Payroll Taxes and Employment

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1 Introduction

While general unemployment in Sweden has been on similar levels as in other OECD countries for the last fifteen years, youth unemployment has been among the highest in the western world. Between 1994 and 2008, the unemployment rate was on average three times higher for individuals younger than 25 years old than for those in the age group 25–54; the same number for the OECD as a whole is 2.3. Moreover, the problem with high and persistent youth unemployment has deepend over time: since 1998, relative unemployment for youths has almost doubled.

Recently, this phenomenon has become a highly debated topic in Sweden—among politicans, in media and within academia—and youth unemployment has become increasingly seen as one of

After the coalition of right-wing parties had won the elections to parliament in 2006, they presented several proposals on how to fight unemployment. One of the most costly reforms aimed at helping young individuals to enter the labor market. In July 2007, payroll taxes were cut for employees aged 18–24 with 11 percentage points (from 32.4% to 21.3%), making the payroll tax for workers in the target group two thirds of that of other workers.

The empirical evidence on the effects of such payroll tax cuts is neither conclusive nor clear-cut. This holds both for studies which use aggregate data to carry out cross-country analyses, and for studies using within-country variation in payroll taxes. A majority of earlier studies seem to establish that some (or whole) of the payroll tax cut is transferred to workers in the form of higher wages, known as *shifting*whereas there is disagreement on how much of the tax relief will be transferred from employers to employees. Furthermore, in some cases there has been a positive effect on employment while in others there seems to be no effect. Consequently, it is difficult for policy makers to predict to what extent a payroll tax cut will actually affect employment.

This paper measures the employment and wage effects of the 2007 reform. Although our study is related to earlier work, several important contributions are worth stressing. First, the size of the group directly affected by the reform is substantial: 18–24 year-olds comprise 14.1% of the population aged 16–64 and around 11% of the labor force aged 16–64. Hence, this was no minor experiment as in some of the earlier studies on the Nordic countries, including Sweden. Secondly, the tax cut we examine is explicitly aimed at employers of a specific age group of the population. There are very few examples of evaluations of such reforms in the past. Thirdly, whether or not an individual is entitled to the payroll tax cut depends only on his/her age. This clear eligibility criteria contrasts many of the earlier tax cuts that have been studied. Importantly, this means that we know exactly who are entitled for the exemptions and who are not and therefore we are not forced to rely on matching assumptions in order to identify effects.

Our results suggest small or no employment effects of the reform, a finding that holds for all three treatment groups we analyze. The support for no reform effect is stronger the more homogenous is the group of workers we study; when we seperate out workers with only high school education there is clear cut evidence that the payroll tax subsidies had no impact on employment.

[Youth unemployment is extra bad because .... referenser] Although Sweden is one of the OECD countries with the highest youth unemployment, it is far from the only country where this problem persists. Many European countries, including Italy, France and Spain, risk ending up in a situation where a large part of the young generations do not have a job. Thus, even though we study a specific country, our results should have a broad interest.

2 Previous Empirical Evidence

2.1 Early work

The literature on the workings of payroll taxes is broad and quite extensive and we only briefly summarize some of the evidence here. For convenience, we focus on work that makes use of disaggregated data and thus leave out older cross-country analyses. There are relatively few studies examining effects outside the Nordic countries; the most important exceptions—which all study the U.S.—are Gruber and Krueger (1990), Gruber (1994), Anderson and Meyer (1997), Anderson and Meyer (2000) and Murphy (2007).

Gruber and Krueger (1990) exploit the fact that in certain industries in the U.S. the cost to employers of providing workers' compensation benefits vary within states over time and across states at a moment in time. Using survey data on a subset of all privately employed workers in the US between 1979 and 1988, they show that higher costs are to a large extent shifted from employers to employees in the form of lower wages (how much of the costs that are offset by lower wages varies between 20% to 86.5%, depending on the specification used). In addition, higher insurance costs have no significant effect on employment.

During the 1975–1979 period, new laws stipulating childbirth to be covered in workers' health insurance were passed in certain U.S. states. This was followed by a federal law in 1978 which more broadly prohibited discrimination against pregnant women. Workers' insurance is paid by the employer and thus the new rules increased the relative cost of hiring young women (and/or their husbands). By exploiting variation in the timing of the new laws, Gruber (1994) is able to identify wage and employment effects. The analysis is conducted in two steps. First, states that passed the laws in the mid 1970's are used as experimental states and those that did not as controls. Secondly, he estimates the effect of the federal law by letting the states that did not yet pass the maternity benifits be treated and those that already did be control states. To be more specific, he compares treatment individuals (young women) in the passing states to a set of control individuals in those same states and estimates the change in the treatments' relative outcomes, relative to states that did not pass the mandates. This Difference-in-Differences-in-Differences approach shows (i) that shifting occured (i.e. the mandates caused lower wages of women of childbearing age and their husbands), and (ii) a negative but small employment effect which is consistent with the finding that the added costs were not fully shifted onto the employees.

Anderson and Meyer (1997), Anderson and Meyer (2000) and Murphy (2007) make use of variation across firms and workers in the unemployment insurance (UI) tax in the U.S. In their early work, Anderson and Meyer use firm level data for eight U.S. states for the period 1978–1984 to study the effects of tax differences across firms within the same labor market. Fixed effects estimates suggest that a large share of the market level tax is born by the worker through lower wages, while individual firms can pass on much less of the within market differences in taxes. The latter is also found to lead to significant employment reallocation accross firms. Their second work estimates what happened to earnings and employment in Washington after the state in

1985 was forced to adopt an experienced-rated tax system that created large variations in UI tax payments across firms. Similar to the first study, their estimates show that most of the industry average tax rates are passed onto workers while firms must bear the major part of the difference between its own tax rate and the industry average taxes. Murphy (2007) instead use between state variation in UI taxes to study the effect on wages paid to workers in different demographic groups. He shows that higher taxes lead to lower earnings for less mobile workers such as married women and youths. For prime-age men however, tax increases have a much smaller and but weakly significant effect.

To sum up, all five studies cited above suggest partial shifting of labor costs, although the size varies, and some impact on employment, albeit often small.

In contrast to these results, Gruber (1997) find evidence of full shifting and no effect on employment after reductions in payroll taxes in Chile in 1981. As noted in Bennmarker et al. (2009), the contrasting results may be explained by the fact that payroll taxes were replaced by income taxes paid by the employee—hence workers should have had a strong position in post reform wage negotiations. Further, Gruber himself stresses that the findings may not be applicable to other countries due to many factors, one of them being high inflation in Chile during the period studied.

2.2 Studies on the Nordic countries

Besides the work on the US and on Chile summarized above, there are several studies on regional differences in payroll taxes in the Nordic countries. Bohm and Lind (1993) study the employment effect, in terms of number of employees, of a 1984 payroll tax reduction in the county Norbotten. They use the Difference-in-Differences estimator combined with a matching procedure in order to find good control groups. No effect on employment could be established for any of the two methods. Further, Bennmarker et al. (2009) evaluate the effect of a ten percentage point reduction in payroll taxes introduced in 2002 for firms situated in a large support area in the Northern parts of Sweden. They find no effect on number of employees but every percentage point reduction in the tax rate increased the average wage bill per employee by about 0.25 percent.

