Labour demand and supply changes in Norway following an imposed harmonization of geographically differentiated payroll-tax rates

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Abstract. The payroll tax is an important source for public financial revenues. Utilizing Norwegian population-wide linked-employer-employee register-data for 2001–12 while exploiting the harmonizing of the geographically differentiated payroll tax imposed by the EU, I show that payroll tax increases are shifted onto workers' hourly wages thus reducing these. Annual earnings also drop as the payroll tax grows. Neither unemployment nor employment levels are affected in the short-run, but the payroll-tax levels and the number of sick leave days appear positively correlated. In the long run, entry into retirement and DI rolls increase. Thus the tax hike induces the withdrawal of workers from the labour force.

1. Introduction

In most countries taxes on firms and people are essential for financing public activities and redistributive purposes. Payroll taxes provide on average 25 per cent of total revenue in OECD countries (OECD, 2008), thus they are obviously important for public revenues. When public finance is pressed, it might be tempting to increase the payroll tax to bolster public tax revenues and thus ensure financial sustainability of public programmes and expenses. Similarly, by providing reduced payroll tax in certain regional areas one might provide regional support. The key question is how the payroll tax affects wages and employment? In addition, important secondary effects might turn up when analysing other outcomes than employment and wages. Little is gained, if extra public revenues gained when increasing the payroll tax, is lost to increased public expenditures on unemployment benefits, social security disability insurance (SSDI) programs and health care.[1]

In this paper, I study how a Norwegian payroll tax reform influenced labour demand, wages and employment outcomes, and long-term outcomes such as retirement and inflow to SSDI rolls. Several studies, which I will return to below, have analysed the impact of payroll tax reforms on wages and employment outcomes revealing mixed results. What

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makes the Norwegian reform unique is that it was imposed on employers by forces external to Norwegian politics, thus allowing causal identification of the reform's impact. Tax reforms often follow from domestic internal reasons which potentially cause bias and make evaluation difficult.

The Norwegian payroll-tax system was characterized by geographically differentiated tax rates, and was supported by all political parties. However, after being criticized and contested by the body surveying the trade agreements between the EU member states and Norway for several years, Norway was forced to gradually disband the system from 2004 to 2006. This induced a forced payroll tax variation that is affecting quite a number of workers, allowing causal identification of how payroll-tax changes, through labour demand, yields labour supply responses.

Theoretically it is well-known that the effects of the payroll tax on employment are ambiguous and depend on the how wages are set, how elastic labour demand is, how elastic labour supply is, and finally, how strong the tax-benefit link (Hamermesh, 1993; Kugler and Kugler, 2009). Empirical studies mostly reveal evidence of a shifting of payroll taxes onto wages, usually observing increased wages when the tax burden is eased (Bennmarker *et al.*, 2009; Gruber, 1997), and negligible labour supply responses (Saez *et al.*, 2012). The evidence on how the entry to DI rolls and retirement are affected by payroll-tax changes is hereto unexplored.

In Section 2 I describe the Norwegian payroll-tax reform. Section 3 reviews the literature on the relationship between labour demand and payroll taxes, it describes how wages are determined in Norway, and it presents the Norwegian sickness absence, disability insurance, and retirement system. The econometric model and empirical strategy are described in Section 4, whereas data are presented in Section 5. My results are presented in Section 6. Section 7 briefly concludes.

2. The Norwegian payroll-tax reform

The Norwegian payroll-tax system with geographically differentiated tax rates is one of the most important government policies supporting activities in areas facing economic hardship, i.e. to provide incentives for and stimulate economic activities in low tax areas and thus to ensure, to stimulate and to help people to live in all parts of Norway. This policy has been up-hold by both left- and centre-right coalition governments.

The Ministry of Finance determines each year the payroll tax for each geographically differentiated zone, and they determine which municipalities belongs to which zones. These tax rates were made public at the end of the previous year, i.e. the policy for year t is made public late in year $t-1$. If or when the Ministry of Finance is of the opinion that the economic environment has changed in a more permanent way for municipalities or areas, they might alter the tax rates appropriately. Mostly, only small changes occurred from one year to another.

This differentiated payroll tax was criticized and contested by the EFTA Surveillance Authorities (ESA), which originally considered this practice against the European Economic Area agreement between EU and the EEA-EFTA countries of Norway, Lichtenstein, and Iceland.[2] On May 20, 1999, the EFTA court ruled against Norway, i.e. it declared that the payroll-tax system was to be considered government subsidies against the agreement. As expected, this decision was heavily contested by the Norwegian government.

As comprise between EU and Norway for 2000–2003 (when the new legislation should be re-evaluated), exemptions from tax harmonization were introduced which in practice

postponed the forced harmonization for most firms.[3] Sweden, a member of the EU who experienced that a similar differentiated tax for Northern Sweden was rejected by the EU Commission in 2000, complained to the EU Commission about the unfairness that Norway was in practice allowed to continue their tax regime. The EU Commission acknowledged this unfairness, and gave ESA the task of enforcing fair practice. ESA requested Norwegian government response by March 25th, 2003. As to bring the case once again to the EFTA court was not expected to yield a new ruling, Norway choose gradually to disband the practice, starting from January 1st, 2004.[4] Thus January 1st, 2004 in practice marks the start of the reform period.[5]

From 2004 and onwards low tax areas gradually experienced higher taxes. Freight-transport-by-road firms were to face the full payroll tax (i.e. 14.1 per cent) regardless of workers' residence. To soften the impact of the reform, firms with workers residing in a zone experiencing increased tax were allowed to pay the original low payroll tax until the difference between this aggregate sum across all their workers and what they should have paid given the new tax rules reached 270,000 Nok. This induced a threshold for firms, which determined those influenced by the new tax (most with more than 20–30 employees faced the new tax).

Table 1 shows the development of the payroll-tax rate during the period 2001–09. Before 2007 only five tax zones exist, comprising tax rates from zero to 14.1 per cent (except for the top 5 percentiles of the income distribution).

Norway's municipalities are grouped into these zones.[6] Zone I, representing the municipalities not being subsidized, faces the full tax rate of 14.1 per cent. Zone V, the zero tax area, constitutes Norway's northernmost county (all municipalities) together with a number of municipalities along the border in the neighbouring county. This is also clearly seen in Figure 1, which shows the geographical location of the zones and how the basic payroll tax (not for those at the top of the income distribution) changed from 2003 to 2006.

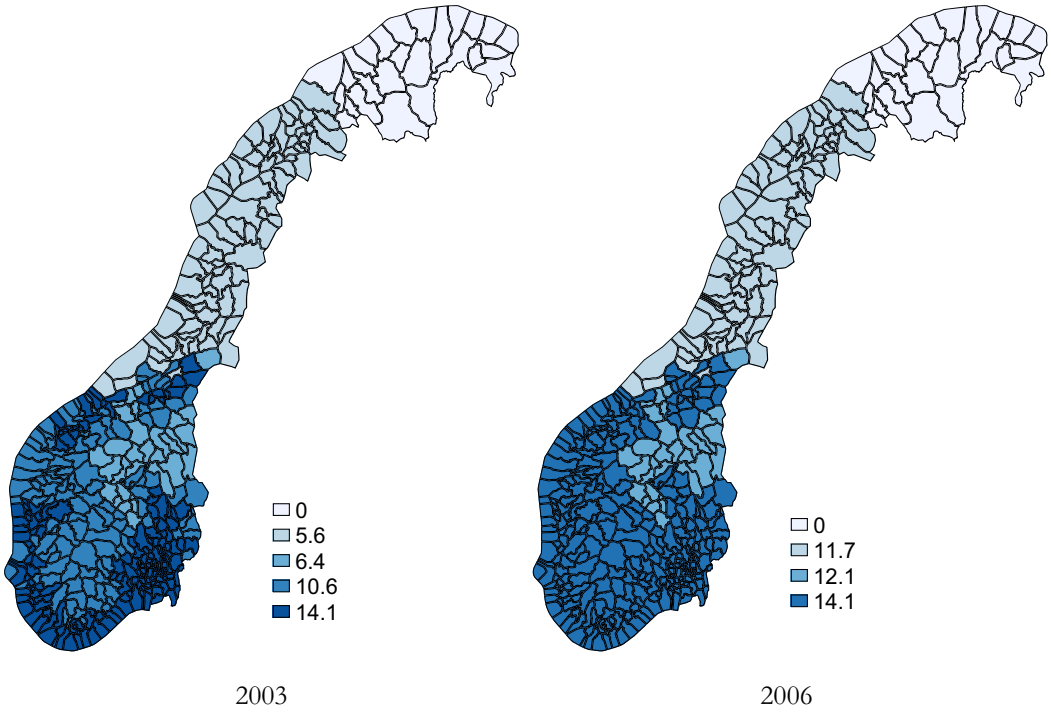
As seen in Table 1, from 2004, following the EFTA court ruling of 1999 and final decision in 2003, the tax rates of zones II-IV were increased slowly. In 2005 the additional top tax for high paid workers was completely abolished.

In 2007, following the 2005 European Commission's guidelines for how to provide regional support, the reform was reversed. However, three exceptions were made: (1) freight-

Table 1. The payroll tax reforms in Norway 1999–2009

	Payroll tax zones								
Year	**I**	**Ia**	**II**	**III**	**IV**	**IVa**	**V**	**Earnings threshold (NoK)**	**Extra percentage points tax**
1999	14.1		10.6	6.4	5.1		0	742,768	12.5
2000	14.1		10.6	6.4	5.1		0	774,032	12.5
2001	14.1		10.6	6.4	5.1		0	809,648	12.5
2002	14.1		10.6	6.4	5.1		0	851,728	12.5
2003	14.1		10.6	6.4	5.1		0	883,728	12.5
2004	14.1		14.1	8.3	7.3		0	930,224	12.5
2005	14.1		14.1	10.2	9.5		0	969,944	12.5
2006	14.1		14.1	12.1	11.7		0	No threshold	0
2007	14.1	10.6 (14.1)	10.6	6.4	5.1	7.9	0	No threshold	0
2008	14.1	10.6 (14.1)	10.6	6.4	5.1	7.9	0	No threshold	0
2009	14.1	10.6 (14.1)	10.6	6.4	5.1	7.9	0	No threshold	0

Figure 1. The imposed payroll tax harmonization in Norway from 2003 and 2006



Note: The period of forced/imposed harmonization of the payroll-tax zones is from 2004 to 2006. After 2006 the payroll-tax rates revert with few exceptions back to the pre-reform levels (see text for details).

transport-by-road firms were still to face the full payroll tax (i.e. 14.1 per cent); (2) 53 municipalities originally belonging to zone 2 were moved to a new sub-zone category (1a), were the size of the workplace total wage bill determined the payroll tax (as regional support due to bad economic performance)[7] ; (3) the municipalities of Bodø and Tromsø would constitute a new zone (IVa) and face an higher tax than the other municipalities in their original zone (7.9 instead of 5.1 per cent). Furthermore, they decided that workers' payroll-tax zone was to be determined by the workplace's location (and not the worker's residence).

