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Abstract

In 2007, the Swedish employer-paid payroll tax was reduced substantially for young workers. We estimate the labor market response for different ages, and at different phases of the business cycle. The overall impact on employment and wages is relatively small, implying an average labor demand elasticity for young workers at around -0.32. While the effect on wages is consistently small, the employment effect differs markedly across ages and over the business cycle. For 21–22 year-old workers, the employment increase is 4–5 times larger than for 25-year-olds, and the estimated demand elasticities are strongly procyclical, approaching zero in the deep 2009 recession. These results suggest that payroll tax reductions need to be narrowly targeted, and carefully timed, in order to be effective. In addition, we find that the payroll tax reduction had no effects on hours worked. There is also little evidence that the employment effect for an individual remained when she was no longer eligible for the tax reduction.

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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 (Gregg 2001; Gregg and Tominey 2005; Nordström Skans 2004), an increasing number of young people risk ending up in long-term unemployment. This has spurred a wide and lively debate on what policies should be undertaken to improve young individuals' labor market prospects. While there are many suggested solutions, there is surprisingly little evidence on which policies actually work.[1]

In this article, we examine whether targeted payroll tax reductions are an effective means to raise youth employment. In 2007–09, the Swedish employer-paid payroll tax was substantially reduced in two steps. The first reduction, in effect 2007–08, lowered the tax rate by 11 percentage points for employers of workers 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 workers who at the start of the year had not yet turned 26 years of age. In addition, the rate was reduced by an additional 6 percentage points for the eligible individuals. The two reductions resulted in considerable variation in payroll tax rates across cohorts. It thus offers a clean testing ground for investigating the causal effects of labor cost reductions on employment and wages.

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 subject to the reductions. Identification is complicated by the fact that individuals

[1] An exception is Blundell et al. (2004), who study the causal effect of a labor market program aimed at young workers in the U.K. While they find large short-run effects on transitions into employment, they find only weak evidence of any long-run effects.

of different ages tend to experience different employment cyclicality, with younger workers displaying larger cyclical variations. We deal with this problem—which essentially constitutes a threat to the identification assumption of parallel counterfactual trends—in two ways. First, we include a large number of covariates in the DiD model in order to control for demographical heterogeneity, thereby netting out the differences in cyclicality across cohorts. Second, we consider treatment-control pairs defined at a very small bandwidth around the treatment-defining age threshold, thus minimizing the age difference between treatment and control groups.

Our study provides several new results that may help to inform policy. We show that the 2007 original reduction significantly raised relative employment for the target group in the first two years. The employment increase for 20–25 year-olds corresponds to 1.8–2.5 percent. We also show that the magnitude of the treatment effect differs substantially across subgroups of workers. First, the effect is higly age dependent: for individuals close to the treatment-defining cutoff the effect was around 1.3 percent, whereas for younger individuals of the target group the effect was as large as four percent. This pattern is consistent with the fact that age is negatively correlated with the number of remaining years of treatment (i.e., treatment dose is decreasing in age). Second, for individuals with a recent record of unemployment, and for foreign-borns, we find no evidence of any effect on employment, indicating that groups with weak attachment to the labor market did not benefit from the lower labor costs. A final finding concerns the important but often overlooked question of whether payroll tax reductions have lasting consequences for an individual. By following the first wave of treated individuals into years when they are no longer eligible, we do find evidence of a one-year treatment persistence, at roughly the same size as the direct effect the year before. That the effect is short-lived may be related to the onset of the great 2008 recession, which had its largest impact in the year 2009. Consequently, we consider the finding of a one-year persistence a lower bound.

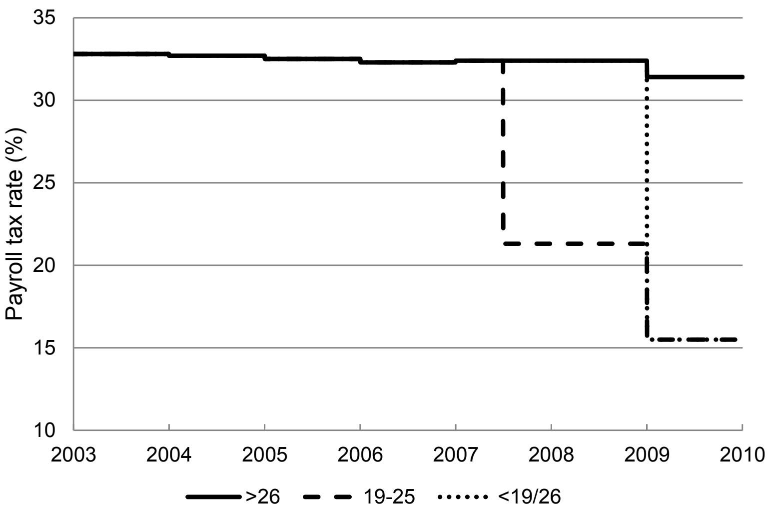
The 2009 extended reduction is evaluated for the years 2009–12. Hence, since we use a time window of six years in total, we are able to draw conclusions about the employment effects of labor cost reductions in the longer run. This makes our study stand out in comparison to the existing evidence, where the studied time periods are usually 1–2 years (Bennmarker et al. 2009; Huttunen et al. 2013; Korkeamäki and Uusitalo 2009; Cahuc et al. 2014; Blundell et al. 2004). In addition, by using a longer time period we cover both years that saw economic expansion and years characterized by recession. This allows us to study whether firms respond differently to the tax cut depending on the state of the economy. For 2009, when the economy hit rock bottom in the recession, we find smaller effects than for the previous years, despite the fact that the reductions were now increased in magnitude (we cannot rule out a null effect). This finding suggests that targeted payroll tax reductions are less potent in strong recessions. For the years 2010–12, the effect sizes increase in proportion to the increase in the tax reduction; in fact, the implied demand elasticities (discussed below) are more or less identical 2007–08 and 2011–12. Thus, normalizing with respect to the magnitude of the cost reduction, the employment effect in the longer run is similar to the effect in the short run.

In addition to studying the effects on employment, we consider two additional margins. First, we show that hours worked remained unaffected over the whole period, both when considering all workers and when restricting the sample to employees with tenure. Second, we show that the wage adjustment was limited: young workers' wages rose by roughly one percent relative to older workers, both in the short run and over a period of six years. We thus conclude that shifting of the tax reduction onto higher wages cannot alone explain the modest employment effects. The finding of only modest wage effects stands in contrast to evidence for the U.S., where the basic result is that of extensive shifting of the incidence of the tax onto workers (see, e.g., Gruber 1994; Anderson and Meyer 1997, 2000; Murphy 2007).

Combining employment and wage estimates, we deduce labor demand elasticities with respect to labor costs (under the assumption of infinite labor supply elasticity). If we were to choose one single figure reflecting the firms' elasticity of demand for young workers in Sweden, −0.32 would be the best estimate. However, this estimate masks the fact that elasticities are highly heterogenous across ages, ranging from −0.11 for the oldest workers to −0.52 for 22-year-olds. Furthermore, the estimated demand elasticities are pro-cyclical in magnitude, even approaching zero in the deep recession. These heterogenities suggest that when designing policies for increasing employment, one need to be careful with the precise delimitation of the target group, as well as timing with respect to the business cycle.

Finally, combining the "cost" of foregone payroll tax revenues due to the tax reduction with the increased tax revenue from the estimated employment and wage increases, we obtain a measure of the gross cost per created job. For 20–25 year-olds, this figure is at around SEK 1.2 million (ca. $140,000). As a comparison, this amounts to more than four times the annual labor cost for a worker at the average wage for this age group. This suggests that targeted payroll tax reductions are not a cost-effective way to boost employment for young individuals.