Similar to these two studies is Korkeamäki and Uusitalo (2006), examining an experiment that reduced payroll taxes for firms in northern Finland between 2003 and 2006. Their tripple differences estimates indicates that for the service sector more than half of the the tax reduction shifted to workers in the form of higher wages (a 1 percent reduction in labour costs increased wages by 0.6 percent). For the manufacturing sector however, estimates are less precise and mostly insignificant. Moreover, the tax cut had no significant effect on firms' number of employees. Johansen and Klette (1998) study wage effects in the manufacturing sector in Norway for the period 1983–1993. By exploiting variation in payroll taxes across regions over time they find evidence of partial shifting: a 1 percent reduction in employers' cost led to an 0.4 percent increase in hourly wages.

In summary, the experience from the Nordic countries is that lowering payroll taxes have no effects on employment measured by number of employees but seems to lead to higher wages. Between 25 and 60 percent of the tax reduction is shifted to employees.

2.3 Similar studies

Few studies analyze payroll tax cuts aimed at promoting employment for a specific subgroup of the labor force. The only other examples we have found—Huttunen et al. (2010), Kramarz and

Philippon (2001), and Goos and Konings (2007)—examine subsidy schemes for workers with low wages.

Huttunen et al. (2010) study a Finnish experiment that decreased payroll taxes for older, full-time, low-wage workers in 2006. Since the eligibility criteria were very specific they are able to define several control groups and thus use a Difference-in-Differences-in-Differences approach. Using different data sets, they show that the subsidies (i) had no effect on employment rates, and (ii) did not significantly affect neither entry to nor exit from unemployment. However, there is some indication of a rise in the probability of part-time workers in the industrial sector obtaining full-time employment due to the reform. The results concerning wages are more ambiguous: even though monthly wages seem to have risen, hourly wages dropped in some cases.

Kramarz and Philippon (2001) study the effects of changed minimum labor costs on the propensity to move into and out of employment. One of the things they find is that tax subsidies, implemented to offset increased minimum wages in France between 1990 and 1998, have no siginificant impact on job entry.

Finally, Goos and Konings (2007) analyze payroll tax exemptions for employers of manual workers in Belgium in the late 1990s. They use firm level data to study the impact on employment and wages and separate between exporting and non-exporting industries. Their findings suggest that (i) full-time manual employment increased 5–8%, (ii) pre-tax wages increased with 1–3%, and (iii) employment increased more in exporting industries.

3 Characterizing the Reform

As in many other countries, Swedish employers are obliged by law to pay mandatory fees to finance welfare services such as pensions, health- and disability insurance, and other social benefits. Payroll taxes were introduced for the first time in Sweden in 1950. Since then, the rate has rosen sharply from around 6% to around 33 % today (Holmlund (1983)). Up until the beginning of the 1980's, the legal tax rate was the same for all firms, but over the last 25 years there have been three major exemptions. First, between 1982 and 1999 firms in the northern parts of Sweden were allowed reductions of 10 percentage points in efforts to boost employment in these areas. Secondly, there was a general cut by five percentage points for all firms in 1997–2008. Lastly, new exemptions in the northern parts of Sweden for private sector employers were introduced in 2002 (see Pro (2007) or Bennmarker et al. (2009) for details). The Swedish payroll tax can be separated into seven different fees, each with a specific use. Besides the statutory payroll tax, collective agreements commit most employers to pay around 10.4% of gross wages to finance job search support, retraining and severance payments when employees are laid off (Bennmarker et al. (2009)).

The reform we study is different in nature than earlier payroll tax subsidies in Sweden. It was ratified in March 2007 and implemented three months later by the newly elected government whose intention was to make it easier for youths to enter the labor market. Before July 1, 2007, employers had to pay the same payroll tax rate irregardless of the age of the employee, whereas after this date, the tax rate for employees 18–24 years of age was cut by 11.1 percentage points (the rate went from 32.4% to 21.3%). The reform halved six out of seven mandatory fees such that the payroll tax for workers in the target group became two thirds of that of other workers.[1] The new rules were in place until January 1, 2009, at which time taxes were cut additionally (down to 15.5%) and the exemptions were expanded to include all individuals younger than 26

[1] Individuals who are self-employed pay "egenavgifter", roughly equivalent to payroll taxes payed by employers. These fees were also cut with the motivation that payroll tax levels should not be a critical factor for young workers' choice between self-employment and regular employment. The level decreased from 30.7% to 20.5%.

years of age.

18–24 year-olds comprise 14.1% of the population aged 16–64 and around 11% of the labor force aged 16–64—thus the size of the group of people directly affected by the new regime is substantial.[2] Further, the gross cost of the reform is calculated to 4.5 billion kr (around $0.6 billion) in 2007 and 10.6 billion kr in 2008 (around $1.41 billion) (see e.g Riksrevisionen (2008) and Pro (2007)) which amounts to 0.5–1 percent of the fiscal budget in these years.[3] The legislation proposal contains no figures on the predicted increase in the number of jobs due to the reform and, noteworthy, it is not clear in whether the purpose was primarily to create new jobs or if also to reallocate jobs between workers. To get a picture of the monetary incentive faced by the employer we note that Swedish 24-year-old workers earned on average 240,000 kr ($32,000) in 2008. Hence, employers who chose to hire a typical 24-year-old worker instead of a 25-year-old in 2008 (with the same salary) saved 25,680 kr ($3,424) this year due to the new rules.

4 Theoretical Remarks [to be expanded/cleaned up]

In this section, we discuss how a labor price shock influences both labor demand and labor supply, and we consider the possible effects on long run labor market equilibrium.

4.1 Labor Demand

4.1.1 Input Substitution and Output Expansion

A change in the price of an input factor i may influence factor demand of a profit maximizing firm in two possible ways. First, holding production output constant, the new factor price ratios will imply a new optimal input vector; this we call an *input substitution effect.* While this effect must be (weakly) negative for the demand for input i itself, the effect on other inputs depends on whether these inputs are substitutes or complements to factor i. Generally, however, we expect most inputs to be at least weak substitutes, and consequently there should be a positive substitution effect of factor i on other inputs. Secondly, since the firm now faces a weakly higher/lower price vector, it may be profitable to decrease/increase production; this is denoted a *scale effect.* Though a change in output is generally associated with a change in input demand in the same direction, this by no means necessarily hold for all inputs.[4] So while the scale effect is likely to be negative for most inputs (i.e. a price decrease leads to increasing demand), it is *a priori* uncertain whether this is positive or negative for a particular input factor.

Formally, consider a profit maximizing firm producing a single output good, with fixed production technology. Let $\mathbf{x}(\mathbf{w}, p)$ and $y(\mathbf{w}, p)$ be the profit maximizing input vector and output quantity, respectively, at factor prices $\mathbf{w}$ and output price p. Now define the *compensated input function* $\boldsymbol{\xi}(\mathbf{w}, y)$ as the cost minimizing input vector producing output y at factor prices $\mathbf{w}$.[5] Noting that $x_i(\mathbf{w}, p) = \xi_i(\mathbf{w}, y(\mathbf{w}, p))$ (see lemma 1 in Sakai (1974)), we find the effect of an

[2] Figures are for 2008. Labor force participation ratios are reported in The Labor Force Survey (Arbetskraftsundersökningen, Statistics Sweden), where estimates are given for age groups 15–19, 20–24, 15–64 and 16–64; the figure 11% for 18–24 year-olds' labor force participation is our rough guess based on these numbers.

