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(No) Effects of Subsidizing the First Employee: Evidence of a Low Take-up Puzzle Among Firms

Annika Nivala*

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Abstract

Finland had a large regional wage subsidy for hiring the first employee in 2007–2011. In this paper, I show that the take-up of the subsidy was very low: only 2% firms that became employers used the subsidy. The subsidy was restricted to hiring a full-time employee, which reduced the take-up. However, even among full-time employers the take-up rate was only 6%. Hence, a large majority of firms left thousands of euros on the table by not using the subsidy. Based on the descriptive evidence, the low take-up seems to be explained by low awareness in addition to costs of using the subsidy. Using a regional difference-in-differences identification strategy, I estimate the effect of the subsidy on the probability of becoming an employer and other firm outcomes. As a consequence of the low take-up, the estimated effect is zero.[1]

JEL Codes: H25; H32; J23; J38; M51

Keywords: Business subsidies, Wage subsidies, Firm behavior, Labor demand, Entrepreneurship, Small Business

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1 Introduction

Employment subsidies are often used as a policy tool to improve employment – whether the goal is to encourage job creation, support employment of a targeted group, or provide a fiscal stimulus during a recession (see e.g. European Commission (2013), Neumark (2013) and Kritikos (2014)). For example, already Kaldor (1936) suggested that wage subsidies could decrease unemployment. In particular, supporting job creation in small firms and new businesses is of interest to governments, as they are considered to have an important role in overall job growth (see e.g. Decker *et al.* (2014) and Haltiwanger *et al.* (2013)).[2] The majority of firms, however, are non-employer firms that never hire an employee. This leads to the question whether employment subsidies could encourage entrepreneurs to take the step in becoming employers. For example, there could be fixed costs in becoming an employer that a subsidy could alleviate, as discussed in Mel *et al.* (2019). In fact, some countries provide incentives for hiring the first employee.[3] However, there is lack of evidence on how entrepreneurs respond to this type of incentive.

In this paper, I study the take-up and effects of a first-employee subsidy in Finland, using data on the full population of Finnish firms. In 2007–2011 Finland had a regional subsidy for non-employer firms to become employers that amounted to 30% of the wage costs of the first employee in the first year and 15% in the second year. This created a sizable incentive to become an employer for a large target population: almost 60% of all firms had no employees other than the entrepreneur(s). The eligibility for the subsidy was clear: a firm in the eligible area that hired on a permanent, full-time contract was eligible for the subsidy. The subsidy was supposed to help firms grow by overcoming the threshold of hiring the first employee.

I start by documenting a low take-up of the subsidy: only 2% of firms that became employers in the eligible area used the subsidy. Many of the non-taker firms do not fulfill the full-time employment criteria, meaning that a large share of firms self-select out of using the subsidy due to the restriction. However, the take-up rate is still very low at 6% for new full-time employers. Consequently, 94% of new full-time employers passed up an average €8,000 by not using the subsidy. The subsidy is a significant share of the first-year wage costs: the wage costs of firms that became full-time employers are on average €34,000, which makes the average subsidy about 25% of that.

In a theoretical model, I consider two reasons for the low take-up: imperfect awareness and compliance costs. First, some firms may not know about the policy. Only the fraction of firms aware of the subsidy can use it and respond. Second, there are compliance costs

[2]Small businesses may not be as important for job growth after accounting for firm age. However, most firms start small so supporting growth of small firms indirectly provides support for growing new firms. Established small firms simply may not use the incentives.

[3]For example, France, Austria, Belgium, Sweden and Finland have had different policies to encourage becoming an employer.

that arise from the subsidy design. These include the costs of applying for the subsidy as well as indirect opportunity costs implied by the restriction to permanent, full-time employees. Compliance costs reduce the effect on the firms aware of the policy, as for some firms the costs of using the subsidy surpass the benefits.

Descriptive evidence suggests that both reasons reduced take-up. The take-up rate is almost zero for firms with less than €2,000 of *ex post* calculated subsidy, based on their realized wage costs, but increases to about 10% for firms with calculated subsidy benefits of €6,000–20,000. Because take-up does not increase in subsidy benefits above €8,000, low awareness seems to be an additional reason for the low take-up: in theory, if imperfect take-up was only due to compliance costs, there would be perfect take-up among firms with a large enough subsidy benefit. In practice, however, the expected benefits may not surpass compliance costs for some of the entrepreneurs with *ex post* calculated subsidy over the threshold. As further support for the low awareness explanation, I find that take-up is positively associated with information channels arising from the institutional context: firms located in municipalities with a regional agency responsible for the subsidy implementation and in regions with higher share of eligible municipalities were more likely to use the subsidy. Also, firms that previously used some business subsidies were more likely to take-up.

In the second part of the paper, I study the effects of the subsidy on becoming an employer using a regional difference-in-differences identification strategy. The subsidy region was specifically defined for the first-employee subsidy and did not follow other administrative borders. There was a regional support motive in that the area included some of the most economically disadvantaged parts of Finland. However, other more prosperous areas were also included, as it had been considered to extend the subsidy to all of Finland. For the causal estimation, I restrict the analysis to the border of the subsidy area and compare firms in eligible municipalities to neighboring ineligible municipalities. This restriction excludes the most disadvantaged and affluent areas, leaving small municipalities closer to the Finnish average in the sample. The descriptive statistics and trends for these areas are similar prior to the subsidy adoption, supporting the parallel trends assumption.

The estimated effect on the likelihood of becoming an employer is statistically indistinguishable from zero. At a 95% confidence level, I can rule out an effect larger than 0.8 ppt, which is 4% of the baseline, on the probability of becoming an employer by the fourth year after the subsidy adoption. The average effect does not seem to hide significant heterogeneity by firm groups: there are no statistically significant positive estimated effects on the studied firm groups.

In the theoretical model, I derive a formula for the awareness rate based on observable quantities and the estimated intent to treat (ITT) effect. In the model, take-up is the result of exogenous awareness and endogenous take-up decision given compliance costs. The simplifying assumptions used to calculate the awareness rate are: i) awareness is not

correlated with labor productivity for firms that would become employers if they were aware of the subsidy, and ii) expected subsidy benefits exceed compliance costs for firms with wage costs above €12,000.

Using the calculated awareness rate of 24% to scale the ITT estimate, I find an estimate for the average effect on the firms aware of the subsidy that is −2.3 ppt by the first year. However, the standard error is large providing a 3.4 ppt upper bound of the 95% confidence interval, which is 36% of the baseline probability. For some firm groups with lower calculated awareness rates even much larger effects cannot be ruled out. Consequently, economically significant effects on the firms aware of the subsidy cannot be excluded. Likely violations to the assumptions bias the calculated awareness rate downwards, meaning that the estimate for the firms aware of the subsidy is an upper bound.

This paper is, to my knowledge, the first to study the effect of an employment subsidy on the probability of becoming an employer.[4] The extent to which subsidies can induce entrepreneurs to become employers is of specific interest, because the decision to hire at the extensive margin (from zero employees to being an employer) is different from hiring for established employer firms and a large share of firms are non-employers.[5] My study is closely linked to Mel *et al.* (2019, 2010), who examine the effects of a temporary wage subsidy offered to micro enterprises in a randomized experiment in Sri Lanka. In the study, firms increase employment temporarily but then return to the baseline, which means there is no evidence of frictions in becoming an employer. In contrast to my results, their paper does not provide estimates for becoming an employer, as they only study the subsidized firms, and their setting in a developing country is very different from this paper. The dynamics of becoming an employer are studied in Fairlie & Miranda (2016) in the US and Lechmann & Wunder (2017) in Germany. The discussion on firm size and the dynamics of firms is related to a more general discussion on the role of entrepreneurship and small firms in job creation (Decker *et al.*, 2014; Haltiwanger *et al.*, 2013; Kritikos, 2014).

Secondly, this paper is one of the few studies on the take-up of business subsidies. Generally, my result of low take-up of a subsidy among small firms is in line with previous studies. The take-up rate of 2–12% that I find is significantly lower than in Zwick (2021), who finds that 37% of eligible firms claim a tax refund for tax losses. Neilson *et al.* (2020) study how firms apply for paycheck protection loans, designed to help small businesses during the COVID-19 crisis, and find that smaller firms are slower to learn about the subsidies, and less likely and slower to apply for them. Additionally, Zwick (2021)

[4]My study is closely related to Cockx & Desiere (2023), who study a similar policy aimed to incentivize non-employers to hire in Belgium: a payroll tax exemption for the first employee that decreased the labor cost of the first employee by 13%. Using the adoption of the policy, they find a 31% increase in the number of new employers.

[5]E.g. Cockx & Desiere (2023), Lechmann & Wunder (2017), and Fairlie & Miranda (2016) show that non-employer firms form a large share of the firm population also in other countries than Finland.

finds that take-up is higher among larger firms, and a similar pattern is also documented in Korkeamäki & Uusitalo (2009) and Huttunen *et al.* (2013), who study the effects of employment subsidies in Finland. Unlike Huttunen *et al.* (2013), who point out that the observed take-up choice seems to be consistent with rational responses to the administrative costs of applying for the subsidy, I find in line with Zwick (2021) that low take-up does not seem to be completely explained by the costs and benefits of take-up. Consequently, my results are more in line with the literature finding that firms make mistakes in profit maximization. For example, Almunia *et al.* (2020) provide evidence that firms in Uganda misreport taxes, increasing their tax liability, and Kremer *et al.* (2019) discuss findings of non-profit-maximizing behavior by firms in developing countries. Also, the importance of institutional issues such as salience and information in take-up may be higher for small firms, as observed by Neilson *et al.* (2020). Thus, small firms may be especially difficult to target by business subsidies. In addition to providing empirical evidence, I develop a theoretical model of take-up that can be used to assess the awareness rate of the subsidy, and consequently the average effect on those firms that are aware of it. The model can be applied in similar settings where there are compliance costs, imperfect awareness and receiving the subsidy is conditional on some action.

My paper is also related to literature studying the effects of employment subsidies on firms. The evidence on the effectiveness of such subsidies is mixed. I contribute by showing that take-up is an important margin of response and may be one reason for the divergent results. The role of take-up has often been overlooked despite the obvious fact that effectiveness requires take-up, and the take-up of social transfers, for example, is recognized as an important factor.[6] My results of no significant effects are in contrast to Ku *et al.* (2020) and Saez *et al.* (2019), who find large employment effects in firms following payroll tax changes in Norway and Sweden, and are more in line with the moderate or insignificant employment effects in response to payroll tax reductions found by Korkeamäki & Uusitalo (2009) in Finland and Bennmarker *et al.* (2009) in Sweden. Lombardi *et al.* (2018) in Sweden and Kangasharju (2007) in Finland look at the effects of employment subsidies targeted at the unemployed and find positive effects on firm performance. Conversely, Lechner *et al.* (2013) find adverse effects of active labor market policies on firms.[7] Regarding take-up, Saez *et al.* (2019) mention that there was full, immediate take-up due to the type of institution involved and, in contrast, Korkeamäki & Uusitalo (2009) and Huttunen *et al.* (2013) mention low take-up of the employment incentives among small firms.

Finally, my paper is linked to the literature on subsidies targeted at firm growth.

[6]For example, Bhargava & Manoli (2015) study the role of psychological issues such as knowledge, complexity and stigma related to the take-up of EITC.

[7]The individual level employment effects of targeted employment subsidies and active labor market policies have been widely studied. See e.g. Huttunen *et al.* (2013), Brown *et al.* (2011) and Card *et al.* (2010). However, the firm responses have received less attention.

Betcherman *et al.* (2010), Cahuc *et al.* (2019) and Hyman *et al.* (2022) find significant effects of marginal employment subsidies or hiring credits on firm employment. Many studies (e.g. Girma *et al.* (2008), Tokila *et al.* (2008), Bronzini & Iachini (2014), Rotemberg (2019) and Criscuolo *et al.* (2019)) find business subsidies to be effective for small firms, especially in manufacturing. Small firms, however, may mean firms with up to 50 employees, which may differ substantially from entrepreneurs considering hiring their first employee.

The rest of this paper is organized as follows. Section 2 describes the institutional setting and data, and Section 3 sets out a theoretical model of the effects and take-up of the subsidy. Section 4 studies the take-up of the subsidy using descriptive evidence. In Section 5, I present the identification method and provide estimates of the effect of the subsidy on the targeted firms. Finally, Section 6 concludes.

2 Institutional Setting and Data

2.1 Institutional Setting

First-Employee Subsidy

Finland had a regional first-employee subsidy in force from 2007 to 2011. The subsidy amounted to 30% of the wage costs of the first employee in the first year and 15% in the second year, excluding payroll taxes. This is a substantial decrease in the labor costs of the first employee. For example, payroll costs are about 25% of wage costs and direct firing costs, as proxied by the wage costs during the notice period, are 4% of the yearly wage costs of an employee dismissed in the first year. As a general employment incentive the subsidy is not as large, as it only affects the costs of the first employee. The subsidy was granted to firms in the eligible municipalities upon application. First, I discuss the subsidy design, followed by a detailed examination of its regional aspects.

The subsidy was supposed to encourage employment and business growth in non-employer firms. The reasoning for this stems from i) there being a large number of non-employer firms and ii) the idea that there is a high threshold to becoming an employer, and this hinders firm growth. In fact, over half of Finnish firms do not have employees (except for the owner(s)). Hence, the target group of the subsidy is large. The second point is related to the idea, that many firms are non-employers because becoming an employer entails high fixed costs.