The rest of the paper is organized as follows. Section 2 describes the Swedish payroll tax reform in detail, and also presents the institutional backdrop in which the reform took place. Section 3 describes our data, details the construction of our employment measure, and gives summary statistics for young workers in Sweden. In section 4, we present our identification strategy and discuss some potential identification pitfalls. The results are presented in section 5, and further analyzed in section 6. Section 7 discusses possible interpretations of our results.



2 Institutional background

2.1 Swedish payroll tax reductions

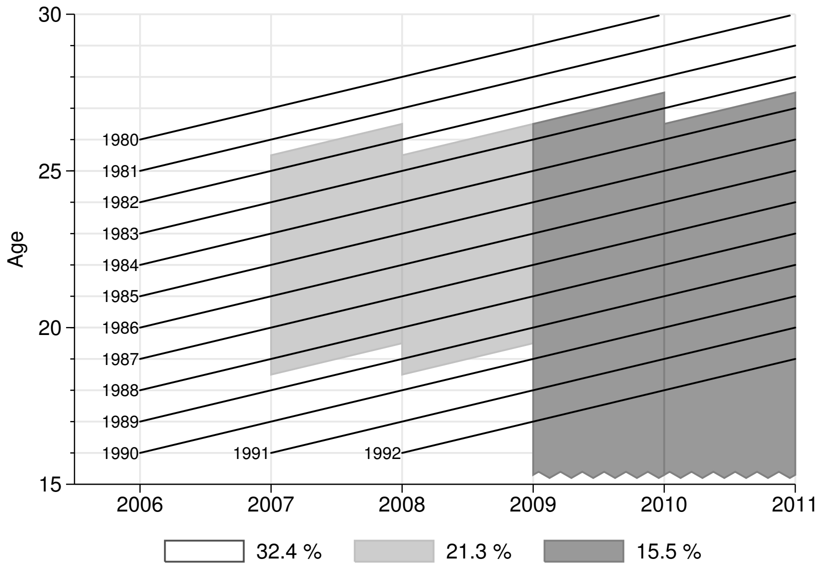
Swedish payroll taxes are proportional to the employee's gross wage and, in contrast to e.g. the U.S., fully levied on the employer. The tax consists of seven mandatory fees, financing social insurance programs such as pensions, health and disability insurances, and other social benefits. Up until the 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 in the northern parts of Sweden were twice subjected to reductions of roughly 10 percentage points in efforts to boost employment in these areas.[2] Second, payroll taxes were cut for small firms in all of Sweden between 1997 and 2008.[3]

In this paper, we study reductions geared towards young workers. Figure 1 provides

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

[3]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 scope. To the best of our knowledge, this reduction has not been evaluated.

Figure 2: Evolution of treatment status across cohorts



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 fees were halved, reducing the total payroll tax rate from 32.42 to 21.32 percent.[4] On January 1, 2009, the 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. Second, 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, implying that there was no effort involved from the employers to benefit from the lower tax rates. Figure 2

[4]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 March 15, 2007. The only fee that was left unchanged was the pension fee. Individuals who are self-employed instead pay self-employed contributions (*egenavgifter*), roughly equivalent to payroll taxes paid by employers. These fees were also cut by about 10 percentage points, in order to avoid distortionary effects with respect to choice of occupation. In addition to 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.

illustrates how different cohorts were subjected to the payroll tax reductions. In 2007, the target group consists of individuals born 1982–88, whereas in 2008 it consists of those born 1983–89. For simplicity, we hereafter let age group a denote 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 ten percent of the labor force aged 15–64 in 2007. Thus, the number of people directly affected by the new regime was substantial. Since the tax reduction applied also to existing employments, the cost of the reform was sizable. Using the tax register data, taking employment rate as constant, we obtain a rough estimate of the yearly foregone tax revenues: 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.

2.2 Other relevant labor market reforms

With the purpose of increasing employment, both overall and for specific groups, several labor market reforms were introduced in Sweden during 2007. First, on January 1, 2007, temporary subsidies for firms that hired individuals having been unemployed or having received sickness or disability benefits—*New Start Jobs* (NSJ)—were introduced. 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 individual's 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.[5] Consequently, in

[5]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

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.

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 misestimating the effect of the payroll tax reductions. Edmark et al. (2012) discuss the difficulty in evaluating this deduction scheme due to the lack of unaffected comparison groups. In this paper, we assume that the response was similar for individuals close in age.

Finally, a third reform concerns employment protection legislation. In 2007, a change in regulation relaxed employer restrictions for using fixed-term contracts. As temporary work is 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.

2.3 Wage formation in Sweden

Wage setting in Sweden has traditionally been characterized by a high degree of centralized bargaining. Over the last 10–15 years, there has been a substantial move toward local negotiations, but many workers still have centrally agreed wages and this is likely

non-employment spell, but restricted to at most 1 year for those aged 20–24 and at most 5 years for those aged 25 or more.

to be more common for young workers.[6] 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). Additional agreements were made in 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 mainly based on age, experience and levels of skill. This means that younger workers are more likely to have wages bound by the minimum wage level.

3 Data

The main data set is collected by Statistics Sweden (SCB) and contains yearly information on employment and demographic characteristics for all individuals living in Sweden at or above 16 years of age, for 2001–12 (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 employment characteristics for a subsample of all employees (measured between July and November each year), including data on actual monthly wages, work intensity (fraction of full-time) and industry affiliation of workplace. For public sector employers, the total population is surveyed through official registers, while firms in the private sector are

[6]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.

sampled using a stratification scheme.[7] The SES subsample, in addition to being used in the analyses of wages and hours worked, is combined with the income data from the tax registers to create monthly measures of employment for all individuals.

3.1 Construction of the employment measure

Our employment measure is constructed in the following way. Starting out from the reduced sample of employed workers (the SES data set), 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 the actual, full-time equivalent wage distribution; these values are to be used as cutoff values, serving as income criteria for full-time employment. The cutoff values thus obtained are now matched to the tax register data, available for all individuals (the RAMS data set). For each month that an individual's taxable income exceeds the appropriate cutoff value, she is, then, classified as being full-time employed. Our preferred 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.[8]

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

[7] 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.

[8] 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 by means of 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 intensity conditions for the outcome variable, such as 10 or 50 percent of full-time employment. Again, the results are not much affected (see section 5). When calculating the income cutoff, we discard employees working less than a quarter of full-time, since these individuals may not be representative for individuals working 25–100 percent of full-time.

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 2007, but a 4-month average for January–April 2008. For these reasons, we use an annual measure of employment, taking the average of monthly employment status for each year.[9] 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 with all control variables, would result in a bias in our DiD estimates.