[3] The net cost, i.e. when for example decreased wage costs for local governments in municipalities is considered, is estimated by Riksrevisionen (2008) to 3.2 billion kr (around $0.43 billion) in 2007 and 7.7 billion kr in 2008 (around $1.1 billion).

[4] Clearly, this depends on how broadly one defines the different factors of production. For instance, broad measures of capital and labor are likely to be positively associated with scale, though surely the exact composition of different types of capital and labor vary with respect to scale (as well as over time).

[5] The cost minimizing input vector is uniquely determined if ...

infinitesimal price change in factor k on the demand for factor i by simply taking the derivative of the composite function:[6]

$$\underbrace{\frac{\partial x_i(\mathbf{w},p)}{\partial w_k}}_{\text{Total effect}} = \underbrace{\frac{\partial \xi_i(\mathbf{w},y(\mathbf{w},p))}{\partial w_k}}_{\text{Substitution effect}} + \underbrace{\frac{\partial \xi_i(\mathbf{w},p)}{\partial y} \cdot \frac{\partial y(\mathbf{w},p)}{\partial w_k}}_{\text{Scale effect}},$$

or compactly, in matrix form

$$\frac{\partial \mathbf{x}(\mathbf{w},p)}{\partial \mathbf{w}} = \frac{\partial \boldsymbol{\xi}(\mathbf{w},y(\mathbf{w},p))}{\partial \mathbf{w}} + \frac{\partial \boldsymbol{\xi}(\mathbf{w},p)}{\partial y} \cdot \frac{\partial y(\mathbf{w},p)}{\partial \mathbf{w}}.$$

As discussed above, a factor's own substitution effect is weakly negative, so the substitution matrix $\frac{\partial \boldsymbol{\xi}(\mathbf{w},y(\mathbf{w},p))}{\partial \mathbf{w}}$ will have non-positive diagonal entries (implying that the matrix is negative semi-definite). Considering the off-diagonal entries however, the signs are contingent on whether factors are substitutes or complements. But we actually know even more: Nagatani (1978) shows that the effect of a factor's price change on its own demand is always non-positive, so also the matrix of total effects is negative semi-definite. (See e.g. Sakai (1973) for a rigorous analysis of substitution and scale effects.) In summary: while it is clear that demand for each input is non-increasing in *its own* price, the effect on demand for another input is *a priori* uncertain.

4.1.2 Labor and capital demand

- Introduce capital: capital is fixed in the short run => labor and capital are substitutes only in the long run, complements in the short run => capital intensive sectors are likely to be less affected by reform and/or have lagged treatment effect – Test!
- Treatment dose (TD): number of years with reduced payroll taxes. The reductions for future years are discounted with probability of being employed in the longer run, i.e. for short-term jobs future years are discounted more heavily. Consequence: TD should matter less for jobs with low expected employment length, i.e. low-skilled jobs – Test!

4.2 Labor Supply

Without presenting an elaborate model, we will briefly discuss the determinants of labor supply, and how it might respond to a reduction in payroll taxes.

An individual deciding whether or not to join the labor force is, presumably, influenced by two main factors: the perceived probability of finding employment (i.e. the matching rate), and, conditional on employment, the expected wage:

$$l_i = f\big(\,\mathbb{E}_i[\Pr(e_i)], \mathbb{E}_i[w_i|e_i]\,\big) \tag{1}$$

[6]Equivalently, the effect of a discrete change in the price of input k on the demand for input i can be expressed thusly:

$$\frac{\Delta x_i}{\Delta w_j} = \underbrace{\frac{\xi_i(\mathbf{w}+\Delta\mathbf{w}, y(\mathbf{w},p)) - \xi_i(\mathbf{w},y(\mathbf{w},p))}{\Delta w_j}}_{\text{Substitution effect}} + \underbrace{\frac{\xi_i(\mathbf{w}+\Delta\mathbf{w}, y(\mathbf{w}+\Delta\mathbf{w},p)) - \xi_i(\mathbf{w}+\Delta\mathbf{w},y(\mathbf{w},p))}{\Delta w_j}}_{\text{Scale effect}}.$$

See Sakai (1974) for details.

Figure 1: The idealized impulse response of the 2007 reform.

Apart from longer-term equilibrium effects, discussed below, change in labor demand may influence labor supply in a more immediate fashion through the expectation operators in the above expression. Indeed, the payroll tax cut in 2007 was explicitly motivated by the supposition that it would increase labor demand. If this conjecture was shared by the general public, the reform had an immediate effect on expected future labor demand. Such an effect would then enter equation 1 through both of its arguments:

1. Increased expected labor demand => increased expected probability of getting employed => increased labor supply

2. Increased expected labor demand => more wage bargaining power => higher expected wage => increased labor supply.

[Labor force participation to be examined later through effects on education participation.]

4.3 Equilibrium Effects

The impact of increased labor demand depends generally on the response of labor supply. However, the labor market in Sweden for age groups 18–24 is essentially demand constrained—indeed, high youth unemployment was the very *rationale* for the reform. This means that the response in wage adaption to higher labor demand will be crucial. But in the short run, wages are likely to be sticky. There are at least two reasons why this should be the case. First, in Sweden a large share of wage contracts are collectively negotiated through unions.[7] This generally means that in the short run, wages cannot adapt to a change in labor demand. This holds in particular for the kind of low skilled jobs that the reform is likely to boost. And even for wages negotiated individually, it presumably takes some time for workers to discover their increased bargaining power. The notion of sticky wages is supported by a rich theoretical literature.[8]

Consequently, labor supply primarily affects measures of unemployment. Since we study employment rate, this is not of major concern to us.

Summary of expected effects (to be cleaned up, obviously):

- short run effect (presupposes pool of unemployed ready to work) depends on elasticity of labor demand

- speed of wage adaption depends on unionization (long-term fixed wage increases)

- degree of wage adaptation depends on elasticity of labor supply (short run: pool of unemployed, long run: labor force entry)

- we should always expect a strictly positive long-term effect (though possibly small)

The causal effect of the reform on a group affected by the reform can be characterized by an impulse response. Figure 1 shows the idealized impulse response on employment for a group positively affected by the reform. The impulse response is flat until the reform occurs. At the time of the reform, we should get a peak as the price of labor decreases. However, as wages

[7] Sources to be added...

[8] The general theory of staggered wage contracts are presented in Taylor (1980) and Calvo (1983). There are also numerous articles in the New-Keynesian field of research, e.g. Ball et al. (1988), or in a dynamic stochastic general equilibrium setting, Blanchard and Galí (2007).

adapt, the reform effect should decrease. In the long run, labor supply should respond as well, limiting the wage increases. So eventually we should approach a small, but strictly positive, reform effect, representing a higher equilibrium demand due to lower factor prices.

