Do Place-based Tax Incentives Create Jobs?*

Hyejin Ku† Uta Schönberg‡ Ragnhild C. Schreiner§

Incomplete

May 2018

Abstract

Large and persistent disparities in regional employment is a main driver of inequality in income and wellbeing. To reduce these regional inequalities, programs of geographically differentiated payroll taxes have been widely used in Nordic countries. In this paper, we evaluate the effects of payroll tax changes on firm behavior, by exploiting a unique policy setting in Norway, where a system of geographically differentiated payroll taxes was suddenly abolished due to an EU regulation. Our key finding is that the increase in regional payroll taxes had only a small impact on the wages of workers. Instead, firms responded to the tax increase primarily by reducing employment. Our findings suggest that in settings characterized by rigid wage setting, place-based tax incentives may be effective in stimulating local employment.

Keywords: Payroll taxes, regional tax incentive, firm behavior, labor demand
JEL Codes: D22, H25, H32, J18, J23

* We thank seminar participants at UCL and Statistics Norway. Ragnhild Schreiner acknowledges funding through a research project financed by the Norwegian Research Council (grant number 219616). This work is part of Oslo Fiscal Studies at the Department of Economics, University of Oslo. Data made available by Statistics Norway have been essential for the research project.
† University College London, Department of Economics and CReAM. h.ku@ucl.ac.uk
‡ University College London, Department of Economics and CReAM. u.schoenberg@ucl.ac.uk
§ University College London, Department of Economics, CReAM, and Ragnar Frisch Centre for Economic Research. uctprcs@ucl.ac.uk

1 Introduction

An individual's place of residence is a strong predictor for her employment status, income and wages. For instance, in 2008, a worker in Flint – the US city with the largest unemployment rate –was nearly five times more likely to be unemployed than a worker in Iowa City (Kline and Moretti, 2013). In Europe, unemployment in 2016 ranged from around 2% in certain regions in Germany and the Czech Republic to around 30% in parts of Greece and Spain (Eurostat, 2017). Moreover, regional differences in employment rates and income are persistent over time (Kline and Moretti, 2013; Blanchard and Katz, 1992). Surprisingly regional differences are also persistent across generations, and regions are a strong predictor for whether a child from a low-income family succeeds in moving up to the top quintile of the earnings distribution (Chetty et al, 2014).

To reduce these regional inequalities, governments have implemented a variety of spatially targeted or place-based policies. A prominent example is large public infrastructure spending such as the Tennessee Valley Authority (Kline and Moretti, 2014) and Appalachian Regional Commission (Glaeser and Gottlieb, 2008) in the US, the Urban Enterprise Zones introduced by Margeret Thatcher in the UK in the 1980s (Devereux, Griffith and Simpson, 2007), and the European Regional Development Fund that grant business subsidies and public investments to low-employment regions of Europe (Becker, 2012).[1] Another important policy tool is to provide tax incentives for firms. In particular, programs of regionally differentiated payroll taxes—flat taxes levied on firms, proportional to workers' earnings—have been widely used in the Nordic countries to stimulate employment in less developed areas. [2] Despite the multitude of programs that have been implemented, empirical knowledge on the effectiveness of place-based policies, in particular the effects of regional tax incentives for firms, is scarce (Bartik, 1991).[3]

[1] Other examples of region-targeted programs are given in Kline and Moretti (2014).

[2] Geographically differentiated *business* taxes provide an alternative means to stimulate employment in under-developed regions; see Bartik (1991) and Glaeser (2001) for a review of this literature and theoretical considerations.

[3] There exist a small literature evaluating the Federal Empowerment Zones (EZ) Program in the US, a program that introduced place-based tax incentives and block grants to stimulate employment and growth in poor areas of the country. The evidence on employment effects from this literature is mixed (see Peters and Fisher (2002) for a review, and Bondonio and Greenbaum (2007); Elvery (2009); Ham, Imrohoroglu and Song (2011); Hanson (2009); Hanson and Rohlin (2011); and Neumark and Kolko (2010) for more

This paper aims to contribute to reducing this gap in knowledge. In particular, we look at the effects of a place-based tax incentive on firms' employment decisions by exploiting a unique policy setting in Norway, where a system of geographically differentiated payroll taxes was suddenly abolished. In 1999, the European Free Trade Association (EFTA) Surveillance Authority ruled that the Norwegian system of geographically differentiated payroll tax rates was not in compliance with EU trade regulations. The EFTA then ordered a tax rate harmonization to take place between 2004 and 2006, with which the government of Norway complied. This created arguably exogenous variation in the payroll tax rates that firms in different regions faced over time, since the reform was adopted and implemented independently of the local labor market developments. We take advantage of this setting to look at firms' adjustments to the abolition of the regional tax incentive, and hence to evaluate the effectiveness of such programs in stimulating local employment.

Our identification strategy exploits the fact that the relevant tax rates are determined by the location of the worker rather than that of the firm. In particular, depending on the pre-reform composition of workers, different firms (even within the same region) are differentially exposed to the tax changes induced by the reform. Alongside the tax reform, a national-level subsidy scheme was implemented to facilitate firms' transitions to paying higher tax rates. Since the subsidy scheme rendered small firms entirely unaffected by the reform, we can use small firms for a placebo analysis. Interestingly, in 2007 the geographically differentiated system was re-introduced, after an appeal by Norway on EFTA's ruling. This setting thus provides an opportunity to assess whether firms' adjustments to the tax increase is reversed once the pre-reform tax incentives are restored.

Our key finding is that overall, the increase in payroll taxes has only a relatively small impact on the wages of workers. Instead, large firms respond to the tax increase primarily by reducing employment. The drop in employment is particularly pronounced in multi-establishment firms, which cut employment primarily through establishment exits and reduced hiring. About half of the tax-induced establishment exits are "strategic" exits, where the firm moves a large part of its workforce to a new firm, likely to receive another subsidy and to thereby essentially circumvent the tax increase. When looking at the subsequent of the reform in 2007, we find that the reform-induced employment effects are not reversed.

recent studies). Busso, Gregory and Kline (2013) provide a comprehensive empirical welfare analysis of the EZ program, and find an increase in employment and wages in EZ neighborhoods compared to control neighborhoods.

Overall, our results suggest that the pre-reform regional tax incentives may indeed have been effective in stimulating regional employment in remote areas in Norway. Moreover, our findings of reduced employment, in part driven by increased establishment shut-downs to circumvent the tax increase, coupled with a small decrease in wages illustrates the importance of the institutional context in determining firm responses to payroll taxes. On the one hand, the high degree of unionization and the centralized wage bargaining scheme in Norway may have left little room for wage cuts by firms. On the other hand, the specific subsidy scheme provided incentives to firms to move entire establishments to a new firm, which amplified the employment effect at the firm level. Our findings further point to the possibility that in a regulated labor market like the Norwegian one, even temporary tax increases may have permanent employment effects. This could be because in our context, a considerable share of the overall employment drop in (multi-establishment) firms is through establishment exits, which might be difficult to reverse.

By using externally imposed changes to payroll taxes and administrative register data, we contribute to understanding the role of place-based policies in creating employment. In particular, our key contribution is to look at how firms respond to the imposition and removal of tax incentives. A few studies have examined the wage and employment effects of regional payroll taxes and they tend to find small employment effects and partial wage shifting (see Bohm and Lind (1993) and Bennmarker, Mellander and Öckert (2009) for Sweden; Korkeämaki and Uusitalo (2009) for Finland; Johansen and Klette (1997) and Stokke (2015) for Norway, and Cruces, Galiani and Kidyba (2010) for Argentina).[4] We depart from the literature in two main ways. First, we focus on tax *increases* rather than *decreases*. Second, we investigate the labor market effects of tax increases at the level of the *firm,* as opposed to the *region,* allowing us to investigate in more detail than the existing literature the channels through which labor markets adjust to tax changes. Part of the employment drop in large affected firms following the tax increase may be absorbed by smaller firms in the region, which were not directly affected by the tax reform.[5] We do not find strong employment gains in small firms located in the same tax zones as the affected firms, relative to other small firms in the post-

[4] The overall findings from this literature is in line with most of the early literature on the incidence of national payroll tax changes, where researchers tend to find substantial wage shifting, and small or no employment effects. See Murphy (2007) and Anderson and Meyer (1997; 2000) for the US and Gruber (1997) for Chile.

[5] When conducting the analysis at the regional level, we lack the power to detect significant employment effects, for two reasons. First, there are only five tax zones with differential payroll tax changes in our context. Second, not all firms in the region were effectively affected by the tax reform.

reform years, suggesting the employment reductions that we uncover at the firm level translate in part to employment reductions also at the regional level.

A second set of papers investigate *national* changes in payroll taxes targeted at a particular group of workers, such as labor market entrants (Saez, Matsaganis and Tsakloglou, 2012; young workers (Saez, Schoefer and Seim, 2017); or low income earners (Lehmann, Marical and Rioux, 2013) and are thus not directly comparable to our set-up. These papers have found mixed evidence on the wage and employment effects of payroll tax changes.

The paper proceeds as follows. In Section 2 we explain how the policy setting allows us to compare firms that were affected by the tax increase with both unaffected firms in other parts of the country, and unaffected firms located in the same regions as the affected firms. In Section 3 we formalize our empirical strategy, and in Section 4 we present the data. Section 5 starts by presenting results from an event study, demonstrating that "treated" and "control" firms indeed experienced similar developments in employment and wages prior to the reform. Next, we present our main results, and contrast them with the results from a set of "placebo" estimations. Section 6 investigates the mechanisms behind the findings of large employment effects, and Section 7 analyzes the wage and employment effects of the subsequent 2007 payroll tax decrease. Finally, Section 8 concludes with a discussion of the effectiveness of place-based tax incentives in stimulating employment and hence in reducing regional inequality.

2 Institutional Setting and Data

2.1 The Payroll Tax Harmonization Reform

Norway runs a generous social security system to finance pension benefits and health insurance, as well as unemployment, disability and welfare benefits. While employees contribute 8.2% of their gross pay to the scheme, regardless of where they reside, employers' contributions—or payroll taxes—are geographically differentiated. Even though some employers pay significantly more into the system than others, all employees draw the same benefits from the scheme. The motivation behind geographically differentiated payroll taxes is to stimulate employment in more remote areas of the country. Until 2006, the country was divided into five tax zones, with payroll tax rates ranging from 14.1% in the central areas to 0% in the northernmost regions (see Panel (a) of Figure A1 in the Appendix). The relevant tax rates faced by a firm was determined by the locations of the workers rather than the location of

the firm. This meant that firms located in the same tax zone could face different average tax rates depending on the locations of residencies of their workers. Even workers within the same firm could be subject to different payroll tax rates if they lived in different tax zones.

In 1999, the European Free Trade Association Surveillance Authority (ESA) ruled that the Norwegian system of geographically differentiated tax rates was not in compliance with trade regulations agreed on by the EU, and the EEA countries Norway, Iceland and Lichtenstein. Norway contested the ruling, arguing that the differentiated tax rates (with only minor changes for certain industries) should be considered as *direct transport aid* in line with EU-EEA legislation. ESA approved the proposal, and Norway was allowed to keep the system until 2003. In September 2002, however, ESA sent a letter to Norwegian authorities requiring that the system had to be changed, and Norway was asked to propose a change by March 25th 2003, that was to be implemented by January 1st. 2004.[6] As a result, a tax rate harmonization was imposed between 2004 and 2006. The resulting payroll tax changes in the different zones are illustrated in Figure 1. Zone 5 (the northernmost region) was allowed to keep its zero payroll tax rate. Zone 1 (central areas) was likewise unaffected, and the payroll tax rate remained constant at 14.1%. In zone 2 the harmonization took place immediately in 2004, raising the tax rate from 10.6% in 2003 to 14.1% in 2004, while the harmonization was more gradual in zones 3 and 4, raising the payroll tax rate by 5.7 and 6.6 percentage points over a three-year period. In 2007, another EFTA ruling allowed Norway to re-introduce the system of differentiated payroll taxes (after an appeal case), and tax rates were reduced to their pre-2004 levels.

The externally imposed harmonization provides an ideal setting to study firm adjustments to payroll taxes, since the changes in the average payroll tax rate faced by firms were imposed by EFTA, and are therefore likely to be independent of the local labor market business cycles. Anticipation adjustments to the 2003 tax reform are possible, as firms knew as of September 2002 that some changes would have to be made. The extent and timing of the changes was however not laid out until March 2003. Anticipation effects of the restoration of the differentiated system in 2007 on the other hand, are unlikely, as the restoration of the system in 2007 was still under negotiation in the summer of 2006 (Aftenposten, 10.02.2006).[7]

[6] Norway's reply to ESA, 25.03.2003: State aid. Differentiated social security contributions in Norway. https://www.regjeringen.no/no/dokumenter/state-aid-differentiated-social-security-4/id91117/

[7] A search for the keywords "Arbeidsgiveravgift" (payroll tax) and "EFTA [ESA]" in the archives of Norway's largest newspaper gives 40 [32] hits in 2003, and only 2-6 [3-6] hits annually for the years 2004-2007.