3.2 Summary statistics

Table 1 shows pre-reform summary statistics divided by age, both for the full population (panel A) and for the SES subsample (panel B). The table highlights some of the large differences in background characteristics across ages. For example, panel A shows that only 8.7 percent of the 20-year-olds have some form of education above high school, whereas for 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, demographic 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 that we use when studying the effect on wages and hours worked. As expected, both (full-time equivalent) monthly wage and the work intensity (fraction of full-time) 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,

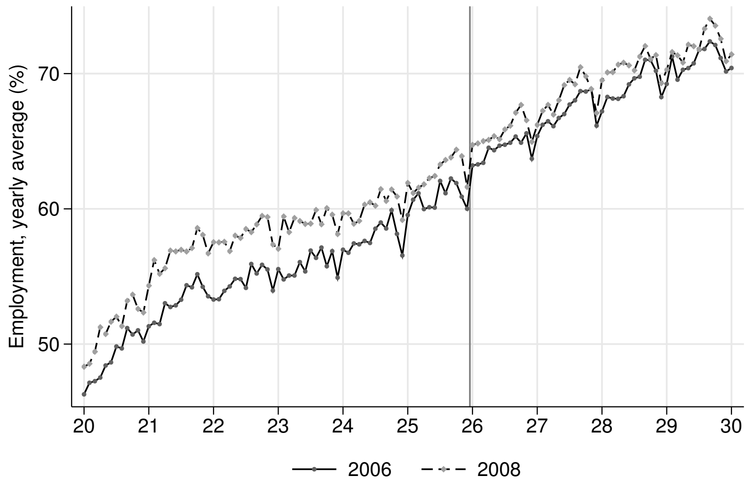
[9] Our measure differs from the official ILO definition of employment, according to which an individual is considered to be employed if working at least one hour per week (ILO 1983). For our purpose, this is too lax a restriction; we are interested in employments that actually have an economic impact 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.

Table 1: Summary statistics, year 2006 (percentages)

	Age cohort, 2006				
	20	23	25	27	30
	Panel A: Full sample				
Employed, $\geq$ quarter-time	49.1	55.9	60.9	67.2	72.0
Employed, full-time	16.3	26.3	33.4	41.6	45.2
Income (year total SEK)	78,218	107,269	130,019	159,531	186,659
Unemployed prev. year	7.6	11.7	9.9	11.9	9.9
Educ. below high school	14.8	12.8	12.0	13.3	8.4
Educ. high school	76.6	53.1	46.0	42.1	45.9
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 (monthly SEK)	17,676	19,428	20,321	21,744	23,587
Work intensity (% of full-time)	84.8	89.2	91.3	94.2	94.2
Hours worked (monthly)	115.0	125.9	130.6	137.5	138.4
Tenured	54.1	68.8	69.7	73.0	80.0
Public sector	17.0	17.3	21.3	24.7	26.6
Employed, yearly average	80.6	87.5	88.8	92.7	94.4
Unemployed prev. year	6.9	9.5	7.0	9.1	6.6
Educ. below high school	7.2	9.6	9.6	10.4	5.3
Educ. high school	89.3	69.0	54.1	44.6	48.9
Educ. above high school	3.5	21.4	36.3	45.0	45.7
Female	49.7	42.7	46.2	45.2	43.6
Foreign-born	6.3	9.9	11.7	10.2	11.1
Sum of strat. weights	42,815	46,946	55,155	64,542	70,803
N	21,424	24,569	30,968	38,092	43,221

Notes: 'Unemployed prev. year' refers to individuals who were registered as unemployed at the Unemployment Office at least 100 days during the previous year. 'Tenured' are those employed with the same employer as previous year. The Structure of Earnings Survey sample is conditioned on working at least quarter-time. Note that the sum of stratification weights indicates population size.

Figure 3: Employment rates by age, 2006 and 2008 (full sample)

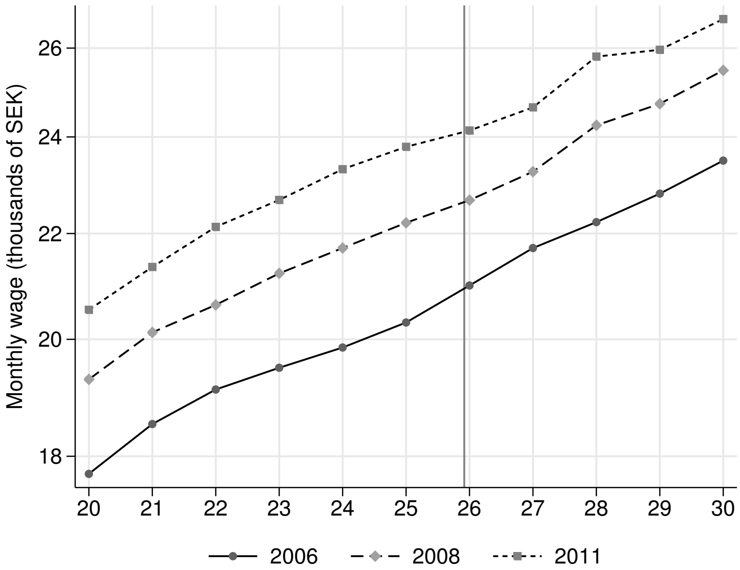


Notes: Employment is defined as working at least quarter–time. The vertical line indicates the age cutoff for the 2007 reform.

e.g., low-educated, women and foreign-born have lower employment rates than other groups. Table A.1 in the Appendix presents the corresponding figures (panel A) for foreign borns and those registered as unemployed the previous year. The employment levels are dramatically lower for both groups, and particularly so for foreign borns. The share in the lowest education category (below high school) is markedly higher for these groups, in comparison to the numbers for the full population.

Finally, we examine the evolution of employment and wages over time. Figure 3 gives the employment rate for different ages, 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 21–23 seem to have gained the most. (A similar picture emerges if we instead consider the age distribution of employments.) This indicates that the 2007 payroll tax reduction did have an impact on employment, and that this impact varies within the target group. 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

Figure 4: Average wage by age, 2006,2008 and 2011, conditional on at least 25% of full-time employment (log scale, SES subsample)



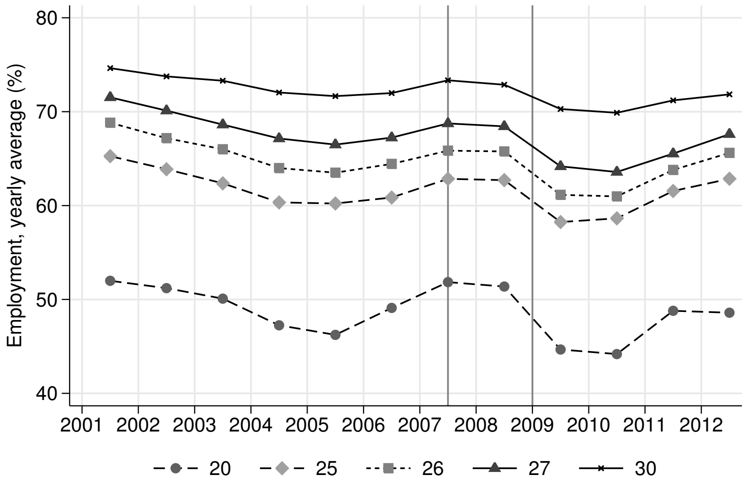
in 2006–08. This concern is further discussed in the next section. Figure 4 plots average wage by age, for 2006 and 2008. In contrast to employment, there is no apparent evidence of larger wage growth for younger workers, neither in the short run nor four years after the first tax reduction.

4 Identification

4.1 Modelling the counterfactual outcome

We rely on the Difference-in-Differences (DiD) framework to estimate the effects of the payroll tax reductions. The fact that treatment is determined by age would, at first glance, appear to make a regression discontinuity analysis on the 25–26 age threshold an ideal candidate for identification. However, inspection of figure 3 (page 14) makes clear why this method is not valid: already in the year prior to the tax reduction, there are employment rate discontinuities between each cohort, corresponding to whether the

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.

individual was born before or after the new year. Since treatment status is determined by year of birth (rather than exact date), treatment status will exactly coincide with an already existing birth year discontinuity, thus rendering a regression discontinuity design invalid.[10] Furthermore, a regression discontinuity design, even if it were possible to implement, would only estimate the effect locally around the treatment cutoff. Hence, using such a strategy, we would not be able to identify heterogeneous treatment effects across ages.