5 Empirical Strategy

5.1 Regression Discontinuity

At first thought, using Regression Discontinuity (RD) seems ideal for estimating the effect of the 2007 reform, estimating employment rate discontinuities at cohort boundaries: Assuming individuals productivity, on average, increases continuously with age and that a workers' probability to be hired depends directly on his/her productivity, we would observe a continuous relation between age and employment rate in absence of payroll tax subsidies, while any reform effects would show up as discontinuities at the break between december and januari for each cohort. The key assumption here is that employment rate, in absence of the reform, depends continuously on age. Unfortunately, this assumption is likely not to hold. In Sweden, children typically start compulsory school the year they turn seven. This means that individuals born just after the new year start school a year later than individuals born just before. Exploiting this school start discontinuity at new year, Fredriksson and Öckert (2005) show that children starting school at an older age (i.e. just after the new year) do better in school, have better educational attainments, and have higher long-run earnings.[9] This implies that individuals born just after the new year are more productive and hence more attractive for employers. The discontinuities stemming from an early school start coincides exactly with our reform effects and thus our estimates of the reform effect is biased upwards.

One possibility, though not further investigated at this point (read term paper submittance), is to acknowledge that the RD estimate is biased, but to look at successive RD estimates over time and examine a potential break at the time of the reform. This leads to a Difference-in-Discontinuities estimator, or if the RD estimates are continuous but not stable in the pre-reform period, a Discontinuity-in-Discontinuities estimator.

5.2 Difference in Differences

We use the method of Difference-in-Differences (DD) to estimate the effect of the 2007 reform. Our period of study is 2001–2008, which means there are 18 months with the new tax regime in place. The most straightforward setup would be to study how employment for 18–24-year-olds evolve over time relative to some well defined control group. Figure 2 plots employment over time for workers of different ages. As seen, there is substantial seasonal variation: for some age groups, employment grows with more than a third during the summer months. Further, it seems as if the seasonal patterns change over time and that they change differently depending on age. This means employment trends for different age groups do not move in parallel before the introduction of the new rules (indicated by the vertical line) and consequently the key assumption when using DD is likely to be violated.

One way to handle the problem with changing seasonal patterns is to model the seasons using time series econometrics but this requires strong assumptions on functional form. We instead handle the problems with seasonality by studying the difference in employment *for given months* over many years. Note that we can still use the DD estimator, the only difference from the standard approach is that we now fix the month under study (instead of analyzing 2 groups of

[9] Since starting school later implies a delayed enter into the labor market the net earnings effect over the entire life-cycle is negative.

Figure 2: Employment rate for different age groups.

time series in 12×8 time periods, we now analyze 12×2 groups of time series in 8 time periods). Note that the parallel trends assumption still needs to be validated.

All workers aged 18–24 are eligible for the subsidy which means defining treated individuals is straightforward. The reform created two cut-off points but we only make use of the upper one since data on employment for individuals under 18 are unreliable. Consequently, the treatment groups in our analysis consist of workers aged 22–24. Importantly, within the target group, a worker's age determines how many years he or she can be exempted. Since remaining treatment years equals 24 minus age, the "treatment dose" is decreasing in age. To see if this results in a decreasing reform effect in age, we estimate treatment effects for each age group seperately.

In principle, there are two reasons for why having historically parallel trends does not a guarantee a valid control group:

1. Control group is also affected by the treatment
2. There are other, omitted, factors that influence the outcome variable, coinciding with the treatment in time – i.e. timing of the treatment is non-random

In the present case, both of the above are potential problems. ...

Finding a suitable comparison group among the untreated (i.e. those older than 24) is one of the major issues we have to deal with in order to estimate casual effects. It is tempting to think that individuals just above the upper cut off constitute the best reference since they should be similar to the treated in many ways. However, one problem with using 25-years-olds is that they are close substitutes to workers in the treatment group and thus they might be suspected to be more negatively affected by the reform than older individuals. This means we would overestimate the reform effect. On the other hand, moving too far away from the cut-off is likely to give a group that is different with respect to other characteristics.[10] Ideally, one would like to find a group of workers who are in fact in the age group 18–24 but still not affected by the new rules. Unfortunately, such a group is difficult to find and thus we face a tough balance act of finding a comparison group that is similar enough in one dimension (observed as well as unobserved characteristics), but not too similar in another one (degree of substitution). With this in mind we choose to use 26-year-olds as our primary reference group. Since individuals who are 25 years old in 2008 are treated the first six months and untreated the last 12 months, they are difficult to use for comparisons; we therefore exclude these individuals from our analysis.

[10] Individuals under the age of 25 face different life decisions than older individuals (choosing education, region of settlement etc).

Formally, we estimate the following basic Difference-in-Differences equation:

$$\begin{aligned} y_{i,t} = \alpha + &\sum_{t=2002}^{2008} \left[\beta_t year_t\right] + \gamma \cdot I(18 \leq age_i \leq 24) \\ + &\sum_{t=2006}^{2008} \left[\delta_t \cdot I(18 \leq age_i \leq 24, t \geq 2006)\right] + \boldsymbol{\theta}\mathbf{X}_{i,t} + \varepsilon_{age,t} \end{aligned} \tag{2}$$

For each month, the DD coefficients δ_t are estimated for 2006, 2007 and 2008, respectively. This means that for January to June, we get a single treatment effect (for 2008) and two pseudo-treatment effects, while for July to December we get pseudo-treatment for 2006 and reform effect for 2007 and 2008. Considering pseudo-treatment effects is crucial for evaluating the plausibility of the parallel trends assumption, i.e. that the control group trend represent the counter-factual trend for the treatment group. $\mathbf{X}_{i,t}$ is a vector of control variables: a dummy for female, and township fixed effects. (Township is the smallest geographical administrative unit in Sweden. There are 2,237 townships in total.)

We also consider the aggregation over age groups, so that we are left with one observation per age group and time period (motivated by Moulton, Bertrand et al, etc.). For this sample, we estimate the following DD regression:

$$\begin{aligned} y_{age,t} = \alpha + &\sum_{t=2002}^{2008} \left[\beta_t year_t\right] + \gamma \cdot I(18 \leq age \leq 24) \\ + &\sum_{t=2006}^{2008} \left[\delta_t \cdot I(18 \leq age \leq 24, t \geq 2006)\right] + \varepsilon_{age,t} \end{aligned} \tag{3}$$

6 Data

We use register data on all individuals in Sweden aged 18–64 over the years 2001–2008, collected by Statistics Sweden (SCB). The data contains matched employer-employee data as well as education attainments and demographics for all individuals. Employments and their characteristics are given on a yearly basis, but with information on start and end months of each employment spell. This allows us to create a *quasi*-panel of monthly employment: while employer-employee matches are measured monthly, other employment characteristics (including total income received from each employer), as well as individual demographics, are given on a yearly basis. In addition to the full set of individuals, we use a subsample of all employees in November each year, containing information on monthly wage and hours worked per month.[11] We use this latter sample to link monthly income to employment.

Since income is only available yearly from each employer, monthly wage is calculated in the following way: for each individual, the employer-specific monthly wage is obtained as the mean monthly income over those months of the year the employment extends. Total monthly wage for each individual is the sum of all employer-specific monthly wages, for each month respectively. Note that this implies that monthly wage will be more precisely measured the shorter the duration of each employment spell (i.e. the more often the individual switches jobs during the year).