While complying with the EU ruling of tax harmonization, the government of Norway at the same time implemented a subsidy scheme, to ease the burden of the higher payroll taxes introduced in 2004 (especially for small firms). In particular, firms (in most sectors) could pay the pre-reform (2003) payroll tax rate (τ_t^l) for the wage bill up to a cap, after which firms would pay the contemporary statutory tax rate (τ_t^o) for the remaining wage bill (τ_t^o and τ_t^l are reported in Table A1 in the Appendix). It is important to emphasize that the subsidy is computed at the level of the *firm* (as opposed to the *establishment*); the wage bill therefore refers to the firm's total wage bill across all establishments. The cap was set such that firms were given a maximum tax relief (subsidy) of 270,000 NOK (38,136 USD) per year.[8] Accordingly, the actual subsidy received by a firm varied over time (as the harmonization proceeded), and depended on the worker composition of the firm. Specifically, the subsidy for firm *j* in year *t* is given by:

$$S_{j,t} = min\left(\sum_{i=1}^{N_{j,t}} w_{i,t} \times (\tau_{i,t}^o - \tau_{i,t}^l)\right), \bar{S}), \qquad (1)$$

where $w_{i,t}$ is the total earnings of worker *i* in year *t* (across all establishments of the firm), $N_{j,t}$ is the number of workers in firm *j* (across all establishments) in year *t*, and $\bar{S}$ is the maximum subsidy. This means that firms below a certain size were *unaffected* by the tax reform (assuming no immediate spillover effects), even if they experienced an increase in the average statutory tax rate.

[FIGURE 1: Statutory Payroll Tax Rate by Tax Zone]

2.2 Wage setting and Firing Costs in Norway

The system of wage bargaining in Norway is characterized by central bargaining and a high degree of unionization. In 2014, 52% Norwegian workers were members of a trade union, and close to 70% of workers in the private sector were employed in firms that were members of an employer federation (FAFO, 2014). Even though only firms that belong to an employer federation are legally required to pay union wages, non-member firms often do so as well. The guiding idea behind the wage bargaining system is that the outcome of wage negotiations in the sectors exposed to foreign competition should set the norm for wage growth also in other

[8] All monetary amounts in this paper are converted to USD using the average exchange rate for 2003 where 1 USD = 7.08 NOK.

sectors of the economy. In this way, overall wage growth is linked to productivity and profitability growth in the exposed sectors.

In practice, the main federation of trade unions (*Landsorganisasjonen i Norge*) and the main private sector employee federation (*Næringslivets Hovedorganisasjon*) bargain over wages in the manufacturing sector, based on a common assessment of the economic situation produced by a committee with broad representation. This centralized wage bargaining typically determines a minimum wage increase, while leaving some room for local negotiations of supplementary wage increases at the firm level. The local negotiations are supposed to take into account a firm's profitability, productivity, expectations for the future and ability to compete (NOU 2013:13). Despite the manufacturing sector being quite small in Norway, the outcome of the centralized negotiations in this sector has usually served as an effective norm for wage growth both in other private sectors and in the public sector (Kahn, 1998; Gjelsvik, Nymoen and Sparrman, 2015). While there is some room for wage negotiations at the firm level, wage setting is more centralized in Norway than in most other comparable countries (OECD, 2016).

Similarly, employment relationships are relatively rigid in Norway[9]. The termination of employment contracts of private sector employees is regulated by the Norwegian Working Environment Act. To terminate a contract, employers must have reasonable grounds, such as firm downsizing (Tekna, 2015).

3 Empirical Strategy

The main challenge to evaluating a regional tax incentive policy is that the policy is usually implemented in response to a poor economic development. This makes it difficult to find a comparable control group to construct a counterfactual outcome – i.e., the outcome in the absence of the place-based policy – for the affected regions. We overcome this challenge by exploiting a setting where the existing place-based policy was suddenly abolished due to an EFTA ruling, together with the fact that the relevant tax rates are determined by the location of the worker rather than that of the firm. There are two reasons why firms employ workers from different locations. First, firms may have establishments in different tax zones. Second, even within establishments, workers may come from different tax zones, in particular if the

[9] In a cross-country comparison of "difficulty of firing" conducted by the World Bank, Norway scores 40 on an index from 0-100. For comparison, the US has a score of 0, France a score of 40 and the UK a score of 10 (World Bank, 2006).

establishment is located close to the tax zone border. Our identification strategy compares changes in firm outcomes before and after the abolition of geographically differentiated payroll taxes between more and less exposed firms. In addition, due to the subsidy scheme in place, small firms remained entirely unaffected by the reform, rendering small firms suitable for a placebo analysis.

We first explain in detail how we make use of the variation in *statutory* tax rates (Section 3.1), and next, how the subsidy scheme creates variation in *effective* tax rates according to the size of the firm (Section 3.2).

3.1 Changes in the Statutory Tax Rate

We start out by constructing a measure of the increase in the statutory tax rate faced by a firm (ignoring the subsidy scheme), for each of the post-reform years 2004-2006, based on the tax zones of residency of the workers employed in the firm in the pre-reform year (2003). Since we fix a firm's worker compositions to the pre-reform year, the variation in our exposure measure is driven by changes in the statutory payroll taxrates, and not by potentially endogenous changes in the worker composition of a firm. The *predicted* average statutory tax rate (hereby "statutory tax rate") faced by firm *j* in year *t* is given by:

$$\bar{\tau}_{j,t} = \sum_{i=1}^{N_{t=2003}} \omega_{i_{(j)}} \times \tau_{i_{(z,j)}t}, \tag{2}$$

where *i* denotes worker and $\tau_{i_{(z,j)}t}$ is the statutory tax rate in year *t* of worker *i* who worked in firm *j* and resided in tax zone *z* in 2003. The worker-specific payroll tax rate is weighted by the worker's share in the firm's total wage bill in 2003 ($\omega_{i_{(j)}}$). Not surprisingly, the average statutory tax rate is strongly correlated with where the firm's headquarter is located: firms primarily employ workers who reside in the same tax zone as the firm's headquarter. However, in 2003, 42% of firms hired at least one worker living in a different tax zone than where the firm's head quarter is located. Overall, 95.8% of the variation in the differential change in the statutory tax rate across firms is between tax zones, and 4.2% within tax zones. Based on the average statutory tax rate of firm *j* in year *t* given by equation (2), we then we construct a measure of a firm's overall exposure to the tax harmonization as follows:

$$\Delta\bar{\tau}_j = \bar{\tau}_{j,t=2006} - \bar{\tau}_{j,t=2003} \tag{3}$$

In parts of the analysis, we will split firms into two groups based on their overall exposure:

$$ST_j = \begin{cases} 1 & if\ \Delta\bar{\tau}_j \geq 4\ pp. \\ 0 & if\ \Delta\bar{\tau}_j < 4\ pp. \end{cases} \quad (4)$$

The cut-off of four percentage points is chosen to ensure that the treated firms experienced a significant tax increase.

Panels (b) and (c) of Figure A1 in the Appendix show the geographical distribution of statutory affected firms. While firms which experienced some change in payroll taxes can be found in all parts of Norway (panel (a)), the statutory treatment firms, which experienced a predicted change in the statutory tax rate of at least four percentage points, are primarily located in tax zones 2, 3 and 4 that were most affected by the tax harmonization (panel (b)).

3.2 Changes in the Effective Tax Rate

Because of the subsidy scheme described in Section 2.1., not all firms are affected by the tax reform, even if they experienced an increase in the predicted *statutory* tax rate. As shown in equation (1), the actual subsidy firms receive in a post-reform year depends on the firm's current wage bill—which may be affected by the tax reform. To compute the firm's predicted *effective* tax rate, we first predict the subsidy that a firm will receive based on the firm's pre-reform (2003) wage bill.[10] The firm's predicted *effective* tax rate then is:

$$\bar{\tau}^*_{j,t} = \bar{\tau}_{j,t} - (\hat{S}_{j,t} / \sum_{i=1}^{N_{j,t=2003}} w_{i,t=2003}), \quad (5)$$

where $\bar{\tau}_{j,t}$ is the statutory tax rate (given by (2)), $\hat{S}_{j,t}$ is the predicted subsidy, and $\sum_{i=1}^{N_{j,t=2003}} w_{i,t=2003}$ is the firm's total wage bill prior to the reform, in 2003. The change in the effective tax from 2003 to 2006 is then given by:

$$\Delta\bar{\tau}^*_{j,t} = \Delta\bar{\tau}_{j,t} - (\hat{S}_{j,2006} / \sum_{i=1}^{N_{j,t=2003}} w_{i,t=2003}) \quad (6)$$

[10] That is, $\hat{S}_{j,t} = min\left(\sum_{i=1}^{N_{j,t=2003}} w_{i,t=2003} \times (\tau^o_{i,t} - \tau^l_{i,t})\right), \bar{S})$.

In Figure 2, we illustrate the 2003-2006 changes in the statutory (equation (3)) and effective (equation (6)) tax rates for "statutory treated" firms (firms that experience an increase in the statutory tax rate of at least four percentage points, panel (a)) and "statutory control" firms (panel (b)), respectively. In the figure, the vertical line indicates the cut-off wage bill below which firms are unaffected in terms of their effective tax rate. Firms with an annual wage bill of less than 4,100,000 NOK (around 580,000 USD) in 2003 are not affected by the tax increase (assuming their work force remains unchanged). From here on, we denote firms with 2003 wage bills below the cutoff as "small firms" and those above the cutoff as "large firms". On average, large, statutory treated firms experience an increase in the statutory and effective tax rates of 6.2 and 2.3 percentage points, while large, statutory control firms on average experienced an increase in the statutory and effective tax rates of 0.4 (0.1) percentage points.

[FIGURE 2: Average Tax Increase from 2003-2006 over Firm Size]

3.4 Event Study and Baseline Regression Equation

We start our empirical analysis by conducting an event study that compares (relative to the pre-reform year 2003) employment and wages in large statutory treated firms (i.e., effectively treated firms) and large statutory control firms (i.e., large firms unaffected by the tax reform) in the years prior to and following the tax reform. The event study allows us to assess whether the two types of firms experienced similar time trends prior to the 2003 reform, but diverge afterwards. This further allows us to trace out the dynamic effects of the payroll tax cuts on economic outcomes.

In a next step, we exploit the variation in the statutory tax rates over time and across firms more fully, and estimate the following regression on the sample of large firms:

$$\ln(y_{j,t}) = \beta \ln(1 + \bar{\tau}_{j,t}) + \rho_t + \delta_j + \varepsilon_{j,t}, \qquad (7)$$

where $y_{j,t}$ is the outcome variable of interest (such as employment and wages) in firm *j* in year *t*; $\bar{\tau}_{j,t}$ is the statutory tax rate based on the firm's worker composition in 2003, as defined in equation (2); ρ_t and δ_j denote year- and firm fixed effects, and $\varepsilon_{j,t}$ is an error term. The parameter of interest, β, measures the impact of a one percent increase in the firms' statutory tax rate on the outcome of interest. It should be noted that we use firm rather than establishment

as the unit for analysis since the subsidy scheme explained in Section 2.1 was enforced at the firm level. We weight the regression by the number of employees in the firm in 2003, and cluster standard errors at the level of the firm's municipality.[11]

We finally conduct a placebo event study and run a set of placebo regressions on the sample of small firms, that because of the subsidy scheme, did not experience an increase in the effective payroll tax rate, even though their statutory tax rate increased. An absence of any effects of the tax reform on small firms provides reassurance that the treatment effects on large firms are indeed caused by the tax reform, and not by differential macroeconomic conditions in different regions of the country.

4 Data

Our analyses make use of several sources of administrative register data, provided by Statistics Norway, that can be linked through unique firm, establishment and worker identifiers. The main data source is the linked employer-employee register that include information on all firms, establishments and workers covered by the social security system in Norway, for the period 1999 to 2009. The data include information on the number of days a worker worked during the year, her wage, the dates when she started and stopped working for a particular establishment, the establishment and firm identification number, as well as the firm's and establishment's location (municipality) and sector affiliation. We match these data to data on worker demographics, including education, labor market experience, age, gender and country of origin. We further make use of a longitudinal database with information on workers' municipality of residence.

We aggregate the worker-level data to the level of the firm, and create a balanced sample of all private sector firms at least two employees every year over the years 2000-2006.[12] Restricting the sample to firms that exist over the entire investigation period could possibly bias our findings if the tax reform affected firm survival. We investigate this in Section 5.3, and find the impact of increases in the statutory payroll tax on firm survival to be close to zero.