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. 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.[11] As 2007 coincided

[10]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.

[11]This heterogeneity is caused by, among other things, differences in labor market attachment, educa-

with an economic expansion, comparing, say, 20- to 26-year-olds would result in an upward-biased reform estimate: even in absence of the 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 and migration patterns within Sweden over time.

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

$$y_{i,t} = \delta_t \cdot D(i,t) + \mathbf{x}'_{i,t}\boldsymbol{\beta} + \varepsilon_{i,t}, \tag{1}$$

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 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 an indicator for being foreign born), gender, geography, and whether the parents immigrated into Sweden. For the foreign born, we also control for country of birth and years since immigration into Sweden.[12]

4.2 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

tional attainment and social situation. See Hoynes et al. (2012) for an extensive treatment of employment cyclicality in the U.S. labor market.

[12]This specification assumes that the impact of demographic factors are homogeneous across different parts of Sweden. We have tested this assumption by combining DiD with exact matching on local labor markets. The results of this exercise, which can be obtained from the authors on request, are very similar to the figures reported in this study.

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. By 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.

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

$$\Delta y_a = \tau_a + \xi_a. \tag{2}$$

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 2, we can now distinguish between a couple of different concepts. The causal effect on the treatment group, ξ_{TG}, we denote the *absolute effect* of the reform. However, the DiD estimator will be biased by the treatment spillover effect, ξ_{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 account 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.[13] 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

[13] 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.

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.

4.3 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 that the majority of 19-year-olds are in their final year of high school for the first half of the year.[14]

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

[14]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 each specific age, any employment differences across cohorts over time can be captured by our control variables. This is a reasonable assumption, since, given age, cohort differences are mainly due to demographic factors, on which we have information. For the pre-treatment years, this is born out in the data and manifested as parallel pre-treatment trends. The corresponding assumption for comparing cohorts over time is that employment differences between ages can be captured by control variables. This is less plausible, 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. Consistent with this line of reasoning, we find that Difference-in-Differences regressions on fixed cohorts tend to display non-parallel pre-treatment trends.

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 showed in the previous subsection, the relative treatment effect bias has the opposite sign to that of the treatment spillover effect. Thus, since 26-year-olds are expected to be adversely affected by the tax reduction, all estimations should be interpreted as upper bounds for the *absolute* treatment effects. 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 the 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 number of remaining treatment years (the treatment dose)—and thus the potential benefit to employers—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

[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.

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.)

4.4 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. If the tax reduction had a persistent effect on individuals, this would, in the above framework, manifest as a positive treatment spillover for the later years in the sample, resulting in a downward bias. We refer back to figure 2 in section 2 (page 6) for an illustration of how different cohorts are subjected to the payroll tax reductions. For example, comparing 25-year-olds to 26 year-olds implies that our control group for 2008 (those born in 1982) was in the target group the year before. As a consequence, we regard the employment estimate for 2007 as being the best identified since there is no earlier intervention, for any age group. One way to handle this concern is to use 27-year-olds instead of 26-year-olds as the control group, when possible.

Figure 2 also shows why it is more difficult to evaluate the 2009 extension. As 26-year-olds are treated from 2009 onwards, we switch to using 27-year-olds as the control group when evaluating the years 2009–12. However, it turns out that 27-year-olds are not comparable—in terms of parallel pre-treatment trends—to age groups below 24 and, thus, for these years we focus on 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. 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 payroll tax reductions fare when labor market conditions worsen.

4.5 Estimating effects on the intensive margin and wages

The impact on employment rate is closely related to what takes place at two other margins: to what extent does the employer increase hours for those already employed (the intensive margin), and how much of the tax cut is shifted onto workers in the form of higher wages? The reform effects on these margins are estimated using the SES subsample of employed workers, using the same main comparison groups as for the employment rate. While lower labor costs should put an upward pressure on hours worked for the target group, this is counteracted by employment regulations that set restrictions on hours. As a result, it is, *ex ante,* an open question whether there is any reform effect on the intensive margin.

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 (Gruber 1997). 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, and to what extent, shifting will occur.

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 δ_t in equation 1 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.

5 Results

5.1 Main findings

Table 2 presents the main results for the 2007 and 2009 reductions. Columns 1–3 report the results for the original 2007 reduction and columns 4–5 show the results for the extended 2009 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.[16]

The first column studies the effect of the 2007 reduction locally around the treatment cutoff, comparing the three oldest birthmonth cohorts (born in January–March) of the 25-year-olds to the three youngest birthmonth cohorts (born in October–December) of the 26-year-olds. We find a statistically significant 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 by roughly 0.8 percentage points, corresponding to a rise in employment of around 1.3 percent. (The percentage increase is relative to the counterfactual outcome and is thus obtained as $\beta/(\overline{y}_{\mathrm{TG}} - \beta)$.) Column 2 looks at the effect of the 2007 reduction for 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 the fact that younger individuals have longer expected exposure to the reduced payroll tax (discussed

[16] Another method to examine the credibility of the DiD assumption, 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.

Table 2: Employment effects of the 2007 and 2009 reductions, main results

	Local (25/26)	20–25 vs. 26	24–25 vs. 26	24–25 vs. 27	Local (26/27)
DD 2005	0.000	−0.002	0.001	−0.003	0.001
	(0.003)	(0.001)	(0.003)	(0.002)	(0.004)
DD 2006	0.000	0.003	−0.000	0.000	0.003
	(0.003)	(0.002)	(0.001)	(0.002)	(0.003)
DD 2007	0.008**	0.014***	0.008**	0.005***	−0.002
	(0.003)	(0.003)	(0.003)	(0.002)	(0.005)
DD 2008	0.008*	0.010***	0.006***	0.008***	0.008**
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
DD 2009				0.002	0.000
				(0.002)	(0.004)
DD 2010				0.009***	0.008*
				(0.002)	(0.003)
DD 2011				0.014***	0.011***
				(0.001)	(0.003)
DD 2012				0.008***	0.005’
				(0.002)	(0.002)
R^2	0.12	0.10	0.11	0.12	0.14
N	419,157	6,015,936	2,588,762	4,038,104	654,222
$\overline{y}_{\text{TG}}$	0.63	0.58	0.61	0.60	0.64

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

Notes: Outcome is average employment status during the year, $\overline{y}_{TG}$ 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 lines indicate reform implementations.

more below). As in column 1, the insignificant pre-treatment estimates support a causal interpretation of the employment increase.

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 six percentage points. As 26-year-olds are part of the target group from 2009 and onwards, we switch to using 27-year-olds as the control group when studying the effects beyond 2008.[17] (For 24–25 year-olds, the result of switching control group can be evaluated by comparing columns 3 and 4. For neither year are the estimates significantly different depending on control group, as verified by estimating the differences in a pooled regression.) The economic expansion in 2007–08 was followed by the great 2009 recession, then succeeded by a recovery phase in 2011–12 (see figure 5, page 16). These fluctuations in the economy allow us to explore whether the effect of the tax reductions depends on the state of the economy.

Column 4 follows 24–25 year-olds—the largest age group of the 2007 treatment group that we can study when comparing to 27-year-olds—who transition from 2007 treatment to 2009 treatment. For 2007–08, this age group displays similar treatment effect magnitudes as the 25-year-olds in column 1. However, moving to the deep recession year 2009, the treatment effect collapses to zero. The null effect for the year 2009 is confirmed in column 5 where we estimate the effect locally around the new cutoff defined by the 2009 extension. In the years 2010–12, the treatment effect is statistically significantly positive again, with on average higher magnitudes than for 2007–08. In section 6, we show that this change in magnitudes corresponds to the increase in tax reduction from

[17] The strategy of alternating the control group can, in theory, be used to get an idea of the magnitude of the substitution 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 4) should decrease with an older control group. Although changing the control group from 26-year-olds to 27-year-olds is in fact associated with lower treatment effect for 2007, the difference is small and not statistically significant. As this may only mean that the substitution effect does not decrease fast enough in age so as to show up between two adjoining age groups, we would have wanted to use even older age groups here. Unfortunately, because of non-parallel pre-treatment trends, this is not possible, and, thus, we cannot make inferences about substitution effects.