[11] Description: register data for state, county and municipal employees; survey data for a stratified sample of the private sector...

Table 1: Summary statistics for different age groups. Mean wage is calculated for the employed subsample (according to our definition, not including self-employed).

		2001	2002	2003	2004	2005	2006	2007	2008
Employment (in %)	20–22	55.7	55.2	53.2	51.4	51.8	55.1	57.7	55.5
	23–24	62.0	60.3	58.3	57.0	57.0	59.5	61.5	60.4
	25–28	70.6	68.9	67.3	65.6	65.7	67.3	68.6	67.5
Years of education	20–22	11.7	11.7	11.7	11.7	11.8	11.8	11.7	11.7
	23–24	12.2	12.2	12.2	12.3	12.3	12.2	12.3	12.2
	25–28	12.5	12.6	12.6	12.6	12.6	12.7	12.7	12.7
Monthly wage (SEK)	20–22	12 182	12 415	12 529	12 763	13 055	13 850	14 675	15 141
	23–24	14 446	14 573	14 600	14 822	15 085	15 798	16 603	17 382
	25–28	16 902	17 122	17 148	17 285	17 596	18 325	19 151	19 894

We conceptually define employment as working at least twenty-five percent of full-time, measured on a monthly basis.[12] Operationally, an individual is considered employed a specific month if she has a total monthly wage at or exceeding 25% of the lower bound of full-time wage. This lower bound is estimated separately for each combination of gender, years of age, years of education, and sector affiliation (whether the employer is a state, county, municipal, or private entity). For each of these subgroups, we obtain the actual distribution of contracted full-time wages from the subsample dataset, and stipulate the lower bound to be equal to the lowest fifth percentile of this distribution. This should give reasonable estimates of the lower bounds while still avoiding disturbances of potential outliers. In this fashion we arrive at an estimate of the number of individuals working at least quarter-time.

In table 1 the development of employment, education and wages is given for three different age groups. Older age groups display consistently better outcomes, and the recession 2001–2004 is visible (though somewhat lagged) in the employment numbers. We also note, as is well-known, that education attainment and wages are not cyclical.

7 Results

7.1 Employment

We start analyzing average monthly employment for individuals aged 22–26. For each age group, the analysis is carried out for two separate subgroups: the first one including all workers and the other one restricted to individuals having 12 years of education (high school). The reason for this is that the latter subgroup should be more homogeneous, making the parallel trends assumption more plausible.

Figures 4 and 5 in the appendix show how employment for specific months evolves over time. Importantly, in each graph, employment for treated and control seems to move in parallel before July 2007 (marked by the dotted vertical line); this indicates that the parallel trends assumption is likely to be satisfied (Note that we perform stronger tests of this assumption below). Judging from the graphs alone, there are no major employment effects of the 2007 reform. Tables 2 and 3 presents the estimated effects from the DD models, measuring the reform effect on employment rate. We start off by interpreting the results for the aggregated sample. The easiest way to

[12]This means that if an individual works full-time the first quarter of a month and is unemployed the rest of the month, she is, falsely, considered to be employed the whole month (since she works 25% of a full-time month), though in reality, she is only employed the first quarter of the month.

read the table is to start at the sixth column which gives estimates for July, the first month of the reform. As seen in the second row, employment for 24-year-olds was significantly higher in July 2007—the first month with the new rules—relative to employment in July 2001-2005. The estimate for July 2008 is also significant at the 5% level, indicating the the effect is still there one year after the reform. Importantly, the insignificant estimate for July 2006 in the first row gives support for our parallel trends assumption. However, we cannot neglect the fact that there is virtually no difference in the point estimates for the seperate years. This suggest that the overall support for a reform effect is small. The last five colums gives estimates for August–December respectively. A close look at the estimates and their standard errors leads us to the same conclusion as before: in none of the months studied there seem to be an impact on employment for 24-year-olds. In the first six columns, which gives coefficients for January–June respectively, the first two rows represents the period before the reform and the third row the period after. As seen, all coefficients for years 2006 and 2007 are significant (some of them even at the 1% level) which means the parallel trends assumption does not hold. Consequently, we should be careful to draw any strong conclusions, though there are weak evidence for an employment effect for 24-year-olds. The lower two sections of the table show estimates for 23- and 22-year-olds respectively. The results for these age groups are difficult to interpret. It becomes clear that 26-year-olds is no longer a good comparison group—a majority of the coefficients in months before the reform are significant—and consequently it is not meaningful to make attempts at conclusions.

This far, we have grouped together all individuals wich means not only do we have the problem with comparing different cohorts but we also compare workers with for example different education (presumably this group is quite heterogenous, especially when it comes to educational background). One way to handle this is to control for education. In Table 3 we use only individuals with 12 years of education, i.e. we exclude those with less education than high school and those with higher education. As expected, the evidence is now much more clear cut. First, all coefficients in periods before the new rules are insignificant. This means we have good chances to say whether the reform had any effect or not. Interestingly, in contrast to the above, we can now compare even 22 and 23-year-olds with 26-year-olds. This is just a result of the fact that we observe a more homogenous group. Second, there is no evidence of any reform effect for any of our treatment groups.

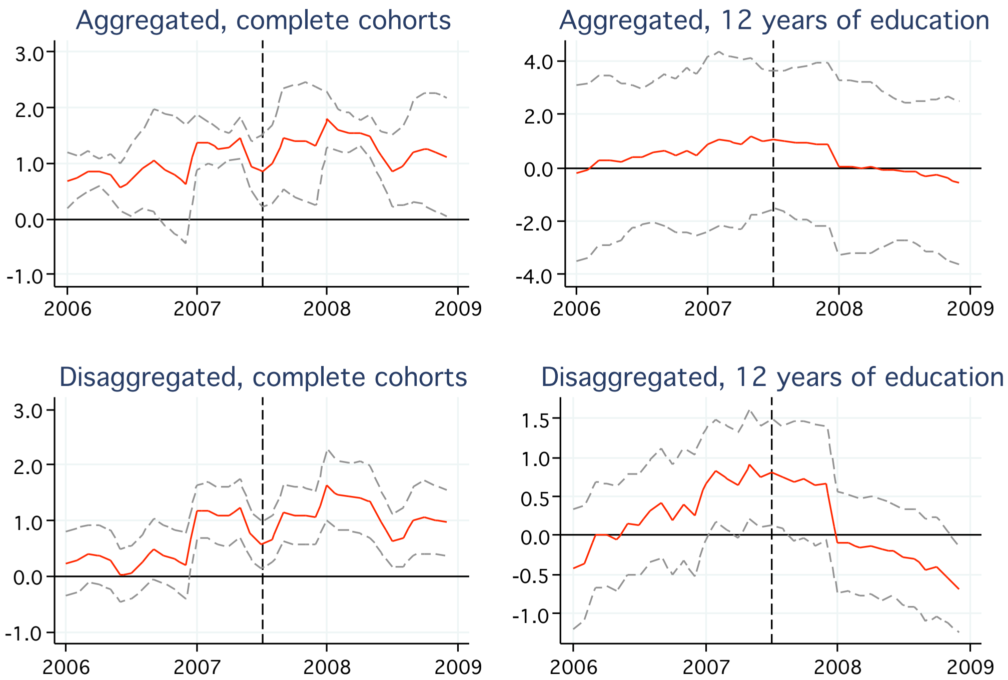
Summing up the conclusions from aggregate data, there is little evidence of any employment effect for 24-year-olds when we use the all individual sample. For 22 and 23-year-olds we cannot draw any conclusions. Studying a subset of individuals with the same educational background, it appears that the reform did not have any effects on employment neither for 24-year-olds nor for 23 and 22-year-olds.