3. Labour demand, wages, benefits, and payroll taxes

3.1 Previous literature on labour demand, wages, and payroll taxes

As pointed out in the introduction, theoretically the effects of the payroll tax on employment are ambiguous. This depends on the wage-setting regime, on the elasticities of labour demand and labour supply, and on how strongly related taxes are to benefits (Kugler and Kugler, 2009). As Kugler and Kugler show, the negative impact of payroll-tax rate on wages is increasingly offset as the elasticity of labour supply increases. The same is true if the levels of benefits and taxes are less related. Similarly, the negative impact is enforced as the elasticity of demand increases. This ability to shift the payroll tax onto wages is then crucial for the impact of the payroll tax on employment. When the tax-benefit link is

perfect, perfectly inelastic labour supply or perfectly elastic labour demand, then payroll tax is fully shifted onto wages and no employment effect follows. However, when wages act motivational, employers might respond to a payroll tax hike by shredding worker while intensifying the required effort of those remaining, thus causing a positive relationship between payroll-tax hikes and wages (Lang, 2003).

Over a more than a decade, empirical studies reveal mixed evidence of a shifting of payroll-taxes onto wages. In as widely different countries as Chile, Finland, Sweden, and Colombia, they find increased wages when the tax burden is eased (Anton, 2014; Bennmarker et al., 2009; Gruber, 1997; Korkeamäki and Usitalo, 2009). Anton (2014) also finds positive employment effects following the reduced tax burden. More seldom, reduced wages are observed when the tax burden is increased in the U.S. (Anderson and Meyer, 2000, 2006). Finally, Saez et al. (2012), exploiting a unique tax reform in Greece allowing analyses of long-term effects, find no evidence of labour supply responses neither along extensive nor intensive margins, which implies that shifting occurs.

On the other hand, when Dyrstad and Johansen (2000) and Carlsen and Johansen (2005) analysed payroll-tax variation in the 1990s on regional data, they found that in the short-run shifting might occur, but in the long-run no shifting was found. Similarly, on French data, Lehmann *et al.* (2013) identify no relationship between the labour income elasticity and the payroll-tax rate elasticity, indicating that no shifting occurs.

The empirical evidence is also mixed when one reviews the literature on regional support. To let some regions experience reduced payroll tax is a regional support policy established in many countries (e.g. Argentine, Norway, Turkey, and Finland). In this literature one finds on one hand, Korkeamäki (2011) which concludes that no aggregate effects are actually found in Finland from payroll tax cuts. This also implies that the regional differentiated payroll tax reform in this case is less successful in providing regional support. Similarly, also Huttunen *et al.* (2013) finds small and negligible effects in Finland when targeting regional support for elderly workers. In Argentine local variation and changes in the payroll tax appears to be shifted unto wages, thus yielding few employment effects (Cruces *et al.*, 2010). On the other hand, when Betcherman *et al.* (2010) analyse a more extensive regional support they find evidence supportive of such systems.

3.2 Norwegian wage setting

Although any impact of the payroll tax on employment is crucially dependent on how it affects wages, it is necessary to describe the Norwegian wage-setting regime. Norway is characterized by a centralized wage-bargaining system. Wallerstein (1999) ranks Norway ranks as the most centralized country, whereas UK, Canada, and the USA rank as the least centralized. However, this system has a considerable element of local wage bargaining and individual pay setting (Bowman, 2002). A firm might be covered by collective bargaining at the local level (workplace or firm), at the sectoral level, at the national level, or a combination of local and sectoral/national bargaining. In practice, 70 per cent of all workers are employed in firms where local bargaining is following the national or sectoral bargaining rounds (Barth *et al.*, 2014). On the other hand, one in five workplaces, admittedly primarily being small, provide individualized pay setting (Bryson *et al.*, 2016). A centralized wage-bargaining system, as seen in Norway, clearly induces wage rigidities (Holden and Wulfsberg, 2014). The centralized bargaining system is not geographically differentiated, which could limit the tax-shifting possibilities of employers. On the other hand, the local element might actually provide this opportunity of shifting.

3.3 Sick leave pay, disability insurance, and public age pension in Norway

Since the key issues in analyses of payroll-tax reforms are how these reforms affect wages and employment, admittedly other outcomes are secondary. Thus the evidence on how payroll-tax changes affect other outcomes on the intensive and extensive margin such as retirement, sick leave, and the entry to DI rolls are also less explored.[8] As we are to explore the impact on such outcomes, which might directly counteract any tax revenue gain by increasing public expenditures on disability and health care, brief introductions to the Norwegian sick pay and disability insurance systems are necessary.

The Norwegian sick pay system provides 100 per cent compensation for a majority of the workers, time limited upwards to 1 year for physician-certified sick leaves (Dale-Olsen, 2013). In March 2007, partial sick leave became the standard when individuals' reduction in work capacity was less than 50 per cent and the work load could be adjusted at the workplace (Brage *et al.*, 2011)(for analysis of partial sick leaves, see Markussen *et al.*, 2012).

As described by Kostøl and Mogstad (2014), the Norwegian disability insurance system target individuals who due to illness, injury, or medical conditions have experienced a permanent drop in work ability.[9] The work ability reduction, as evaluated by the Social Security Administration's medical staff and disability examiners, has to be at least 50 per cent and have to have lasted at least a year. For individuals between 18 and 67 years of age, the disability insurance compensation level is determined as the level of old age pension. After the mandatory retirement age of 67 DI recipients are transferred to the public age pension system. The DI system is financed by the insured (e.g. employee-paid taxes) and the employers (e.g. payroll taxes).

Until January 1st 2011, workers might retire and enter the Norwegian public age pension system from the age of 67 years (but trade union agreements might allow earlier retirement and certain occupations have earlier mandatory retirement). The public age pension system ensured an average pre-tax compensation of just above 50 per cent (government employees were ensured 66 per cent compensation), but the progressive tax system which also made retired face lower tax rates than non-retirees, ensured that the post-tax compensation increased to roughly 65 per cent. Private agreements ensured additional compensation, and the possibility of early retirement.

These major welfare benefit systems are thus not explicitly geographically differentiated (they are related to income and thus indirectly geographically differentiated). Thus if an imposed harmonization of the payroll-tax levels across regions occurred with few apparent changes to the provision of benefits, then this might actually influence the wage-shifting possibilities and thereby employment.

4. Econometric strategy

The main empirical focus of the paper is to determine the short- and long-run causal impact of the payroll-tax reform. The empirical strategy of the paper is basically very simple, mostly based on a basic difference-in-difference approach applied to linear regressions:

$$\begin{aligned} Y_{igt} = {} & \text{intercept} + \beta_1 \text{payroll\_zone}_g + \beta_2 T_t + \beta_3 \text{Edu}_{it} + \beta_3 \text{Edu}_{it} X T_t \\ & + \beta_{\text{reform}} \text{Payroll-tax rate}_{gt} + \varepsilon_{igt}, \end{aligned} \tag{1}$$

where Y could express outcomes such as the hourly wage, earnings or weekly working hours, or indicator variables for being employed next year, newly hired (last year), newly

hired due to establishment entry (last year), being unemployed at the end of the year, entering early retirement, moved to another payroll-tax zone, being in the labour force 3 years later, having entering temporary disability insurance rolls 3 years later, and finally receiving permanent disability insurance 3 years later. T and Edu express year dummies and years of educational qualification in excess of compulsory schooling, respectively, and Edu X T expresses the interaction of the year dummies and years of education. These latter terms takes into account educational-specific time trends.[10] All fixed variation between the payroll-tax zones are taken into account by dummies. The key parameter of interest is β_{reform}, which now is identified by the variation in the payroll-tax rate driven by the reform.

In an ideal setting, the tax rules (except for the payroll-tax rate-changes) would have been unchanged during 2001–2009, thus making it possible to pool the data and conduct a coherent analysis of the whole period. However, I have chosen to split the period into 2001–06 and 2004–09 for five reasons. First, the reform was reversed in the period 2007–09 and such a reversal might have different impact on outcomes, e.g. wages. Second, the number of payroll tax zones is increased from 5 to 7 in the period 2007–09 and this makes comparisons with 2001–03 difficult. Third, from 2007 and onwards, the legislation changed in how workers are designated to payroll-tax zones. Fourth, the reversal of the tax reform in 2007 introduced payroll-tax differences depending on firms' wage costs. Fifth, the Financial Crisis of 2008–09 might have differential geographical outcomes.[11] On the other hand, the period of 2001–06 is characterized by no other major change than the payroll tax reform. These considerations imply that comparisons of the period 2007–09 with the earlier period are more likely to be affected by what can be interpreted as measurement errors, and this makes outcomes of the reversal of the reform (2007–09) and the pooled data (2001–09) much more difficult to evaluate.

Thus my prime analysis is of the reform using the years 2001–06, where the years 2001–03 act as pre-reform years. The parameter associated with the payroll tax rate is identified by changes in the payroll-tax rates within the reform zones over time. The reform or treatment zones comprise zones 2, 3, and 4, whereas zones 1 and 5 act as the control group. Fixed payroll-zone dummies take into account fixed within treatment/control group variation. In the reversal analysis (2004–09), I impose the new zones to the pre-period years of 2004–06, and thus add dummies for these zones as controls. Otherwise the reversal analysis mirrors the analysis of the reform. Finally, I also supplement the analyses above by conducting a limited analysis on the pooled sample (2001–09). Both the analyses of the reversal of the reform (2004–09) and those on the pooled sample (2001–09) are supplemented primarily for completeness and robustness reasons.

Because the number of clusters are few (i.e. only five payroll tax zones in the main reform analysis, seven zones otherwise)(see discussion of Cameron and Miller (2015) regarding the caveats of few clusters, ordinary clustered standard errors might be inappropriate, thus I also report wild-bootstrapped *p*-values (400 bootstrap resamples) based on Rademacher weights.[12]

Employment and wage relationships are measured during the first two weeks of each year. Thus in the last year of the pre-period (2003), all active employment relationships had started long before ESA's deadline of March 25th (2003) for the Norwegian government response, and before this issue had gained public interest (see note [5]), which limit pre-reform selection issues. Furthermore, in the centralized wage-bargaining system of Norway as wage negotiations between employer unions and labour unions primarily occur during the spring, employment relationships (and their wages) in January year t are largely

determined by the year $t-1$ spring bargaining outcome (although some wage drift occur during autumn). For those receiving performance pay, bonuses for performance year t (e.g. sales, revenues, profits) will usually be paid after the fiscal year, thus in year $t + 1$.

5. Data

My data set is based on public administrative register data provided by Statistics Norway comprising *all* firms, workplaces and employees in Norway 2001–12 employed at least January 1st and the next fortnight each year. This linked employer-employee data set provides information on workers (sick pay spells, diagnoses, gender, educational qualifications), jobs (seniority, spell-specific earnings and fringe benefits, working hours, wages, and firm-and establishment identifying numbers), and workplaces (sales per employee, industry, sector, and municipality).