2009 onwards. Notably, in 2011, when the economy recovered substantially, the effect seems to be much larger than for the surrounding years, corresponding to a 2.3 percent employment increase. Overall, these results suggest that the effect of a payroll tax reduction is procyclical.[18]

Finally, we note that in column 5 we have pre-treatment effects in 2008 (26-year-olds were subjected to reduced payroll taxes for the first time in 2009). However, since those at 26 years of age in 2008 were treated the year before, the positive 2008 estimate may rather indicate that the treatment effect in 2007 had a persistent effect. We return to the question of persistence in subsection 5.3.

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 any particular month). We have also tested 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 significantly 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. Finally, Appendix table A.2 shows the result of gradually increasing the treatment group, from 25-year-olds to 20–25 year-olds.[19]

5.2 Treatment effect heterogeneity

There are strong *a priori* reasons to expect that the treatment effect varies depending on age. First, since the payroll tax reamins reduced up until the year the individual turns 25,

[18]This stands in stark contrast to the results by Neumark and Grijalva (2015), who study the effect of state hiring credits implemented in the U.S. during 1969–2012. They find suggestive evidence that refundable credits and credits targeting the unemployed were more effective at promoting job growth during recessions.

[19]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 too blunt to study the full age group 20–25, for the age groups 25 and locally around the age cutoff, this measure 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 on request.

Table 3: Employment effects of the 2007 reduction: Age heterogeneity

	21 vs. 26	22 vs. 26	23 vs. 26	24 vs. 26	25 vs. 26
DD 2005	−0.003	0.004	−0.001	0.000	0.002
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
DD 2006	0.004	0.009**	0.005	0.002	−0.002’
	(0.004)	(0.003)	(0.004)	(0.002)	(0.001)
DD 2007	0.020***	0.023***	0.015***	0.010**	0.006**
	(0.004)	(0.004)	(0.002)	(0.004)	(0.002)
DD 2008	0.014***	0.021***	0.016***	0.009***	0.004*
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)
R^2	0.11	0.11	0.11	0.11	0.11
N	1,731,273	1,727,814	1,725,068	1,727,851	1,735,846
$\overline{y}_{\text{TG}}$	0.57	0.58	0.59	0.60	0.63
$\mathbb{E}[l_{\text{subs.}}]$	1.30	1.22	1.11	0.91	0.49

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

Notes: See notes for table 2.

the younger workers have longer remaining time with reduced taxes. Additionally, the target group of 19–25 year-olds is a highly heterogeneous group. The younger individuals are mostly fresh out of high-school, and many of these take a sabatical before proceeding to higher education. The older ones, on the contrary, tend to either participate in higher education or have started their working careers. The existence of heterogeneous treatment effect across ages is, to some extent, already indicated in table 2, but here we will study this topic in more detail.

Table 3 estimates treatment effects of the 2007 reduction for single ages down to 21, which is the lowest age for which pre-treatment trends are parallel with the control group. These estimates appear less stable than those in table 2 (there is even a significant pre-treatment effect for 22-year-olds), perhaps caused by the fact that any cohort heterogeneity will have a large impact when studying single ages. However, there is a very clear pattern emerging from the table: the employment effects of the tax reduction are highly age dependent, with higher employment increases for younger workers. (This same pattern emerges also when studying 24- and 25-year-olds over the entire period

2007–12.)

The explanation closest at hand is that mentioned above—younger individuals have longer expected exposure to the reduced payroll tax. However, the extent to which this has economic relevance to the employer depends on the expected spell lengths of new hirings. For short-term jobs during the summer months, treatment dose differentials should be close to irrelevant, but for long-term hirings it may be important. To make this more clear, an additional bottom row has been added to table 3, giving the expected years of subsidized employment for new hirings, for each age group. This figure is obtained as the intersection of the total age-specific expected spell length and the time remaining until the start of the year the worker turns 26:

$$\mathbb{E}[l_{\text{subs.}}] = \mathbb{E}\left[\min\left\{26 - a - (t - \lfloor t \rfloor), L_i\right\} \mid a\right],$$

where t is start time of employment spell, L_t is full spell-length, and a is age in cohort terms (present year minus year of birth).[20] If we study how $\mathbb{E}[l_{\text{subs.}}]$ varies over age, it seems that treatment dose tend to be most limiting for the oldest age groups. In particular, 25-year-olds hired late in the year enjoy only a few months of reduced payroll tax, and this is reflected in the considerably lower figure for this age group. However, for younger ages, treatment dose appears less important: since few employments for 22-year-olds last longer than four years, the 21-year-olds' additional year of lower employment cost has but a small impact on $\mathbb{E}[l_{\text{subs.}}]$. By examining how $\mathbb{E}[l_{\text{subs.}}]$ covaries with the age-specific treatment effects, we can assess to what degree the latter can be explained by treatment dose differentials. While differences in $\mathbb{E}[l_{\text{subs.}}]$ seem to fully explain the difference between 24- and 25-year-olds, this is not the case for the younger ages. Thus,

[20] Thus, $(t - \lfloor t \rfloor)$ measures the fraction of the year that has passed when the employment spell starts. The values of $\mathbb{E}[l_{\text{subs.}}]$ reported in table 3 are conditional on starting out at least 25 percent of full-time, and are calculated over all employments initiated in 2006, so as not to risk the unexpected 2009 recession contaminating the estimates. However, using the corresponding calculations for any adjoining year yields very similar results.

explanations for the considerably higher treatment effects for 21–22 year-olds must be sought elsewhere.[21]

Other potential explanations for age-heterogeneous treatment effects are related to labor force composition. First, 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. Second, as individuals get older, the non-employed are an increasingly selected group of low-productive workers which may respond less to the payroll tax reductions.

We have described in section 4 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 columns 1–4 of table 3, 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 for 22-year-olds is around 1.7 percentage points higher 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

[21] Note that when looking at the *expected present value of the reduction*—perhaps the best measure of treatment dose—age differences are even smaller than suggested by $\mathbb{E}[l_{\text{subs.}} \mid a]$, since the latter does not take into account discounting for future wage commitments, nor the fact that average wage increases in age; both these factors will work to diminish the age differences in expected treatment dose. This further weakens the case for treatment dose as an explanation for age-heterogeneous effects.

[22] Using the terminology of section 4—where δ_g is the DiD estimate and ξ_g is the causal effect of the reform for group g—the result of comparing 22-year-olds against 25-year-olds (assuming parallel counterfactual trends) is

$$\delta_{22/\text{CG}} - \delta_{25/\text{CG}} = (\xi_{22} - \xi_{\text{CG}}) - (\xi_{25} - \xi_{\text{CG}}) = \xi_{22} - \xi_{25}.$$

Table 4: Employment effects for foreign-born and previously unemployed

	FOREIGN-BORN		UNEMPLOYED
	20–25 vs. 26	24–25 vs. 27	25 vs. 26
DD 2005	−0.001	−0.004	−0.000
	(0.002)	(0.007)	(0.005)
DD 2006	−0.001	−0.002	−0.005
	(0.003)	(0.005)	(0.006)
DD 2007	0.005’	0.001	0.007
	(0.003)	(0.006)	(0.007)
DD 2008	−0.002	0.001	0.002
	(0.004)	(0.006)	(0.007)
DD 2009		−0.005	
		(0.005)	
DD 2010		0.003	
		(0.006)	
DD 2011		0.001	
		(0.004)	
DD 2012		−0.009	
		(0.006)	
R^2	0.18	0.19	0.04
N	890,942	730,312	153,931
$\overline{y}_{\mathrm{TG}}$	0.35	0.36	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.

can use the 25–26 estimates as upper bounds for the negative substitution effect for the 26-year-olds. This implies that the absolute employment increase for 22-year-olds is *at least* around 1.7 percentage points.