So far we have only looked at aggregate data. Even though this should give a first indication, analyzing individual data is necessary to complete the picture. For convenience we follow the same structure as above, i.e. we first look at the full age groups, then looking at a less heterogenous group of workers. Table 4 and 5 show estimates. (Though the left hand side is a dummy variable, coefficients have been scaled by 100 for ease of interpretation, giving reform effect as percentage point change in probability of being employed.)

The results are quite similar to the aggregate case. When studying all individuals within age groups, it is only meaningful to draw conclusions for 24-year-olds, and again the evidence for any effect is small. Again, using the subsample of workers with only high school education, interpreting the coefficients gets easier. As before, there is now support for the parallel trends assumption, making it safer to draw the conclusion that there was no effect for any of our age group studied.

The estimates for each month can be compiled into a time series of estimates for 2006–2008; in

Figure 3: Series of DD estimates for 24-year-olds with 95% confidence intervals.



this fashion we obtain the impulse response of the reform, as discussed in section 4. Reiterating, the idealized impulse response should be flat pre-reform, rise to an initial peak at the time of the reform, and gradual attenuating towards a small, but positive long-run effect. It is clear that for the 24-year-olds, as depicted in figure 3, only the subsample restricted to 12 years of education comes anywhere close to this. Needless to say, however, we study but a short time period. [...]

7.2 Wages

7.3 Marginal groups

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Table 2: Difference-In-Differences estimations, aggregated (employment in %). Complete cohorts.

	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
24 vs. 26 (16 observations)												
DD 2006	0.69*	0.75*	0.86**	0.84**	0.78*	0.57*	0.70’	0.90’	1.05’	0.89	0.80	0.63
	(2.92)	(4.23)	(5.15)	(7.58)	(4.32)	(2.79)	(2.31)	(2.72)	(2.43)	(1.88)	(1.59)	(1.26)
DD 2007	1.38**	1.38**	1.27**	1.30***	1.46**	0.96**	0.86*	0.99*	1.45*	1.40*	1.39*	1.31’
	(5.85)	(7.81)	(7.56)	(11.7)	(8.04)	(4.67)	(2.81)	(3.00)	(3.35)	(2.96)	(2.78)	(2.62)
DD 2008	1.79**	1.59***	1.55***	1.55***	1.49**	1.14**	0.86*	0.96*	1.21*	1.26’	1.21’	1.12’
	(7.60)	(8.98)	(9.28)	(13.9)	(8.20)	(5.55)	(2.83)	(2.89)	(2.81)	(2.66)	(2.41)	(2.24)
R^2	1.000	1.000	1.000	1.000	1.000	0.999	0.998	0.998	0.998	0.998	0.998	0.998
23 vs. 26 (16 observations)												
DD 2006	1.12*	1.13*	1.18**	1.27***	1.32***	1.08’	1.12	1.31*	1.69**	1.60**	1.45**	1.34**
	(3.20)	(3.15)	(4.87)	(13.3)	(12.1)	(2.18)	(1.97)	(2.95)	(7.96)	(5.62)	(4.99)	(5.81)
DD 2007	2.48**	2.44**	2.37***	2.39***	2.52***	1.76*	1.58*	1.78*	2.36***	2.34**	2.34**	2.30***
	(7.08)	(6.84)	(9.77)	(24.9)	(23.1)	(3.55)	(2.79)	(4.01)	(11.1)	(8.22)	(8.07)	(10.0)
DD 2008	3.53***	3.34***	3.31***	3.40***	3.23***	2.20*	1.61*	1.79*	2.34***	2.37**	2.25**	2.14***
	(10.1)	(9.35)	(13.7)	(35.5)	(29.6)	(4.43)	(2.83)	(4.03)	(11.0)	(8.30)	(7.79)	(9.29)
R^2	1.000	1.000	1.000	1.000	1.000	0.997	0.995	0.998	1.000	1.000	1.000	1.000
22 vs. 26 (16 observations)												
DD 2006	1.77*	1.82’	1.78*	1.72*	2.07*	1.30’	1.22	1.49*	2.00**	2.01**	1.99**	1.88**
	(2.84)	(2.75)	(2.98)	(3.07)	(4.30)	(2.71)	(2.05)	(3.21)	(7.72)	(5.62)	(6.41)	(5.28)
DD 2007	3.38**	3.43**	3.34**	3.37**	3.64**	2.60**	2.15*	2.43**	3.33***	3.41***	3.49***	3.50***
	(5.42)	(5.17)	(5.59)	(6.01)	(7.58)	(5.46)	(3.62)	(5.25)	(12.8)	(9.54)	(11.2)	(9.81)
DD 2008	4.57**	4.36**	4.15**	4.14**	4.04**	2.74**	2.14*	2.30**	2.94***	3.14***	2.97***	2.82**
	(7.32)	(6.57)	(6.96)	(7.38)	(8.40)	(5.73)	(3.60)	(4.97)	(11.3)	(8.79)	(9.57)	(7.92)
R^2	0.999	0.999	0.999	0.999	0.999	0.998	0.996	0.998	1.000	1.000	1.000	1.000

t-statistics in parentheses. Year fixed effects included but not reported.
*** p<0.001, ** p<0.01, * p<0.05, ’ p<0.1

Table 3: Difference-In-Differences estimations, aggregated (employment in %). Conditional on 12 years of education