[11] There are 422 municipalities in Norway.

[12] We drop firms with missing information on municipality, and we further exclude firms in the 99th percentile of the firm size distribution (measured by number of employees). Private sector firms include the following organizational forms: general partnership, general partnership with shared liability, limited companies, public limited company, limited liability companies, sole proprietorship and Norwegian division of foreign business enterprise.

Table 1 presents descriptive statistics on the balanced sample of 43,561 firms. There are 4,890 "statutory treated" firms, which together employ 59,425 workers. Out of the 4,890 statutory treated firms, 954 are large and hence experienced changes in both the statutory and *effective* tax rates, whereas 3,936 firms are small, and hence experienced an increase in the statutory tax rate only. The 954 large firms make up 56% of total employment among the "statutory treated" firms. There are 38,671 statutory control firms, of which 9,822 are large and 28,849 small. The statutory control firms together employ 551,014 workers, out of which the large statutory control firms employ 67%.

Large statutory treated firms are somewhat larger than large statutory control firms, and are more likely to be in agriculture/oil/mining, manufacturing and construction, and less likely to be in wholesale and retail and finance/insurance/property management. Differences between small statutory treated and statutory control firms are smaller. It should be noted that our empirical approach will account for any time-constant differences between statutory treated and statutory control firms through the inclusion of firm fixed effects. We will further probe the robustness of our results by allowing for industry-specific time trends.

[TABLE 1: Descriptive Statistics]

5 Results

5.1 Event Study

We start our empirical analysis with an event study that compares, relative to the pre-reform year 2003, employment and wages in large statutory treated firms (i.e., large firms primarily located in more remote areas that experienced an increase in their effective tax rate; "treated" firms) and large statutory control firms (i.e., large firms primarily located in central areas that neither saw an increase in their statutory tax rate nor in their effective tax rate; "control" firms). The results are presented in panel (a) and (b) of Figure 3, and show that employment (head counts) and wages (per day worked) evolved similarly in the two type of firms in the years prior to the reform (panel (a)). After the reform, however, employment sharply declines in treated firms relative to control firms. Firm wages also grow at a slightly lower rate in treated relative to control firms in the post-reform years—although the difference is not statistically significant at conventional levels (panel (b)). Panel (c) and (d) of Figure 3 show the

corresponding series for small firms, that, according to their worker composition in 2003, would be (almost) unaffected by the reform, regardless of where they are located. The results are striking: Whereas we see a dramatic drop in employment among large statutory treated firms relative to large statutory control firms after the tax reform, employment in small statutory treated and statutory control firms grow at similar rates both before and after the tax reform. This provides reassurance that the employment drop observed in large firms is indeed caused by the tax reform, and not by differential macroeconomic conditions in remote and central regions. The findings on small firms further suggest that spillover effects from large statutory treated to small statutory treated firms are not large.

[FIGURE 3: Firm Outcomes over Time]

5.2 Regression Results

We next exploit the variation in the statutory tax rates over time and across workers more fully, by estimating equation (7) separately for large and small (placebo) firms. We report the estimated coefficients Table 2. The results confirm the findings from the event study: increases in the statutory tax rate strongly reduces employment in large firms (columns (1) and (2)), but not in small firms (columns (4) and (5)). The estimate for large firms in column (1) imply that a one percent increase in in the statutory tax rate reduces employment—measured as the head count of workers—by about 1.9%. Estimates are similar when we use man-days as the dependent employment variable (column (2)), suggesting that firms adjust employment almost entirely on the extensive margin. Column (3) shows that the tax increases also lead to a modest drop in wages: a one percent increase in in the statutory tax rate reduces wages by about 0.26%. In line with the findings from the event study in Figure 3, changes in the statutory tax rate have no impact on employment and wages in small firms who were effectively unaffected by the reform (columns (5) to (7)).

[**TABLE 2: Main Regression Results]**

Table 1 revealed some differences between large statutory treated and control firms in industry affiliation. To rule out that the drop in employment in treated firms relative to control firms after the tax reform simply reflects differential macroeconomic trends across industries,

we next allow for fully flexible time trends that are allowed to vary by industry (panel (a) of Table A2 in the Appendix). This has little effects on our estimates.

The tax reform was implemented the same year as the 2004 expansion of the EU, and we might therefore worry that the inflow of labor from Eastern Europe affected treated and control firms differently.[13] To assess this, we restrict the sample to Norwegian-born workers (panel (b) of Table A2). The results from this robustness exercise are very similar to the baseline results.

5.3 Firm Survival

The analysis so far is based on a balanced sample of firms that operate all years between 2000 and 2006. The tax reform, however, may have affected the probability of firm survival. To investigate this, we create an unbalanced sample of firms that operate (with at least two workers) in 2003. As in our main sample, we drop firms in the 99th percentile of the firm size distribution. This gives an unbalanced sample of 14,677 large firms, of which 1274 are statutory treated firms that experienced an increase in their effective pay roll tax rate and 13,403 are statutory control firms that experienced neither an increase in the statutory nor in the effective tax rate.

Panel (a) of Figure 4 shows the share of firms that are operating for each of the years 1999 to 2006, separately for large statutory treated and large statutory control firms. In each year prior to the tax reform, control firms are on average less likely to operate than treated firms. This might be explained by control firm on average being younger than treated firms; as of 2003, 5% (70%) of the control firms are one (at least six) years, compared to 4 (74%) of treated firms [14] Following Saez et al. (2017), we re-weight the group of control firms to match the age distribution in the group of treated firms, using DFL-weighting as developed by DiNardo, Fortin, and Lemieux (1996). The DFL weights are given by the following equation:

$$DFL(age_j) = \frac{\widehat{Pr}\left(ST_j = 1 \middle| age_{j,t=2003}\right)/\widehat{Pr}(ST_j=1)}{\widehat{Pr}\left(ST_j = 0 \middle| age_{j,t=2003}\right)/\widehat{Pr}(ST_j=0)} \quad (8)$$

where ST_j is the treatment indicator defined by equation (4) and $age_{j,t=2003}$ is a variable for firm age in year *t*. As shown in panel (b) of Figure 4, once DFL-weighted by firm age, the

[13] From 2003-2006, the share of workers from "new EU" countries increased from 0.005 to 0.012 in large statutory control firms, and from 0.002 to 0005 in large statutory treated firms.
[14] We can only observe whether firms are up to six years old. The age variable therefore takes on six values where the highest value means that firm is aged six years or more.

statutory treated and control firms seem comparable in term of firm survival (based on the pre-reform survival shares), and we conclude that the payroll tax increase has no differential effect on the survival of treated versus control firms.

[FIGURE 4: Firm Survival]

5.4 Single- versus Multiple- Establishment Firms

Figure 5 highlights that the employment drop following the tax reform is particularly pronounced in multi-establishment firms. In our sample, 17% of the large firms are multi-establishment firms in 2003; these firms employ 73% of all workers in large firms in that year. The number of establishments per multi-establishment firm ranges from 2 to 45, with an average of 3.8. The figure compares, in an event study, employment and wages in large statutory treated and control firms, separately for single-establishment firms (panels (a) and (b)) and multiple-establishment firms (panels (c) and (d)). In both single- and multi-establishment firms, employment evolves at a roughly similar rate prior to the 2003 tax reform. In contrast, three years after the tax reform, employment declines in treated relative to control firms by 17% in multi-establishment firms, but by only half as much (8%) in single-establishment firms. The employment adjustment is immediate in multi-establishment firms, but more gradual in single-establishment firms. At the same time, there is some evidence that single-establishment firms—but not multiple-establishment firms—responded to the tax reform by reducing wages.

In sum, the large overall employment effect appears to be primarily driven by multi-establishment firms.

[FIGURE 5: Firm Outcomes Over Time – Single- and Multi-Establishment Firms]

6 Adjustment Mechanisms

How did firms adjust their employment? And which factors can account for the particularly large drop in employment in multi-establishment firms following the tax reform? Two important channels through which multi-establishment (but not single-establishment firms) may adjust employment are increased establishment exit and reduced establishment entry.

Moreover, the subsidy scheme that rendered small firms unaffected by the reform gives firms an incentive to move part of their work force (or entire establishments) to a new firm, in order to receive another subsidy and thereby essentially circumvent the tax increase. We call this "strategic exits". Next, we provide evidence that a large part of the overall employment drop in multi-establishment firms, in response to the tax reform, can be accounted for by increased establishment exit, and that about half of these exits are strategic exits.

6.1 Establishment Entry and Exit in Multi- Establishment Firms

We begin by conducting an event study that compares (relative to 2003) the number of establishments per firm in large statutory treated, multi-establishment firms, and large statutory control, multi-establishment firms in the years before and after the tax reform (Figure 6). Panel (a) of the figure illustrates that the number of establishments per firm evolved similarly in treated and control firms prior to the 2003 tax reform. Immediately after the reform, however, the number of establishments per firm declines in treated firms, but increases in control firms. Three years after the tax reform in 2006, there are 12% fewer establishments in treatment relative to control firms.

To further understand the contribution of establishment exit and entry to the overall employment drop in multi-establishment firms, we estimate the following difference-in-differences (event study) regression equation on the sample of large multi-establishment firms:

$$y_{j,t} = \rho_t + \lambda\, ST_j + \sum_{\kappa \neq 2003} \gamma_\kappa\, ST_j \cdot I(t = \kappa) + v_{j,t}, \qquad (9)$$

where ρ_t is a vector of year dummy parameters, ST_j is the treatment indicator equal to 1 if the firm experienced an increase in the statutory tax rate of at least 4% (equation (4)), and $I(t = \kappa)$ is an indicator variable equal to 1 for the year $t = \kappa$. We weight regressions by the number of workers in the firm in 2003, and cluster standard errors at the level of the municipality of the firm. In panel (b) of Figure 6, we plot the estimated coefficients $\hat{\gamma}_\kappa$ (for $\kappa = 2000 - 2006$) from this regression, where the dependent variable is the number of establishments per firm, scaled by the number of establishments in the firm in 2003 ($\frac{estab_t}{estab_{t=2003}}$). The coefficients measure the percentage change in the number of establishments between year *t* and the pre-reform year 2003 in treated relative to control firms, and correspond to the difference between

the dashed blue and solid black lines in panel (a). As implied by panel (a), the figure reveals a drop in the number of establishments in treated relative to control firms immediately after the tax reform.

To decompose the decline in the number of establishments in treated firms into increased establishment exits and reduced establishment entry, we start with the identity that the change in the number of establishments per firm between year *t* and 2003 is equal to the difference between the number of establishments that entered and that exited over that period: $estab_t - estab_{2003} = estab_entry_t - estab_exit_t$. Next, we estimate regression equation (9) using the number of establishments that entered or exited the firm, scaled by the number of establishments in 2003, as the dependent variable ($\frac{estab_entry_t}{estab_{t=2003}}$ and $\frac{estab_exit_t}{estab_{t=2003}}$). The results, presented in panel (c) of Figure 6, show that the drop in the number of establishments per firm in treated relative to control firms is primarily accounted for a by an increase in establishment exit, rather than a reduction in establishment entry.

The large increase in establishment exits in treated firms in response to the tax reform may seem surprising. Panel (d) of Figure 6 shows however, that nearly half of the overall establishment exits are strategic exits, where the firm moves at least 60% of an establishment's workforce to a new firm (the blue dashed line relative to the black solid line in the figure).[15] This finding is consistent with the idea that firms strategically restructure in order to qualify for another annual tax subsidy, and thereby avoid the increased tax rates.

How much of the overall employment reduction in multi-establishment can be accounted for through establishment exits and entry, as opposed to employment changes in continuing establishments? To address this question, we start with the identity that the change in firm employment between year *t* and 2003, $E_t - E_{2003}$, is equal to the difference in the number of workers who joined the firm because of establishment entry and who left the firm because of establishment exit between 2003 and *t*; plus the number of workers who joined or left continuing establishments in the firm between 2003 and *t*:

$$\underbrace{\frac{E_t - E_{2003}}{E_{2003}}}_{\text{total change in employment}} = \underbrace{\frac{workers_estab_entry_t}{E_{2003}} - \frac{workers_estab_exit_t}{E_{2003}}}_{\text{external adjustment}} + \underbrace{\frac{workers_hire_cont_t}{E_{2003}} - \frac{workers_sep_cont_t}{E_{2003}}}_{\text{internal adjustment}} \quad (10)$$

[15] The coefficients for strategic exits are obtained from estimating regression equation (9) using the number of strategic establishment exits, divided by the number of establishments in 2003, as the dependent variable.