To qualify for the subsidy the firm had to: i) had no external employees[8] for at least 12 months and ii) hire an employee on a permanent employment contract with at least 25 hours of work per week. Condition (i) defines the *target group* of the subsidy and

[8]other than the initial entrepreneur(s)

condition (ii) defines the *hiring criteria* on which firms can receive the subsidy based on their employment decision. There were no restrictions on the type of employee hired, because the subsidy was meant to support firm growth. Minimal discretion was used when granting the subsidy.[9] Due to EU regulations, the subsidy could not be granted to businesses in fisheries, agriculture, forestry or the processing or marketing of agriculture products.

Firms had to apply for the subsidy before hiring their first employee. After being granted the subsidy and hiring an employee, firms had to again apply for payment of the subsidy, which was paid semi-annually. The subsidy was administered and granted by the regional ELY Centres (Centre for Economic Development, Transport and the Environment). There are 15 ELY Centres in Finland and they are responsible for the regional implementation of central government policy in areas of business and industry, labor force, skills, and cultural activities.

Geographical variation

The institutional setting of the subsidy creates geographical variation in i) eligibility and ii) administration that may affect take-up. First, I discuss the eligible area, and then I address the potential variation created by the regional administration.

The subsidy area was defined centrally by the government and the municipalities were specifically chosen for this subsidy.[10] Hence, the subsidy area does not precisely follow provincial or other administrative borders.[11] The area is a consequence of a dual motive behind the subsidy: i) a regional motive of supporting disadvantaged areas and ii) possible extension of the subsidy to the whole country, using the regional implementation as an experiment. Consequently, the area included the economically most disadvantaged areas of Finland, but there were municipalities considered to be more representative of all Finland both inside and outside the subsidy area.

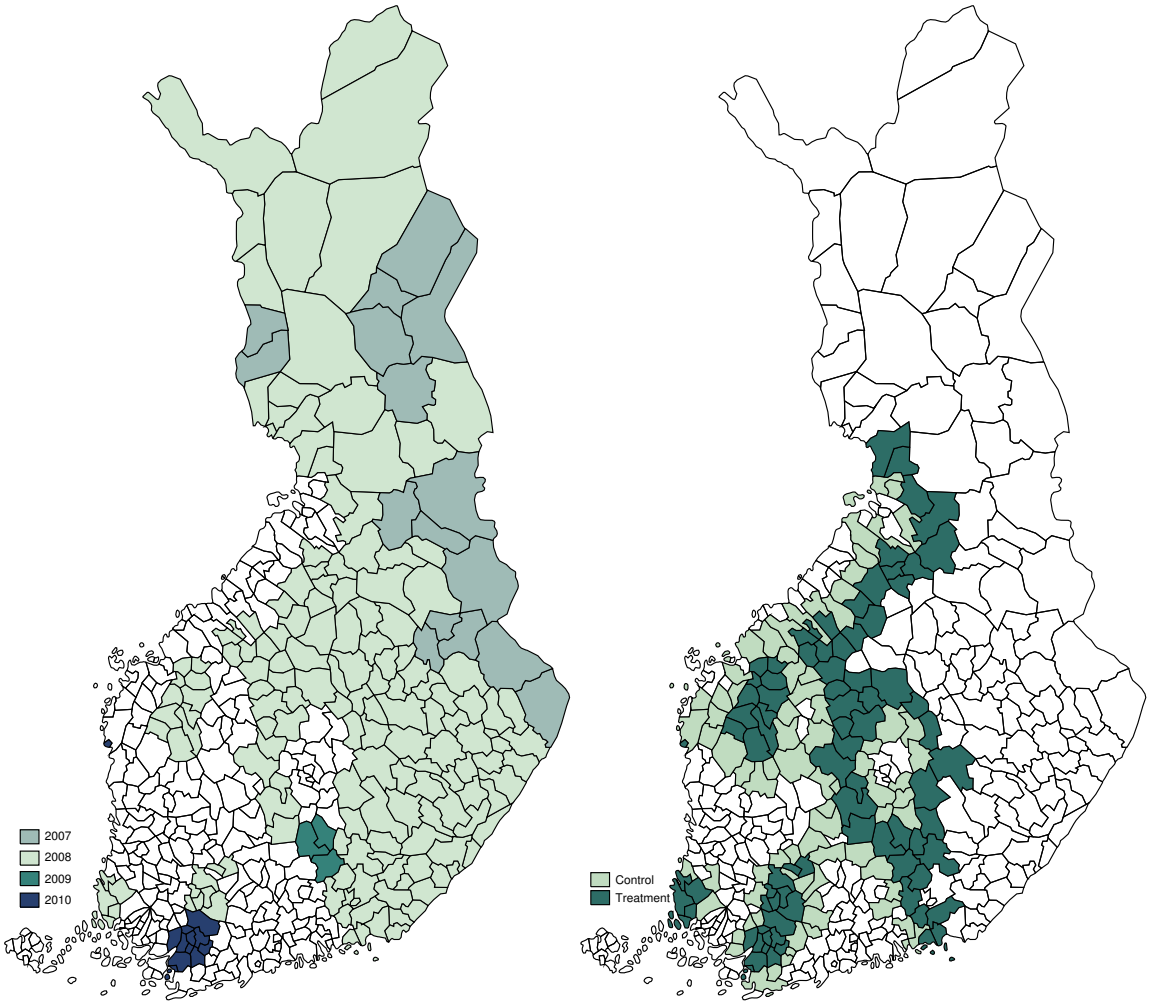
Figure 1a depicts the development of the subsidized area. The subsidy program started in June 2007 in a few municipalities in Northern and Eastern Finland. In 2008, the subsidy area covered about one third of Finnish municipalities, including all of Lapland and Eastern Finland, large parts of Northern and Central Finland as well as some municipalities in Southern and Western Finland. Small areas were added in 2009 and 2010.

In general, the subsidy area is concentrated in the economically worse-performing Northern and Eastern Finland, which are losing population and have have higher unem-

[9]Firms could be refused the subsidy if they did not have the prerequisites for viable business or if subsidizing was considered to considerably distort local competition or markets. In practice, these reasons were not often used for refusing the subsidy.

[10]The law lists EU I assisted area, and specific subregions (EU LAU 1 areas or former NUTS 4).

[11]In particular, the subsidy area does not follow the general business subsidy assisted area types I-III that are defined for business subsidies according to EU criteria, where Type I receives most business subsidies in amounts. The subsidy area included the whole Type I assisted area and a minority of municipalities in the Type II and III assisted areas.



(a) Development of the subsidy area (b) Treatment and control areas

Figure 1: Map of the subsidized area

Notes: Maps drawn using the municipality borders of 2007. The treatment area in panel (b) refers to municipalities in the subsidy area that are on the border of the subsidy area. The control area corresponds to those municipalities neighboring the treatment area where the subsidy was not available.

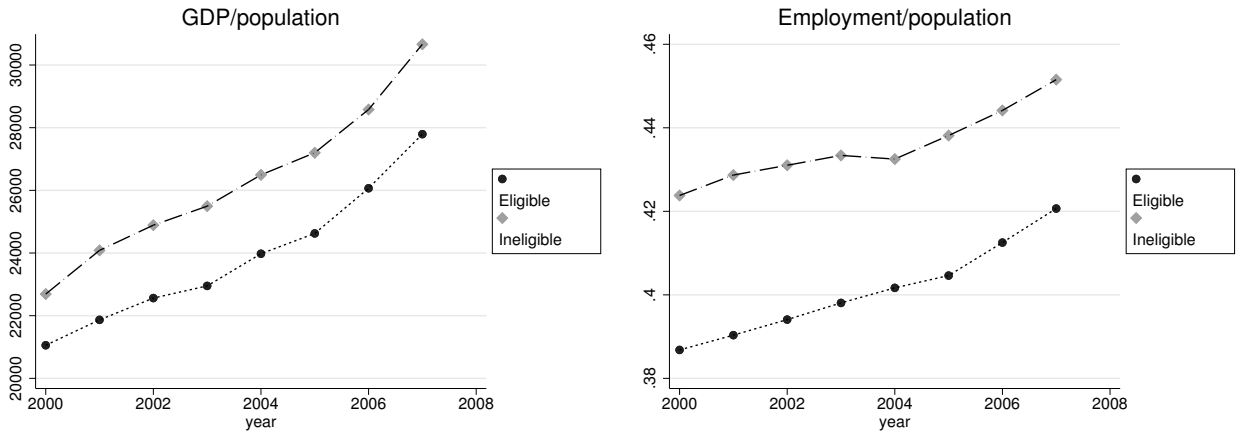


Figure 2: Trends GDP and employment share in the subsidy and no-subsidy areas
Notes: GDP (in market prices) and total employment divided by population, excluding capital city area. Data: Statistics Finland, Regional accounts, Transactions by NUTS 2, NUTS 3 and LAU 1 regions 2000–2007

ployment than other areas. Figure 2 depicts the trends in GDP and the employment rate in the eligible and ineligible areas (excluding the capital city area) before the subsidy period. The levels are clearly higher in the ineligible area, but the trends are quite similar. For the empirical strategy, it is important that the development in the areas is comparable over time. To improve the credibility of the causal estimation below, I focus on neighboring municipalities on the border of the subsidy area. This area is depicted in Figure 1b. Section 5.1 discusses the identification strategy.

The administration of the subsidy was regional: The ELY Centres were responsible for the implementation, including informing firms about the subsidy and processing the applications. This can result in geographic differences in, for example, information availability. First, firms located closer to an ELY Centre may be more likely to visit one and obtain information on subsidies. Secondly, ELY Centres may differ in how they distribute information on business subsidies. In particular, there are differences between ELY regions in the share of eligible municipalities: the shares vary between 3.7% and 100%. ELY Centres with higher share of eligible municipalities may distribute information more efficiently, resulting in higher take-up rates in those regions.

Related institutions

There are some partially overlapping business and employment subsidies in Finland, but none of them coincide with the geographical area of the first-employee subsidy, and there were no simultaneous changes. There are two subsidies that are substitutes for the first-employee subsidy, but they do not have such clear qualification and payment criteria and are granted with discretion, which makes it more uncertain for a firm. First is a discretionary subsidy for developing a business that can be granted for the labor costs of

a small start-up or a growing incumbent company. Second is a business starting subsidy targeted at companies in rural areas for expanding or starting a business amounting to a maximum of 50% of the labor costs of the first employee for two years. In the robustness section, I check for an effect on receiving business subsidies.

Firms could use other employment subsidies for hiring that include, for example, a hiring voucher for the unemployed, a wage subsidy for over 54-year-old low-wage employees and an exemption from payroll taxes up to a threshold in some rural municipalities in 2003–2011. These are targeted at all firms and specific groups of employees (with the exception of the payroll tax exemption) and do not specifically affect the incentives for becoming an employer. In addition, there is a business start-up grant to ensure a secure income for a new entrepreneur for at most 12 months when starting a business or when transitioning to full-time entrepreneurship but this does not affect labor costs.

Employment protection may decrease willingness to take-up the subsidy because the first-employee subsidy requires hiring on a permanent contract. In Finland the costs of dismissals are relatively modest especially for new employees in small businesses. For example, direct firing costs during the first year are 4% of yearly wage costs (as proxied by the length of the notice period) and dismissals are permitted for financial reasons. More details on employment protection in Finland are given in the Appendix. However, the entrepreneur's perceived costs of dismissals may differ from the true costs implied by employment protection regulation. In particular, new employers that are not well informed about the responsibilities and rights of employers may misperceive the costs.

2.2 Data and Descriptive Statistics

I use three register data sets: tax return data on the population of Finnish firms, the first-employee subsidy grants and other business subsidies. This allows me to study the hiring behavior of all firms and the subsidy take-up.

First, the main data set is yearly firm tax returns from 2000 to 2013 from the Finnish Tax Administration.[12] This includes information on firms' revenue, cost items including wage costs, number of employees, company form, industry and home municipality. I exclude non-business company forms, firms with non-positive revenue, and firms in agriculture that are not eligible for the first-employee subsidy. In addition, I exclude firms in the capital city area as this area differs the most from the subsidy area. After these restrictions there are 3,642,506 observations of 596,740 unique firms.

Throughout this paper, number of employees refers to the total number of employees that worked in the company during the year excluding the entrepreneur. I construct this variable as the total number of employees minus so called entrepreneur employees, which means firm owners who received wage income from the firm. Accordingly, wage costs is

[12] Additionally, I use payroll tax data on wage costs for robustness analysis.

constructed as the total wage costs minus entrepreneur wage costs.[13]

Second, I use data on the first-employee subsidy decisions obtained from the Finnish Ministry of Economic Affairs and Employment to identify the subsidized firms and document the use of the subsidy. The data consists of the 1,351 positive decisions[14] and includes a firm identifier, the amount of the subsidy granted and paid to the firm, as well as background information about the firms in the year of application. The subsidy was granted to 1,349 different firms as two firms applied for the subsidy twice. The number of firms that used the subsidy is small but not negligible: 1,349 subsidy users compared to 40,241 active non-employer firms in the eligible area in 2006. I match the subsidy data to the firm tax return panel using firm (pseudonymized) identifiers. There are 1,020 matched observations – 331 subsidized firms cannot be identified in the tax return data.

Third, I use business subsidy data from Statistics Finland to identify which firms have received some type of business subsidy. This data includes all state subsidies for firms through ELY Centres as well as other business subsidy agencies. Information on the specific type of subsidy (e.g. first-employee subsidy) is not included in this data.

The size distribution of firms is highly concentrated at zero employees, with almost 60% of firms having zero employees - as depicted in Figure 3. Therefore the target population of the first-employee subsidy is very large. Only about 10% of firms have one employee and the distribution is highly skewed.