	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
24 vs. 26 (16 observations)												
DD 2006	-0.19	-0.10	0.27	0.29	0.23	0.42	0.42	0.59	0.67	0.44	0.66	0.49
	(-0.12)	(-0.068)	(0.18)	(0.19)	(0.17)	(0.33)	(0.35)	(0.48)	(0.50)	(0.33)	(0.45)	(0.34)
DD 2007	0.89	1.08	0.99	0.90	1.16	0.99	1.05	0.99	0.91	0.93	0.88	0.88
	(0.57)	(0.70)	(0.66)	(0.60)	(0.84)	(0.78)	(0.87)	(0.79)	(0.68)	(0.69)	(0.61)	(0.61)
DD 2008	0.026	0.043	-0.0071	0.016	-0.050	-0.062	-0.14	-0.16	-0.32	-0.28	-0.40	-0.56
	(0.017)	(0.028)	(-0.0047)	(0.011)	(-0.036)	(-0.049)	(-0.12)	(-0.13)	(-0.24)	(-0.21)	(-0.28)	(-0.39)
R^2	0.946	0.947	0.945	0.942	0.945	0.937	0.939	0.934	0.941	0.948	0.942	0.945
23 vs. 26 (16 observations)												
DD 2006	-0.32	-0.20	0.065	0.42	0.57	0.64	0.67	0.92	1.13	1.09	1.16	0.88
	(-0.17)	(-0.11)	(0.036)	(0.24)	(0.38)	(0.43)	(0.46)	(0.65)	(0.74)	(0.73)	(0.77)	(0.59)
DD 2007	1.43	1.54	1.54	1.38	1.63	1.24	1.16	1.23	1.46	1.40	1.42	1.43
	(0.76)	(0.85)	(0.85)	(0.77)	(1.09)	(0.84)	(0.80)	(0.86)	(0.95)	(0.93)	(0.94)	(0.96)
DD 2008	1.50	1.45	1.46	1.52	1.44	1.11	0.74	0.76	0.77	0.69	0.49	0.27
	(0.80)	(0.80)	(0.80)	(0.86)	(0.95)	(0.75)	(0.51)	(0.53)	(0.50)	(0.46)	(0.33)	(0.18)
R^2	0.964	0.965	0.961	0.958	0.963	0.941	0.935	0.940	0.958	0.965	0.967	0.970
22 vs. 26 (16 observations)												
DD 2006	0.100	0.27	0.55	0.57	1.10	1.03	0.91	1.24	1.35	1.33	1.60	1.34
	(0.039)	(0.11)	(0.23)	(0.26)	(0.60)	(0.61)	(0.55)	(0.81)	(0.81)	(0.77)	(0.89)	(0.73)
DD 2007	2.51	2.80	2.84	2.85	2.99	2.39	2.15	2.23	2.69	2.80	2.98	2.92
	(0.98)	(1.11)	(1.19)	(1.31)	(1.63)	(1.41)	(1.29)	(1.45)	(1.62)	(1.63)	(1.66)	(1.58)
DD 2008	3.02	3.05	2.80	2.66	2.42	1.81	1.40	1.48	1.67	1.68	1.56	1.32
	(1.18)	(1.21)	(1.17)	(1.23)	(1.32)	(1.07)	(0.84)	(0.97)	(1.01)	(0.98)	(0.87)	(0.72)
R^2	0.970	0.970	0.969	0.970	0.972	0.952	0.944	0.956	0.975	0.978	0.977	0.979

t-statistics in parentheses. Year fixed effects included but not reported.
*** p<0.001, ** p<0.01, * p<0.05, ' p<0.1

Table 4: Difference-In-Differences estimations, disaggregated (employment in %). Complete cohorts.

	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
					24 vs. 26 (1,728,428 observations)							
DD 2006	0.22	0.29	0.40	0.37	0.29	0.018	0.068	0.25	0.49'	0.38	0.30	0.19
	(0.77)	(0.97)	(1.53)	(1.37)	(1.07)	(0.075)	(0.28)	(0.99)	(1.76)	(1.44)	(1.09)	(0.62)
DD 2007	1.17***	1.19***	1.08***	1.08***	1.22***	0.74**	0.57**	0.67**	1.14***	1.09***	1.09***	1.05***
	(4.75)	(4.56)	(4.17)	(3.93)	(4.53)	(3.31)	(2.65)	(3.08)	(4.43)	(4.06)	(4.21)	(4.32)
DD 2008	1.65***	1.46***	1.44***	1.41***	1.34***	0.98***	0.64**	0.70**	1.00**	1.07**	1.02**	0.96**
	(5.12)	(4.62)	(4.75)	(4.26)	(4.09)	(3.42)	(2.61)	(2.61)	(3.17)	(3.18)	(3.22)	(3.16)
R^2	0.049	0.049	0.048	0.048	0.046	0.034	0.031	0.032	0.041	0.044	0.045	0.044
					23 vs. 26 (1,725,640 observations)							
DD 2006	0.66*	0.66*	0.71*	0.78**	0.80*	0.45	0.41	0.61*	1.12***	1.10***	0.97***	0.89**
	(2.20)	(2.15)	(2.36)	(2.66)	(2.56)	(1.46)	(1.50)	(2.21)	(4.17)	(3.83)	(3.39)	(2.97)
DD 2007	2.15***	2.12***	2.05***	2.04***	2.17***	1.38***	1.13***	1.31***	1.92***	1.96***	1.95***	1.95***
	(7.57)	(7.23)	(6.68)	(6.50)	(6.43)	(5.10)	(4.65)	(4.94)	(7.41)	(7.08)	(6.84)	(6.46)
DD 2008	3.26***	3.08***	3.05***	3.12***	2.96***	1.94***	1.31***	1.48***	2.04***	2.12***	2.02***	1.90***
	(10.9)	(9.78)	(9.75)	(9.14)	(8.81)	(5.85)	(4.22)	(4.49)	(5.75)	(5.62)	(5.43)	(5.17)
R^2	0.053	0.054	0.053	0.053	0.051	0.034	0.031	0.033	0.046	0.050	0.051	0.051
					22 vs. 26 (1,728,378 observations)							
DD 2006	1.23***	1.32***	1.29***	1.25***	1.57***	0.76**	0.62*	0.88**	1.47***	1.54***	1.53***	1.47***
	(4.18)	(4.45)	(4.68)	(4.48)	(5.65)	(2.80)	(2.25)	(3.02)	(4.65)	(5.09)	(5.30)	(4.77)
DD 2007	2.90***	2.98***	2.91***	2.95***	3.23***	2.27***	1.77***	2.03***	2.85***	2.97***	3.03***	3.09***
	(11.2)	(12.1)	(10.9)	(10.9)	(12.3)	(9.04)	(6.93)	(8.97)	(11.1)	(11.6)	(11.6)	(11.1)
DD 2008	4.15***	3.99***	3.80***	3.81***	3.74***	2.54***	1.91***	2.06***	2.65***	2.88***	2.71***	2.58***
	(13.1)	(12.1)	(11.5)	(11.6)	(12.1)	(8.74)	(7.16)	(7.02)	(7.75)	(8.19)	(8.05)	(7.75)
R^2	0.055	0.055	0.055	0.054	0.051	0.034	0.030	0.032	0.047	0.052	0.053	0.054

t-statistics in parentheses robust to clustering on 291 municipalities. Female, year and township fixed effects included but not reported.
*** $p<0.001$, ** $p<0.01$, * $p<0.05$, ' $p<0.1$

Table 5: Difference-In-Differences estimations, disaggregated (employment in %). Conditional on 12 years of education.