We thus estimate regression equations (9) using the three variables from equation (10) (i.e., the total change in employment, external and internal adjustments) as dependent variables. The results are presented in panel (a) of Figure 7. One year after the tax reform, nearly the entire employment drop among multi-establishment, treated firms is accounted for by external adjustments. Three years after the reform, about 40% of the overall employment reduction is due to internal adjustments in continuing establishments.

6.2 Internal Adjustments: Hiring versus Separations

How do continuing establishments internally adjust employment—through reduced hiring or through increased separations (as shown in equation (10))? In panel (b) of Figure 7, we decompose internal adjustments three years after the reform, in 2006, into these two components. The figure shows that in multi-establishment firms, the reduction in employment in continuing establishments is driven entirely by reduced hiring. In single-establishment firms, on the other hand, hiring and separations each account for around half of the overall drop in employment.

To summarize, multi-establishment firms respond to the tax increase in a somewhat extreme way. Whereas wages in these firms barely decline, employment sharply drops—primarily through increased establishment exits and reduced hiring. Single-establishment firms, in contrast, respond to the tax increase both through a reduction in wages and, compared to multi-establishment firms, a much more moderate drop in employment—which is driven both by increased separations and reduced hiring.

[FIGURE 6: Number of Establishment, Entry and Exit]

[FIGURE 7: Decomposing Employment Reductions]

6.3 Following Workers

Large tax-induced declines in employment in treated firms do not necessarily imply similarly large employment declines at the regional level, as small firms, which were not directly affected by the tax reform, may have absorbed some of the employment decline in large firms. On the one hand, the placebo figure based on small firms (panel (c) in Figure 3) does not point to large spillover effects from large to small firms, as employment in small statutory treated

firms does not increase relative to employment in small statutory control firms after the tax reform. On the other hand, multi-establishment firms downsized in part through strategic exits, moving an entire establishment to a new firm—which substantially reduces *firm* employment, but leaves *regional* employment largely unchanged. In a final step of the empirical analysis, we investigate worker movements from large (effectively treated) firms to smaller firms in more detail, by tracing out career outcomes of workers who were employed in a large, statutory treated firm in 2003, before the tax increase came into place. Specifically, we investigate whether workers in large treated firms are more likely to move to smaller firms in the region in the post-reform years, compared to workers in large control firms.

- To be included -

7 Subsequent Tax Decrease

In 2007, the system of geographically differentiated taxes was re-introduced (with some adjustments), and most of the large statutory treated firms went back to paying a lower tax rate. This tax decline (probably) was a surprise for Norwegian firms, as the details of restoration of the geographically differentiated tax were unclear as late as six months before the changes were implemented. One might therefore wonder whether large statutory treated firms bounced back and grew relative to large statutory control firms after 2007. To address this question, we follow the large statutory treated and control firms not only until 2006 (as in our baseline results), but until 2009. Between 2006 and 2007, the treated firms experienced a decrease in the statutory tax rate of 5.6 percentage points, compared to 0.25 percentage points for the control firms. The results are presented in Figure 8, and suggests that the employment drop in treated firms was not reversed after the statutory tax rates were decreased in 2007. These results should however, be interpreted with some caution. First, Figure 8 defines treatment based on the firm's exposure to the increase in the statutory tax rate in 2003 (using the firm's 2003 workforce composition), but we look at outcome variables measured a long time after 2003. Second, the financial crisis occurred just after the reversal of the tax reform, and treated and control firms might have been differentially affected by the financial crisis. Nevertheless, the figure points to the possibility that even temporary tax increases may lead to long-lasting employment reductions in affected firms. In our context, this could be because a considerable share of the overall employment drop in (multi-establishment) treated firms is through plant exits (in part to avoid the tax increase), which might be difficult to reverse.

[FIGURE 8: Firm Outcomes over Time - Subsequent 2006 Tax Decrease]

8 Conclusion

This paper evaluates firm responses to a place-based tax incentive, by exploiting a unique policy setting in Norway, where a system of geographically differentiated payroll taxes was suddenly abolished. In 1999, the European Free Trade Association (EFTA) Surveillance Authority ruled that the Norwegian system of geographically differentiated tax rates was not in compliance with EU trade regulations. As a result, EFTA imposed a tax rate harmonization to take place between 2004 and 2006. The reform was adopted and implemented independently of the local labor market developments, and thereby created exogenous variation over time in payroll tax rates across firms in different regions.

Our key finding is that the increase in payroll tax rates affected worker wages only moderately. Instead firms, in particular multi-establishment firms, respond to the tax increase primarily by reducing employment. In 2004, the first year into the reform, the outcome of the centralized bargaining between the main federation of trade unions and the main private sector employee federation implied a wage growth for industry workers of 3.6% (*Landsorganisasjonen i Norge*, 2015), while inflation measured by the growth in the CPI from 2004 to 2005 was at 1.6% (Statistics Norway). Similarly for 2005 and 2006, the outcomes of the central negotiations implied significant increases in real wages. These outcomes might have made it difficult for firms to cut wages in response to the local payroll tax increases, leading instead to a sizable reduction in employment. Through a subsidy scheme, the Norwegian tax reform further provided an incentive for firms to move a large part of the workforce to a new firm, to receive an additional annual subsidy, and thereby essentially circumvent the tax increase. We find that a substantial part of the overall employment reduction in multi-establishment firms is indeed accounted for by such "strategic" establishment exits.

Overall, our study shows that regional tax incentives may be an important policy tool in stimulating employment in underdeveloped regions, as firms' employment strongly responds to the tax increase. However, firms' employment responses are likely to depend on a country's labor market institutions, in particular the system of wage bargaining, as well as on the incentives and possibilities to circumvent tax payments.

References

Aftenposten *(18.07.2003): Kjemper for å forhindre skattesmell for bedriftene.* https://eavis.aftenposten.no/aftenposten/80220/17/?query=arbeidsgiveravgift+esa. Accessed 25.11.2017.

Aftenposten (10.02.2006): *Kjempet for avgift i Brussel.* *https://eavis.aftenposten.no/aftenposten/81728/8/?query=arbeidsgiveravgift+esa.* Accessed 25.11.2017.

Anderson, P. M. and B. D. Meyer (1997): The effects of firm specific taxes and government mandates with an application to the US unemployment insurance program. *Journal of Public Economics,* 65 (2), 119-145.

Anderson, P. M. and B. D. Meyer (2000): The effects of the unemployment insurance payroll tax on wages, employment, claims and denials. *Journal of Public Economics,* 78 (1-2), 81-106.

Bartik, Timothy J. 1991. *Boon or Boondoggle? The Debate Over State and Local Economic Development Policies,* In Who Benefits from State and Local Economic Development Policies? Kalamazoo, MI: W.E. Upjohn Institute for Employment Research, pp. 1-16.

Becker, S. O. (2012). *EU Structural Funds: Do They Generate More Growth?* Chatham House.

Bennmarker, H., E. Mellander, and B. Öckert (2009). Do regional payroll tax reductions boostemployment? *Economics,* 16 (5), 480-489.

Bohm, P. and H. Lind (1993). Policy evaluation quality: A quasi-experimental study of regional employment subsidies in Sweden. *Regional Science and Urban Economics,* 23 (1), 51-65.

Bondonio, D., and Greenbaum, R.T. (2007): Do Local Tax Incentives Affect Economic Growth? What Mean Impacts Miss in the Analysis of Enterprise Zone Policies. *Regional Science and Urban Economics*, 37 (1): 121–36.

Busso, M., Jesse, G. and Kline, P. (2013): Assessing the Incidence and Efficiency of a Prominent Place Based Policy. *American Economic Review*, 103(2): 897–947.

Chetty, R., Hendren, N., Kline, P. and Saez, E,)2014): Where is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States, *Quarterly Journal of Economics* 129(4): 1553-1623, 2014.

Cruces, Galiani and Kidyba (2010): Payroll taxes, wages and employment: Identification through policy changes. *Labour Economics*, 17: 743-749.

Devereux, M. P., Griffith, R., & Simpson, H. (2007). Firm location decisions, regional grants and agglomeration externalities. *Journal of Public Economics*, *91*(3-4), 413-435.

DiNardo, J., Fortin, N. M. and Lemieux, T. (1996): Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach. *Econometrica*, 64: 1001-1044.

Elvery, J.A. (2009): The Impact of Enterprise Zones on Resident Employment: An Evaluation of the Enterprise Zone Programs of California and Florida, *Economic Development Quarterly*, 23 (1): 44–59.

European Commission - Eurostat (2017): Unemployment in the EU regions in 2016, Brussels/Luxembourg.

FAFO (2014): «Stabil organisasjonsgrad i 2014, men litt færre tariffdekkede». http://www.arbeidslivet.no/Lonn/Fagorganisering/Litt-flere-fagorganiserte-i-2014-men-lavere-tariffspredning/, accessed 12.05.2018.

Gjelsvik, M.L., Nymoen, R. and Sparrman, V. (2015): *Have inflation targeting and EU labour immigration changed the system of wage formation in Norway*, Statistics Norway Discussion Papers No. 824 (October 2015).

Glaeser, E.L. (2001): The Economics of Location-Based Tax Incentives, Harvard Institute of Economic Research Discussion Paper 1932.

Glaeser, E. and Gottlieb, J. (2008). The economics of place-making policies. B*rookings Papers on Economic Activity*, Spring, 155-239

Hanson, A. and Rohlin, S. (2011): Do location-based tax incentives attract new business establishments? *Journal of Regional Science,* 51(3): 427-449.

Hanson, A. (2009): Local employment, poverty, and property value effects of geographically-targeted tax incentives: An instrumental variables approach, *Regional Science and Urban Economics,* 39(6): 721-731.

Ham, J.C., Ayse Imrohoroglu, C. S., and Song, H. (2011): Government Programs Can Improve Local Labor Markets: Evidence from State Enterprise Zones, Federal Empowerment Zones and Federal Enterprise Community. *Journal of Public Economics,* 95 (7-8): 779–97.

Johansen, F. and T. J. Klette (1997): Wage and employment effects of payroll taxes and investment subsidies. Report 194/1997, Statistics Norway.

Kahn, L.M. (1998): Against the wind: Bargaining recentralisation and wage inequality in Norway 1987-91. *The Economic Journal*, 108: 603-645.

Kline, P. and Moretti, E. (2013): Place based policies with unemployment. *The American Economic Review*, 103(3): 238-243.

––––– (2014): Local economic development, agglomeration economies, and the big push: 100 years of evidence from the Tennessee Valley Authority. T*he Quarterly Journal of Economics,* 129(1): 275-331.

Korkeamäki, O. and R. Uusitalo (2009): Employment and wage effects of a payroll-tax cut – evidence from a regional experiment. *International Tax and Public Finance*, 16 (6), 753-772.

Landsorganisasjonen i Norge (2015): Resultat tariffoppgjørene 1990-2014. http://www.lo.no/Lonn--tariff/Nyheter-lonn-og-tariff/Resultat-tariffoppgjorene-1990---2014/. Accessed 25.11.2017.

Lehmann, E., F. Marical, and L. Rioux (2013): Labor income responds differently to income-tax and payroll-tax reforms. *Journal of Public Economics*, 99, 66-84.

Murphy, K. J. (2007): The impact of unemployment insurance taxes on wages. *Labour Economics*, 14 (3), 457-484.

Neumark, D. and Kolko, J. (2010): Do Enterprise Zones Create Jobs? Evidence from California's Enterprise Zone Program, *Journal of Urban Economics* 68 (1): 1–19.

NOU 2013:13 (Norges Offentlige Utredninger (2013)), *Lønnsdannelsen og utfordringer for norsk økonomi* [Wage Setting and Challenges for the Norwegian Economy].

OECD (2016), *Economic Policy Reforms 2016: Going for Growth Interim Report*, OECD Publishing, Paris.

Peters, A.H., and Fisher, P.S. (2002): *State Enterprise Zone Programs: Have They Worked?* Kalamazoo, MI: W. E. Upjohn Institute for Employment Research.

Saez, E., M. Matsaganis, and P. Tsakloglou (2012). Earnings determination and taxes: evidence from a cohort-based payroll tax reform in Greece. *The Quarterly Journal of Economics*, 127 (1), 493-533.

Saez, E., Schoefer, B. and Seim, D. (2017): *Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers Tax Cut in Sweden*. NBER Working Paper No. 23976.

Stokke, H. E. (2017): *Regional payroll tax cuts and individual wages: Heterogeneous effects across education groups*. Mimeo, Department of Economics, Norwegian University of Science and Technology.

Tekna (2015): «Termination of the employment relationship». https://www.tekna.no/en/salary-and-negotiations/legal-advice/termination-of-the-employment-relationshi/, accessed 26.05.2018.

World Bank (2006) Doing Business in 2006: Creating Jobs, Washington, World Bank.

Appendix

[FIGURE A1: Geographical distribution of tax rates and treated firms, 2003.]