Table 1 presents descriptive statistics for 2006–2007 on all firms, non-employer firms and firms that were non-employers in the previous year. The average number of employees is 4.9 and revenue €641,000. The average revenue of non-employer firms is €65,000. Of the firms that were non-employers in the previous year, 7.8% became employers, with an average of 0.14 employees and a revenue of €77,000, indicating small growth in non-employer firms on average.

Most non-employer firms are sole proprietors with 73% share compared to 53% of all firms. The second largest company form is corporation, which account for a third of all firms but only about 17% of non-employer firms. Partnerships account for about 10% of firms in all groups. The industry distribution of non-employer firms resembles the distribution of all firms. Human health and social work and other service activities are more common among non-employer firms.

[13]Some company forms such as corporations can pay wage income to the owner i.e. entrepreneur. 59% of firms are non-employers, which decreases to 55% when including entrepreneur employees. The subsidy was only targeted to external employees and could not be used for the wage costs of the owner. Results are robust to using the number of total employees instead of the number of external employees.

[14]According to a report by Aaltonen *et al.* (2011), a total of 1,635 firms applied for the subsidy and 1,351 were granted it. Most of the negative decisions were because the firms did not qualify for the subsidy. The most common reason for a negative decision was hiring the first employee before applying for the subsidy.

Table 1: Descriptive statistics of firms in 2006–2007

	(1) All	(2) Non employer	(3) Non employer in t-1
Revenue	640,536	64,734	77,176
	(1.2e+07)	(2,445,443)	(2,726,074)
Employees	4.9	0	.14
	(35)	(0)	(1.1)
Employer	.41	0	.078
	(.49)	(0)	(.27)
Wage costs	84,100	220	800
	(855,925)	(3,381)	(10,894)
Profit	47,591	14,568	16,990
	(824,017)	(98,268)	(110,631)
Sole Proprietor	.53	.73	.7
	(.5)	(.45)	(.46)
Partnership	.13	.1	.11
	(.34)	(.3)	(.32)
Corporation	.33	.17	.19
	(.47)	(.38)	(.39)
Manufacturing	.11	.091	.099
	(.31)	(.29)	(.3)
Construction	.15	.13	.14
	(.35)	(.33)	(.35)
Wholesale and retail trade	.19	.17	.18
	(.39)	(.38)	(.38)
Transportation and storage	.092	.062	.074
	(.29)	(.24)	(.26)
Professional, scientific and technical activities	.1	.11	.11
	(.3)	(.31)	(.32)
Human health and social work activities	.075	.098	.11
	(.26)	(.3)	(.31)
Other service activities	.065	.097	.11
	(.25)	(.3)	(.31)
New	.26	.34	.21
	(.44)	(.47)	(.41)
Previous employer	.58	.34	.29
	(.49)	(.47)	(.45)
Observations	428,564	251,731	192,539

Notes: Table presents sample means and standard errors in parentheses. The sample includes firms with positive revenue. Firms in the capital area and agriculture are excluded. Standard Industrial Classification according to Statistics Finland 2008. Largest industries are included in the table. Industries not in table include: mining and quarrying, electricity, gas, steam and air conditioning supply, water supply, accommodation and food services, information and communication, financial and insurance activities, real estate activities etc.

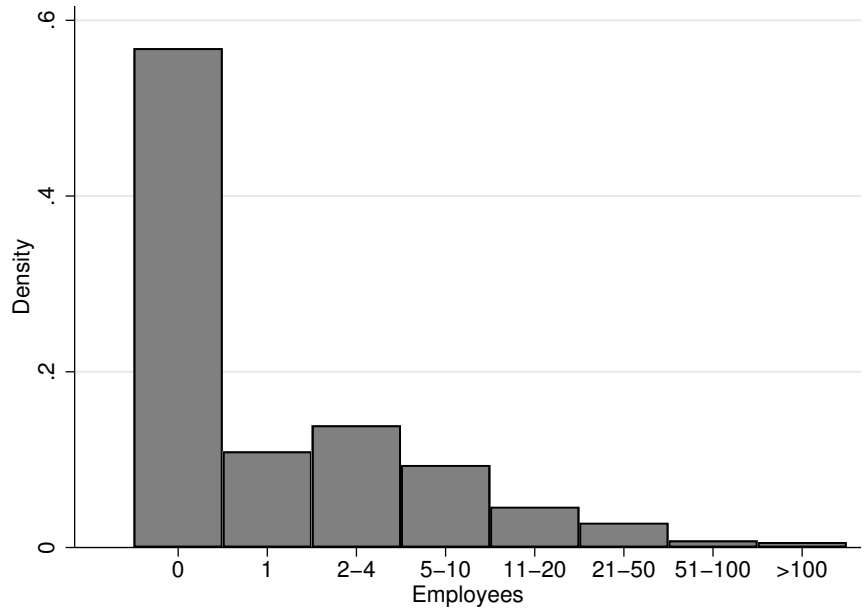


Figure 3: Size distribution of firms in 2006 by employment

Notes: Sample includes all firms with positive revenue in 2006. Employment is the number of external employees reported in the tax return, referring to all employees that worked in the firm during the tax year excluding the entrepreneur.

3 Theoretical Framework

3.1 Employment Incentive

The subsidy decreases the cost of labor for firms with no prior employees, increasing employment incentives. It can increase employment through i) increasing the share of firms that become employers and ii) increasing employment in firms that would have become employers even without the subsidy.

The effect of the subsidy depends on the distribution of labor productivities. Consider a simple model of firm production using only labor as input where production of firm i is a function of labor $f_i(l)$, where $f_i(l) \geq 0$ for $l \geq 0$, is twice differentiable and has standard production function properties $f_i'(l) \geq 0$, $f_i''(l) \leq 0$ (*Assumption 1*).

Profit function of firm i is $\pi_i(l_i) = f_i(l_i) - wl_i$ that implies the first order condition

$$FOC : f_i'(l_i^*) = w \tag{1}$$

determining optimal labor choice for the firm. A firm becomes an employer if $l_i^* \geq 0$. Denote $f_i'(0) = f_{0i}'$ as the marginal productivity of labor at zero. Now the probability of becoming an employer as a function of wage level is

$$p(w) = P(f_{0i}' \geq w) = 1 - D_0(w) \tag{2}$$

where $D_0(.)$ is the cumulative density function of labor productivities evaluated at $l = 0$ defined by firm specific production functions $f_i(.)$.

This core intuition holds even though, more realistically, the decision is based on expected or perceived labor productivities and costs, as firms may not know the true productivities and costs before becoming an employer. More generally, $f_i(l)$ can refer to the entrepreneur's utility from production including the entrepreneur's risk preference or preference for working alone, which would just introduce an optimality condition in addition to the FOC in equation 1. For example, a clear preference for working as a solo-entrepreneur would take a form where $f_i(l) < f_i(0)$ for all $l > 0$.

The subsidy enters the profit function by decreasing the cost of labor. Let $\bar{s} \in (0, 1)$ be equal to the fraction of labor costs covered by the subsidy and $s = 1 - \bar{s}$, i.e., the firm's share of labor costs after the subsidy. The labor choice and probability of becoming an employer are as presented in equations 1 and 2 replacing w with sw, for example, a firm becomes an employer if $f'_{0i} \geq sw$. Consequently, the subsidy induces an increase in the probability of becoming an employer:

$$\tilde{\delta} = p_1 - p_0 = D_0(w) - D_0(sw), \tag{3}$$

where $p_1 = 1 - D_0(sw)$ is the probability with the subsidy and $p_0 = 1 - D_0(w)$ probability without the subsidy.[15]

The effect is defined by the size of the subsidy and the mass of firms at the margin of becoming an employer – those that have labor productivities between w and $(1 - s)w$. Adapting the instrumental variable terminology, I call the group of firms with $f'_{0i} \geq w$ *always-employers* and those that become employers only under the subsidy, i.e. firms with $sw \leq f'_{0i} < w$, *subsidy-employers*.[16]

The increase in employment is $\Delta l = l^*_{i1} - l^*_{i0} \geq 0$ where l^*_{i1} satisfies the FOC with the subsidy (equation 1) and l^*_{i0} without the subsidy. In the subsidy-employers group $l^*_{i0} = 0$. As the subsidy is only for the wage costs of the first employee, it mainly affects at the hours margin for the first employee in the always-employers group. However, a firm could use the extra cash from the first employee to hire a second employee instead of just one. Because the subsidy does not affect the price of all labor, labor market equilibrium effects are attenuated in comparison to a general wage subsidy.

Additional Effects

In addition to the direct employment incentive the subsidy can have other effects through different mechanisms. First, the subsidy may have long-term effects on labor if there are fixed costs of becoming an employer, even though the decrease in labor costs is only

[15] $\tilde{\delta} = p_1 - p_0 = 1 - D_0(sw) - (1 - D_0(w)) = D_0(w) - D_0(sw)$

[16] Subsidy-employers is similar to "compliers" in the instrumental variables literature, that relates to the group that changes behavior because of the treatment. Always-employers relates to always-takers.

temporary. In this case, the subsidy may improve efficiency by enabling firms to overcome the costs of becoming an employer. For example, Mel *et al.* (2019) discuss some reasons why a temporary subsidy may have a lasting impact on employment: labor market frictions or imperfect information may bring about one-time hiring constraints. For instance, some firms may have under-evaluated their managerial ability and only discover their true (higher) ability after hiring, or finding a suitable match for the job or training the employee becomes easier after experience.

The fixed costs of becoming an employer could be either due to institutional requirements (e.g. compliance costs of payroll taxation, different legal duties of employers) or due to production technology (e.g. learning to recruit and manage employees). The subsidy can help firms to overcome this barrier induced by fixed costs and choose optimal employment in the long run.

Second, if the subsidy has an effect on labor it can affect other firm outcomes such as revenue (or production) and profitability. Of course, the subsidy can affect the wage costs through both employment and wages. As the subsidy decreases the operating costs of employer firms, it can increase entry or decrease exit of employer firms.

Third, the subsidy may affect firm compliance to other policies. For example, the subsidy could increase job formalization if it causes firms to substitute towards formal employment from informal employees. In addition, the subsidy may have unintended consequences as some firms strategically try to take advantage of the subsidy such as splitting up firms to hire the first employee in multiple firms, or remain longer as a non-employers to qualify for the subsidy.

3.2 Take-up and Effectiveness

Due to the subsidy design it does not simply reduce labor costs (unlike e.g. a payroll tax cut) but firms have a decision whether or not to apply for the subsidy. Therefore, I consider take-up of the subsidy as an additional firm decision and margin of behavior. I consider two factors that can affect take-up: i) awareness and ii) compliance costs. Both of these are affected by the subsidy design and administration.

Consider first awareness of the subsidy. It is clear, that only firms aware of the subsidy can respond and use it. How many and which firms know about the subsidy depends on how the ELY Centres distribute information on the subsidy availability. Second, consider compliance costs, i.e. costs of using the subsidy. These reduce the benefits of the subsidy, causing some firms to self-select out of using the subsidy. The compliance costs can be direct costs, such as time or money spent on application, or indirect costs. Indirect costs include the opportunity cost of the full-time, regular employee requirement: the labor costs of a full-time employee are higher than of a part-time employee and there is higher

risk as it may not be as easy to end the employment contract. The full-time requirement as a separate eligibility condition cannot be empirically distinguished from compliance costs. This is because both imply that firms below a labor threshold do not use the subsidy, as as shown below. In addition, indirect costs include the financing costs arising from the time lag in receiving the subsidy half year after the realized wage costs.

Let $\alpha_i(l_{i1}^*, c, \bar{l}, a_i) = a_i * b_i$ denote firm take-up, where $a_i = \{0, 1\}$ is firm awareness of the subsidy, with $a_i = 1$ meaning a firm is aware of the subsidy, $b_i(c, \bar{l}) = \{0, 1\}$ the decision to use the subsidy, c the compliance costs of using the subsidy, and the full-time threshold as $\bar{l}$. I assume firm awareness is exogenous, meaning I do not model searching for information on the subsidy. For simplicity, I focus on constant compliance costs. Below, I discuss shortly the case with heterogeneous compliance costs. For firms with $a_i = 0$ the profit maximization problem is not affected by the subsidy. However, for firms with $a_i = 1$ there is the additional decision to use the subsidy or not compared to the simple case without compliance costs presented above. Without unawareness or compliance costs $\alpha_i = p_1$.

Firstly, to analyze how compliance costs and the full-time requirement affect firm choice, consider the case that $a_i = 1$. The firm's choice to use the subsidy is determined by whether the subsidy benefits are higher than the compliance costs: $swl_{i1}^* \geq c$, which can be rearranged as $l_{i1}^* \geq \frac{c}{sw}$. The full-time requirement is simply $l_{i1}^* \geq \bar{l}$. The full-time requirement is binding if $\bar{l} \geq \frac{c}{sw}$ and the compliance costs if $\bar{l} < \frac{c}{sw}$. Consequently, compliance costs or the full-time threshold restrict the subsidy use to firms with high enough labor demand with the subsidy. For simplicity, let us assume the full-time constraint is binding. Because $f_i'(l_{i1}^*) = sw$ and $l_{i1}^* \geq \bar{l}$ we get the following condition for using the subsidy:

$$b_i = I(f_{\bar{l}i}' \geq sw), \tag{4}$$

where $f_{\bar{l}i}' = f_i'(\bar{l})$. Hence, subsidy use decision depends on the firm's marginal labor productivity evaluated at the full-time labor threshold (or threshold implied by compliance costs).

Table 2 panel (a) classifies firm types by observed hiring and take-up behavior under assumption that firms are aware of the subsidy. Because of the compliance costs, subsidy-employers not willing to hire on a full-time contract decide not to use the subsidy, and, hence do not hire. This decreases the effect of the subsidy compared to the case without compliance costs. Some always-employers also might not use the subsidy, which decreases the effect on labor demand.