I mainly focus on workers between 20 and 62 years of age during the observation period to avoid measurement problems associated with other tax rules.[13] Furthermore, as the so-called 'fat-cat' top payroll tax was abolished after 2005, we exclude all workers with earnings susceptible for the 'fat-cat' top payroll tax during the whole period (also as if it had existed the years 2006–09) (this constitutes 1.7 per cent of the workers). The empirical analyses focus on the changes in the payroll-tax rates for the large majority of the workers, not the very few highly paid workers. Finally, firms exempted from the reform are discarded from the data (they employ 6 and 3.5 per cent of the workers 2003–06 and 2006–09, respectively).[14]

To measure correctly the impact of the reform on wages will be crucial for the analyses that follow. To ease interpretation, in the remainder of the paper, all wage measures is reported in 2006 U.S. dollar [deflated by the Norwegian CPI (KPI-JAE)].The hourly wage measure is calculated as the job spell-specific earnings reported to the tax authorities relative to the spell-specific contracted weekly working hours multiplied the spell duration measured in week. This measure comprises all wages, sick pay, and taxable fringe benefits (such as a company car, stocks and stock options). However, as hourly wages are calculated from information on spell-specific earnings, spell length, and weekly working hours, measurement problems might cause observations with very low wages or very high wages. Thus we exclude 62 observations with hourly wages over of 1,247€ and those observations with hourly wage less than 2.5 U.S. $ (1.8 per cent).[15]

Unfortunately, data from 2001 and 2002 are problematic for two reasons. First, before 2001 information on weekly work hours were only reported in form of three work hour intervals.[16] In 2001 still 26.6 per cent of the jobs were registered with intervals for work hours. Second, in 2002 Statistics Norway started to keep track of work hour changes within a year, identifying roughly 23 per cent such changes. After this start-up year, however, these occurrences increased to 45.5 per cent from 2003 and onwards. As the measurement problems associated with work hour changes occur within a job and job changes occur more frequently in central areas, these could problems affect the treatment and control group differently.

Let me return briefly to the key outcome measures. As key short-term variables I focus on continuous variables such as the spell-specific hourly wage, weekly working hours, total annual work hours, annual earnings, and the number of sick leave days. These variables measure changes along the intensive margin. As key short-term variables measuring the extensive margin I apply indicator variables expressing employed at the beginning of the

year (only applied when analysing all individuals and not limited to workers), still being employed at the end of the year, being newly hired during the last year, being newly hired due to establishment entry during the last year, unemployed (receiving unemployment benefits and having no active employment relationship at the end of the year), retirement, and moved to another payroll-tax zone during the year. Long-term outcome variables of interest are: indicator variables for being employed, receiving unemployment benefits, retiring, entering temporary disability insurance rolls, and finally receiving permanent disability insurance. These long-term variables measure labour market attachment along the extensive margin.

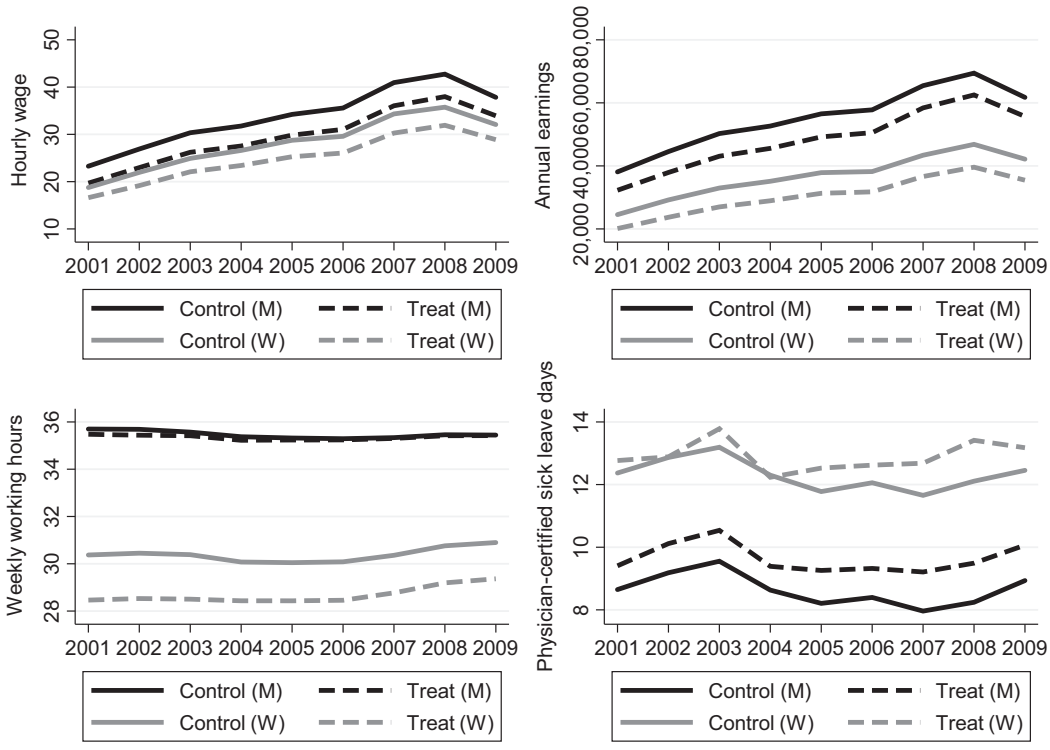
Figures 2–4 show the outcomes separately for men (M) and women (W) and separately for the two groups of payroll tax zones (the reform zones, i.e. our treatment group marked by a dashed line, and those not affected) during the period 2001–09 for workers between 20 and 61 years of age. Figure 2 focuses on short-term outcomes on the intensive margin, such as hourly wage, earnings, work hours, and sick leave. Similarly, Figure 3 focuses on short-term outcomes on the extensive margin such as employment (for all and for workers) and unemployment. Finally, Figure 4 shows long-term outcomes on the extensive margin. In several cases we see that both for men and women outcomes are different for the control group compared with the treatment group, but nothing seems to inviolate the common trends assumption. Such differences are as expected, because one of the motivations for the geographically differentiated payroll tax system is to provide economic support to areas less successful. On the other hand, the figures do not reveal any strong reform impact on outcomes.[17]

From these data I construct the main sample. For impacts related to the intensive margin (wages, work hours, sick leaves, earnings) I require workers to be employed at least a fortnight in the beginning of January 2003 (pre-period) and at least a fortnight in the beginning of January 2004 (post-period). This is necessary to observe variation in the payroll-tax rate within zones. For analysis of the impacts on outflow from employment, outflow from work to unemployment and other outcomes related to the extensive margin we do not add further requirements. However, as the overall impact of the reform on the employment levels is also important, in one analysis we extend the main sample to comprise all individuals within 20–62 years of age living in the municipalities. Thus this sample is neither limited to workers nor the private sectors. Employment status is measured at the beginning of each year.

For robustness checks I construct three additional samples which will then be used for the supplementary analyses or robustness checks of how the payroll-tax reform affected short-term and long-term outcomes. First, since the reform was basically annulled from 2007 and onward I also construct a 'reversal' sample comprising the period 2004–09, similarly as the main sample. Note that when analysing outcomes related to the intensive margin, identification is ensured by variation in payroll-tax rates within zones, i.e. only two years of observations are needed because the rates vary each year during the period 2004–09.

Second, I create samples based on the main and reversal samples, but where treatment workers are *matched* by pre-period characteristics (years of education, dummies for age deciles, dummies for firm wage costs deciles, dummies for hourly wage deciles, dummies for manufacturing, welfare (women), trade, hotel, transport, construction (men))[18] to control workers based on gender-specific 1-nearest-neighbor matching without replacements. The matching of the reform sample is based on characteristics from the pre-year 2003, whereas the matching of the reversal sample is based on 2006 information.

Figure 2. Short-term intensive outcomes over time

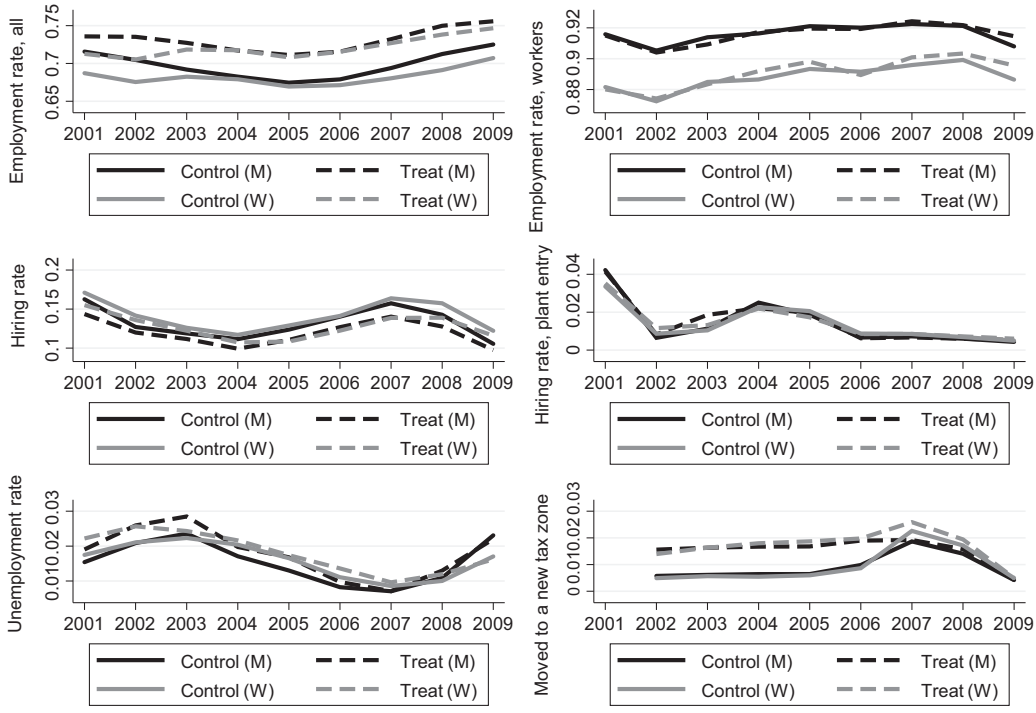


Note: Population: all male (M) and female (W) workers between 20 and 61 years of age, employed January 1st at least one year and then at least for a fortnight. Employment and unemployment are measured at the end of the year. The EFTA-treaty industries are excluded (see note [5]).

In Table 2 I present descriptive statistics on the key outcome variables for the main sample of workers, whereas Table A1 in the Appendix presents descriptive statistics on background characteristics for the main sample. Table A2 in the Appendix presents descriptive statistics on outcome and background characteristics for the reversal-sample. Descriptive statistics on the matched samples are available from the author upon request.