We next turn to the subsample of young foreign-borns, in columns 1–2 of table 4. 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 population of young workers as a whole, as reported in the bottom rows of tables 2 and 4. Strikingly, there is no evidence that the tax reduction had any impact at all for this group. On the other hand, it should be noted that the estimates are not sufficiently precise for us to rule out a positive effect (the upper confidence limit is at around a one percentage point increase). We can, however, conclude that if there were positive employment effects for foreign-borns, they were limited.[23]

As a final subgroup, we consider unemployed workers. More precisely, we follow the group of previously unemployed 25–26 year-olds, defined here as those individuals who were registered as 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.[24] As column 3 of table 4 shows, there is no evidence that the effect for unemployed 25-year-olds was larger than in the general case, albeit the estimates are somewhat noisy.

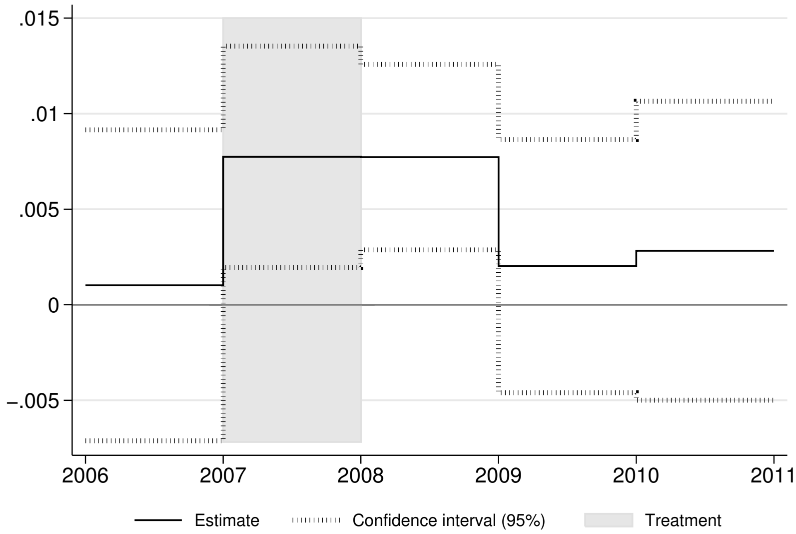
5.3 Persistence of treatment effects

An important question, especially from a welfare perspective, is whether past treatment causes a higher relative employment rate also in the years when individuals are no longer treated. In order to examine this question, we follow the 1982 cohort over time in figure 6, limiting the sample to individuals born within three months around the turn of the year (this is necessary to get tight specifications also when studying older ages). Since, due to non-parallel pre-treatment trends, we cannot follow pairs of cohorts over time, each year in the figure corresponds to a separate local estimate, from 25/26 for 2007, to 28/29 for 2010. The 1982 cohort was treated only in 2007 (cf. figure 2, page 7), and the positive

[23]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.

[24]For this group, labor supply constraints should be less of a problem than for the target group as a whole: by definition, registered unemployed are not taking part in education, and the fact that these individuals are attending the unemployment office should indicate a willingness to take a job. (On the flip side, the group of unemployed is a highly selected sample with worse employment prospects than the population on average, and may thus respond differently to the tax reduction.)

Figure 6: Treatment effect persistence for the 1982 cohort



Notes: Estimates taken from consecutive age−fixed regressions.

estimate for 2007 in figure 6 is the same as that in the left-most column of table 2. If the payroll tax reduction had a lasting impact on individuals, we expect to see significant differences in employment also in 2008 (and possibly onwards). Evidently, there is some evidence of persistence: there appears to be a lagged treatment effect in 2008, one year after the treatment ended for this cohort, at roughly the same size as the direct effect the year before. (This is the local 26/27 estimate from table 2.) However, from 2009 onwards there is no longer any trace of the previous treatment. It is an open question whether the effect would have persisted longer, had the economic downturn not taken place in 2009 (recall that we also do not find any effect for the target group for 2009).

5.4 Intensive margin and wages

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

Table 5: Effects on hours (subsample of employed)

	All workers				Tenured			
	Loc 25/26	20–25 / 26	24–25 / 27	26 / 27	Loc 25/26	20–25 / 26	24–25 / 27	26 / 27
DD 2005	−1.17	−1.06	−0.55	−0.32	−2.19	−0.10	−0.94	−1.54
	(2.37)	(0.85)	(0.83)	(0.95)	(2.85)	(1.29)	(1.18)	(1.25)
DD 2006	−3.22’	0.64	−0.50	0.11	−1.10	0.95	−1.32	−0.88
	(1.79)	(0.93)	(1.02)	(1.22)	(2.53)	(1.22)	(1.05)	(1.06)
DD 2007	0.93	0.19	−0.31	0.17	3.66	0.94	−0.50	0.21
	(2.83)	(0.95)	(1.07)	(1.21)	(3.25)	(0.74)	(1.29)	(1.28)
DD 2008	−3.28*	0.58	0.08	0.19	−3.51’	0.88	−1.00	0.31
	(1.43)	(1.44)	(1.06)	(0.89)	(1.95)	(1.59)	(1.14)	(1.13)
DD 2009			−0.59	0.55			−2.18’	0.40
			(1.22)	(1.12)			(1.29)	(1.48)
DD 2010			−0.96	−1.82			−2.93*	−3.43*
			(1.09)	(1.79)			(1.44)	(1.72)
DD 2011			2.36*	3.02’			2.48	1.97
			(1.10)	(1.74)			(1.86)	(2.24)
DD 2012			−2.21	−1.57			−2.04	−1.94
			(2.00)	(1.52)			(2.17)	(1.80)
R^2	0.08	0.10	0.08	0.07	0.08	0.10	0.08	0.07
N	131,886	1,651,545	1,227,608	880,190	96,789	1,143,575	906,489	667,047
$\sum w_i$	235,405	3,066,698	2,237,243	1,579,714	167,512	2,051,916	1,611,855	1,167,837
$\overline{y}_{\mathrm{TG}}$	120.6	108.3	115.2	123.5	122.0	111.4	117.5	125.2

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

Notes: Outcome is the monthly total of hours worked, $\overline{y}_{TG}$ denotes treatment group average outcome in the treatment period. ‘Tenured’ are those employed with the same employer as in the previous year. $\sum w_i$ reports the sum of stratification weights. 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 government sectors (municipality and county).[25] We also look specifically at the subsample of previously employed, so as to avoid the potential composition problem arising from the employment increase. Table 5 shows how hours worked were affected for the main age groups studied above, for the original tax reduction as well as for the 2009 extension. Evidently, there appears to have been no effects on the intensive margin, irrespectively of whether we consider all workers or those with tenure. (The local estimation for all employees in column 1 appears unstable, with tendencies of pre-treatment effects.) For 24–25 and 26-year-old tenured workers there is a significant, but small, negative effect in 2010. While, at first glance, a negative effect might appear counterintuitive, this could be the result of adjustments made in the wake of the recession. For example, this might be a composition effect if, as a response to weaker demand, firms cut down on hours more for younger, on average less productive, workers. Our finding of no adjustments along the intensive margin is different from the results in Huttunen et al. (2013), who find that decreasing payroll taxes for employers of older low-wage workers in Finland *increased* working hours among those already at work.