As a result, the effect on the probability of becoming an employer is the fraction of subsidy-employers that us the subsidy

$$\overline{\delta} = \tilde{\delta} - (1 - b) \int_{sw}^{w} d_{f_o'|f_{\bar{l}} < sw}(u) du \leq \tilde{\delta} \tag{5}$$

Table 2: Observed Choice and Firm Type

	(a) Full awareness		(b) Incomplete awareness	
Em-ployer	Use Subsidy		Use Subsidy	
	Yes	No	Yes	No
Yes	Always-employer, full-time	Always-employer, not full-time	Always-employer, full-time, aware	Always-employer, full-time, not aware
				Always-employer, not full-time, aware
				Always-employer, not full-time, not aware
	Subsidy-employer, full-time		Subsidy-employer, full-time, aware	
No	X	Subsidy-employer, not full-time	X	Subsidy-employer, full-time, not aware
				Subsidy-employer, not full-time, aware
				Subsidy-employer, not full-time, not aware
		Never-employer		Never-employer

Notes: Always-employer refers to firms that become employers even in the absence of the subsidy, at the given wage level. Subsidy-employers become employers when there is a subsidy but not without it. Never-employers do not hire even with the subsidy. The subsidy is restricted to full-time employees, which causes some firms to self-select out of using the subsidy.

where $(1-b)\int_{sw}^{w} d_{f'_o|f_{\bar{l}}<sw}(u)du \geq 0$ is the fraction of subsidy-employers that do not satisfy the full-time requirement with the subsidy, and $b = E(b_i) = 1 - D_{\bar{l}}(sw)$ i.e. the fraction of firms that satisfy the full-time requirement (under the subsidy).[17] As b gets closer to p_1 (full take-up) the probability is closer to the case without compliance costs. The derivation of $\overline{\delta}$ and the corresponding probability of becoming an employer $\bar{p_1}$ is presented in the Appendix B.1. The production functions $f_i()$ define the joint distribution of f'_0 and $f'_{\bar{l}}$.

Secondly, imperfect information further reduces the effect as depicted in panel (b) of Table 2. The main difference to full awareness is that there are now full-time subsidy-employers (and always-employers) that do not use the subsidy. Consequently, only the fraction of full-time subsidy-employers that are aware of the subsidy become employers. The effect on the probability of becoming an employer with compliance costs and imperfect

[17] $\overline{\delta} = \bar{p_1} - p_0 = D_0(w) - D_0(sw) - D_{\bar{l}}(sw)\int_{sw}^{w} d_{f'_o|f_{\bar{l}}<sw}(u)du$

awareness is

$$ITT = \delta = a\overline{\delta} = a\tilde{\delta} - a(1-b)\int_{sw}^{w} d_{f'_o|f_{\bar{l}}<sw}(u)du \tag{6}$$

where $a = E(a_i|f'_{\bar{l}} \geq sw, sw < f'_0 < w))$ is the awareness rate for subsidy-employers with $b_i = 1$. The effect on becoming a full-time employer can be defined simply using equation 3 and substituting $D_{\bar{l}}()$ for $D_0()$ as follows:

$$ITT_f = \delta_f = a\tilde{\delta_f} = a(D_{\bar{l}}(w) - D_{\bar{l}}(sw)). \tag{7}$$

The effect on becoming a full-time employer is larger than the effect on becoming an employer because some always-employers may become full-time employers due to the subsidy. The proof is in the Appendix B.1.

This δ in equation 6 is the *intent to treat* (ITT) effect or, in other words, the effect on the eligible firms. The ITT scaled by awareness rate of subsidy-employers is the average treatment effect on the treated (ATT) i.e. those aware of the subsidy, which is also equal to the treatment effect under full awareness:

$$ATT = \overline{\delta} = \frac{\delta}{a} \tag{8}$$

Hence, compliance costs reduce the ATT while awareness only reduces ITT. In a sense, only the firms aware of the subsidy are "treated" in the sense that their price of labor is changed. Both parameters are relevant: while the ITT is the total effect of the policy, ATT tells how firms respond to the employment incentive of the subsidy.

With heterogeneous compliance costs the core intuition is the same but there is gradual increase in take-up rate as subsidy benefits increase instead of a sharp increase at a threshold, assuming that awareness rate is fixed. The difference arises because the subsidy benefit threshold that determines the take-up decision differs by firm according to their compliance costs. This complicates inferring awareness from data: Above any subsidy benefit threshold the take-up rate is the result of awareness rate and fraction of firms that have compliance costs below that threshold. Consequently, firm-specific compliance costs cannot be empirically distinguished from awareness.

3.3 Estimating Effects and Awareness

Finally, I discuss estimating the quantities from the data. I first discuss how presence of unawareness or compliance costs can be detected in the data. Then I discuss how awareness can be calculated using observable quantities, which can then be used to calculate ATT given ITT estimate. ITT simply corresponds to the estimated effect on the targeted firms. The estimation strategy is discussed in 5.1. If awareness was directly observed, the ATT could be estimated using the instrumental variables approach. In contrast, the

ATT without compliance costs cannot be quantified because we need to add a constant equal to $(1-b)\int_{sw}^{w} d_{f_o'|f_{\bar{i}}<sw}(u)du$ to ATT, which is not observable in the data.

Under full awareness and no compliance costs the take-up rate in the target group equals the employer rate, i.e., $\alpha = p_1$, or all employers use the subsidy. This can be assessed by observing the take-up rate of employers:

$$\alpha_1 = \alpha_{|I(employer)=1} = \frac{ab}{\overline{p}_1}. \tag{9}$$

If the take-up rate of employers is one there is full awareness and no compliance costs. Similarly, if there is no unawareness, the take-up rate of full-time employers should be equal to one (see Table 2).

Although awareness itself is not observed, awareness rate can be calculated from the data assuming a flat awareness rate for subsidy- and always-employers. Granted, this may be a strong assumption. Using the model, the observed take-up rate can be written as a function of awareness and full-time employer shares. First, take-up rate of full-time employers (or alternatively firms with subsidy above a fixed compliance cost level) equals their awareness rate, because the only non-takers in this group are always-employers that are not aware of the subsidy (see table 2). This is the average take-up rate of full-time always-employers and subsidy-employers that are aware of the subsidy:

$$\bar{a}_{|\text{full-time employer}} = \frac{ab_0 + a\overline{\delta}_f}{\bar{b_1}} = \frac{ab_0 + \text{ITT}_f}{b_0 + \text{ITT}_f}$$

where $\bar{b}_1$ is the full-time share, and the always-employer full-time share is $b_0 = \bar{b_1} - \text{ITT}_f$. This is higher than the awareness rate because the denominator excludes subsidy-employers that are unaware of the subsidy, namely $b_1 = b_0 + \overline{\delta} \geq b_0 + a\overline{\delta} = \bar{b_1}$. With some algebra the awareness rate of always- and subsidy-employers can be written

$$a = \frac{\bar{a}\bar{b_1} - \text{ITT}_f}{\bar{b_1} - \text{ITT}_f} = \frac{\bar{a}\bar{b_1} - \text{ITT}_f}{b_0} \tag{10}$$

using only observable and estimable quantities.[18]

Consequently, the estimated ATT is the ITT scaled by the calculated awareness rate in equation 10 under the assumption of flat awareness rate. Variance of awareness and ATT are derived in the Appendix B.2.

The assumption of flat awareness rate may not hold in reality, as awareness may be correlated with the firms' productivity. However, the formula provides an observable quantity for assessing the average awareness rate and ATT. In particular, flat awareness rate is more realistic in groups of firms with similar productivities and characteristics.

[18]Similarly, if there are no compliance costs awareness rate can be inferred by substituting the full-time shares by employer shares.

Hence, to estimate more realistic ATTs, the awareness rate and effect can be estimated in more narrowly defined groups.

In the presence of heterogeneous compliance costs, the calculated awareness rate using formula 10 is a lower bound of the true awareness rate. Correspondingly, ATT calculated using the lower bound for awareness is an upper bound of the true ATT.

4 Subsidy Take-up

In this section, I document the take-up of the subsidy and present descriptive evidence on the reasons for the low take-up rate.

4.1 Full-time Eligibility Restriction

Here, I document the take-up of the subsidy among targeted firms that became employers and what is the role of the full-time employee restriction in explaining take-up. In particular, I first look at take-up by observed labor choice and second by predicted probability of becoming a full-time employer.

I focus on take-up among employer firms that had zero employees in the previous year. I use this eligibility criteria because it is clear that firms in this group are eligible for the subsidy if they hired an employee on a full-time permanent contract. The zero employees in the previous year criteria does not match exactly with the criteria of the law, specifically, the 12 month non-employer spell duration. However, using a different eligibility criteria does not matter much for the take-up rates, which I discuss in more detail in the Appendix C. In short, the group I focus on does not include all firms that are eligible for the subsidy for two main reasons. First, the yearly data does not include all 12 month long non-employer periods. Second, the ELY Centres may have interpreted the no employees condition to mean no full-time employees. In addition, it is not clear whether firms that hire in their entry year are eligible to the subsidy. In fact, 34% of subsidy users do not fulfill this eligibility criteria in the data. Hence, the group I study is a subset of eligible firms but the results are robust with respect to different eligibility specifications.

The eligibility was restricted to full-time employees with at least 25 hours of work per week, which I do not observe in the data. I measure "full-time" employers using total wage costs as a proxy. First, I call firms full-time employers if they have total wage costs above median wage cost per employee, which is €12,000. These firms have total labor above a "full-time equivalent" level, although it may be divided among multiple employees or contracts. Second, I try to capture firms that have at least one full-time employee by looking at firms with wage costs per employee above median wage cost per employee. These firms have per employee wage costs above a "full-time equivalent" level. In the

	(1) Subsidy takers	(2) Em-ployers	(3) Full-time employers	(4) Employers, full-time employees
% Subsidy takers	100	1.9	5.6	7.5
	(0)	(14)	(23)	(26)
Paid subsidy	8,131	7,673	8,940	9,105
	(3,846)	(3,682)	(3,373)	(3,644)
Calculated subsidy	6,155	2,673	8,418	10,434
	(4,174)	(4,117)	(5,946)	(6,292)
Employees	2.4	1.7	2.3	1.9
	(5.8)	(3.1)	(6.1)	(7.2)
Wage costs	16,971	8,840	34,172	42,707
	(22,286)	(99,861)	(209,911)	(294,117)
Observations	1,011	18,556	2,586	1,300

mean coefficients; sd in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Subsidized firms and take-up

Notes: Sample includes firms in the subsidy area during the subsidy period that had zero employees in the previous year and the subsidy takers identified in the tax data. Employers have a positive number of (external) employees. Full-time employers have at least one effective employee, i.e. total labor costs above the median labor cost per employee. Full-time employee firms have at least one full-time employee i.e. their wage costs per employee are above median wage costs per employee. Calculated subsidy is defined as $0.3lc_t + 0.225lc_{t+1} + 0.075lc_{t+2}$ using the actualized labor costs per employee lc_t and the subsidy rule, assuming that the first employee was hired in the middle of the first year.

Appendix C, I describe that take-up rates are not particularly sensitive to how I define the "full-time equivalent" level.

Table 3 summarizes the take-up and employment information for the subsidy takers and eligible firms: all employers, full-time employers and employers that have at least one full-time employee. Only 2% of the eligible employer firms used the subsidy. Take-up increases to 5.6% and 7.5% for full-time employers and employers with full-time employees respectively. Number of employees and total wage costs are lower for all employers than for the subsidy takers and full-time employers. This suggests that many targeted firms did not use the subsidy because their chosen level of labor was below the full-time eligibility restriction. Moreover, the take-up rate is still very low for full-time employers. In other words, there are full-time always-employers that did not use the subsidy.

Secondly, I ask whether subsidy use is focused on firms that are more likely to become full-time employers even without the subsidy. This is relevant, as the effectiveness of the subsidy depends on the take-up rates of subsidy-employers, which are not as likely to become employers at the baseline. In addition, deadweight spending and, hence, efficiency depends on the take-up rates of always-employers. I predict the probability of

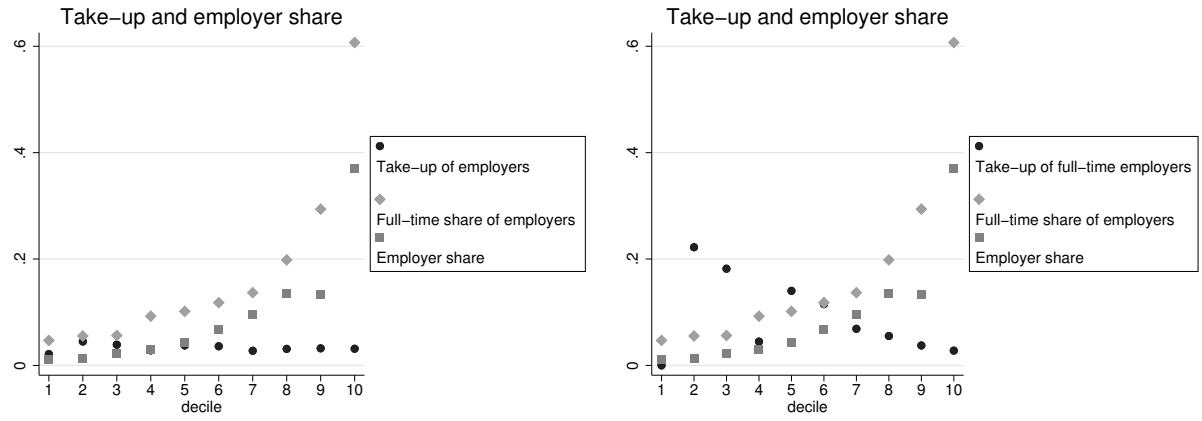


Figure 4: Take-up and full-time employer share by predicted probability of becoming an employer

Sample includes firms in the treatment area with 0 employees in the previous year. Full-time employer is defined as having total wage costs above median of wage costs per employee (€12,000). X axis has deciles of predicted probability of becoming a full-time employer using logit regression coefficients summarized in table 13.

becoming a full-time employer using a logit regression and data from 2005–2006 before the subsidy period. The observed firms characteristics included in the regression are firm size, industry, company form, age and hiring history, and local labor market measures. The regression results and predictive power are presented in the Appendix D. Of course, even after the predicted probability there are unobserved differences in firms' probability of becoming an employer.