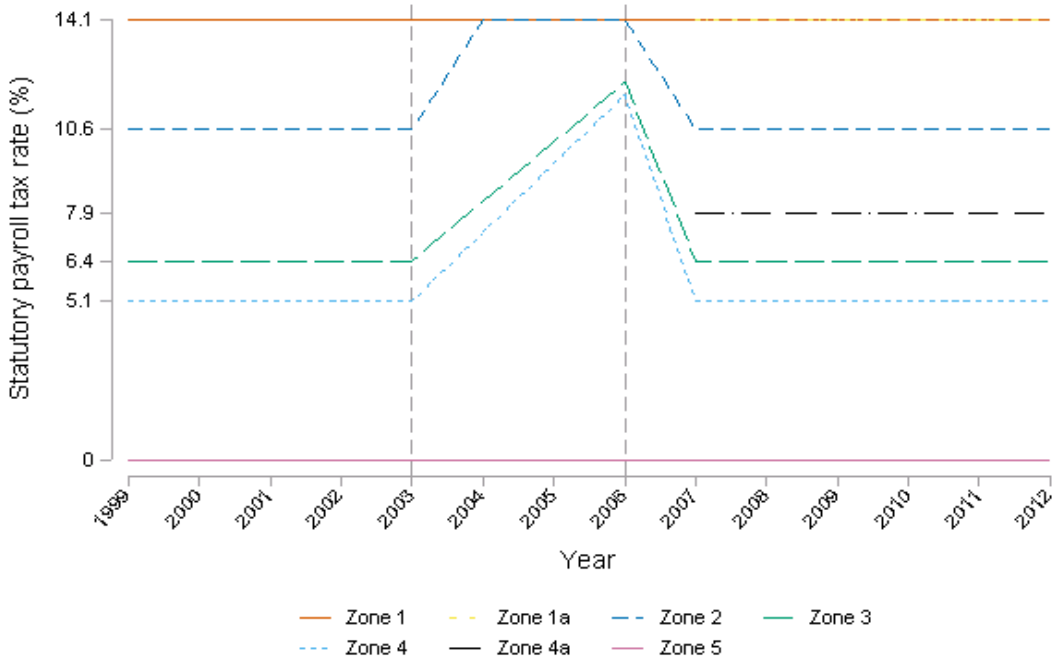
[TABLE A1: Table A1: Payroll Tax Rates τ_t^o (τ_t^l in Parenthesis), 2000-2009]

[TABLE A2: Regression Results – Robustness]

[TABLE A3: Firm Survival]

[TABLE A4: Regression Results: Large, Single- and Multi-Establishment Firms]

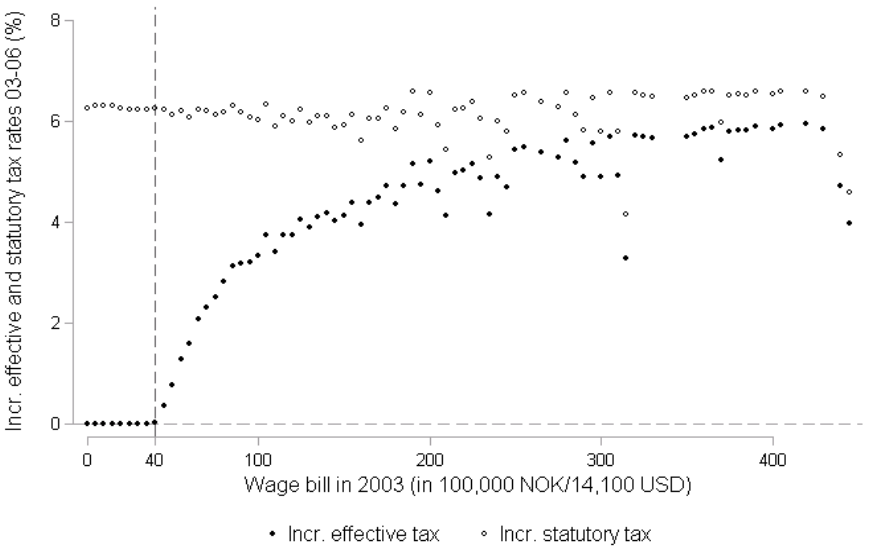
Figure 1: Statutory Payroll Tax Rate by Tax Zone



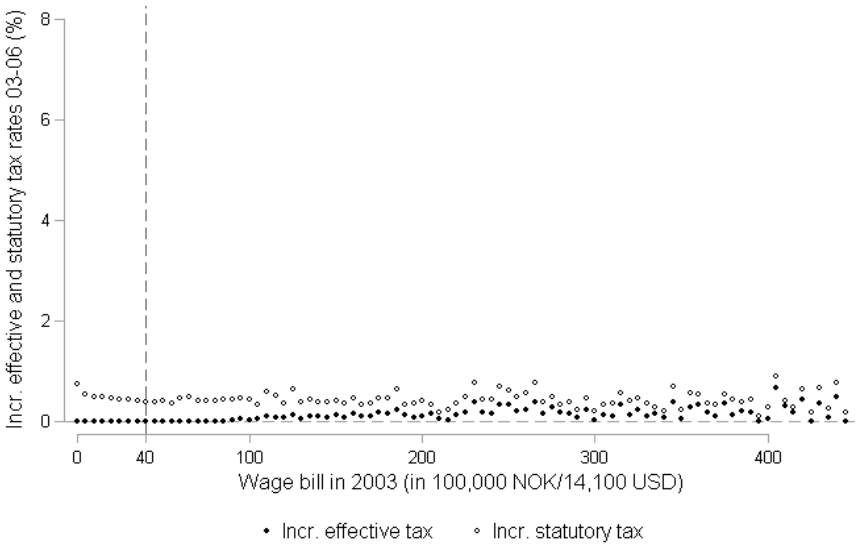
Notes: Statutory payroll tax changes (by tax zone) in Norway imposed by the EFTA Surveillance Authority, 2003-2006. In zones 1 and 5, the tax rates remained unchanged at 14.1% and 0%. In zone 2 the payroll tax rate increased from 10.6% in 2003 to 14.1% in 2004, and in zone 3 and 4, the tax rates increased by 5.7 and 6.6 percentage points from 2003 to 2006. (see Table A1 in the Appendix). In 2007 the geographically differentiated system was re-introduced, after an appeal by Norway on EFTA's ruling.

Figure 2: Average Tax Increase from 2003-2006 over Firm Size.

(a) Increase in Statutory and Effective Tax Rates over Wage Bill: Statutory Treated Firms



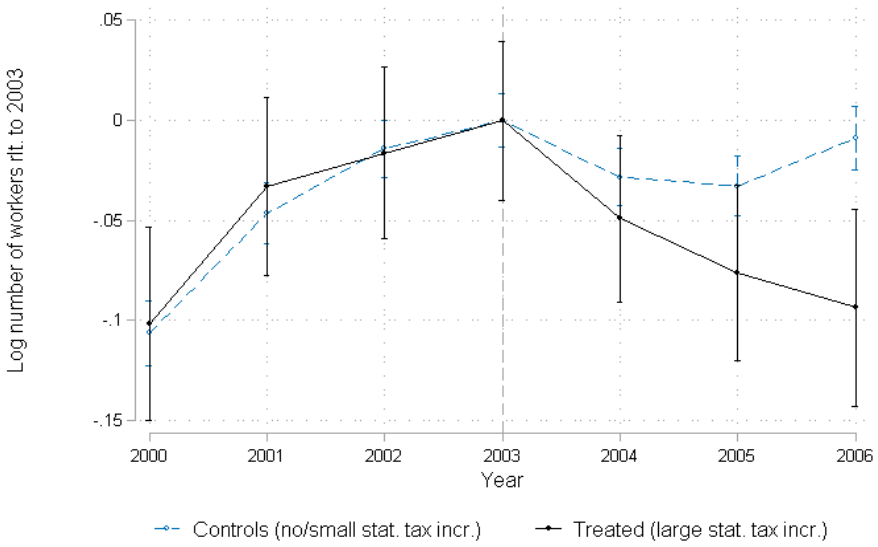
(b) Increase in Statutory and Effective Tax Rates over Wage Bill: Statutory Control Firms



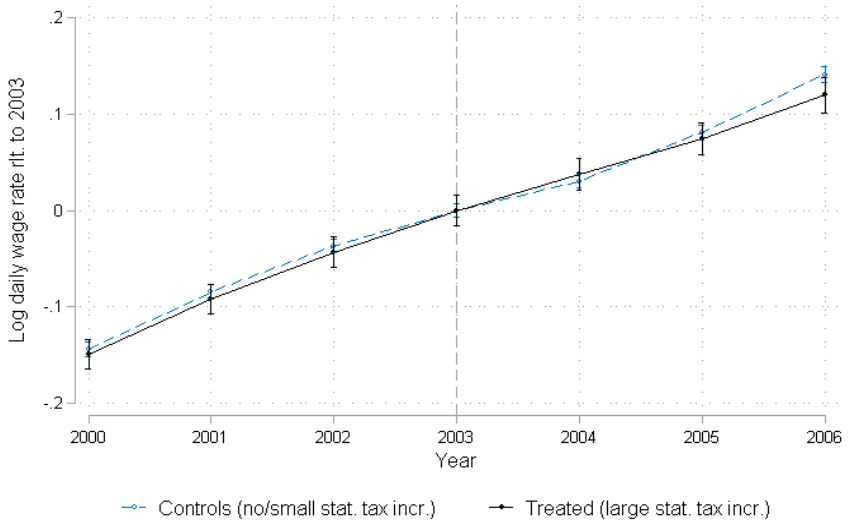
Notes: Balanced sample of 43,561 firms, where 4,890 are statutory treated and 38,671 are statutory controls. Treatment status is defined by equation (4). The figure shows the expected average statutory and effective tax increases from 2003 to 2006 over firms' 2003 wage bills for statutory treated firms (panel (a)) and statutory control firms (panel (b)). Firms with an annual wage bill of less than 4,100,000 NOK (580,000 USD) in 2003 were not affected by the tax increase (assuming their work force remained unchanged).