	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
24 vs. 26 (685,657 observations)												
DD 2006	-0.43	-0.35	0.0050	0.012	-0.041	0.15	0.14	0.31	0.42	0.20	0.39	0.26
	(-1.10)	(-0.93)	(0.015)	(0.035)	(-0.12)	(0.46)	(0.42)	(0.94)	(1.18)	(0.57)	(1.07)	(0.65)
DD 2007	0.66'	0.83*	0.74*	0.64'	0.92*	0.76*	0.81*	0.75*	0.70'	0.72'	0.65	0.68'
	(1.82)	(2.49)	(2.18)	(1.86)	(2.59)	(2.27)	(2.36)	(2.22)	(1.79)	(1.89)	(1.64)	(1.83)
DD 2008	-0.091	-0.096	-0.15	-0.13	-0.18	-0.19	-0.28	-0.29	-0.44	-0.40	-0.53'	-0.68*
	(-0.27)	(-0.31)	(-0.47)	(-0.40)	(-0.56)	(-0.62)	(-0.87)	(-0.92)	(-1.30)	(-1.22)	(-1.78)	(-2.41)
R^2	0.037	0.037	0.037	0.037	0.037	0.036	0.035	0.035	0.036	0.037	0.036	0.036
23 vs. 26 (712,350 observations)												
DD 2006	-0.68*	-0.58'	-0.33	0.054	0.23	0.25	0.28	0.53'	0.79*	0.79*	0.87*	0.62'
	(-2.11)	(-1.76)	(-1.01)	(0.18)	(0.70)	(0.77)	(0.92)	(1.73)	(2.32)	(2.37)	(2.54)	(1.69)
DD 2007	1.03**	1.11***	1.10***	0.98***	1.28***	0.85***	0.77**	0.83***	1.08***	1.04***	1.07***	1.11**
	(3.15)	(3.65)	(3.66)	(3.38)	(4.71)	(3.48)	(3.27)	(3.50)	(3.95)	(3.43)	(3.33)	(3.04)
DD 2008	1.25***	1.19***	1.19***	1.29***	1.25***	0.90**	0.53'	0.53'	0.55'	0.49	0.30	0.086
	(3.91)	(3.73)	(3.62)	(3.90)	(3.71)	(2.71)	(1.71)	(1.77)	(1.70)	(1.47)	(0.98)	(0.29)
R^2	0.037	0.036	0.036	0.036	0.036	0.034	0.033	0.033	0.035	0.037	0.037	0.037
22 vs. 26 (760,701 observations)												
DD 2006	-0.52	-0.34	-0.041	0.025	0.63'	0.54	0.40	0.73*	0.87*	0.85**	1.11***	0.87*
	(-1.44)	(-0.99)	(-0.12)	(0.077)	(1.80)	(1.54)	(1.20)	(2.23)	(2.54)	(2.60)	(3.36)	(2.47)
DD 2007	1.85***	2.14***	2.19***	2.24***	2.47***	1.87***	1.62***	1.68***	2.13***	2.23***	2.39***	2.35***
	(5.68)	(7.34)	(7.54)	(7.12)	(8.71)	(6.79)	(5.98)	(6.27)	(7.50)	(7.74)	(8.22)	(7.75)
DD 2008	2.48***	2.51***	2.28***	2.18***	2.04***	1.46***	1.04**	1.09**	1.26***	1.26***	1.14***	0.89**
	(7.21)	(7.65)	(6.82)	(6.41)	(6.00)	(4.50)	(3.16)	(3.18)	(3.65)	(3.71)	(3.37)	(2.76)
R^2	0.038	0.038	0.037	0.036	0.035	0.031	0.030	0.030	0.035	0.037	0.037	0.038

t-statistics in parentheses robust to clustering on 291 municipalities. Female, year and township fixed effects included but not reported.
*** $p<0.001$, ** $p<0.01$, * $p<0.05$, ' $p<0.1$

Figure 4: Employment trends for age groups 26, 24, 23, 22 (highest to lowest trend). Yearly changes, for each month respectively.

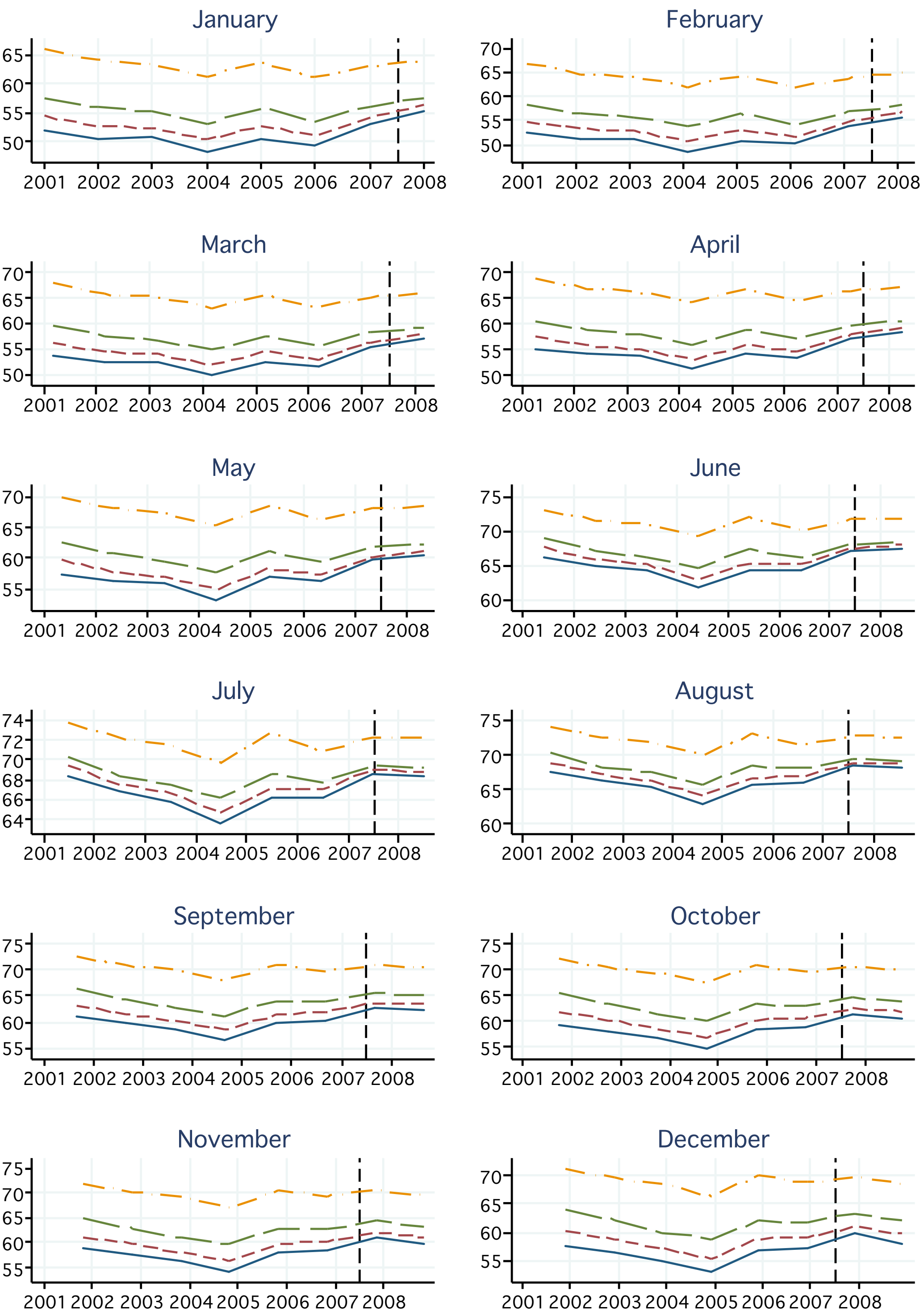

Figure 5: Employment trends conditional on 12 years of education, for age groups 26, 24, 23, 22 (highest to lowest trend). Yearly changes, for each month respectively.