Figure 4 plots the observed take-up of employers in the left panel and take-up of full-time employers in the right panel, full-time employer share of employers and the employer share by the decile of predicted probability of becoming a full-time employer. The non-take-up of full-time employers reflects lack of take-up that is not directly explained by the full-time eligibility constraint. Total take-up of employers is very similar across deciles between 2 and 4%, although the share of full-time employers increases a lot by decile - first slowly from about 5 to 15% in the first 7 deciles and then sharply from 20% in the 8th decile to 60% in the 10th decile. This reflects that take-up of full-time employers varies greatly by the predicted probability: from 0 in the first decile to 22% in the second decile to again about 3% in the 10th decile. The lack of take-up not explained by the full-time restriction is higher for firms with high probability of becoming a full-time employer.

4.2 Subsidy Benefits

Here I describe how take-up is related to the monetary value of the subsidy. With compliance costs of using the subsidy, only firms with subsidy benefits above the costs take-up the subsidy. Consequently, the monetary value of the subsidy can shed light on how compliance costs explain take-up.

To measure the subsidy benefits for all firms, not just takers, I construct a measure *calculated subsidy* that is based on the *ex post* reported wage costs of the firm and the subsidy rule. The calculated subsidy is

$$\text{Calculated subsidy} = 0.3lc_t + 0.225lc_{t+1} + 0.15lc_{t+2},$$

where lc_t are wage costs per employee in year t when the firm became an employer. This is based on simplifying assumptions i) the wage costs are divided equally between the first and other employees and ii) the first year wage costs of the first employee are divided equally in years t and t+1 (similarly for the second year), as the true wage costs of the first employee and timing are not observed in the yearly data. Assumption (i) means that the first employee wage costs are proxied by the firms total wage costs divided by the number of employees. The calculated subsidy is on average close but smaller than the observed subsidy for full-time employers. Robustness of this measure is discussed in the Appendix C.

Table 3 summarizes the paid subsidy and calculated subsidy for takers and eligible employer firms. The subsidy takers received €8,100 on average with a smaller calculated subsidy of €6,100, and they had on average 2.4 employees with €17,000 wage costs. The paid subsidy amounts to about 45% of the total wage costs in the first year.

The average calculated subsidy of all employers is only €2,600 that may in part explain the low take-up of 2% in this group. For full-time employers and for employers with full-time employee(s) the calculated subsidy is €8,300 and €10,356 that are close to the paid subsidy for the takers at €8,900 and €9,100 and higher than the average calculated subsidy for all takers. In addition, table 12 in the Appendix shows that subsidy amounts are similar for takers and non-takers in the full-time employer group. However, take-up rates are low despite the relatively high calculated subsidy benefits.

Figure 5 depicts the take-up rate by calculated subsidy. The take-up is close to zero for firms with calculated subsidy under €2,000 and gradually increases to 12% for firms with calculated subsidy between €8,000 and €10,000. For firms with calculated subsidy of €6,000-20,000 the take-up rates vary between 9 and 12%.

Compliance Costs

There are two key observations that suggest that compliance costs decrease take-up but cannot explain the extent of it. First, take-up rates increase from 2% to about 12% with sufficiently high subsidy benefits. Second, there are full-time employers with relatively high subsidy benefits that do not use the subsidy. By Table 2, this suggests that incomplete awareness is part of the explanation. But, there are three factors that complicate drawing clear conclusions of compliance costs based on the observations.

First, lack of take-up in groups with sufficiently high subsidy benefits cannot be ex-

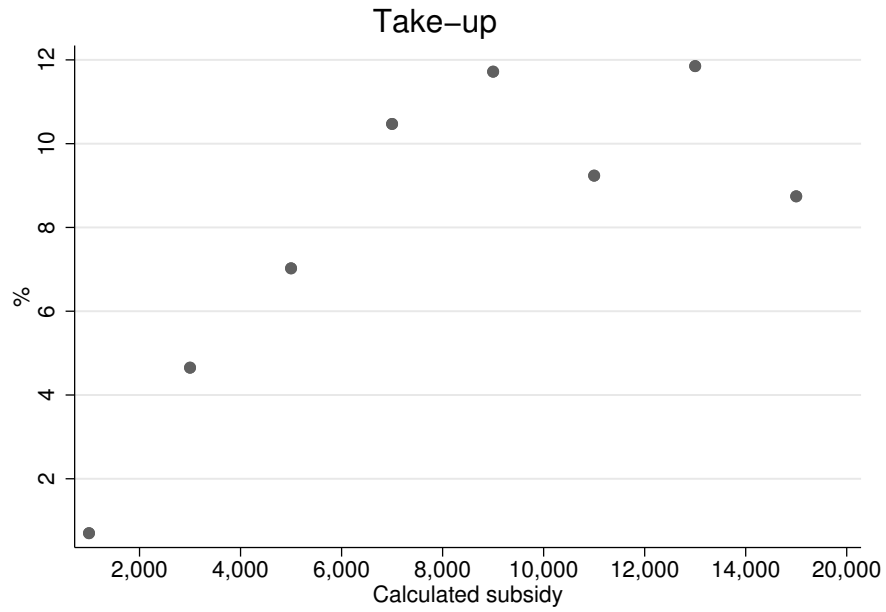


Figure 5: Take-up by calculated subsidy

Notes: Calculated subsidy is $0.3lc_t + 0.225lc_{t+1} + 0.15lc_{t+2}$, where lc_t is the wage costs per employee at t. Sample of firms includes firms in the treatment area with calculated subsidy amount above 0 and below €20,000.

plained by homogeneous compliance costs but with heterogeneous compliance costs some firms may still have benefits below their compliance costs. In particular, the costs implied by the full-time threshold are likely heterogeneous but application costs may also depend on the entrepreneur's ability. However, the observed pattern is not likely to arise from heterogeneous compliance costs. First, heterogeneous costs imply increasing take-up in subsidy benefits, but take-up does not increase after €8,000 of subsidy benefit. Second, take-up rates of firms more likely to become full-time employers are not higher despite that they should have lower compliance costs. Alternatively, if heterogeneous compliance costs explain the lack of take-up, the compliance costs surpass €9,000 for 94% of full-time employers.

Second, compliance costs of the subsidy include the opportunity cost of hiring on a permanent full-time contract as opposed to a temporary contract with fewer hours. The difference in the firms' wage costs between the optimal contract and the full-time restriction equals this component of compliance costs. With higher wage costs the subsidy benefits are higher but compliance costs are lower as well. Because of this, compliance costs and the full-time requirement cannot be empirically separated. Arguably, the other compliance costs are rather low as getting the subsidy requires very little bureaucracy. For example, there is no need for lengthy project plans in application unlike in, e.g. applying for R&D grants.

Third, take-up is based on *expected* subsidy benefits, which reflect the *expected* wage

costs of the first employee, not the actualized amounts discussed here. For example, the expected wage costs could be lower than actualized if a firm expects to hire only shortly but ends up keeping the employee(s) for a longer period. There is also risk involved in the future wage costs. Consequently, a firm may not take-up the subsidy because its expected value is below the compliance costs even though its *ex post* calculated subsidy benefits exceed the costs. This may result in the observed take-up pattern if a large proportion of firms have low expected subsidy benefits.

4.3 Awareness

The observed low take-up of full-time employers with high subsidy benefits may be explained by incomplete awareness. Here I present descriptive evidence on how take-up is associated with information proxies and other firm characteristics.

I use a logit regression to study how awareness proxies and firms characteristics are associated with using the subsidy. The institutional setting provides potential variation in awareness through ELY Centres as an information channel. I use three proxies related to this information channel: i) a dummy for firm location in a municipality with an ELY Centre, ii) share of eligible municipalities in the ELY Centre region, and iii) a dummy for firms that have used some type of business subsidy before. First, firms located close to ELY Centres may be more likely to visit them and, consequently, receive information. Second, ELY Centres may distribute information of the subsidy more effectively if larger share of their area is eligible for the subsidy. Admittedly, these proxies may correlate with other firm characteristics associated with firm location, but the regressions control for local differences. Third, firms that have used business subsidies before may be more likely to search or receive information from ELY Centres. This indicator for using business subsidies is based on business subsidy data, that includes all state subsidies for businesses.

I estimate the logit regression:

$$\log(\frac{p_i}{1-p_i}) = c + \beta X_i + \epsilon_i$$

where p is the probability of using the subsidy, c is the constant, and X_i is a vector of firm characteristics and ϵ_i is the error term. Firm characteristics include the awareness proxies, industry mean wage cost per employee to reflect industry differences in wage costs, company form, age, hiring history, size and region and industry fixed effects when possible. In an alternative specification, I also include the predicted probability of becoming a full-time employer. Controlling for firm size, wage costs, and predicted probability of becoming a full-time employer help to control for the selection due to compliance costs.

Table 4 summarizes the regression results from 3 different specifications: (1) including region and industry fixed effects and, hence, excluding regional subsidy availability (= share of subsidy municipalities in the region) and industry mean wage costs, and (2) and

Table 4: Logistic regression results of subsidy take-up

	(1)		(2)		(3)	
ELY Center municipality	1.359*	(0.181)	1.466**	(0.181)	1.462**	(0.180)
Previous business subsidy user	2.282***	(0.396)	2.237***	(0.378)	2.289***	(0.386)
Regional subsidy availability_50p			1.225	(0.251)	1.229	(0.252)
Pirkanmaa	1.125	(0.498)				
Central Finland	0.655	(0.274)				
South Ostrobothnia	1.454	(0.435)				
Central Ostrobothnia	0.490	(0.377)				
North Ostrobothnia	0.940	(0.348)				
New	1.619***	(0.220)	1.621***	(0.219)	1.571**	(0.247)
Revenue_50p	4.289***	(0.572)	4.037***	(0.512)	4.381***	(0.963)
Net assets_50p	0.881	(0.112)	0.830	(0.104)	0.825	(0.104)
Partnership	0.193***	(0.0886)	0.194***	(0.0876)	0.203***	(0.0911)
Corporation	0.479***	(0.0721)	0.443***	(0.0651)	0.411***	(0.0735)
Construction	1.111	(0.825)				
Wholesale and Retail Trade	1.380	(1.026)				
Other service activities	2.328	(1.752)				
Previous Employer	0.590***	(0.0738)	0.570***	(0.0709)	0.575***	(0.0864)
Industry mean wage cost per employee _50p			1.126	(0.123)	1.122	(0.123)
Decile 2					2.281	(1.216)
Decile 3					2.677	(1.384)
Decile 4					1.888	(0.982)
Decile 5					2.681*	(1.342)
Decile 6					2.119	(1.091)
Decile 7					1.506	(0.808)
Decile 8					1.833	(1.012)
Decile 9					1.485	(0.873)
Decile 10					2.735	(1.721)
Observations	18,437		18,555		18,547	
pseudo R^2	0.117		0.097		0.102	

Exponentiated coefficients; Standard errors in parentheses
Reporting Odds Ratio
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The sample includes eligible employer firms: firms in the subsidy area and period with positive employment and zero employees in the previous year. Net asset and revenue percentiles are according to the previous year relative to full population. Regression (1) includes firm covariates and industry and region fixed effects, while (2) and (3) use industry mean wage costs instead of industry fixed effects and regional subsidy municipality share instead of region fixed effects. Standard industrial classification according to Statistics Finland, only reporting selected industry coefficients that represent the largest differences. Region defined according to home municipality of firm and ELY Centre municipalities are those that have the regional ELY Centre. New means firms that are at most 3 years old and previous employer means firms that have had a positive number of employees in some prior year. The regressions control for year fixed effects. The reported odds ratios are exponentiated coefficients from the logit model.

(3) including a dummy for regional subsidy availability and industry mean wage costs above median. Specification (3) includes deciles of predicted probability of becoming a full-time employer, which also uses firm characteristics including industry and region fixed effects. The coefficients are fairly robust across specifications.

The results show, firstly, that take-up is positively associated with the information proxies in all specifications. Firms in ELY Centre municipalities and with prior business subsidy use are statistically significantly more likely to take-up the subsidy with odds ratios of 1.4–1.5 and 2.3–2.9, respectively. The association with subsidy municipality share is also positive but statistically weaker. This is consistent with an information channel through ELY Centres.

Secondly, take-up is associated with some characteristics related to higher probability of hiring even after controlling for the predicted probabilities. For example, new and relatively large firms are more likely to use the subsidy. Hence, the subsidy benefited firms that were larger and more likely to hire even without the subsidy, which also supports that the full-time restriction or compliance costs reduced take-up for smaller firms. However, some firm characteristics that are associated with higher probability of becoming an employer, namely corporations and firms with prior hiring experience, have negative association with subsidy use. Third, industry differences do not seem to arise from wage cost differences. This could mean that wage level is less important in explaining take-up than firm size.