Figure 3: Firm Outcomes over Time

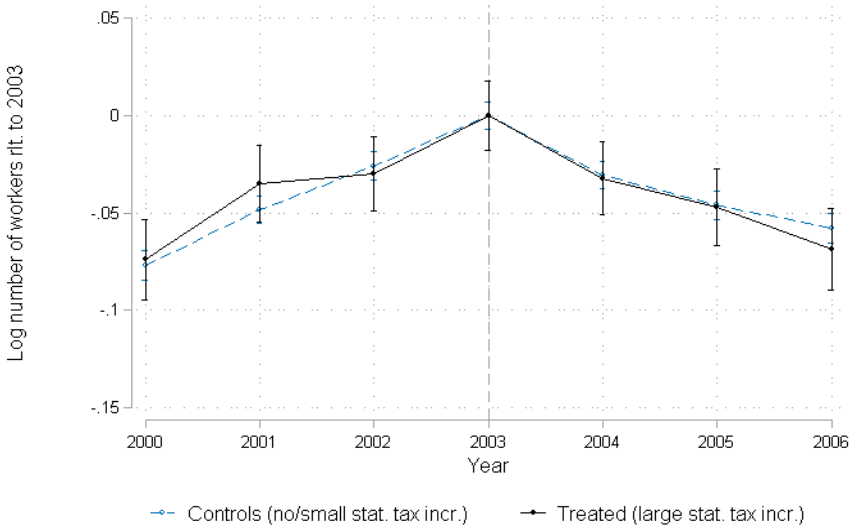
(a) Large Firms: Log Number of Workers



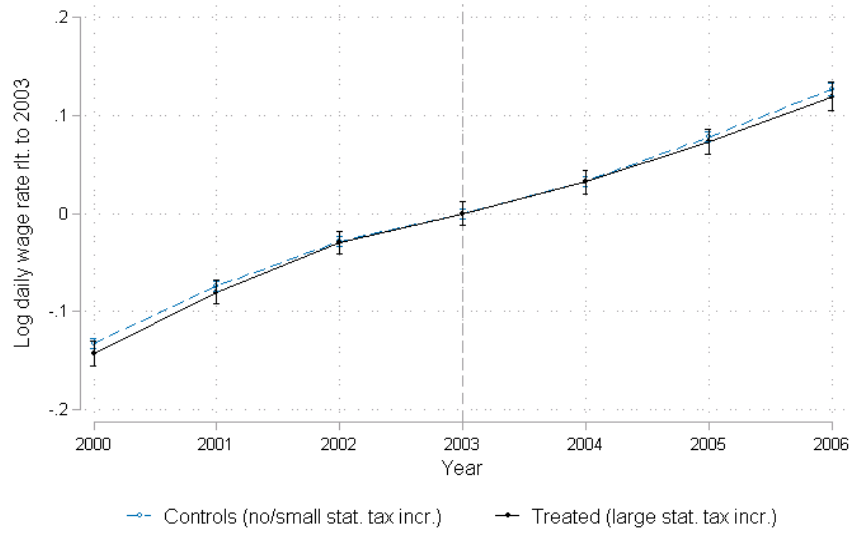
(b) Large Firms: Log Daily Wage Rate



(c) Small Firms - Log Number of Workers



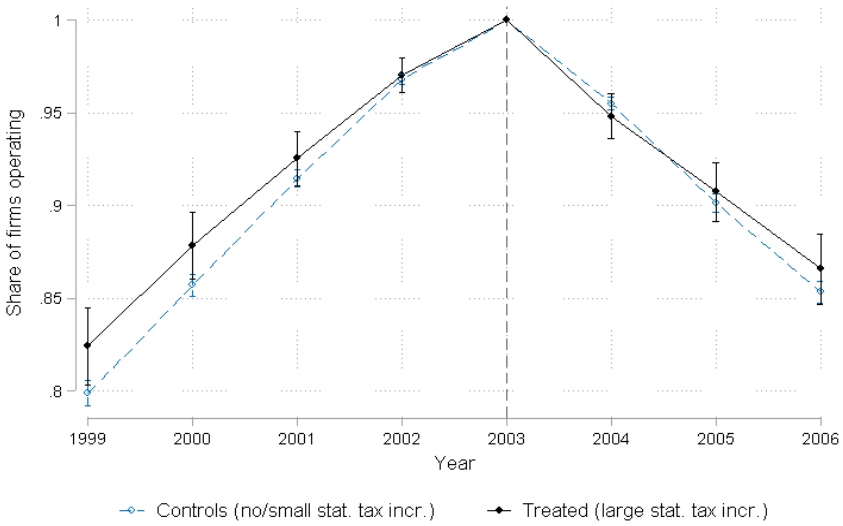
(d) Small Firms: Log Daily Wage Rate



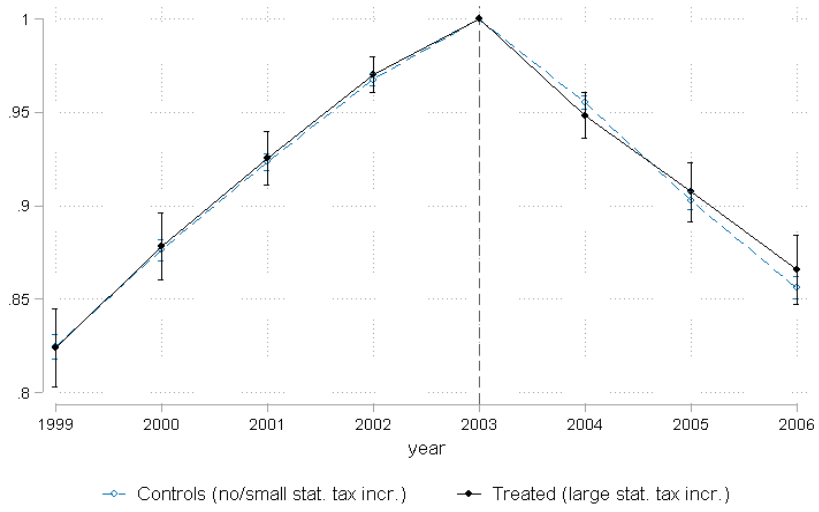
Notes: Panel (a) and (b): Balanced sample of 10,776 large firms, where 954 are statutory treated and 9,822 are statutory controls. Panel (c) and (d): Balanced sample of 32,785 small firms, where 3,936 are statutory treated and 28,849 are statutory controls. Treatment status is defined by equation (4).The figures show the time series of average outcomes for the group of treated and control firms. Means are weighted by the number of employees in the firm in 2003.

Figure 4: Firm Survival

(a) Share of Firms Operating



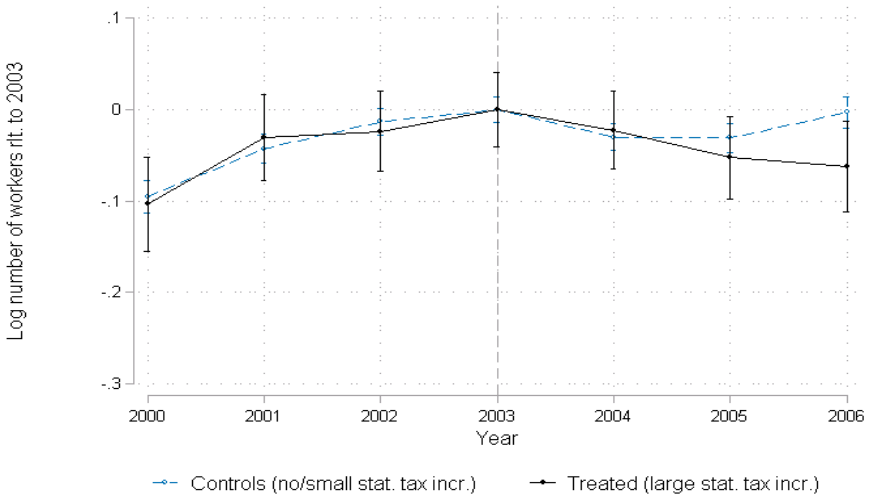
(b) Share of Firms Operating, DFL-Re-Weighted



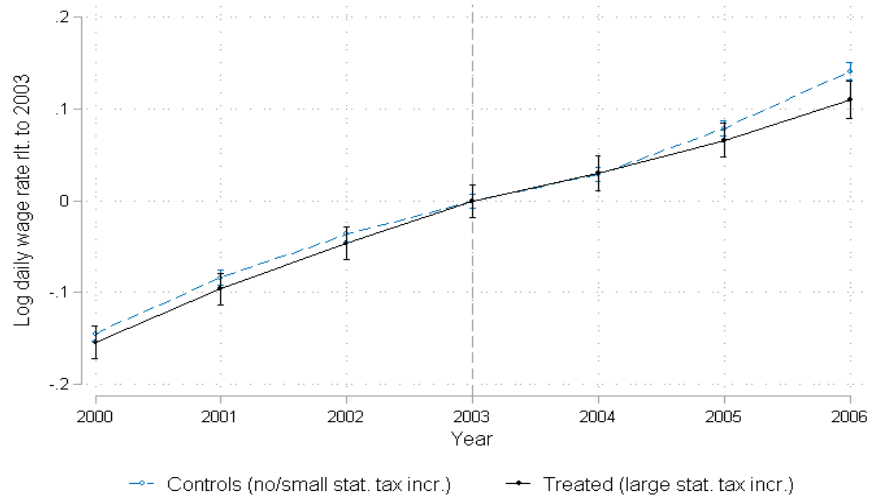
Notes: Unbalanced sample of 11,599 large firms, where 962 are treated and 10,637 are not treated. Treatment status is defined by equation (4). The figure shows the share of firms operating (with at least two workers) in 2003 that operate between the years 1998 and 2006. In panel (b) the group of control firms has been DFL- weighted to match the distribution of firm age in the group of treated firms. Corresponding estimatesa are reported in Table A3 in the Appendix.

Figure 5: Firm Outcomes Over Time – Single- and Multi-Establishment Firms

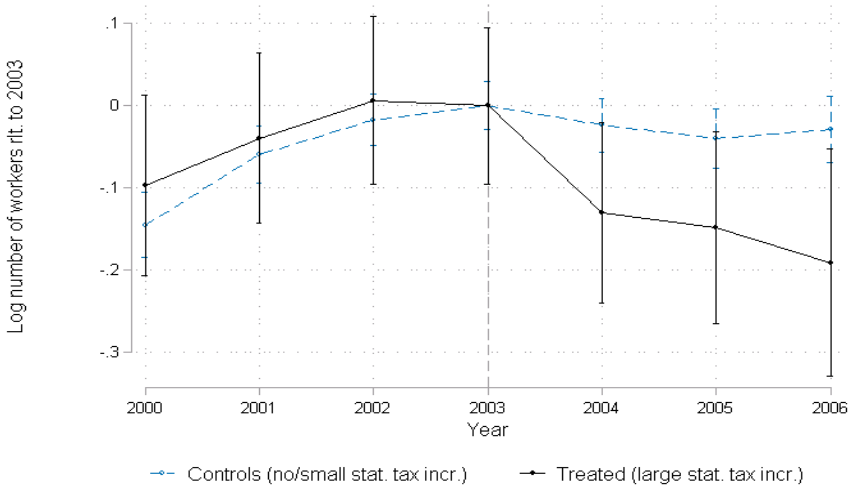
(a) Single-Establishment Firms: Log Number of Workers



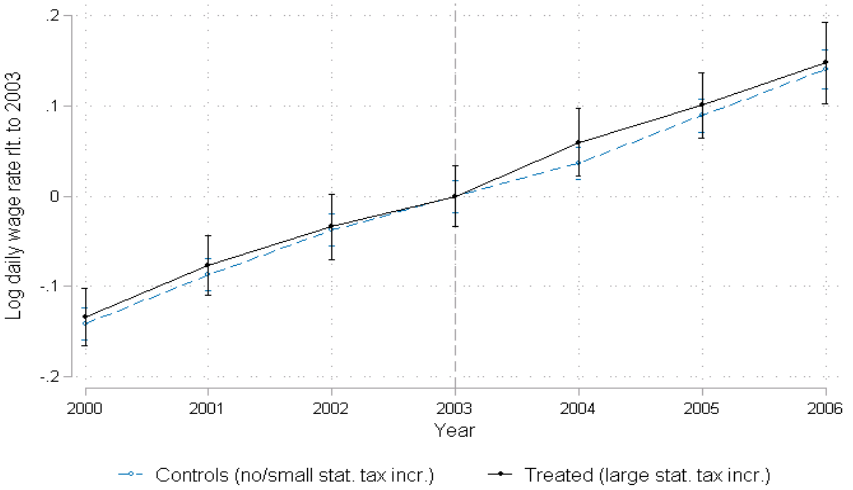
(b) Single-Establishment Firms: Log Daily Wage Rate



(c) Multi-Establishment Firms: Log Number of Workers



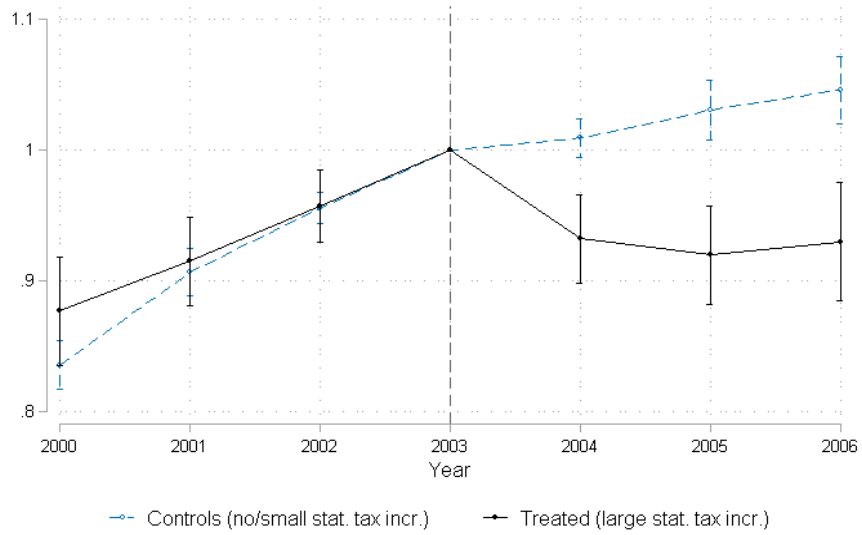
(d) Multi-Establishment Firms: Log Daily Wage Rate



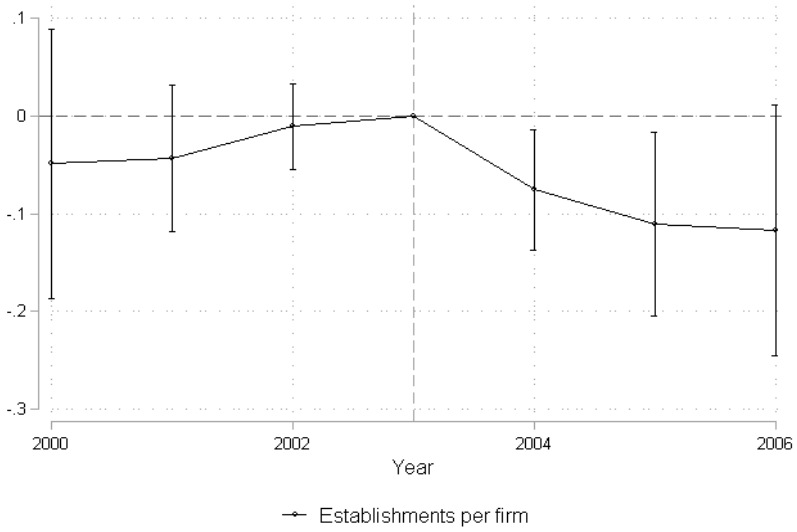
Notes: Panel (a) and (b): Balanced sample of 8,988 large, single-plant firms, where 774 are statutory treated and 8,214 are statutory controls. Panel (c) and (d): Balanced sample of 1,788 large multi-plant firms, where 180 are statutory treated and 1,608 are statutory controls . Treatment status is defined by equation (4). All outcome variables are in logs. The figures show the time series of average outcomes for the group of treated and control firms. Means are weighted by the number of employees in the firm in 2003. Regression estimates of regression equation (7) for single- and multi-establishment firms separately are reported in Table A4 in the Appendix.