We next examine whether part of the tax reduction was passed on to employees as higher wages. This analysis uses the sample of those employed at least a quarter of full-time (in symmetry with the employment definition used above), and the outcome measure is the log of monthly full-time equivalent wage. Table 6 gives the impacts of both the 2007 reduction and the 2009 extension (again, also including estimates for the subsample of previously employed workers so as to spot potential compositional bias). Starting with the 2007 reduction, there is no effect locally around the cutoff; the point estimates are small in economic terms and statistically insignificant. For 20–25 year-olds there is, however, a small relative wage increase, slightly above one percent both in 2007 and in 2008.[26]

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

[26]For each of the two age groups that we consider, we have tested for heterogeneity with respect to

Table 6: Effects on wages (subsample of at least quarter-time employed)

	All workers				Tenured			
	Loc 25/26	20–25 / 26	24–25 / 27	26 / 27	Loc 25/26	20–25 / 26	24–25 / 27	26 / 27
DD 2005	−0.017	0.004	−0.000	0.005	−0.003	0.007’	−0.002	−0.001
	(0.013)	(0.003)	(0.005)	(0.004)	(0.010)	(0.004)	(0.005)	(0.004)
DD 2006	−0.011	0.006	−0.004	−0.001	−0.008	0.008’	−0.003	−0.001
	(0.013)	(0.004)	(0.003)	(0.004)	(0.013)	(0.004)	(0.004)	(0.005)
DD 2007	−0.007	0.012***	0.009*	0.003	−0.006	0.011**	0.011*	0.006
	(0.007)	(0.002)	(0.004)	(0.004)	(0.007)	(0.004)	(0.005)	(0.005)
DD 2008	0.003	0.013**	0.014*	0.005	0.009	0.013*	0.018***	0.007
	(0.007)	(0.005)	(0.006)	(0.004)	(0.010)	(0.006)	(0.004)	(0.004)
DD 2009			0.010***	0.011**			0.011**	0.010*
			(0.003)	(0.004)			(0.004)	(0.004)
DD 2010			0.009**	0.004			0.005	0.000
			(0.003)	(0.003)			(0.005)	(0.005)
DD 2011			0.025***	0.011’			0.019**	0.005
			(0.007)	(0.007)			(0.006)	(0.005)
DD 2012			0.010	0.004			0.011’	0.003
			(0.006)	(0.004)			(0.006)	(0.005)
R^2	0.21	0.25	0.31	0.31	0.21	0.25	0.31	0.32
N	128,383	1,485,392	1,191,502	890,199	94,790	1,039,925	886,057	678,953
$\sum w_i$	223,666	2,758,228	2,125,447	1,545,146	160,763	1,868,284	1,545,044	1,151,902
$\overline{wage}_{\mathrm{TG}}$	21,790	20,543	22,625	23,475	21,971	20,888	22,894	23,676

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

Notes: Outcome is the log of monthly full-time equivalent wage (truncated below to 0), $\overline{wage}_{TG}$ denotes treatment group average outcome in the treatment period, in non-log form. ‘Tenured’ are those employed with the same employer as in the previous year. $\sum w_i$ reports the sum of stratification weights. 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.

Comparing 24–25 year-olds to 27-year-olds allows us to study the evolution of wages into the 2009 extension. As seen in columns 3–4 and 7–8 of table 6, wages did not adjust more in the longer run: with the exception of the recovery year 2011, the relative wage increase for 24–25 year-olds remains at around one percent throughout 2007–12. The figures for 26-year-olds are similar, except that there is weaker evidence for wage effects in the longer run for this age group. (We remind the reader that 26-year-olds are treated only from 2009 onwards.) These small wage effects contradict the prediction made by the standard competitive labor market model where the incidence of payroll taxes falls on individual workers. Our findings also stand in contrast to evidence for the U.S., where the basic result is that of extensive shifting of the incidence of the tax onto workers (see, e.g., Gruber 1994; Anderson and Meyer 1997, 2000; Murphy 2007).

Understanding the small wage effects that we find requires making a few observations. To start with, 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. (As mentioned in section 2, 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, however, is the evidence of wage effects even for 24–25 year-olds, who typically have wages strictly above the minimum wage level.[27] This would suggest that shifting instead works through individual wage bargaining. Such an impact, if it exists, is likely to be more immediate than union-negotiated wage increases. This being said, we conclude that given the small size of the wage increase, shifting alone cannot explain the modest employment effects we have found.

private or public sector, and for blue collar or white collar workers. The results for these subgroups are similar to the general cases.

[27] 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.

6 Implied elasticities and cost-benefit analysis

6.1 Implied demand elasticities

We combine employment and wage estimates to obtain the elasticity of demand for young workers with respect to labor costs. This is under the assumption that the Swedish labor market, for these age groups, is essentially demand constrained (and so we disregard supply constraints).[28] When calculating demand elasticities, we have, for increased efficiency, reestimated the models, using pooled treatment effect estimates for pairs of years (2007–08, 2009–10, 2011–12). For 20–25 year-olds, our estimate of the joint 2007–08 employment increase is 2.1 percent, and the corresponding wage increase is 1.2 percent, implying a labor demand elasticity of about -0.32.[29] How does this figure fit into the previous evidence on demand elasticities? Overall labor demand elasticity is generally considered to lie between -0.15 and -0.75.[30] However, studies on labor cost reductions targeted at smaller (disadvantaged) sub-groups of the labor market have produced somewhat mixed results. Katz (1996) estimates an own-wage labor demand elasticity for young disadvantaged workers in the U.S. of -0.5. Cahuc et al. (2014) study temporary hiring credits targeted at low-wage workers in France in 2009 and find substantial short-run effects on employment, corresponding to a labor demand elasticity as high as -4 (which, additionally, the authors considers to be a lower bound in absolute terms). One potential explanation for the larger effects in these studies is that the tax reductions

[28]Formally, we need to assume that the supply elasticity of young workers is infinity. This assumption is common in the literature, e.g. in Cahuc et al. (2014), and Katz (1996).

[29]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

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

where 32.42 percent is the original payroll tax rate, and 11.1 percent represents the tax cut.

[30]Hamermesh (1996) summarizes more than 70 studies on labor demand elasticity estimation, cited in Cahuc et al. (2014). In the words of the latter: "If a single figure were to be chosen, [−]0.30 would surely be the best estimate." (Cahuc et al. 2014, p. 117).