As discussed in section 3.2, ITT estimate is needed to calculate awareness rates assuming. Consequently, I now estimate the effect of the subsidy on the targeted firms. Then, section 5.4 below derives awareness rates and discusses the implications of awareness on effectiveness.

5 Effects of the Subsidy

5.1 Empirical Identification

I use a standard difference-in-differences (DD) method to estimate the effect of the subsidy, exploiting the geographical criteria and the timing of the policy. Firms in the area without the subsidy are controls for firms in the subsidy area. The effect can be estimated using a regression model:

$$Y_{it} = \gamma D_{it}^{AREA} + \lambda D_{it}^{PERIOD} + \delta(D_{it}^{AREA} * D_{it}^{PERIOD}) + \alpha_i + X_{it}^{'}\beta + \epsilon_{it} \qquad (11)$$

for firm i in period t, whereY_{it} is the dependent variable, D_{it}^{AREA} is a subsidy dummy that equals one in the subsidy area and zero in the control area, D_{it}^{PERIOD} is a dummy for the subsidy period, α_i is a firm fixed effect and X_{it} is a vector of additional covariates.

For some of the specifications, I use an OLS regression omitting the α_i in the regression. The estimate δ is the difference in the change between the subsidy and control areas, which is the intent to treat effect of the subsidy (ITT) given that the DD assumptions hold. Standard errors are clustered at the municipality level, as that is the level of the treatment assignment. I also estimate the dynamic form DD with yearly coefficients:

$$Y_{it} = \alpha_i + \gamma D_{it}^{AREA} + \sum_{t=2000}^{2013} \lambda_t D_{it} + \sum_{t=2000}^{2013} \delta_t (D_{it}^{AREA} * D_{it}) + X_{it}^{'}\beta + \epsilon_{it} \qquad (12)$$

where δ_t are the yearly DD coefficients, to examine parallel trends assumption and study the dynamics of the effect.

The DD identification relies on (i) the parallel trends assumption, i.e. that the areas would have developed similarly without the subsidy, (ii) that there are no other simultaneous policy changes between the areas and (iii) no spillovers between the areas because of the subsidy. Assumption (ii) holds as there are no simultaneous policy changes according to the first-employee subsidy area. Regarding assumption (iii), spillovers between areas are not likely because the subsidy only affects a small fraction of jobs in the labor market. Below I firstly discuss the parallel trends assumption (i) and then the no spillovers assumption (iii) in more detail.

As discussed in section 2.1 the subsidy was targeted mainly to disadvantaged areas. To improve the comparability between the subsidy and control areas, I restrict the analysis to the border of the subsidy area. The treatment and control areas are defined in Figure 1b. The treatment area includes the eligible municipalities that have a neighboring ineligible municipality, and the control area is the neighboring ineligible municipalities. While the economic conditions of firms can differ vastly by location, large differences between neighboring municipalities are not likely. In addition, the border municipalities exclude the economically weakest (East and North) and strongest (e.g. the capital region) areas. Spillover effects between eligible and ineligible neighboring municipalities do not cause a major concern for the identification strategy, because the subsidy only affects a small fraction of jobs in the labor market.

Table 5 presents descriptive statistics of firms and municipalities in 2006 in the subsidy, treatment, ineligible and control areas and, in the last column, all of Finland (including the capital region). The sample is restricted to firms that are non-employers in some year and have at most 50 employees, to exclude large firms irrelevant for the subsidy. Non-employer shares and number of employees are similar in all areas, but firms in the ineligible area have higher revenue, profits and assets on average. The differences are smaller in the border area (i.e., treatment and control). There are larger differences between the subsidy and ineligible areas in the municipal-level statistics but, again, the control and treatment areas are similar. Unemployment was 13% in the subsidy area compared to 8.4% in the no-subsidy area. Restriction to the border area decreases the

Table 5: Descriptive statistics of firms and municipality labor market by area in 2006

	Subsidy	Treatment	Ineligible	Control	All Finland
Non-employer	.74	.75	.76	.76	.75
	(.44)	(.43)	(.43)	(.43)	(.43)
Revenue	91,614	93,143	110,660	88,906	100,931
	(433,417)	(453,863)	(1,744,270)	(580,395)	(1,258,377)
Employees	.73	.68	.66	.65	.68
	(2.4)	(2.4)	(2.4)	(2.2)	(2.4)
Wage Costs	6,301	6,130	6,761	5,871	6,452
	(35,154)	(33,404)	(43,833)	(47,194)	(38,930)
Net Assets	38,773	31,438	57,449	35,242	48,292
	(770,773)	(249,488)	(1,470,378)	(463,025)	(1,151,172)
Profit	16,353	16,066	17,961	17,260	17,408
	(62,644)	(38,814)	(80,475)	(61,654)	(90,537)
Observations	51,119	18,875	69,951	20,901	143,340
Municipality level statistics:					
Employment share	.86	.86	.84	.85	.85
	(.032)	(.033)	(.046)	(.046)	(.041)
Unemployment rate	13	10	8.4	9	10
	(4.4)	(3.7)	(2.8)	(2.7)	(4.2)
Population	8,565	6,731	11,954	6,682	12,651
	(11,712)	(8,021)	(23,545)	(5,748)	(35,933)
Observations	195	87	216	84	415

Notes: Table presents descriptive statistics of firms and municipalities: mean and standard errors in parenthesis. The sample includes firms with at most 50 employees, non-zero revenue in 2006 and number of employees zero in some year between 2000 and 2013. Non-employer means firms with zero employees. Number of employees refers to all employees that worked in the firm during the tax year excluding the entrepreneur(s).

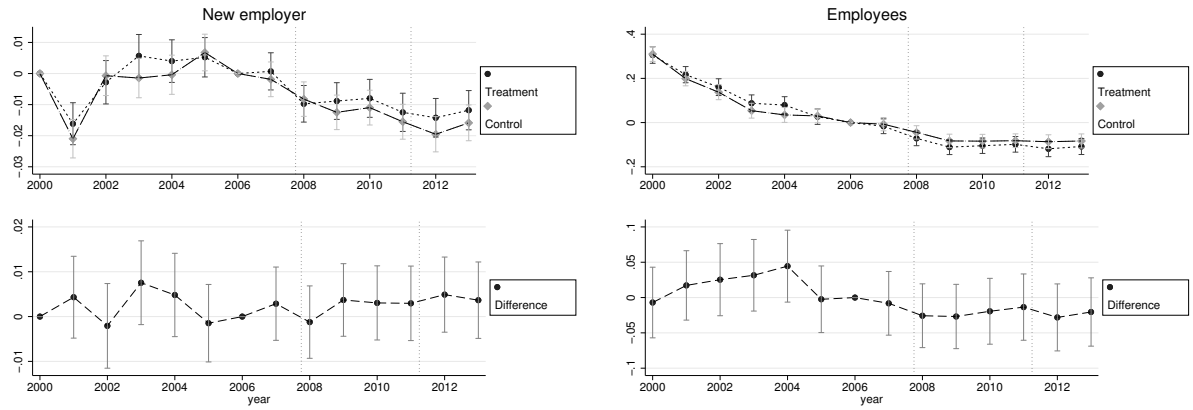


Figure 6: New employer share and firm employment trends in the treatment and control municipalities

Notes: The figures plot the estimated yearly coefficients λ_t and the 95% confidence intervals from equation 12 with k=2006 as the reference year, estimated separately for the treatment and the control areas in the top panels, and the coefficients δ_t in the lower panels that correspond to the annual difference-in-differences estimates. The specification includes firm fixed effects. The outcome variables are: dummy for being a new employer that equals one for firms that have positive employment and had zero employees in the previous year, and (number of) employees. The sample includes firms with at most 50 employees with a non-zero revenue that have zero number of employees in some year between 2000 and 2013. The treatment and control areas are as defined in Figure 1b. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighbor municipalities, to include only areas treated at the same time.

difference significantly: The unemployment rate was 10% in the treatment area and 9% in the the control area – both close to the national average of 10%. The treatment and control municipalities are also similar in size, while they are smaller than municipalities on average in Finland. Employment shares are similar in all areas.

Figures 6 and 7 depict firm trends in the treatment and control areas in the upper panel and the trend in the difference in the lower panel relative to year 2006 for the share of new employers, firm level employment, wage costs and (log of) revenue.[19] There are no significant differences in firm trends between the areas before the subsidy period. In addition, the decline in revenue during the financial crisis (in 2009) is similar in both areas. Appendix F includes placebo regressions corresponding to the estimation results below and firm trend figures using alternative size restrictions to further evaluate the parallel trends assumption. The placebo treatment effects are close to zero and statistically insignificant, and the firm trends are similar using different size thresholds.

In a similar manner, Figure 8 plots the trends in the local labor market trends in the treatment and control areas and their difference. There are no significant differences

[19]The sample uses the same selection criteria of firms with at most 50 employees. Hence, the selection can affected by the subsidy after the policy adoption. However, this is not a problem as I use a different sample for the actual estimation strategy. This sample is only used for descriptive statistics before the subsidy, trend comparisons between areas and robustness checks.

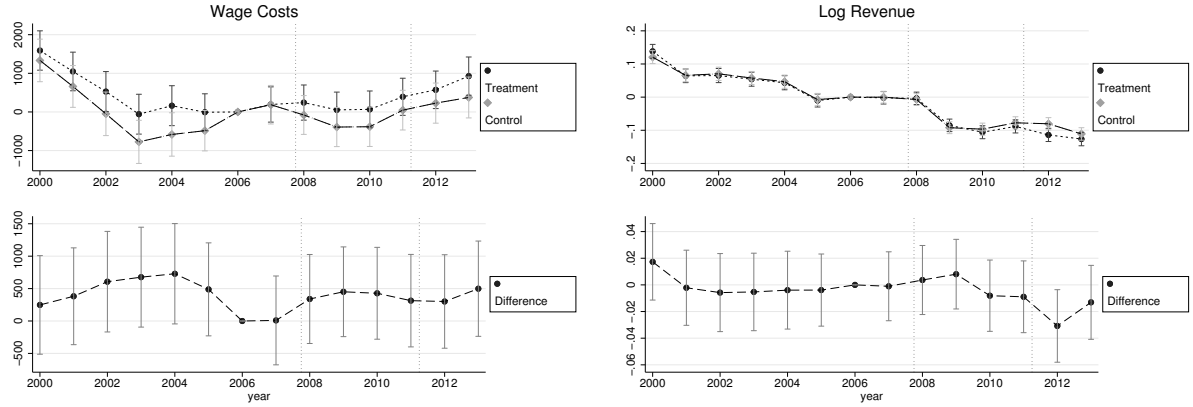


Figure 7: New employer share and firm employment trends in the treatment and control municipalities

Notes: The figures plot the estimated yearly coefficients λ_t and the 95% confidence intervals from equation 12 with k=2006 as the reference year, estimated separately for the treatment and the control areas in the top panels, and the coefficients δ_t in the lower panels that correspond to the annual difference-in-differences estimates. The specification includes firm fixed effects. The outcome variables are: wage costs, and log of revenue. The sample includes firms with at most 50 employees with non-zero revenue that have zero number of employees in some year between 2000 and 2013. The treatment and control areas are as defined in Figure 1b. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighboring municipalities, to include only areas treated at the same time.

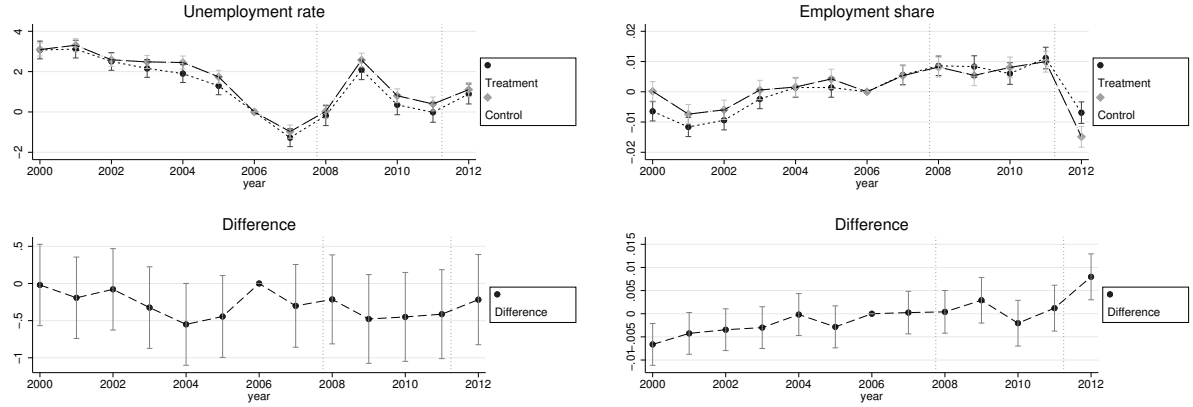


Figure 8: Labor market trends at the municipality level

Notes: The figures plot the estimated yearly coefficients λ_t and the 95% confidence intervals from equation 12 with k=2006 as the reference year, estimated separately for the treatment and the control areas in the top panels, and the coefficients δ_t in the lower panels that correspond to the annual difference-in-differences estimates. The specification includes firm fixed effects. The outcome variables are municipal employment rate and employment share. The treatment and control areas are as defined in Figure 1b. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighboring municipalities, to include only areas treated at the same time.

in the trends of unemployment but the employment percentage grows slightly faster in the treatment area from 2000 to 2004. To account for this, I control for municipal level employment share in the estimation below.