Figure 6: Number of Establishment, Entry and Exit

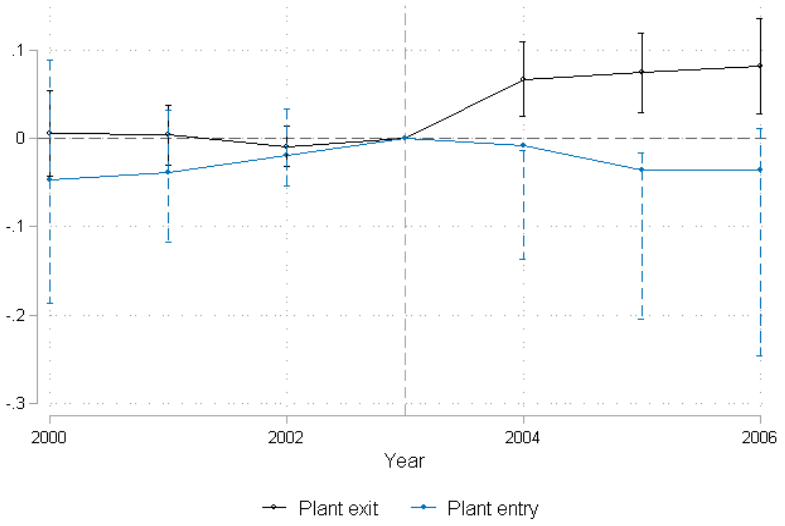
(a) Event Study: Number of Establishments per Firm



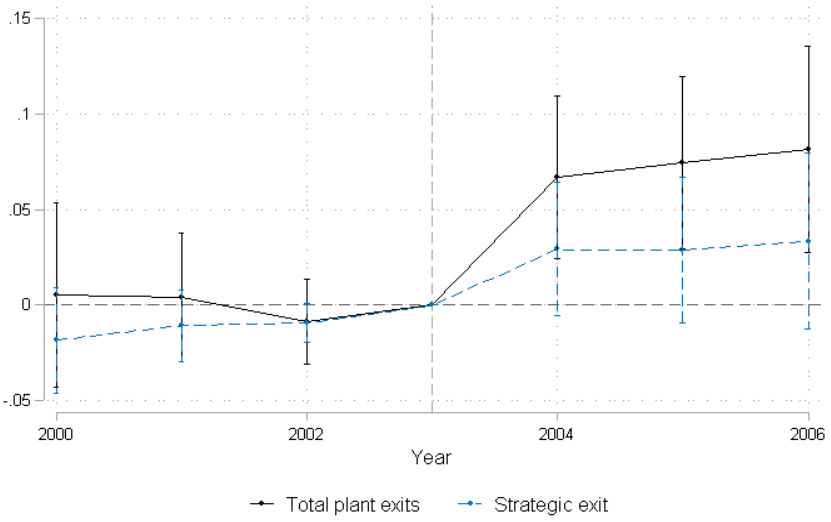
(b) DiD: Number of Establishments per Firm



(c) Establishment Entry and Exit



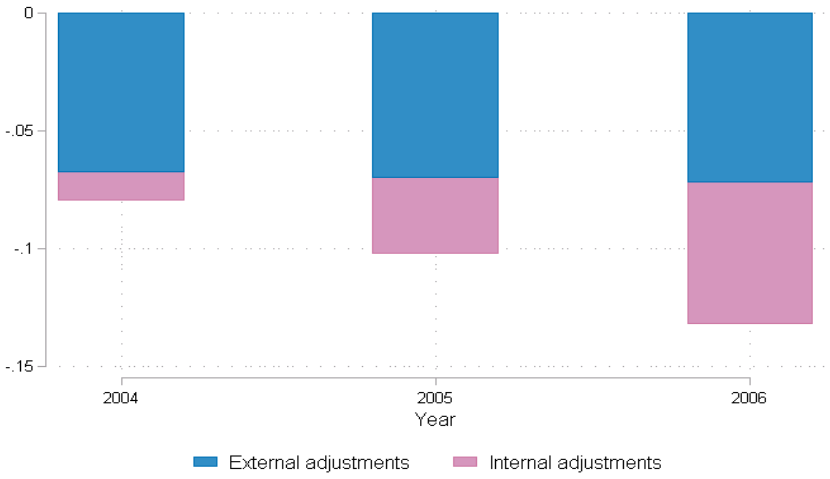
(d) Total Exits and Strategic Exits



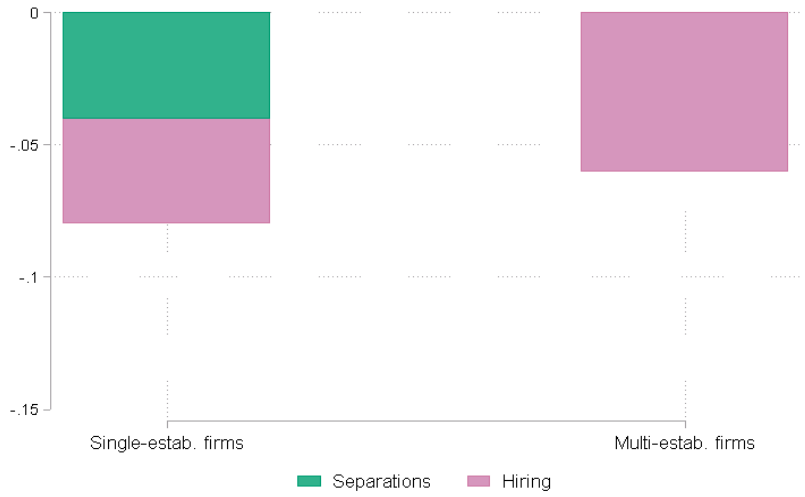
Notes: Balanced sample of 10,776 large firms, where 954 are statutory treated and 9,822 are statutory controls. Treatment status is defined by equation (4). The figure show the time series of average number of establishments per firm (a), number of establishment exits and entries (panel (b)) and number of exits and strategic exits (panel (c)) (all variables scaled by the number of establishments in 2003) for the group of treated and control firms. Means are weighted by the number of employees in the firm in 2003. Panel (b) shows the estimated coefficients on the interacted treatment and year dummy variables in equation (9), with the number of establishments per firm (scaled by number of establishments in 2003) as outcome variable. The regression is weighted by the number of workers in the firm in 2003, and standard errors are clustered at the level of the municipality of the firm.

FIGURE 7: Decomposing Employment Reductions

(a) External and Internal Adjustments, Multi-Establishment Firms



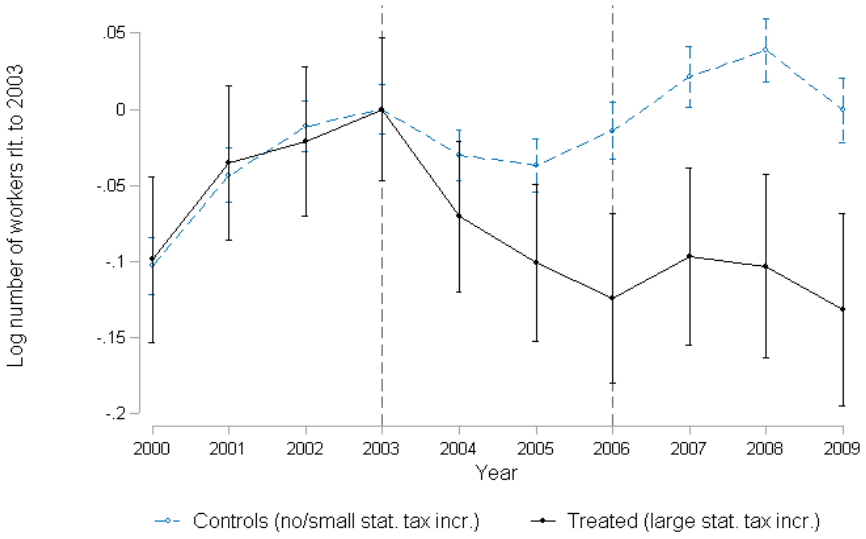
(b) Hiring and Separations: Single-and Multi-Establishment Firms



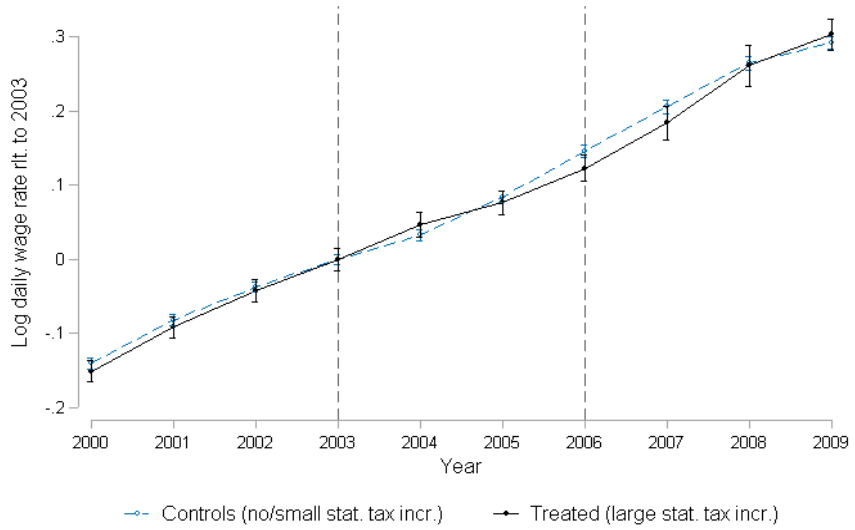
Notes: Balanced sample of 10,776 large firms, where 954 are statutory treated and 9,822 are statutory controls. Treatment status is defined by equation (4). Panel (a) decomposes the employment reductions among multi-establishment firms resulting into internal and external adjustments. Panel (b) decomposes the internal employment adjustments among single-establishment and Multi-establishment firms into reductions through reduced hiring and increased separations. Estimates are obtained from regressing equation (9) with the different variables defined in equation (10) as outcome variables. Regressions are weighted by the number of workers in the firm in 2003.