Table 2 clearly shows that the size of reform zones constitute roughly one fifth of the control zones. Among the control zones, zone 1 by far constitutes the majority (98 per cent). The treatment zones are more equal in size, although zone 2 constitutes roughly half of the observations. We see from the tables (Tables 2 and A1) that workers pre-reform in the affected payroll-tax zones (the treatment workers) are on average slightly less educated, they receive lower wages and are less recently hired, and they are less involved in construction and transport industries compared with the control workers. On the other hand, the treatment workers are more likely to move to a new tax zone and they are more likely to be unemployed 3 years after 2003 or have entered the DI rolls. Otherwise, w.r.t. age, other industries and other outcomes the differences between the treatment and control workers are minor. Post-reform these relationships are still true, except that the prevalence of construction and transport workers has dropped significantly among the control workers. Also, note that the treatment workers appear more likely to move to a new tax zone also

Figure 3. Short-term extensive outcomes over time



Note: Population: all male (M) and female (W) workers between 20 and 61 years of age, employed January 1st each year and at least for a fortnight. Wages and earnings are measured in in 2006 U.S. dollar. The EFTA-treaty industries are excluded (see note [5]).

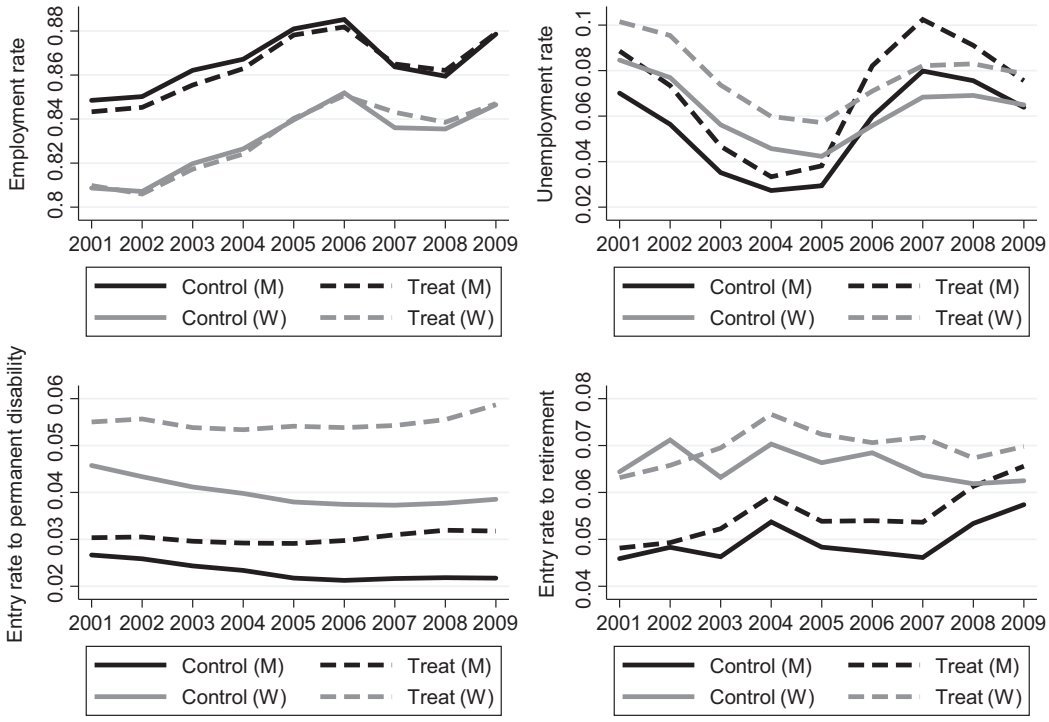
from 2006 to 2009 (Table A2), i.e. a period when it becomes more beneficial to stay in the treatment zones. Thus worker movement from the treatment zones to the control zones are always higher and not particularly related to payroll tax reform changes, i.e. worries of worker selection issues related to the reform(s) is somewhat mitigated.

6. Main results

As the impact of the payroll-tax reform on labour demand depends on whether the tax changes are shifted onto wages, we start by analysing the reform impact on wages. Table 3 presents the estimated parameter associated the payroll-tax rate from several linear hourly wage regressions. One set of regressions takes into account all variation related to the combination of industry (5-digit) and educational qualifications (6-digit) (in total 377,745 combinations) and years, thus controlling for educational- and industry-specific time trends. Wild-bootstrapped *p*-values are reported in brackets. The first four columns report the results from the reform, whereas the last four columns focus on the reversal of the reform.

The analyses of the main reform reveal that the payroll-tax harmonization, causing higher payroll-tax levels for the treatment zones, result in wage reductions, so the tax hike is partly shifted onto workers' wages. The wage cuts appear to be rather modest, with

Figure 4. Long-term outcomes over time



Note: Outcomes are measured after 3 years. Population: all male (M) and female (W) workers between 20 and 61 years of age, employed January 1st each year and at least for a fortnight. The EFTA-treaty industries are excluded (see note [5]).

elasticities of hourly wage w.r.t. the payroll-tax rate around 5–10 per cent (evaluated at mean). For each percentage point the payroll-tax increases, the hourly wage is cut by 15–20 cents for men and 15–26 cents for women, i.e. less than 1 per cent for men and women (compared with the pre-reform average hourly wage for the treatment workers). As seen in Table 1, the tax rate increased 4–6 percentage points depending on zone, so overall the wage cuts could be expected to constitute around 0.8–1 dollar. On the other hand, based on the pre-reform hourly wage observations we can calculate the hourly post-tax wage cost growth for the employers in the treatment zones due to the reform, which constitutes to 1.38 and 1.20 dollar for men and women, respectively. In other words, 60–80 per cent of the increase in wage costs due to the reform is shifted onto workers' wages. This indicates that the shifting is non-negligible. When the reform is reversed we observe no impact of economic size whatsoever. Thus when the payroll-tax is cut from 2006 to 2009, this appears not to be shifted onto workers' wages.

How robust are these findings? One potential worry has been that differences might exist between the payroll-tax zones when it comes to background characteristics, which thus might inviolate the common trends assumption essential in difference-in-difference analyses. I shed light on this issue, by doing four different sets of analyses. First, I study whether this could be related to differences in the returns to educational qualification within industry and time, effectively controlling for education-industry time trends. Secondly, I study whether these

Table 2. Descriptive statistics on outcomes. Main reform. 2001–06. All workers

	Men				Women			
	Pre-period		Post-period		Pre-period		Post-period	
	Treat	Control	Treat	Control	Treat	Control	Treat	Control
Zone 1	–	0.981 (0.135)	–	0.982 (0.134)	–	0.984 (0.127)	–	0.984 (0.124)
Zone 2	0.506 (0.500)	–	0.505 (0.500)	–	0.481 (0.500)	–	0.480 (0.500)	–
Zone 3	0.092 (0.289)	–	0.092 (0.290)	–	0.089 (0.285)	–	0.098 (0.297)	–
Zone 4	0.402 (0.490)	–	0.402 (0.490)	–	0.069 (0.255)	–	0.422 (0.494)	–
Zone 5	–	0.019 (0.135)	–	0.018 (0.134)	–	0.013 (0.127)	–	0.015 (0.124)
Payroll-tax rate (%)	8.204 (2.435)	13.836 (1.909)	11.534 (2.697)	13.843 (1.885)	8.086 (2.433)	13.871 (1.784)	11.398 (2.697)	13.879 (1.751)
Hourly wage	22.949 (13.942)	26.817 (18.077)	30.215 (17.648)	34.880 (22.239)	19.256 (14.661)	21.852 (17.384)	25.069 (17.108)	28.733 (21.853)
Earnings	37731.43 (19077.8)	44257.75 (23814.4)	49837.74 (24655.9)	57589.74 (30886.5)	23565.17 (14644.5)	28871.33 (18506.4)	31079.07 (19288.3)	37999.81 (24663.4)
Employment-all	0.733 (0.443)	0.710 (0.484)	0.715 (0.452)	0.684 (0.448)	0.712 (0.452)	0.683 (0.465)	0.714 (0.452)	0.675 (0.469)
Employment-workers	0.909 (0.287)	0.912 (0.284)	0.920 (0.271)	0.922 (0.268)	0.879 (0.329)	0.880 (0.325)	0.894 (0.308)	0.892 (0.311)
Newly hired	0.125 (0.331)	0.136 (0.343)	0.111 (0.314)	0.126 (0.329)	0.138 (0.345)	0.147 (0.354)	0.111 (0.314)	0.128 (0.334)
Newly hired through workplace entry	0.023 (0.149)	0.020 (0.141)	0.004 (0.062)	0.004 (0.062)	0.020 (0.141)	0.018 (0.132)	0.006 (0.080)	0.006 (0.075)
Work hours	35.446 (6.731)	35.654 (6.515)	35.318 (6.792)	35.431 (6.780)	28.499 (10.740)	30.402 (10.133)	28.541 (10.606)	30.252 (10.176)
Annual work hours	1704.07 (528.02)	1714.515 (524.009)	1706.092 (528.380)	1708.626 (532.523)	1331.475 (646.099)	1414.858 (639.814)	1331.721 (653.521)	1407.968 (649.637)
New zone	0.016 (0.125)	0.006 (0.077)	0.018 (0.131)	0.008 (0.087)	0.015 (0.122)	0.005 (0.073)	0.019 (0.136)	0.007 (0.081)
Unemployed	0.024 (0.154)	0.020 (0.140)	0.015 (0.123)	0.012 (0.110)	0.024 (0.153)	0.021 (0.141)	0.018 (0.131)	0.016 (0.0124)
Employed $t + 3$	0.848 (0.359)	0.854 (0.353)	0.878 (0.327)	0.881 (0.324)	0.811 (0.391)	0.818 (0.386)	0.840 (0.367)	0.841 (0.366)
Unemployed $t + 3$	0.070 (0.255)	0.054 (0.226)	0.049 (0.216)	0.037 (0.189)	0.090 (0.287)	0.073 (0.260)	0.062 (0.241)	0.047 (0.212)
Temp. DI $t + 3$	0.047 (0.211)	0.040 (0.197)	0.039 (0.193)	0.034 (0.181)	0.073 (0.261)	0.064 (0.244)	0.074 (0.261)	0.062 (0.241)
Perm. DI $t + 3$	0.030 (0.171)	0.026 (0.158)	0.029 (0.167)	0.021 (0.145)	0.055 (0.228)	0.043 (0.204)	0.053 (0.225)	0.038 (0.191)
Retired $t + 3$	0.050 (0.218)	0.047 (0.211)	0.057 (0.231)	0.051 (0.220)	0.066 (0.249)	0.066 (0.249)	0.074 (0.261)	0.067 (0.253)
NXT	384,854	1,584,907	376,025	1,582,964	192,486	958,091	186,411	949,741

Note: Population: Employed-all: all individuals (not limited to workers). Otherwise private sector workers in non-EFTA-treaty industries between 20–61 years of age, employed at least a fortnight in January at least once during 2001–09. $t + 3$ denote that the outcome is measured in 3 years. The hourly wage and annual earnings are measured in 2006 U.S. dollar.

Table 3. The impact of payroll-tax reforms on the hourly wage. Panel data analysis.