The assumption of no spillovers means that the subsidy does not affect outcomes in the control area. Although it is not likely that the subsidy affects the labor market in the control area, spillovers if some firms locate to the subsidy area from the control area to be eligible for the subsidy. However, this is not common in the data.[20] Moreover, the problem of movers is solved by fixing the treatment status of firms before the subsidy period. Consequently, moving to the subsidy area does not cause problems for the identification strategy.

5.2 Effect on Becoming an Employer

Here I study whether the subsidy increases the probability of becoming an employer or a full-time employer, as suggested by the model. I estimate the ITT effect on the targeted firms i.e. non-employer firms before the subsidy in year 2007.[21] For the pre-period needed in the DD strategy I use the four year period 2004–2007 preceding the subsidy. I use the sample of non-employer firms in 2003 for the reference period to have similar samples in the subsidy and reference periods i.e. the non-employer firm population in the preceding year. This sample selection includes the target population of the subsidy, and a similar population of firms in the reference period. Restricting the analysis to the non-employer population in 2007 would exclude new firms in the subsidy period, and they are a particularly important group for the policy. In addition, the firms in the subsidy period would be older by construction than in the pre-period and the sample size would decrease significantly. Appendix E includes results for the 2007 non-employer population and the results are similar. To fix the same treatment timing for all firms, I only include the municipalities where the subsidy came into effect in 2008. This accounts for the majority of the treated municipalities.[22]

Figure 9 depicts the cumulative probability of becoming an employer and a full-time employer during the four year treatment period 2008–2011 and during the preceding four year reference period 2004–2007 for the sample of non-employer firms in 2007 and 2003,

[20] Only 31 of the 1,349 subsidized firms moved to the subsidy area during the subsidy period, which is less than the number of subsidized firms (39) that moved to the subsidy area in the five years before the subsidy period. The number of firms that relocated from the ineligible area in 2006 to the subsidy area by 2009 is similar to the number of firms with an opposite relocation: 1,274 and compared to 1,269, or 541 compared to 514 in the border area.

[21] I exclude firms that become non-employers during the subsidy period, because this would introduce selection bias to the sample: as the subsidy increases the probability of becoming an employer, the non-employer population in the eligible area is affected by the subsidy but the non-employer population in the control area is not.

[22] The municipalities where the subsidy came into effect in 2007 are already excluded by the restriction to the border area. In addition, municipalities added in 2009 and 2010 only include small areas and may have been more likely selected due to local economic developments.

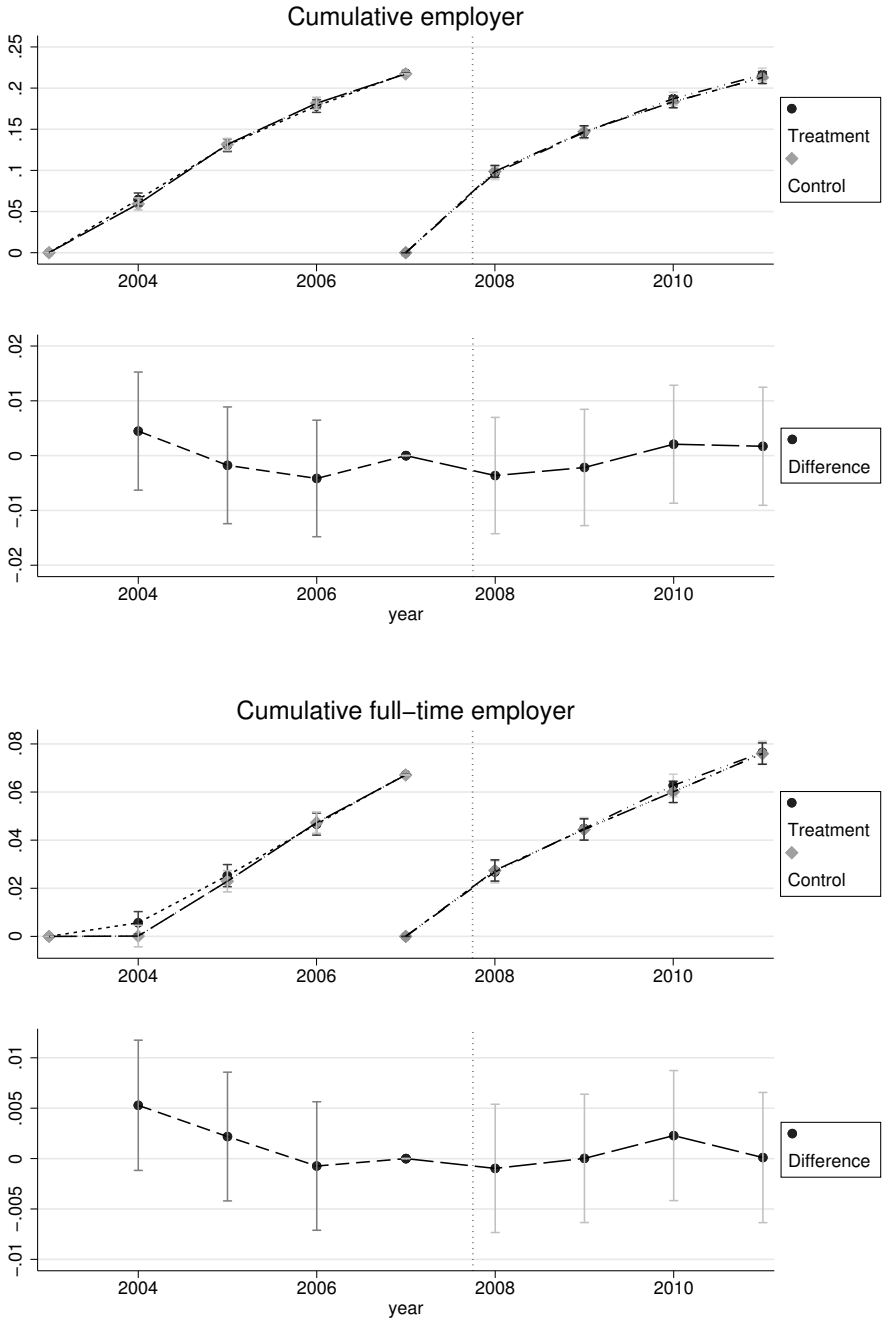


Figure 9: Effect on the cumulative probability of becoming an employer

Notes: The upper panel plots the cumulative probability of becoming a (full-time) employer for the sample of non-employer firms in 2003 in years 2004-2007 and for the sample of non-employer firms in 2007 for years 2008–2011. The plotted estimates come from a single regression including both samples in years 2004–2011 and excluding the sample selection year. The figure plots the yearly coefficient plus the constant $\lambda_t + c$ from and the 95% confidence intervals from equation 12 with k=2007 as the reference year, estimated separately for the treatment and the control areas in the top panels. The lower panel plots the annual DD coefficients δ_t from equation 12 relative to year 2007. The treatment and control areas are as defined in Figure 1b. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighboring municipalities, to include only areas treated at the same time.

respectively. The sample includes the balanced panel of non-employer firms in 2007 in the treatment period and non-employer firms in 2003 in the reference period.[23] The outcome variable is a dummy equal to 1 if the firm is an employer by the observation year and zero otherwise. The panel above depicts the rate of becoming an employer in the treatment and control areas and the panel below depicts the DD coefficients relative to year 2007.

The figure shows a similar hiring pattern in the treatment and control areas before the treatment period, validating the identification strategy. There is no change in the difference after the treatment with coefficients very close to zero.

Table 6 summarizes the regression results for the cumulative probability by the first and fourth (the last subsidy) year. None of the coefficients are statistically different from zero and the estimates are rather precise: effects larger than 10% of the baseline can be ruled out at the 95% confidence level. The coefficients for the first year are negative ranging from -0.5 to -0.3 percentage points depending on the model specification compared to a mean probability of 7.8 per cent. Effects larger than 0.6 to 1.2 ppt, i.e., close to 10% of the outcome mean can be ruled out at the 95% confidence level. The coefficients by the fourth year are also not statistically distinguishable from zero, but the coefficients from models without controls are positive. The largest estimate from model (1) is 0.3 ppt. The upper bound of the 95% confidence interval is 2 ppt relative to a mean probability of 20.4%.

Table 7 presents results on the probability of becoming a full-time employer. Again, none of the estimates are statistically significant at the 5 per cent level. The estimates are quite precise but because the outcome means are very low (1.2% by the first and 5.7% by the fourth year) the relative effects are not as precise. The coefficients by the first year are again negative and effects larger than 0.5 ppt or 38% of the outcome mean can be ruled out. By the fourth year the coefficients are positive in models (1) and (2) and negative in (3) and (4). The largest estimate is 0.2 ppt in model (1) with 1.2 ppt upper bound of the 95% confidence interval, which is 21% of the outcome mean.

The results are for firms that are active for the full observation period. Hence, the results may be biased if firms in the treatment area are less likely to exit because of the subsidy. Figure 12 and Tables 14 and 15 in the Appendix present the results estimated using the full non-employer sample that accounts for attrition. The outcome is zero if the firm i) has zero employees or ii) is not observed by the year. The results are similar. For robustness, Appendix E.1 uses a duration model approach to estimate the effect on becoming an employer. Using the duration model specification, I find small positive estimates but no statistically significant effect of the subsidy.

[23]Some firms may be in both samples, if they are non-employers in 2003 and 2007. The results for a filled unbalanced panel accounting for exits are included in the Appendix **??**.

Table 6: Estimated effect on the probability of becoming an employer

	(1) OLS	(2) OLS with covariates	(3) FE	(4) FE with covariates
By 1st year				
Treatment Effect	−0.00344	−0.00548	−0.00537	−0.00260
	(0.00537)	(0.00632)	(0.00595)	(0.00743)
Observations	38,778	38,778	38,778	38,778
Adjusted R^2	0.002	0.029	0.000	0.005
95% CI upper bound	0.00717	0.00699	0.00638	0.0121
Outcome mean				0.0782
By 4th year				
Treatment Effect	0.00361	−0.0111	0.0000556	−0.0101
	(0.00828)	(0.00943)	(0.00780)	(0.0106)
Controls	No	Yes	No	Yes
Observations	38,778	38,778	38,778	38,778
Adjusted R^2	0.005	0.062	0.007	0.014
95% CI upper bound	0.0200	0.00750	0.0155	0.0108
Outcome mean				0.204

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DD regression coefficients δ from equation 11 using 4 different models: (1) OLS with only the subsidy area and period dummies, (2) OLS with controls, (3) firm FE with subsidy area and period dummies controlling for firm fixed effects and (4) firm FE with controls. The dependent variable is a dummy for having a positive number of employees in some year by the first, and fourth year. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region fixed effects and region*subsidy period fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period. The table includes the upper bound of the 95% confidence interval for the estimates. The sample includes active (non-zero revenue) firms for the observation period excluding firms in agriculture that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period. The treatment period is 2008–2011 and the treatment area is defined as in 1b, but excluding municipalities that were added to the subsidy area in 2009 and 2010.

Table 7: Estimated effect on the probability of becoming a full-time employer

	(1) OLS	(2) OLS with covariates	(3) FE	(4) FE with covariates
Treatment Effect	−0.00346	−0.00407	−0.00152	−0.00306
	(0.00283)	(0.00335)	(0.00287)	(0.00397)
Observations	38,778	38,778	38,778	38,778
Adjusted R^2	0.000	0.021	0.003	0.019
95% CI upper bound	0.00213	0.00255	0.00415	0.00478
Outcome mean				0.0125
By 4th year				
Treatment Effect	0.00156	0.000864	−0.00184	−0.00187
	(0.00532)	(0.00629)	(0.00429)	(0.00535)
Controls	No	Yes	No	Yes
Observations	38,778	38,778	38,778	38,778
Adjusted R^2	0.000	0.050	0.015	0.023
95% CI upper bound	0.0121	0.0133	0.00663	0.00870
Outcome mean				0.0569

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DD regression coefficients δ from equation 11 using 4 different models: (1) OLS with only the subsidy area and period dummies, (2) OLS with controls, (3) firm FE with subsidy area and period dummies controlling for firm fixed effects and (4) firm FE with controls. The dependent variable is a dummy for being a full-time employer (i.e. wage costs above median wage cost per employee) in some year by the first, and fourth year. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region fixed effects and region*subsidy period fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period. The table includes the upper bound of the 95% confidence interval for the estimates. The sample includes active (non-zero revenue) firms for the observation period excluding firms in agriculture that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period. The treatment period is 2008–2011 and the treatment area is defined as in 1b, but excluding municipalities that were added to the subsidy area in 2009 and 2010.

5.3 Heterogeneity: Do takers respond more?

Despite the average zero effect, is there a significant effect on a smaller group of firms more likely to hire or use the subsidy? In particular, is the estimated effect larger for firm groups with higher take-up rates?

Here I present evidence on heterogeneous effects by estimating the effect separately for different groups of firms. I show results for different types of firms: partnerships and corporations, new firms, firms with higher subsidy availability, and firms with different predicted probability of becoming a full-time employer. Firms with higher subsidy availability include firms located ELY Centre municipality or in regions with above median eligible municipality share. Partnerships and corporations are more likely to become employers in the baseline, but they have lower take-up rates as do firms with higher probability of becoming a full-time employer. New firms are both more likely to become employers and take-up, and take-up rates are higher for higher subsidy availability and lower probability firms.

For all time-variant characteristics, I fix the population to firms that fulfill the criteria before the subsidy, e.g. new firms includes firms that were new in 2007 for years 2008–2011. For the predicted probability of becoming a full-time employer I use the same logit results as in section 4.3. I exclude firms in the first and tenth decile of the probability, because these groups have very different probabilities and take-up rates than the middle deciles.