Table 7: Implied labor demand elasticities, by age and over the business cycle

	$\epsilon_{2007/08}$
Age 21	−0.46
Age 22	−0.52
Age 23	−0.40
Age 24	−0.24
Age 25	−0.11
Age 20–25	−0.32

	Age 24–25	
	yearly	2-year ave.
ϵ_{2007}	−0.14	−0.18
ϵ_{2008}	−0.23	
ϵ_{2009}	−0.04	−0.10
ϵ_{2010}	−0.16	
ϵ_{2011}	−0.26	−0.19
ϵ_{2012}	−0.13	

are conditioned on *new* hires. (When Cahuc et al. (2014) adjust their elasticity estimate to account for this, they obtain −1.1.) Moreover, Cahuc et al. (2014) study a credit that firms knew would only be valid for one year. Finally, Huttunen et al. (2013) find that employment was unaffected by reductions targeted at the employers of older, full-time, low-wage workers in Finland.[31]

Table 7 makes clear that labor demand elasticity for young workers vary substantially, both across ages and over the business cycle. The left panel shows the 2007–08 elasticity estimated for different ages; this pattern roughly mirrors the heterogeneous treatment effects found in Table 3. Elasticities vary between −0.52 for the youngest age-groups to −0.11 for the oldest. As discussed in the previous section, this pattern is partly, but not wholly, explained by differences in expected exposure to the reduced taxes. The right panel of table 7 follows the age group 24–25 in the longer run, up to the year 2012. Although the year-specific estimates are noisy, the two-year averages displays a pattern: in the expansion years of 2007–08 and 2011–12, the estimated elasticity is slightly below −0.20, while in the deep recession years 2009–10, the elasticity is estimated at −0.10. Thus, the potency of payroll tax reduction for increasing youth employment appears to be strongly pro-cyclical. Furthermore, it is striking that the larger effect estimates, in

[31]See also Bohm and Lind 1993; Bennmarker et al. 2009; Korkeamäki and Uusitalo 2009.

absolute numbers, found for 2011–12 are fully explained by the increase in reductions associated with the 2009 extension, in combination with business cycle.

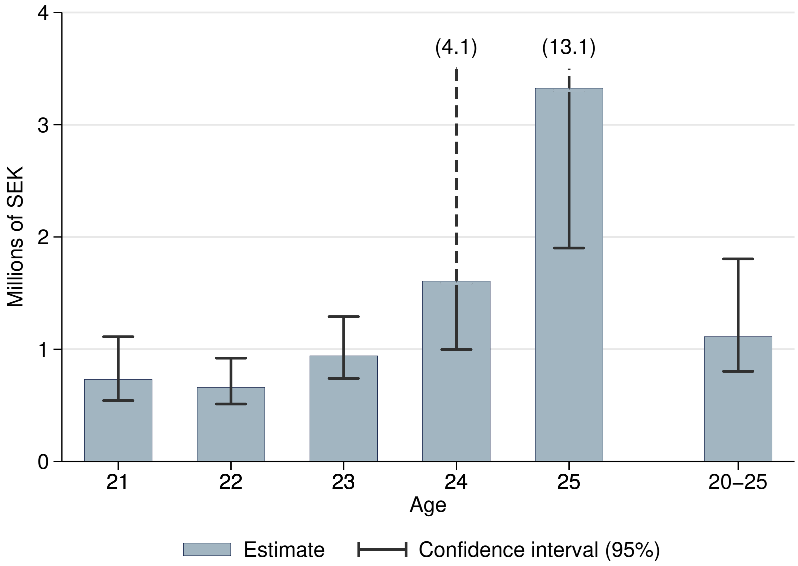
6.2 How much money was spent on each job?

We define the *gross cost* of the payroll tax reductions as the sum of foregone payroll taxes, taking into account the positive effects of higher employment and wages, but disregarding other channels.[32] This figure can be straightforwardly calculated by combining information on total taxable income, available to us in the tax registers, with the employment and wage estimates from the previous section. 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.[33] Since the gross cost increases in age—as both employment rate and average wage correlates with age— and, additionally, the estimated number of new jobs decreases in age, it is not surprising that the cost per job soars 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.

[32] If the tax reduction had an effect on, say, firms' profits, this would have resulted also in increased corporate tax revenues. Since we do not take this potential factor into account, we prefer the term *gross* cost. When estimating increased revenues from higher employment, we assume that the new jobs have average wages, conditional on employee age and working at least a quarter of full-time. This should be a reasonable approximation, given the dominance of centrally negotiated wages for young workers.

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

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



7 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. Moreover, hours worked was completely unaffected, ruling out also this potential channel of adjustment. 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.

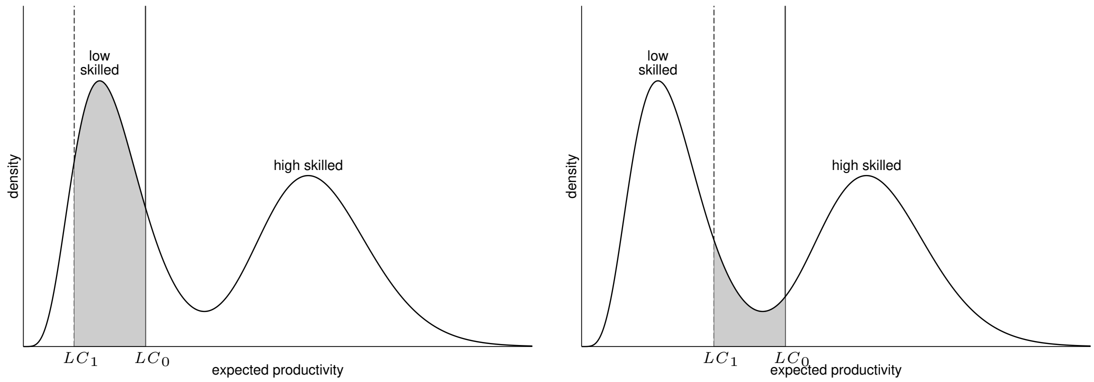
It is, in principle, possible that the lack of employment response is caused by low labor supply. There are many alternatives to employment for young individuals. Many are taking part in higher education, others spend a couple of years after high school travelling the world. It is also possible that 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 stronger social stigma both of being unemployed and of living with one's parents. 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 consider 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 intended as a temporary policy, but even here, we find small 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 needed 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 sector, 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 reduction. 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

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.



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 A.1, page 49, 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 resulted in at most weak effects for these groups, if at all.

It is, finally, important to stress that the estimates reported in this study may not reflect net effects on the labor market as a whole. In section 4, we described 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 4), 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.[34] As a 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.

[34]As an example, in 2013 an employer of low-skilled labor turned down a 35-year-old applicant with the explicit motivation that they only hire workers who are subjected to the lower payroll tax, which prompted the Swedish Trade Union Confederation to sue the employer (Svenska Dagbladet 2013). At the time of writing, the case has not been settled.

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A Additional tables

Table A.1: Summary statistics (mean percentages) for foreign-borns and previously unemployed, year 2006 (see Section 3)

	Foreign-born			Prev. unempl.		
	20	23	25	20	23	25
Employed, $\geq$ quarter-time	26.3	33.0	37.1	42.7	43.9	45.3
Employed, full-time	6.3	12.8	17.0	11.2	14.6	17.3
Income (year total SEK)	41,335.8	63,710.5	78,417.1	63,695.9	76,066.0	84,562.4
Unemployed prev. year	7.4	13.9	13.1	100.0	100.0	100.0
Educ. below high school	28.5	22.4	20.6	25.5	24.1	23.9
Educ. high school	62.3	46.1	39.4	72.3	65.6	55.3
Educ. above high school	9.2	31.5	40.1	2.2	10.3	20.9
Female	49.5	51.5	50.6	48.3	43.4	44.9
Foreign-born	100.0	100.0	100.0	12.0	19.8	23.3
N	13,982	17,437	19,156	8,585	12,286	10,754

Table A.2: Employment effects of the 2007 reduction: Gradually increasing the treatment group (see Section 5)

	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	0.001	0.002	0.001	−0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
DD 2006	−0.002’	−0.000	0.001	0.003’	0.004	0.003
	(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***	0.010***	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	1,735,846	2,588,762	3,438,895	4,291,774	5,148,112	6,015,936
$\overline{y}_{\text{TG}}$	0.63	0.61	0.61	0.60	0.59	0.58

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

Notes: See notes for table 2.