	2001–06 (Main reform)					2004–09 (Reversal)					2001–09 (Pooled)	
	Main	Edu-industry time trends	Regional time trends	Match	Within job	Main	Edu-industry time trends	Regional time trends	Match	Within job	Main	Regional time trends
(A) Men												
Payroll tax rate	**−0.218****	**−0.141****	**−0.215****	**−0.155****	**−0.201****	−0.029	0.042	−0.047	0.004	0.089	**−0.219****	**−0.207***
	(0.047)	(0.039)	(0.048)	(0.038)	(0.038)	(0.109)	(0.039)	(0.100)	(0.092)	(0.039)	(0.042)	(0.056)
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.76]	[0.27]	[0.74]	[0.65]	[0.18]	[0.01]	[0.05]
(%-points)												
(B) Women												
Payroll tax rate	**−0.267****	**−0.152****	**−0.266****	**−0.239****	**−0.263****	0.007	−0.011	−0.002	0.068	0.083	**−0.156***	**−0.138^x**
	(0.013)	(0.016)	(0.014)	(0.018)	(0.018)	(0.049)	(0.019)	(0.045)	(0.072)	(0.033)	(0.034)	(0.061)
(%-points)	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.84]	[0.58]	[0.99]	[0.44]	[0.09]	[0.05]	[0.09]
A:NxT	3,433,086	3,433,086	3,433,086	1,135,112	3,433,086	3,403,014	3,403,014	3,403,014	1,102,935	3,403,014	5,331,331	5,331,331
B:NxT	1,876,700	1,876,700	1,876,700	550,287	1,876,700	2,018,804	2,018,804	2,018,804	530,588	2,018,804	3,037,760	3,037,760
Controls:	Basic	Basic+ Fixed	Basic+ trends	Basic	Basic+ Fixed	Basic	Basic+ Fixed	Basic+ trends	Basic	Basic+ Fixed	Basic	Basic+ trends
Elasticity of the hourly w.r.t. payroll-tax rate												
Men	−0.095	−0.061	−0.094	−0.067	−0.087	−0.010	0.014	−0.010	0.004	0.031	−0.083	−0.079
Women	−0.141	−0.080	−0.140	−0.126	−0.139	0.003	−0.005	−0.005	0.028	0.035	−0.072	−0.063

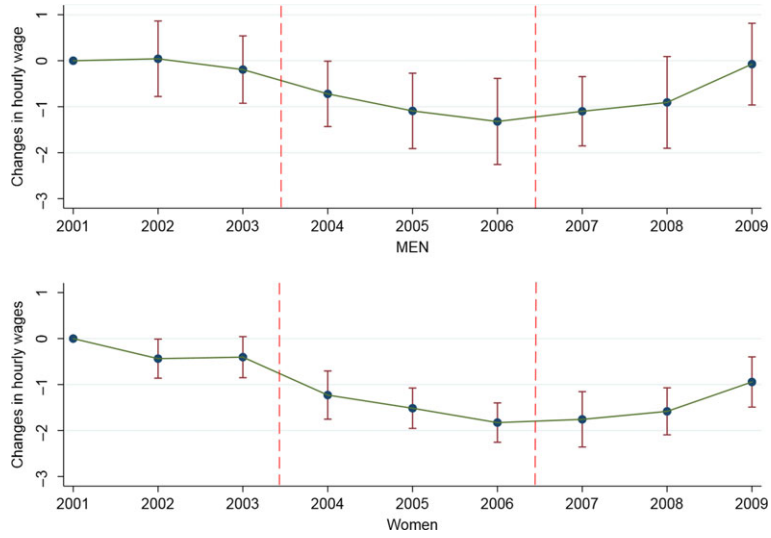
Note: The table (Panel A and B) reports the estimated parameter associated with the payroll-tax rate (in percentage points) from separate linear regressions. Panel A) and B) report the results for men and women, respectively. Dependent variable: hourly wage, measured in 2006 U.S. dollar. Populations: Main reform: Observations of workers between 20 and 61 years of age, from 2001 to 2006 at least employed for a fortnight in January 2003 and in January 2004 in non-EFTA-treaty industry firms; Reversal: Observations of workers between 20 and 61 years of age, observed from 2004 to 2009. *Match*: Matched to gender-specific 1-nearest-neighbour without replacements based on pre-period characteristics (2003 or 2006) (pre-period matching characteristics: years of education, dummies for age deciles, dummies for total wage cost deciles, dummies for hourly wage deciles, dummies for manufacturing, welfare (women), trade, hotel, transport, construction(men)). See text for details on matching. Control vectors (additional control variables): *Basic*: time dummies, dummies for payroll-tax-zones, years of education and the interaction between years of education and time dummies; *Edu-industry time trends*: controls for the combination of 5-digit industry codeX6-digit educational qualification codeXyear, thus effectively controlling for industry-educational time trends. *Regional time trends*: county-specific linear time trends added. *Within job*: controls for fixed job effects in addition to the *basic* control vector. Cluster-adjusted robust standard errors are presented in parentheses, whereas wild-bootstrapped *p*-values are presented in brackets, respectively. *, ** and ^x denote 1, 5, and 10 per cent level of significance, respectively, based on the wild-bootstrap *p*-values. These estimates are also shown in bold. The bottom rows of the table (Panel C) show the elasticity of hourly wages w.r.t. the payroll tax rate for men and women. The elasticity is calculated at the mean for men and women and during the reform period (2001–06), the reversal period (2004–09), and the pooled period (2001–09).

results are due to different regional time trends. Thirdly, I conduct a propensity score 1-to-1 matching on pre-period characteristics invoking common support, and after ensuring satisfying balancing tests (as implemented by Leuven and Sianesi, 2015), then repeat the difference-in-difference regression analysis on the matched sample (on matching see e.g. Heckman *et al.*, 1998; and with difference-in-difference, see Blundell and Dias, 2009). To avoid bias, matching is based on dummies and only one continuous background variable (Abadie and Imbens, 2006, 2011). Finally, I conduct a set of regressions controlling for fixed job characteristics, effectively controlling for all variation between combinations of individualsXworkplaces, thus letting the identification only rest on payroll tax variation within a job.

The results of these analyses are also presented in Table 3. As is seen in the table, the findings appear quite robust. In most cases, the point estimates are quite comparable across samples and measures. The main reform (2003–06) clearly implies a shifting of the payroll-tax increase unto workers' hourly wages, thus reducing these. The reversal of the reform (2006–09) appears similarly, given the caveats of note 12, to have negligible or insignificant impact on wages.

Finally, we present for completeness sake, selected regression results on the pooled data. These estimates are weaker in strength, but are otherwise similar to the main reform estimates. This comes as no surprise, because the estimates from the reversal of the reform yielded small and insignificant results. The strong impact is identified by what happens when the reform was introduced in 2004. As I have pointed out earlier in Section 4, it could be more difficult to evaluate the impact of the reform reversal, and these problems could thus affect the pooled sample as well. The pooled sample does, however, allow us to study the reform (and the reversal) in an event-history perspective, i.e. we can see how the reform

Figure 5. Event-study approach. The reform effects across the observation period



Note: The figures report yearly mean effect (and 95 per cent confidence interval) associated with the reform zones during 2001(reference) to 2009. The hourly wage is measured in 2006 U.S. dollar. The dashed lines indicate when the tax rates in the zones start to change. Until 2003, the tax rates are unchanged. During 2004–06 a harmonization of the tax rates occurs. From 2007 and onwards the tax rates are more and less changed back to what it was during 2001–03, except for a minor sub-population.

impacts changes over the reform period. We use the same model and controls as in the model with regional time trends (rightmost column of Table 3).[19] Figure 5 shows the development from 2001 to 2009 of the hourly wages in the reform zones relative to the control zones for men and women. The upper panel shows the graph for men, whereas the bottom panel shows the similar graph for women. 2001 is the reference year, normalized to zero.

We see that for the next 2 years the hourly wage in the reform zones hovers statistically around zero, although the point estimates are weakly negative. 2004 is the first reform year. During 2004–06 the deviation in the hourly wage for the reform zones from 2001 turn significantly negative, and in 2006 men and women in the reform zone earn 1.32 and 1.83 dollar (in 2006-dollar), respectively, less than in 2001. In 2007 the reform was more or less reversed. We see that the deviations for men and women from 2001 are considerable in the beginning, but then start to diminish (and actually disappears for men). Thus when we observe no significant effects of the reversal of the reform in Table 3, this reflects two facts. First, it follows from a comparison of the average of 2004–06 with the average of 2007–09, and this difference is less than if we had compared with 2006 (when the reform was at maximum). Second, it appears that the reversal process takes longer time to be fully absorbed.

The wage impacts found in Tables 3 are all average treatment effects, but these effects might be different depending on whether one consider high wage or low wage workers. The centralized wage-bargaining system as seen in Norway clearly induces wage rigidities, particularly at the bottom of the wage distribution, because firms in many sectors have trade union agreements with binding minimum wages. This should, ceteris paribus, make it more difficult to shift payroll tax increases unto workers at the lower end of the wage distribution. To shed light on this issue Equation [5] is estimated by quantile regressions (Koenker and Hallock, 2001).

Table 4 therefor presents the results of several quantile hourly wage regressions, analysing the impact at the 10, 50, and 90 percentile. It clearly reveals that the reform affects high wage workers more strongly than low wage and medium wage workers. At the 10^{th} percentile, an increase in the payroll-tax rate by 1 percentage point implies 10–18 cent reduced hourly wages for men and women. At the 90th percentile, such an increase in the payroll-tax rate yields 30–40 cent reduced hourly wages for men and women. Once again the table reveals that the reversal of the reform has negligible impact on wages, this time across the wage distribution.

After having established that the payroll-tax reform affected wages by reducing these modestly, we then ask the at least equally important question about what happens with employment? Table 5 reports the results from linear regressions of individuals and workers' employment and hiring outcomes, for all individuals, workers and for the matched samples.

These analyses of both the reform and the reversal show a quite robust finding: the payroll-tax rate changes caused by the reform and the reversal appear to have had negligible impacts on employment and hirings. Close to none of the estimations yield any significant impacts associated with the payroll-tax changes. One exception is the reduced female employment rates overall in the economy as the payroll-tax rates grow. Another exception is the impact on the hiring activity following workplace entry, which appears reduced following increased payroll-tax rates. We have also tested out specifications with different linear trends and controls for composition (not shown), but these yield qualitatively similar results to what is seen in Table 5. While it is hard to draw strong conclusions from the estimates of Table 5, it clearly appears that the shifting of the tax hike unto wages keeps labour demand at the pre-reform level thus leaving employment unchanged.

Table 4. A differential impact of the payroll-tax reforms on wages depending on position in the wage distribution?

	2001–06 (Main reform)			2004–09 (Reversal)		
	10	**50**	**90**	**10**	**50**	**90**
(A) Men						
All	**−0.143**** (0.041)	**−0.150**** (0.048)	**−0.382**** (0.046)	−0.088 (0.089)	−0.055 (0.079)	−0.009 (0.076)
Matched	**−0.071**** (0.015)	**−0.079**** (0.017)	**−0.289**** (0.026)	−0.059 (0.085)	−0.030 (0.077)	−0.003 (0.079)
(B) Women						
All	**−0.182**** (0.041)	**−0.107**** (0.040)	**−0.369**** (0.046)	−0.008 (0.023)	0.027 (0.015)	0.090 (0.057)
Matched	**−0.120**** (0.036)	**−0.075**** (0.006)	**−0.356**** (0.025)	−0.050 (0.088)	0.010 (0.045)	**0.120*** (0.052)

Note: Each cell reports the estimated parameter associated with the payroll-tax rate (in percentage points) or the corresponding payroll-zone cluster-adjusted standard error (in parenthesis) from separate quantile hourly wage regressions (on clustering, see Parente and Santos Silva, 2015), controlling for the basic control vector. Column head denotes percentile in the wage distribution. Dependent variable: Hourly wage, which is measured in 2006 U.S. dollar. All models incorporate the basic control vector. See note to Table 3 for details on population, controls, and matching. * and ** denote 1 and 5 per cent level of significance, respectively. These estimates are also shown in bold.