Figure 8: Firm Outcomes over Time - Subsequent 2006 Tax Decrease

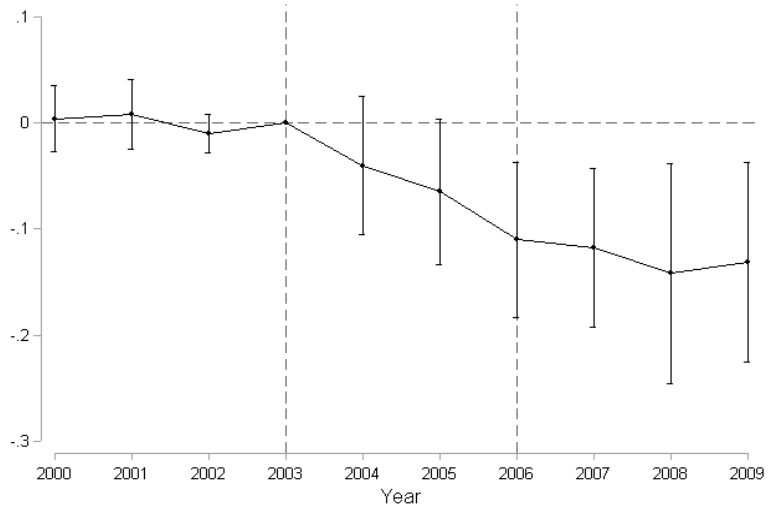
(a) Log Number of Workers



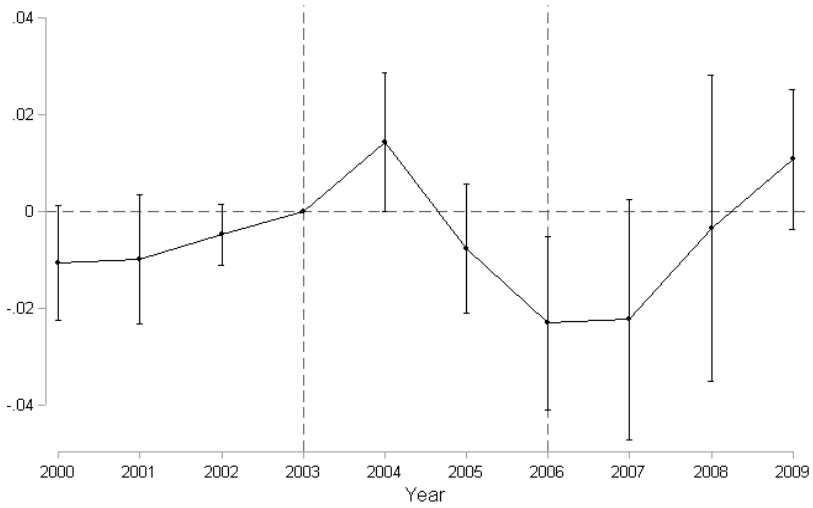
(b): Log Daily Wage Rate



(c) DiD: Log Number of Workers



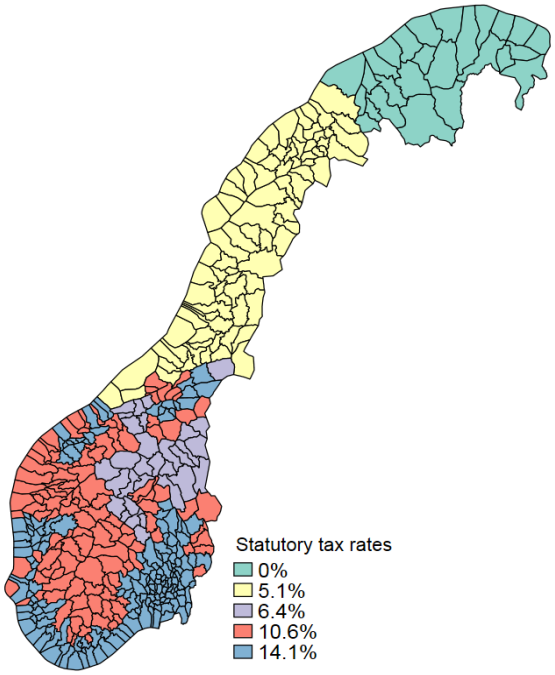
(d) DiD: Log Daily Wage Rate



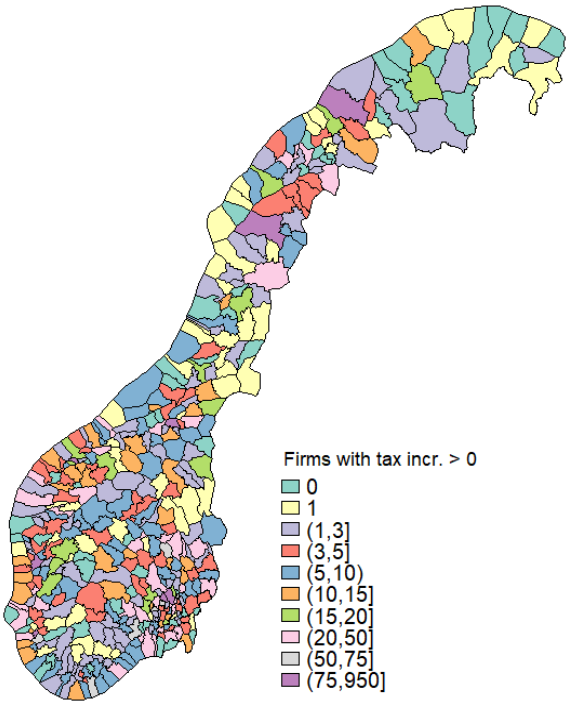
Notes: Main sample of 10,776 large firms, where 954 are statutory treated and 9,822 are statutory controls. Treatment status is defined by equation (4). Panels (a) and (b) show the time series of average outcomes for the group of treated and control firms. Panels (c) and (d) show the estimated coefficients on the interacted treatment indicator and year dummies in equation (9). Regressions are weighted by the number of the workers in the firm in 2003, and standard errors are clustered at the level of the municipality of the firm.

Figure A1: Geographical Distribution of Tax Rates and Treated Firms, 2003.

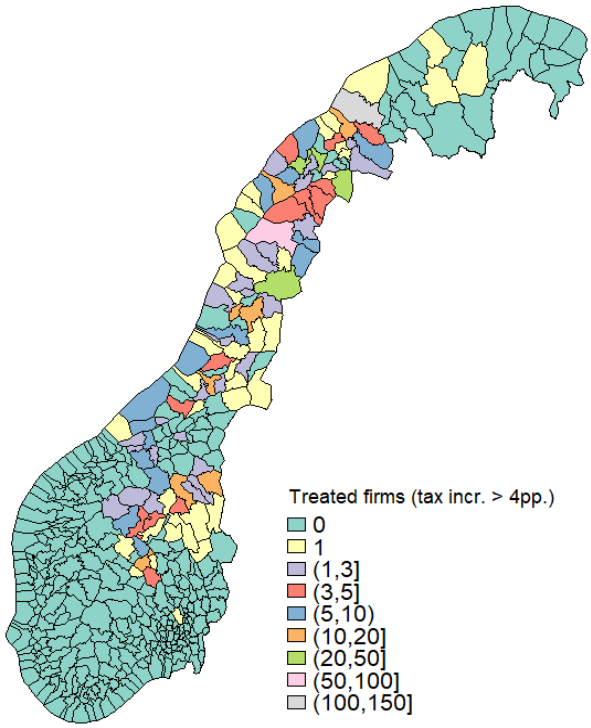
Panel (a): Statutory Tax Rates, 2003



Panel (b): Firms with Tax Incr. > 0



Panel (c): Firms with Tax Incr. >= 4pp.



Notes: Panel (a) shows the geographical distribution of tax rates in Norway in 2003 (pre-reform). Panel (b) shows the number of firms experiencing a positive tax increase between 2003 and 2006 , and panel (c) shows the distribution of the number of large treated firms according the definition of treatment status given by equation (4).

Table 1: Descriptive Statistics

	Treated (large stat. tax incr.)		Control (zero/small stat. tax incr.)	
	Large	Small	Large	Small
Daily wages	864.458	676.220	1074.718	737.859
Workers	34.833	6.655	37.442	6.352
Days	11785.187	2257.687	12683.574	2166.741
Statutory tax rate 2003	0.055	0.055	0.134	0.132
Change in stat. tax rate 03-06	0.062	0.063	0.004	0.005
Change in eff. tax rate 03-06	0.023	-0.000	0.001	0.000
Industries				
Agriculture/oil/mining	0.058	0.044	0.021	0.031
Manufacturing	0.206	0.093	0.196	0.098
Construction	0.178	0.126	0.134	0.125
Wholesale and retail	0.215	0.359	0.278	0.355
Hotel/restaurants/catering	0.050	0.053	0.044	0.039
Transport	0.088	0.090	0.066	0.073
Finance/insurance/property mng.	0.100	0.120	0.173	0.156
Health	0.059	0.063	0.039	0.065
Other	0.046	0.051	0.049	0.058
Number of firms	954	3936	9822	28849

Notes: The table shows descriptive statistics for the balanced sample of 43,561 firms with at least two employees in each year 2000-2006. The sample is split into two groups by treatment status as defined by equation (4), and further into two groups by size as defined by figure (2). All variables are measured in 2003. Monetary amounts are given in NOK (1 USD = 7.08 NOK in 2003).

Table 2: Main Regression Results

	Large firms			Small firms		
	(1)	(2)	(3)	(5)	(6)	(7)
	Workers	Days	Daily wage rate	Workers	Days	Daily wage rate
Stat. tax rate	-1.865***	-1.984***	-0.260**	-0.315*	-0.264	-0.017
	(0.567)	(0.551)	(0.118)	(0.174)	(0.179)	(0.093)
Observations	75,432	75,432	75,432	229,495	229,495	229,495
R-squared	0.884	0.874	0.897	0.826	0.806	0.839

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Balanced samples of 10,340 large firms (columns (1) - (3)), and 32,785 small firms (columns (5) - (7)). The table shows estimates of β and corresponding standard errors, from regression equation (7) for three different outcome variables (workers (head count), days and daily wage rate; all outcome variables in logs)), for large and small firms separetely. The regressions are weighted by the number of employees in the firm in 2003, and standard are clustered at the level of the municipality of the firm.

Table A1: Payroll Tax Rates, 2000-2009

	Zone 1	Zone 1a	Zone 2	Zone 3	Zone 4	Zone 4a	Zone 5
2000	14.1		10.6	6.4	5.1		0
2001	14.1		10.6	6.4	5.1		0
2002	14.1		10.6	6.4	5.1		0
2003	14.1		10.6	6.4	5.1		0
2004	14.1		14.1 (10.6)	8.3 (6.4)	7.3 (5.1)		0
2005	14.1		14.1 (10.6)	10.2 (6.4)	9.5 (5.1)		0
2006	14.1		14.1 (10.6)	12.1 (6.4)	11.7 (5.1)		0
2007	14.1	14.1	10.6	6.4	5.1	7.9	0
2008	14.1	14.1	10.6	6.4	5.1	7.9	0
2009	14.1	14.1	10.6	6.4	5.1	7.9	0

Notes: Payroll tax rates by tax zone and region. (Pre- Reform Low Tax Rate in Parenthesis.) Source: The Norwegian Tax Authorities.

TA2: Regression Results - Robustness

	Large firms	Small firms (placebo)
	(1)	(2)
	Workers	Workers
(a) Sector specific time trends		
Stat. tax rate	-1.571***	-0.237
	(0.567)	(0.172)
Observations	75,432	229,495
R-squared	0.884	0.826
(b) Norwegian-born workers		
Stat. tax rate	-1.294**	-0.067
	(0.552)	(0.189)
Observations	75,371	226,712
R-squared	0.884	0.813

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Balanced samples of 10,340 large firms (column (1)) , and 32,785 small firms (column (2)). The table shows estimates of β and corresponding standard errors, from regression equation (7) for the outcome variable log number of workers (head count), for large and small firms separetely. Panel (A) shows results when sector specidic time trends (fully interacted sector and time dummy variables), and panel (B) shows results when only counting the number of Norwegian-born workers in the firm. The regressions are weighted by the number of employees in the firm in 2003, and standard are clustered at the level of the municipality of the firm.

Table A3: Firm Survival

	(1) No controls	(2) Firm Age FEs	(3) DFL-reweighted
2000.year#1.Treated	0.027***		0.002
	(0.010)		(0.008)
2001.year#1.Treated	0.012*		0.002
	(0.007)		(0.007)
2002.year#1.Treated	0.002		0.003
	(0.006)		(0.005)
2004.year#1.Treated	-0.008	-0.001	-0.007
	(0.010)	(0.006)	(0.006)
2005.year#1.Treated	0.003	-0.000	0.004
	(0.014)	(0.006)	(0.009)
2006.year#1.Treated	0.008	0.000	0.010
	(0.015)	(0.006)	(0.010)
Observations	117,368	58,684	117,368
R-squared	0.044	0.898	0.039

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Unbalanced sample of 14,677 firms, where 1,274 are treated and 13,403 are not treated. The table shows estimated coefficients and standard errors on interacted year and treatment fixed effects in regression of an indicator of firm existence on year and treatment fixed effects as well as their interactions, without and with controls for firm age (column 1 and 2 respectively). The regressions in column (1) and (2) are weighted by the number of employees in the firm in 2003, while in column (3), the firms in the control group is re-weighted to match the firm age distribution in the group of treated firms (using the DFL weight given by equation (8)).

Table A4: Regression Results: Large, Single- and Multi-Establishment Firms

	Single-Esablishment Firms			Multi-Establishment Firms		
	(1)	(2)	(3)	(4)	(5)	(6)
	Workers	Days	Daily wage rate	Workers	Days	Daily wage rate
Stat. tax rate	-0.984**	-1.147**	-0.351***	-4.275**	-4.264***	-0.043
	(0.469)	(0.462)	(0.113)	(1.660)	(1.611)	(0.295)
Observations	62,916	62,916	62,916	12,516	12,516	12,516
R-squared	0.887	0.876	0.901	0.826	0.809	0.887

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Balanced sample of large firms, where are 8,988 are single plant-firms (774 statutory treated and 8,214 statutory controls), and 1,797 multi-plant firms (180 statutory treated and 1,608 statutory controls). Treatment status is defined by equation (4). The table shows estimates of β and corresponding standard errors, from regression equation (7) for three different outcome variables (workers, days and daily wage rate; all outcome variables in logs), and for single- and multi-plant firms separately. The regressions are weighted by the number of employees in the firm in 2003, and standard are clustered at the level of the municipality of the firm.