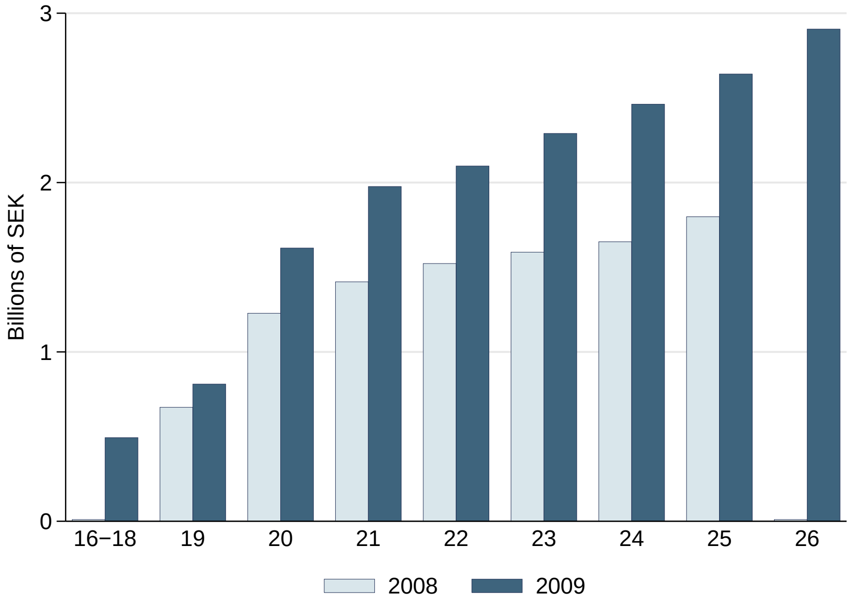
The gross cost of the payroll tax reductions—the sum of foregone payroll taxes, disregarding potentially increased revenues due to, e.g., higher profits—can be straightforwardly calculated since total taxable income is available to us in the tax registers.[23] Figure 6

[22]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

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

[23]Skedinger (2013) provides indications that part of the payroll tax reduction ended up as firm profits.

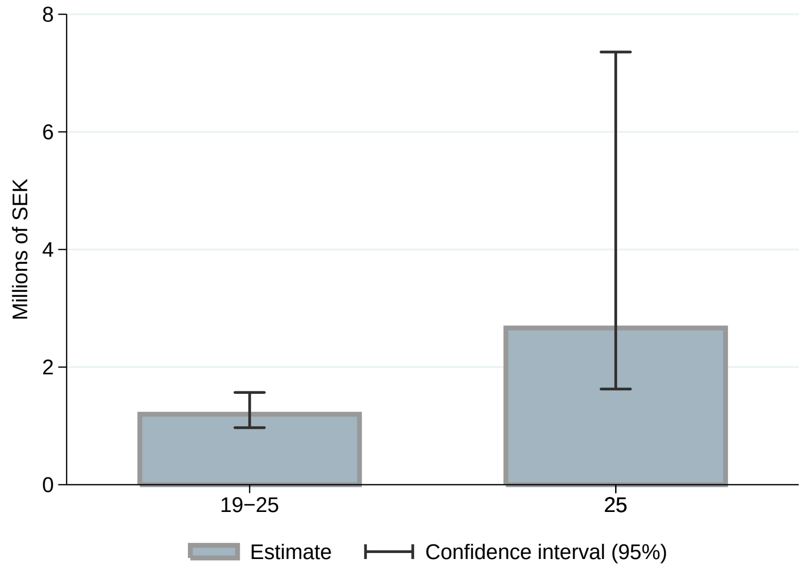
Figure 6: Gross cost per age group, 2008 and 2009



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 ($1.5 billion) in 2008 to 17 billion ($2.6 billion) in 2009. These high numbers reflect the fact that all employments were subsidized, not only new ones.

Using the pooled 2007–08 estimates of the treatment effect, we can deduce the total number of new jobs created each year by the payroll tax reduction. For 25-year-olds, a 95 percent confidence interval gives an estimate of 250 to 1,100 new jobs (with a point estimate of 675), whereas for the the target group as a whole, the number of new jobs amounted to 6,000 to 10,000. In combination with the gross cost, we now get an estimate

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



of the gross cost per created job, depicted in figure 7. For the entire target group, the gross cost for each job is SEK 1.0 to 1.6 million ($150,000 to $240,000), with a point estimate at SEK 1.2 million ($180,000). Notably, this is more than four times the hiring cost, assuming that the created jobs had the average annual income for this group.[24] 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 cost amounts to between SEK 1.6 and 7.4 million ($250,000 to $1,100,000), with a point estimate at SEK 2.7 million ($410,000)—more than eight times the average hiring cost for 25-year-olds working at least quarter-time.

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

[24] 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). We have chosen not to consider payroll taxes as government revenues, as they are, mostly, linked to insurances and social benefits for the employed. However, the figures change only marginally if we instead subtract increased payroll tax revenues from the cost figures (taking into account both new jobs and the wage increase associated with the tax reduction).

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 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 begs 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 explanations. 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 lower taxes were implemented, 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 explanation 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 (2013) shows that the effects were small 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 explanation is corroborated by the fact that for previously unemployed, where labor costs should correspond even less to productivity, we find no effects at all. Conversely, when restricting the sample to individuals with vocational training—for whom there is known to be a supply shortage—we find notably larger effects on employment. This suggests that targeted payroll tax reductions can be effective only if they are very selective.

It is important to stress that the figures reported in this study may not reflect net effects on the labor market as a whole. In section 5 we describe how control group substitution 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 19–25 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.[25] As a consequence, it

[25]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). The case has not yet been

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. Similarly, 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 at 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 elasticity of demand for young workers in Sweden is at around -0.31. Using a different metric, the estimated cost per created job for 19–25 year-olds was SEK 1.0 to 1.6 million ($150,000 to $240,000). Since we are likely to overestimate the employment increase, these figures are likely to be overly optimistic. We conclude that targeted payroll tax cuts are an expensive way to boost employment for young individuals.

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settled.

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A Additional results

Table A.1: Alternative control group specifications

	25 vs. 27		25 vs. 26–27		19–25 vs. 26–27	
	Linear	Matched	Linear	Matched	Linear	Matched
DD 2005	0.002	0.002	0.002	0.002	−0.003*	−0.003**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
DD 2006	0.001	0.001	−0.000	−0.000	0.004	0.003**
	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)
DD 2007	0.007***	0.007***	0.007***	0.007***	0.013***	0.013***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)
DD 2008	0.010***	0.010***	0.008***	0.007***	0.010**	0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)
R^2	0.10	0.11	0.10	0.10	0.11	0.12
N	1,753,297	1,753,297	2,628,227	2,628,227	7,794,643	7,794,643
$\overline{y}_{\mathrm{TG}}$	0.59	0.59	0.59	0.59	0.52	0.52

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

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 (Linear) or obtained by bootstrapping with 250 replications (Matched).