Table 5. The impact of payroll-tax reforms on employment and hires. Panel data analysis.

	2001–06 (Main reform)				2004–09 (Reversal)				2001–09 (Pooled)	
	Individuals	Workers			Individuals	Workers			Individuals	Workers
			Hirings				Hirings		Employed	
	Employed Main	Employed Main	All	Plant entry	Employed Main	Employed Main	All	Plant entry	Main	Main
(A) Men										
Payroll	$2.7e^{-3}$	$-2.9e^{-5}$	$1.6e^{-3}$	$\mathbf{-3.6e^{-4}}^{**}$	$6.3e^{-4}$	$8.9e^{-5}$	$-2.2e^{-4}$	$-2.1e^{-4}$	$-1.7e^{-4}$	$-1.6e^{-4}$
tax rate	$(3.8e^{-4})$	$(3.0e^{-4})$	$(7.3e^{-4})$	$(8.8e^{-5})$	$(9.3e^{-4})$	$(7.5e^{-4})$	$(4.4e^{-4})$	$(3.4e^{-4})$	$(3.8e^{-3})$	$(7.9e^{-4})$
(%-points)	[0.27]	[0.97]	[0.44]	[0.01]	[0.54]	[0.82]	[0.63]	[0.55]	[0.67]	[0.83]
(b) Women										
Payroll	$3.3e^{-3}$	$4.1e^{-4}$	$1.5e^{-3}$	$-3.1e^{-4}$	$\mathbf{-1.2e^{-3}}^{**}$	$-6.8e^{-4}$	$-1.3e^{-3}$	$\mathbf{-5.3e^{-4}}^{**}$	$-1.1e^{-4}$	$3.6e^{-4}$
tax rate	$(6.8e^{-4})$	$(8.0e^{-4})$	$(2.8e^{-4})$	$(6.7e^{-4})$	$(6.8e^{-4})$	$(5.1e^{-4})$	$(5.9e^{-4})$	$(3.1e^{-4})$	$(3.8e^{-3})$	$(1.4e^{-3})$
(%-points)	[0.39]	[0.52]	[0.29]	[0.53]	[0.02]	[0.38]	[0.15]	[0.01]	[0.29]	[0.91]
A:NxT	7,586,424	3,929,102	3,929,102	3,929,102	7,742,137	3,702,742	3,702,742	3,702,742	1,1597,473	5,331,331
B:NxT	7,173,153	2,286,732	2,286,732	2,286,732	7,373,153	2,261,275	2,261,275	2,261,275	10,909,040	3,037,760
Controls	Basic	Basic	Basic	Basic	Basic	Basic	Basic	Basic	Basic	Basic

Note: The table reports the estimated parameter associated with the payroll-tax rate (in percentage points) from separate linear regressions. Panel (A) and (B) report the results for men and women, respectively. Dependent variable: dummy for being employed at the beginning of the year (all individuals), dummy for still being employed the end of the year (for workers), and dummy for being newly hired (all hires or hires caused by workplace/plant entry)(workers). The heading *Individuals* in second-row denotes that the population comprises all individuals between 20 and 61 years of age living in the municipalities, regardless of labour market attachment. See otherwise note in Table 3 for details on populations and controls. Cluster-adjusted robust standard errors are presented in parentheses, whereas wild-bootstrapped *p*-values are presented in brackets, respectively. * and ** denote 1 and 5 per cent level of significance, respectively, based on the wild-bootstrapped *p*-values. These estimates are also shown in bold.

Table 6. The impact of the payroll-tax reform (2001–06) on the short-term intensive and extensive outcomes and long-term outcomes (3 years later).

Short-term:	**Weekly work hours**	**Annual work hours**	**Earnings**	**Sick leave days**	**Unemployed**	**New zone**
Men-All	−0.009	0.789	**−357.213****	**0.086****	$6.3e^{-5}$	**−0.003****
	(0.010)	(0.367)	(59.068)	(0.010)	($1.9e^{-4}$)	(0.001)
	[0.55]	[0.16]	[0.01]	[0.01]	[0.53]	[0.01]
Men-Matched	0.009	1.050	**−235.278****	0.065	$6.3e^{-5}$	**−0.006****
	(0.008)	(0.425)	(45.796)	(0.024)	($1.9e^{-4}$)	(0.001)
	[0.54]	[0.28]	[0.01]	[0.29]	[0.53]	[0.01]
Women-All	**0.023****	**1.746****	**−382.539****	0.038	**$-4.8e^{-4}$****	**$6.2e^{-4}$****
	(0.008)	(0.570)	(53.356)	(0.119)	($7.3e^{-5}$)	($1.1e^{-4}$)
	[0.01]	[0.01]	[0.01]	[0.78]	[0.01]	[0.01]
Women-Matched	**0.047****	**3.470****	−234.561	**0.137****	$-4.8e^{-4}$	**−0.006****
	(0.012)	(0.995)	(71.928)	(0.029)	($1.9e^{-4}$)	(0.001)
	[0.01]	[0.01]	[0.26]	[0.01]	[0.54]	[0.01]

Long-term	**Employed *t* + 3**	**Unemployed *t* + 3**	**Temporary DI *t* + 3**	**Permanent DI *t* + 3**	**Retired *t* + 3**
Men-All	$-1.8e^{-4}$	$-4.4e^{-4}$	$3.2e^{-4}$	**$1.0e^{-3}$****	$9.7e^{-4}$
	($6.8e^{-4}$)	($1.6e^{-4}$)	($2.1e^{-4}$)	($5.5e^{-4}$)	($6.5e^{-4}$)
	[0.79]	[0.83]	[0.30]	[0.01]	[0.53]
Men-Matched	−0.005	0.007	0.003	**0.002****	**0.002****
	(0.008)	(0.014)	(0.002)	(0.000)	(0.000)
	[0.51]	[0.50]	[0.50]	[0.01]	[0.01]
Women-All	$2.7e^{-5}$	**$-7.5e^{-4}$****	$5.9e^{-4}$	$1.1e^{-3}$	$1.1e^{-3}$
	($1.6e^{-4}$)	($2.8e^{-4}$)	($3.9e^{-4}$)	($2.4e^{-4}$)	($2.6e^{-4}$)
	[0.86]	[0.01]	[0.47]	[0.33]	[0.26]
Women-Matched	**$1.4e^{-3}$****	**$-1.5e^{-3}$****	**$7.4e^{-4}$****	**0.002****	**$1.4e^{-3}$****
	($2.7e^{-4}$)	($3.3e^{-4}$)	($2.2e^{-4}$)	(0.001)	($2.7e^{-4}$)
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]

Note: Each cell in the table reports the estimated parameter associated with the payroll-tax rate (in percentage points) from separate linear regressions. Dependent variable: denoted by the dark rows. Population and controls, see Table 3. Cluster-adjusted robust standard errors are presented in parentheses, whereas wild-bootstrapped *p*-values are presented in brackets, respectively. * and ** denote 1 and 5 per cent level of significance, respectively, based on the wild-bootstrap *p*-values. These estimates are also shown in bold.

What other impacts can we observe following the main tax reform? Table 6 reports the results from linear regressions of workers' short- and long-term labour supply outcomes, for all workers and for the matched samples. The results are usually qualitatively similar across the populations and thus quite robust, but appear enforced on the matched samples.

As is seen in the table, few short-term effects on the extensive or on the intensive margin are observed. Weekly working hours and annual work hours vary little, if anything they increase slightly. This increase is, however, not enough to offset the cut in hourly wages, and we see that when labour became more expensive, earnings dropped. The sick leave rate appears not very sensitive to payroll-tax rate changes, but if anything higher tax rates imply higher sick leave rates. Workers' movement to new zones appears to have been reduced. This reduction in worker mobility might reflect that a withdrawal process has been initiated (e.g. expressed through sick leaves and filing for disability insurance).

What then about the long-term impacts of the main reform? As is seen in the table, we mostly observe no significant long-term effects, with two important exceptions: the entry onto retirement and permanent DI increases for both men and women as the payroll-tax rate increases. While the pre-reform 2003 DI entry rates were 0.014 and 0.029 for men and women, respectively, these rates increase by 7 and 10 per cent, respectively. Even if part of the payroll-tax increase is shifted onto workers' wages, the wage cut is not sufficient to hinder all negative long-term supply outcomes. Thus some of the increased payroll-tax revenues following the tax hikes will have to be spent on DI expenditures and pensions.

This means that the concern raised in the introduction is well founded. However, is the magnitude of this problem such that it is economically unsound to increase the payroll tax? The withdrawal of any individual onto DI caused by tax increases cannot be interpreted as something positive. However, we can shed light on the economic side of this issue by considering the case of men employed in the treatment zones (117,400 in 2003) and conducting the following back of the envelope calculation. In 2003 the inflow to disability (within 3 years) was 1.4 per cent (i.e. 1,643 men). The reform causes an increase in the inflow to DI by 0.1 percentage points, around 117 men. In the matched samples these figures are twice as large, but still less than 300 workers. For each of these men one thus has to pay roughly 50 per cent of their hourly wage in DI (see Section 2 on DI compensation levels), i.e. about on average 11.9 dollar per hour for each worker. In addition, one has to pay 0.6 day extra in sick pay compensation. On the other hand, 85.5 per cent of the workers (over 100,000) remain in employment, generating 1.38 dollar per work hour for each worker in extra payroll-tax revenues. Thus it appears that the increase in tax revenues by those employed when the reform was introduced by far exceeds the increase in DI insurance spending. Even if employers appear to hire fewer workers at entry, the loss in tax revenue caused by less hiring cannot out-weight the increase in tax revenues.

As we already know that the payroll-tax reduction, i.e. the reversal of the reform did not affect hourly wages, labour should have become cheaper and thus increased labour demand. Table 5, however, did not reveal any evidence for the impact on employment and hiring but we saw that the point estimates for women indicated that lower payroll-tax rates was associated with higher employment and hiring rates. What about other outcomes?

Except for the impact on annual work hours, Table 7 on other outcomes reveals few robust effects across samples associated with the reversal of the reform, on the intensive and extensive margin and in the short- and in the long-run (during the next 3 years). Lower payroll tax implies higher annual work hours. However, for women it also appears that such a decrease in the tax causes the long-term job probability to increase. Although not significant for men, we see that lower payroll tax is associated with higher long-term

Table 7. The impact of the reversal of the payroll-tax reform (2004–09) on the short- and long-term intensive and extensive outcomes. Reversal analyses.

	Work hours	Annual work hours	Earnings	Sick leave days	Unem-ployed	New zone	Employed $t + 3$	Unemployed $t + 3$
Men-All	−0.024	**−3.091****	−197.118	**−0.059***	$2.1e^{-4}$	0.002	$-6.1e^{-5}$	$-8.7e^{-4}$
	(0.020)	(0.876)	(217.816)	(0.015)	$(3.7e^{-4})$	(0.001)	$(5.8e^{-4})$	$(8.4e^{-4})$
	[0.24]	[0.01]	[0.41]	[0.05]	[0.65]	[0.46]	[0.99]	[0.44]
Men-Matched	**−0.029***	−3.467	−200.118	−0.051	$2.1e^{-4}$	0.002	$-4.3e^{-4}$	$-4.3e^{-4}$
	(0.017)	(1.192)	(216.816)	(0.038)	$(1.8e^{-4})$	(0.001)	$(4.4e^{-4})$	$(1.1e^{-3})$
	[0.04]	[0.17]	[0.39]	[0.23]	[0.28]	[0.32]	[0.37]	[0.66]
Women-All	−0.033	**−3.777****	34.148	**−0.092****	$1.9e^{-4}$	0.002	$-3.3e^{-4}$	$-3.3e^{-5}$
	(0.054)	(1.802)	(122.534)	(0.033)	$(1.1e^{-5})$	(0.001)	$(6.8e^{-4})$	$(4.8e^{-4})$
	[0.75]	[0.01]	[0.61]	[0.01]	[0.19]	[0.18]	[0.64]	[0.99]
Women-Matched	−0.077	**−3.848****	20.148	−0.109	**$3.8e^{-4}$****	0.002	**$-1.2e^{-3}$****	$1.2e^{-4}$
	(0.063)	(1.534)	(120.534)	(0.043)	$(1.0e^{-4})$	(0.001)	$(4.2e^{-4})$	$(7.5e^{-3})$
	[0.41]	[0.01]	[0.71]	[0.11]	[0.01]	[0.14]	[0.01]	[0.72]

Note: Each cell in the table reports the estimated parameter associated with the payroll-tax rate (in percentage points) from separate linear regressions. Dependent variable: denoted by top row. Population: Observations of workers between 20 and 61 years of age, observed from 2004 to 2009 in non-EFTA-treaty industry firms; Matched: Matched to gender-specific 1-nearest-neighbour without replacements based on pre-period characteristics. All regressions comprise additional control variables as year dummies, years of educational qualification in excess of compulsory schooling and the interaction of this and the year dummies, and payroll-tax-zones-fixed effects. * and ** denote 1 and 5 per cent level of significance, respectively, based on the wild-bootstrap p-values. These estimates are also shown in bold.

employment rates for them as well, which indicate that workers have become less likely to withdraw from the labour force. Due to DI and retirement reforms (from 2010 and onwards), we cannot study these outcomes, and can only speculate that the inflow to DI and retirement would drop when the payroll-tax rate was cut.

7. Conclusion and discussion

The payroll tax is a major source for public financial revenues. Many countries experience an ageing population and thus increased pension and health care expenditures, and during the last decade many Western countries faced increased public expenditures on unemployment and disability benefits and lost tax revenues due to the Financial Crisis and the following debt crises. In times of austerity revenues become more important, and increased payroll tax could be an easy answer to strengthen public finances. At the same time, the empirical evidence on the relation between the payroll tax, labour demand and outcomes in and out of the labour market is scarce, and causal evidence is hard to come by. In particular, it is important to gain insight into how the payroll tax affect employment and wages, as these outcomes are important for workers levels of living, they affect public tax revenues through the income tax system and they might influence the withdrawal of workers from the labour market thus feed future public expenditures. In this paper I analyse the impact of a shock to the Norwegian payroll-tax system, when trade agreements between EU and Norway, combined with EU's competition rules, forced Norway to disband the geographically differentiated payroll tax.

The analyses reveal that higher payroll-tax levels were shifted onto workers' wages, with few other short-term effects. The long-term employment effects generally appeared limited, increased entry to DI rolls was, however, an unfortunate consequence. Thus part of the growth in tax revenues generated by the payroll-tax increase ended up being spent on public DI expenditures, but the extra revenues out-weighted these expenditures by a wide margin. When the reform was revoked in 2007, and thus tax rates were cut, these tax cuts were not shifted sufficiently or quickly enough onto workers' wages. Cheaper labour should thus have increased demand in the short-run, but we only observe weak short- and long-term employment gains for men and women. Firms might, however, have anticipated that a future shifting will occur, thus already adjusted labour demand accordingly. Unfortunately, the Financial Crisis and other tax reforms occurring late in the period might also affect the results relating to the reversal of the reform.

Finally, from my results, it is tempting to conclude that raising the payroll tax would be an easy way to improve fiscal revenues for many countries, and thereby finance growing enrolment in social security programs. Viewed in isolation, this is true. At least the Norwegian labour market seemed quite resilient to payroll-tax rate hikes, with the exception that workers' wages were cut. However, due to the partial equilibrium nature of my analyses, such a conclusion is premature. This requires an appropriate general equilibrium model, which then should be applied to consumer- and labour market data jointly. I leave this for future research.

Notes

[1]On average in the OECD-area, countries spend 1.2 per cent of GDP on disability benefits, and whereas incapacity-related benefits constitute on average 10 per cent of total public spending (OECD,

2010:57), in a redistributive economy such as the Norwegian, it constitutes as much as 23 per cent (OECD, 2010, Table 2.1). Enrolment in social security disability insurance (SSDI) programmes and growing health-related expenses cause concerns in many countries (Burkhauser and Daly, 2012; OECD, 2010), and one set of suggestions to ensure the financial sustainability of the SSDI programmes has therefore been to increase the payroll-tax rates, experience rating the payroll tax and to introduce private employer-financed DI (Autor and Duggan, 2010; Morton, 2014; Ruffing and Van de Water, 2014).

[2]The aim of the EEA Agreement is to guarantee the free movement of persons, goods, services, and capital; to provide equal conditions of competition; and to abolish discrimination on grounds of nationality in all 31 EEA States – the 28 EU States and 3 of the EFTA States. The EU Member States are monitored by the EU Commission and the EFTA States party to the EEA Agreement by the EFTA Surveillance Authority. The latter has been given powers corresponding to those of the Commission in the exercise of its surveillance role. The EFTA Court has jurisdiction with regard to EFTA States which are parties to the EEA Agreement. The jurisdiction of the EFTA Court largely corresponds to the jurisdiction of the Court of Justice of the European Union over EU States.

[3]Firms involved in the so-called EFTA-treaty industries have always payed 14.1 per cent payroll tax: production of electricity by water power, extraction of crude petroleum and gas, services related to extraction of crude petroleum and gas, mining of non-ferrous metal ores, quarrying/mining of olivine and nepheline syenite, production of ESCS-steel, shipyards, production of telecom services, and finance involving activities outside Norway in the EFTA-area. Exemptions were given to all other firms not competing with EU-firms, and to those located in the outskirts of Norway were transport costs constitute a significant burden. According to the NGO 'No to EU' fact sheet No 2/2002 only 150 firms was forced to pay higher payroll taxes.

[4]See http://omega.regjeringen.no/nn/dep/fin/Dokument/proposisjonar-og-meldingar/stortingsmeldingar/20022003/stmeld-nr-2-2002-2003-/3/3/4.html?id=328073.

[5]This is also seen by the public interest in these matters. A textual search on 'payroll tax' in the data base of Norwegian journals and newspapers reveal a hit of 200–300 each year until 2003. January 2003 reveals only 40 hits, but later during 2003 the reform gets national attention thus generating over 900 articles featuring the word 'payroll tax'. During 2004–05 one observes 4–500 occurrences, and then a new spike in 2006 with over 700 occurrences.

[6]From 2002 workers older than 61 years of age experienced reduced payroll-tax rates.

[7]Firms located in this zone would pay 10.6 per cent payroll tax until the difference between what the firm actually pays in payroll tax and what it would have paid given 14.1 per cent tax reached 530,000 NOK. Above this threshold, firms would have to pay 14.1 per cent payroll-tax rate for the wage bill above the threshold.

[8]Changing labour demand is clearly important for variation in the retirement rates and the inflow to DI. Bad times, recessions, job displacements, and plant closures, all reflecting reduced labour demand, induce higher entry to DI (Black *et al.*, 2002; Huttunen *et al.*, 2011; Korkeamäki and Kyyrä, 2010; Rege *et al.*, 2009). Bratsberg *et al.* (2013) even argue that since job loss is a major reason for becoming a DI recipient in Norway, this disability recipiency is really unemployment in disguise. Experience-rating of the employer's portion of the payroll tax has been found to increase employers' preventive actions to reduce the inflow to DI rolls in Netherlands, contingent on employer awareness (Koning, 2009, 2012). Employer-sponsored private disability insurance is also advocated by Autor and Duggan (2010). Current Population Survey data reveal that recessions significantly accelerate retirements of older workers over a 30-year period (Coile and Levine, 2011), while a linking of the US Census 1980, 1990, and 2000 to the 2009–11 American Community Survey reveal that local demand conditions do affect the labour and retirement behaviour of the older segment of the population, particularly in the service industry (Maestas *et al.*, 2013).

[9]In 2004 the system was reformed by the introduction of temporary DI benefits. Individuals might become temporarily unable or less able to engage in substantial gainful activity, but these conditions could improve over time. Thus temporary DI benefits were introduced for a period of 1–4 years for individuals which the Social Security Administration anticipated an improvement in work ability conditions.

[10] In one of the robustness check specifications, we take into account all variation related to the combination of industry (5-digit) and educational qualifications (6-digit) (in total 377,745 combinations) interacted with the time dummies. Thus this within industryXeducationXtime regression completely control for industry- and education-specific time trends. In another robustness check specification regional (county) linear time trends are added. In a final specification I even take into account all fixed variation between jobs, i.e. thus identification of the impact of the payroll-tax changes rests on variation over time within the same job.

[11] In the treatment zones only firms with total wage costs above the threshold experienced raised payroll taxes. The thresholds were (in nominal NOK) 270,000/0.033, 270,000/0.057, and 270,000/0.066 in zones 2, 3, and 4, respectively. In the control zone I imposed a threshold at 270,000/0.033.

[12] Arguably one should apply the 6-point distributed weights of Webb (2013) when conducting the wild bootstrap.

[13] During 1.7. 2002–31.12.2006 those 62 years of age or older got 4 percentage points reduced payroll-tax rates. See Ellingsen and Røed (2006) for an analysis of this reform.

[14] These firms are involved in the so-called EFTA-treaty industries and always pay 14.1 per cent payroll tax: electricity production by water power, extraction of crude petroleum and gas, services related to extraction of crude petroleum and gas, mining of non-ferrous metal ores, quarrying/mining of olivine and nepheline syenite, production of ESCS-steel, shipyards, production of telecom services, and finance involving activities outside Norway in the EFTA-area.

[15] A legal minimum wage does not exist in Norway. Sector-specific agreements and union recommendations make it difficult to cut wages below 10–15$ for adults in well-regulated industries.

[16] This makes it difficult to use data before 2001 to analyse the impact of payroll-tax changes on the hourly wage.

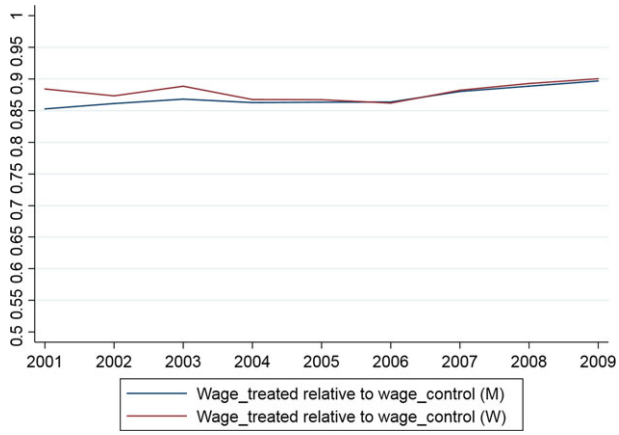
[17] The lack of a strong discontinuity in 2004 regarding wages is slightly puzzling. On the other hand, these aggregated wage averages are unconditional on the observation period, i.e. they comprise quite a few workers only present in the data before the reform (only in the pre-period) or only after the reform (in the post-period). To be easily seen, such a discontinuity has also to be considerable. In Figure A1, we plot the average wages of the reform zones relative to the control zones (for men and women). Here it appears that a minor drop occurs during 2004–06.

[18] Details on the matching procedure and results are available from the author upon request. Matching tests reveal no significant difference between the control and treatment group when it comes to the matching variables. Note that the matching variables mainly reflect labour market characteristics of employed workers (e.g., wage, industry). Because most of the analyses of the reform impact on intensive and extensive margin are based on workers (only the employment regression is relevant for non-workers), a good matching along these characteristics are deemed more relevant than matching on non-labour market characteristics.

[19] In practice, this is achieved by estimating regressions of the form Y_{igt} = intercept + $\beta_X X_{igt}$ + $\beta_1$payroll_zone$_g$ + $\beta_2 T_t$ + β_YReform_zone$_g X\ T_t$ + ε_{igt}, where T_t expresses a time vector (2002–09), Reform_zone$_g$X T_t expresses the interaction between a dummy for being a reform zone and the time vector, and X expresses a control vector.

Appendix

Figure A1. Relative average wages of the treatment and control zones.



Note: M and W denote men and women, respectively.

Table A1. Descriptive statistics on background characteristics. Main reform (2001–06).

	Men				Women			
	Pre-period		Post-period		Pre-period		Post-period	
	Treat	Control	Treat	Control	Treat	Control	Treat	Control
Years of education	2.599 (2.015)	3.175 (2.497)	2.685 (2.032)	3.281 (2.518)	2.370 (2.211)	2.941 (2.487)	2.563 (2.205)	3.177 (2.554)
Age	40.310 (10.853)	40.285 (10.801)	41.234 (10.980)	40.879 (10.877)	40.156 (11.038)	39.853 (10.954)	40.903 (11.128)	40.345 (11.021)
Manufacturing	0.293 (0.454)	0.249 (0.432)	0.270 (0.444)	0.231 (0.421)	0.168 (0.374)	0.137 (0.344)	0.152 (0.359)	0.125 (0.330)
Construction	0.160 (0.367)	0.119 (0.324)	0.174 (0.379)	0.127 (0.333)	0.022 (0.144)	0.018 (0.134)	0.022 (0.146)	0.018 (0.133)
Trade	0.170 (0.375)	0.219 (0.413)	0.171 (0.376)	0.220 (0.415)	0.373 (0.484)	0.319 (0.466)	0.379 (0.492)	0.326 (0.468)
Hotel	0.022 (0.145)	0.024 (0.153)	0.021 (0.142)	0.024 (0.154)	0.089 (0.285)	0.058 (0.233)	0.082 (0.274)	0.057 (0.231)
Transport	0.137 (0.343)	0.114 (0.318)	0.136 (0.343)	0.117 (0.321)	0.069 (0.255)	0.079 (0.270)	0.076 (0.264)	0.079 (0.269)
Welfare-service	0.123 (0.328)	0.203 (0.402)	0.128 (0.334)	0.208 (0.406)	0.268 (0.443)	0.354 (0.478)	0.277 (0.447)	0.361 (0.480)
NXT	384,854	1,585,259	376,025	1,582,964	1,92,489	958,091	186,411	949,741

Population: All workers in non-EFTA-treaty industries between 20 and 61 years of age, employed at least a fortnight in January at least once during the period 2001–09. Years of education measures years of schooling in excess of compulsory schooling.

Table A2. Descriptive statistics. Reversal of the reform (2004–09).

	Men				Women			
	Pre-period		Post-period		Pre-period		Post-period	
	Treat	Control	Treat	Control	Treat	Control	Treat	Control
Zone 1	–	0.976 (0.153)	–	0.976 (0.153)	–	0.981 (0.134)	–	0.982 (0.131)
Zone 2	0.412 (0.492)	–	0.408 (0.491)	–	0.377 (0.485)	–	0.371 (0.483)	–
Zone 3	0.092 (0.289)	–	0.087 (0.282)	–	0.098 (0.297)	–	0.094 (0.292)	–
Zone 4	0.293 (0.455)	–	0.300 (0.458)	–	0.296 (0.456)	–	0.300 (0.458)	–
Zone 5	–	0.017 (0.129)	–	0.017 (0.129)	–	0.015 (0.121)	–	0.014 (0.118)
Zone 6	0.110 (0.313)	–	0.116 (0.320)	–	0.109 (0.312)	–	0.116 (0.320)	–
Zone 7	0.116 (0.320)	–	0.113 (0.317)	–	0.134 (0.341)	–	0.133 (0.339)	–
Payroll-tax rate (%)	11.794 (2.729)	13.861 (1.821)	8.625 (2.490)	13.861 (1.820)	11.664 (2.711)	13.889 (1.711)	8.582 (2.566)	13.899 (1.670)
Hourly wage	30.215 (17.649)	34.880 (22.239)	37.058 (20.418)	41.758 (25.553)	25.068 (17.108)	28.733 (21.853)	30.640 (21.595)	34.625 (25.309)
Annual earnings	49837.7 (24655.9)	57589.7 (30886.5)	60803.2 (31230.0)	67864.4 (37631.2)	31079.1 (19288.3)	37999.8 (24663.4)	37895.1 (24611.1)	45476.8 (30950.7)
Employed all	0.715 (0.452)	0.684 (0.464)	0.746 (0.435)	0.712 (0.452)	0.714 (0.452)	0.674 (0.469)	0.737 (0.440)	0.694 (0.461)
Employed workers	0.920 (0.271)	0.922 (0.268)	0.923 (0.266)	0.920 (0.271)	0.894 (0.309)	0.892 (0.311)	0.901 (0.298)	0.895 (0.306)
Newly hired	0.110 (0.313)	0.124 (0.329)	0.120 (0.325)	0.134 (0.340)	0.111 (0.314)	0.128 (0.334)	0.130 (0.337)	0.148 (0.354)
Newly hired due to workplace entry	0.016 (0.124)	0.016 (0.125)	0.006 (0.075)	0.006 (0.074)	0.016 (0.125)	0.017 (0.129)	0.007 (0.084)	0.007 (0.080)
Work hours	35.318 (6.792)	35.431 (6.780)	35.427 (6.591)	35.489 (6.701)	28.541 (10.606)	30.252 (10.176)	29.211 (10.437)	30.859 (9.943)
Annual work hours	1706.092 (528.379)	1708.62 (532.523)	1691.0 (540.319)	1676.36 (562.1019	1331.72 (653.521)	1407.97 (649.637)	1330.99 (673.277)	1387.85 (676.07)
New zone	0.018 (0.131)	0.008 (0.087)	0.013 (0.111)	0.012 (0.107)	0.019 (0.136)	0.007 (0.081)	0.016 (0.127)	0.014 (0.119)

Table A2. Continued

	Men				Women			
	Pre-period		Post-period		Pre-period		Post-period	
	Treat	Control	Treat	Control	Treat	Control	Treat	Control
Unemployed	0.015 (0.123)	0.012 (0.110)	0.014 (0.116)	0.013 (0.115)	0.017 (0.131)	0.016 (0.123)	0.012 (0.111)	0.012 (0.108)
Employed $t + 3$	0.878 (0.327)	0.881 (0.324)	0.872 (0.339)	0.871 (0.335)	0.840 (0.367)	0.841 (0.366)	0.844 (0.362)	0.842 (0.365)
Unemployed $t + 3$	0.049 (0.216)	0.037 (0.190)	0.088 (0.284)	0.071 (0.257)	0.062 (0.241)	0.047 (0.212)	0.081 (0.273)	0.066 (0.249)
Years of education	2.685 (2.032)	3.289 (2.518)	2.750 (2.077)	3.368 (2.557)	2.563 (2.205)	3.177 (2.554)	2.793 (2.320)	3.440 (2.636)
Age	41.234 (10.980)	40.879 (10.877)	41.372 (11.157)	40.574 (10.956)	40.903 (11.128)	40.345 (11.021)	40.984 (11.237)	40.074 (11.021)
Manufacturing	0.270 (0.444)	0.259 (0.438)	0.251 (0.434)	0.203 (0.402)	0.152 (0.359)	0.125 (0.330)	0.129 (0.334)	0.102 (0.302)
Construction	0.174 (0.379)	0.127 (0.333)	0.139 (0.346)	0.105 (0.307)	0.022 (0.146)	0.018 (0.133)	0.019 (0.138)	0.016 (0.125)
Trade	0.171 (0.376)	0.220 (0.415)	0.135 (0.341)	0.159 (0.366)	0.379 (0.485)	0.326 (0.469)	0.269 (0.443)	0.226 (0.418)
Hotel	0.021 (0.142)	0.024 (0.154)	0.017 (0.128)	0.019 (0.137)	0.082 (0.275)	0.057 (0.231)	0.067 (0.253)	0.045 (0.208)
Transport	0.136 (0.343)	0.117 (0.321)	0.098 (0.297)	0.096 (0.295)	0.076 (0.264)	0.079 (0.269)	0.064 (0.245)	0.070 (0.254)
Welfare-service	0.128 (0.334)	0.208 (0.406)	0.203 (0.402)	0.303 (0.459)	0.277 (0.447)	0.361 (0.480)	0.423 (0.494)	0.499 (0.500)
NXT	376,025	1,582,964	394,220	1,717,771	186,411	949,741	190,523	1,033,436

Note: Population: Population: Employed-all: all individuals (not limited to workers). Otherwise private sector workers in non-EFTA-treaty industries between 20–61 years of age, employed at least a fortnight in January at least once during 2001–09. Years of education measures years of schooling in excess of compulsory schooling. Variables denoted by $t + 3$ imply that the outcome is measured in 3 years. The hourly wage and annual earnings are measured in 2006 U.S. dollar.

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