Table 8 summarizes the results by subgroups. I only report the coefficients from the model without firm fixed effects corresponding to model (2) in Table 6 to have the same model for all groups, as the fixed effects specification cannot be estimated for new firms because there is only one observation per firm. In addition, the fixed effects model does not improve precision much and many firms are only observed once. The estimates are similar across models.

There are no statistically significant positive estimates on any firm group. Effects larger than 17% on partnerships and corporations to 25% on new firms can be ruled out by the 95% confidence intervals. Thus, economically meaningful effects on these firms cannot be ruled out despite no evidence in favor of a positive effect. The fourth year coefficient on the lower likelihood firms negative: -3.6 ppt and significant at the 1% significance level.

Table 16 in the Appendix includes additional firm groups: sole proprietors, firms with revenue above €40,000 and deciles 1-5 and 6-10 of the predicted probability of becoming a full-time employer. The coefficient on sole proprietors is negative and statistically significant at the 5 % significance level, but the upper bound on the confidence level is close to zero. Including the 1st decile into the lower probability group increases the estimate to -2.5 ppt and it is only significant at the 5% level. Other coefficients are not statistically significantly different from zero.

Table 8: Estimated effect on the probability of becoming an employer in different subsamples

	(1) All	(2) Partnership & Limited	(3) New	(4) Local subsidy availability	(5) Decile 2-5	(6) Decile 6-9
By 1st year						
Treatment Effect	−0.00548	0.00280	−0.0287	−0.00998	−0.00392	−0.00364
	(0.00632)	(0.0152)	(0.0158)	(0.00723)	(0.00712)	(0.0126)
Observations	38,778	12,693	9,786	22,540	14,665	11,810
Adjusted R^2	0.029	0.017	0.058	0.030	0.005	0.007
95% CI upper bound	0.00699	0.0329	0.00252	0.00436	0.0101	0.0212
Outcome mean	0.0782	0.124	0.0821	0.0802	0.0373	0.0898
By 4th year						
Treatment Effect	−0.0111	0.0142	0.0129	−0.0178	−0.0363**	0.00991
	(0.00943)	(0.0171)	(0.0202)	(0.0118)	(0.0130)	(0.0193)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	38,778	12,693	9,786	22,540	14,665	11,810
Adjusted R^2	0.062	0.028	0.093	0.061	0.019	0.020
95% CI upper bound	0.00750	0.0481	0.0528	0.00567	−0.0107	0.0480
Outcome mean	0.204	0.288	0.210	0.208	0.131	0.254

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DD regression coefficients δ from equation 11 estimated separately for subgroups of firms omitting the firm fixed effect. New firms include firms that are at most 3 years old in 2003 or 2007. Higher subsidy availability means firms located in i) ELY Centre municipalities or ii) regions with above median share of subsidy municipalities. Decile refers to the decile of predicted probability of becoming a full-time employer estimated as described in section 4. The dependent variable is a dummy for having a positive number of employees in some year by the first and fourth year. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region fixed effects and region*subsidy period fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period. The table includes the upper bound of the 95% confidence interval for the estimates. The sample includes active (non-zero revenue) firms for the observation period excluding firms in agriculture that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period. The treatment period is 2008–2011 and the treatment area is defined as in 1b, but excluding municipalities that were added to the subsidy area in 2009 and 2010.

5.4 Awareness and Bounds on ATT

Finally, this chapter derives awareness rates and ATTs, i.e. the treatment effect on the firms aware of the subsidy, based on the theoretical model and the estimates (see chapter 3). The assumptions used to derive the awareness rate in equation 10 are: i) compliance costs equal the full-time employer threshold, which is €12,000 here, and ii) awareness is not correlated with firm labor productivity for subsidy- and always-employers.

There are two potential violations to the assumptions that could bias the calculated awareness rate downwards. In this case, the estimated ATT is the upper bound because the calculated awareness rate is a lower bound. First, there may be heterogeneous compliance costs making it suboptimal for some full-time employers to use the subsidy. Consequently, in the group of full-time employers there could be firms aware of the subsidy that do not use it. Second, awareness could be correlated with the likelihood of becoming an employer. Negative correlation could arise, if firms that are close to the margin of hiring search of information on subsidies. Figure 4 suggests there may be negative correlation, because take-up rates of full-time employers are lower for firms with a higher likelihood of becoming full-time employers. In this case, the calculated awareness rate, that is the average of always- and subsidy-employers, is lower than the awareness rate of subsidy-employers, which should be used to calculate the ATT. However, awareness could also be positively correlated with labor productivity, which would bias the calculated awareness rate upwards.

The assumption (i) entails that only full-time employers use the subsidy. This is not, however true in the data and already suggests a violation to this assumption. Because of this I take the maximum of two candidates as the awareness rate: i) the awareness rate calculated using the take-up rate of full-time employers and the full-time share assuming only full-time employers surpass compliance costs, ii) the equivalent awareness rate assuming no compliance costs. i.e. using take-up rate of employers, employer share and ITT instead of full-time employer shares in formula 10. The logic is that there are non-taker employers that were aware of the subsidy but did not use it because of compliance costs. Hence, the awareness rate cannot be lower than what is calculated assuming no compliance costs. If the awareness rate calculated assuming compliance costs is higher, this is a better assessment of true awareness. A definite lower bound of the awareness rate is the population take-up rate.

Table 9 presents the estimated ITTs by the first year (as already reported in Table 8), take-up and employer rates, the calculated awareness rate and, finally, ATT. The employer share by the first year in all firms is 6.8% and 3.1% of the employers used the subsidy. With the -0.5 ppt ITT estimate this translates to a calculated awareness rate of 10% for employers, i.e., assuming no compliance costs. However, the take-up rate of full-time employers is larger at 6.1% with a 1.8% of firms being full-time employers. Given

Table 9: Bounds on awareness and ATT by firm groups

	(1) All	(2) Partnership & Corporation	(3) New	(4) Local subsidy availability	(5) Decile 2-5	(6) Decile 6-9
By 1st year						
ITT	−0.00548	0.00280	−0.0287	−0.00998	−0.00392	−0.00364
	(0.00632)	(0.0152)	(0.0158)	(0.00723)	(0.00712)	(0.0126)
Population take-up rate	0.00232	0.00313	0.00271	0.00363	0.00159	0.00253
Employer share	0.0684	0.120	0.0625	0.0705	0.0345	0.0786
Take-up of employers	0.0312	0.0255	0.0340	0.0456	0.0414	0.0291
Awareness rate, employers	0.103	0.00222	0.338	0.164	0.139	0.0721
	(0.0767)	(0.130)	(0.115)	(0.0751)	(0.160)	(0.142)
ITT, full-time	−0.00407	−0.00676	−0.00499	−0.00197	−0.00554	−0.00612
	(0.00335)	(0.00938)	(0.00778)	(0.00373)	(0.00230)	(0.00590)
Full-time share	0.0176	0.0443	0.0140	0.0186	0.00420	0.0166
Take-up of full-time empl.	0.0613	0.0398	0.0538	0.0948	0.194	0.0702
Awareness rate, full-time empl.	0.238	0.167	0.302	0.181	0.653	0.321
	(0.118)	(0.153)	(0.285)	(0.148)	(0.0781)	(0.176)
Awareness	0.238	0.167	0.338	0.181	0.653	0.321
	(0.118)	(0.153)	(0.115)	(0.148)	(0.0781)	(0.176)
ATT	−0.0231	0.0168	−0.0849	−0.0551	−0.00600	−0.0113
	(0.0290)	(0.0924)	(0.0549)	(0.0601)	(0.0109)	(0.0397)
95% CI upper bound	0.0337	0.198	0.0227	0.0627	0.0154	0.0664
Outcome mean	0.0932	0.128	0.118	0.0949	0.0415	0.107
ATT, full-time	−0.0171	−0.0405	−0.0148	−0.0109	−0.00849	−0.0191
	(0.0165)	(0.0673)	(0.0236)	(0.0224)	(0.00366)	(0.0212)
95% CI upper bound	0.0152	0.0914	0.0315	0.0330	−0.00131	0.0224
Outcome mean	0.0179	0.0351	0.0231	0.0178	0.00429	0.0208

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients δ or ITTs, take-up and employer shares in the treatment group, awareness rates and ATTs estimated separately for subgroups of firms by the first year. The formulas for calculating awareness under the assumption of flat awareness rate and the ATT are presented in section 3 and the formulas for calculating the variance of the estimates in Appendix B.2. Awareness of employers uses only the employer share and take-up of employers in the formula and corresponds to assuming there are no compliance costs. Awareness of full-time employers uses the ITT on full-time employership, full-time share and take-up of full-time employers and corresponds to assuming only full-time employer firms use the subsidy. Awareness rate is the maximum of take-up in the population, awareness of employers and awareness of full-time employers. ATTs are calculated using the awareness rate.

the ITT estimate of −0.4 ppt on full-time employer share, the calculated awareness rate of full-time employers is 24% with a standard error of 0.12. As the calculated awareness rate is higher for full-time employers, I use this to calculate the ATT. The standard error of awareness rate is large: it implies a 95% confidence interval from 0.7 to 47%.

The ATT on becoming an employer is −1.7 ppt with a 3.4 ppt upper bound of the 95% confidence interval and for becoming a full-time employer −1.7 with a 1.5 upper bound. Consequently, the results are consistent with economically significant effects on the firms aware of the subsidy because of the low estimated awareness rates: 36% on becoming an employer or 84% a full-time employer. However, the ATT estimates are imprecise because of large standard errors in the awareness rates.

The assumption of flat awareness rate may not hold in reality. I relax this assumption by estimating the awareness rate in groups of similar firms, where the assumption is more

realistic. The results for different groups of firms are in columns (2)-(6). There are large differences in the employer shares and take-up rates between groups that also result in large differences in the awareness rates. The calculated awareness rates of employers i.e. assuming no compliance costs range between 0.2% (partnerships and corporations) and 34% (new firms). The awareness rates of full-time employers range between 17% (partnerships and corporations) and 65% (lower probability firms).

Interestingly, the awareness rate is low for partnerships and corporations that have the largest ITT. This results in an ATT of 1.7 ppt on becoming an employer with a 19.8 ppt upper bound of the 95% confidence interval – over 100% of the outcome mean. Consequently, even a very large effect on the treated firms cannot be excluded given the results. For other groups the ATTs are more moderate as the awareness rates are higher or the ITT estimates smaller. For the lower probability group the ATT estimate is -0.6 ppt and rather precise with a 1.5 ppt upper bound of the confidence interval. For this group a large ATT can be ruled out, but even this is significant relative to the low outcome mean of 4.1%.

Because of low awareness and imprecisely estimated ATTs even economically large effects cannot be ruled out by the evidence with upper bounds of the confidence interval ranging from 36% to 155%. Likely deviations from the model assumptions suggest that the estimated ATT is the upper bound on the true ATT.

5.5 Additional Results and Robustness

I have conducted a variety of robustness analysis. First, I estimate the effect using the full eligible area and the ineligible area, excluding the capital region. Including the year*region fixed effects here means that the variation in the eligibility only comes from regions with both eligible and ineligible municipalities, therefore, excludes the Northern and Eastern Finland that are economically most disadvantaged. The results presented in Tables 22 and 23 in the Appendix are similar as in the main approach using only the border area.

To assess the parallel trends assumption, I conduct placebo regressions with 2006–2007 as the placebo period and non-employer firms in 2005 as the placebo target group. The results are included in the Appendix in Tables 18 and 19. Overall, there is no evidence of violating to the assumption. The exception is that the coefficient for new firms on becoming an employer by the first year is -2.8 pp and statistically significant at the 5% level. If the deviation in the parallel trend for new firms was -3 ppt, the treatment effect by the fourth year would be 3.3 ppt – with a standard error of 2 ppt this still would not be statistically significant.

I also estimate the effect on additional firm outcomes: the probability of being an employer, employment, wage costs and (log) revenue. These results depict how the subsidy affects the average employer share, employment, wage costs and revenue in the eligible

firms during the treatment period. The results are summarized in Tables 24 and 25 and in Figure 16 in the Appendix. Again, the estimates are mostly small and none of them are statistically significant at the 5% significance level. The estimated effect on average employer share is −0.6 ppt with a 13.2% baseline, and on wage costs 51.85, which is 3.2% of the baseline. The exception is that some of the estimates on new firms are large – 4 ppt on the probability of being an employer and 15% on log revenue – but with large standard errors. The estimates on employment and wage costs are negative.

To address whether there are effects on other subsidies, I regress the dummy for receiving business subsidies excluding the first-employee subsidy. The results are non-significant and they are summarized in Table 25. The estimate on new firms is, however, large but imprecise at 5.6 ppt which is more than the outcome mean. Consequently, it cannot be excluded that other subsidies - that can be substitutes or complementary for the first employee subsidy, for new firms in the eligible area increased simultaneously. More importantly, there is no decrease in receiving other subsidies that could bias the estimated effect downwards.

The subsidy can also affect firm entry and exit decisions. Therefore, I estimate the effect on firm entry and exit by aggregating the data into municipality*industry units following the approach in Bennmarker *et al.* (2009). This approach is based on the assumption that firm entry increases the total number of firms with reported positive revenue. There is no statistically significant effect on the number of firms, non-employer firms or employer firms. The results are summarized in Table 28 in the Appendix.

To include firms that may be eligible to the subsidy despite having positive employment in 2007,[24] I use a wider sample of firms and estimate the effect on multiple firm outcomes. I use a sample of firms that had zero employees in some year and at most 50 employees, i.e. the sample used for trend assessment in section 5.1. I run regressions with multiple firm-level outcomes: employer status, employment, wage costs and revenue. The results are summarized in Tables 26 and 27 in the Appendix. There is a statistically significant positive effect on being a full-time employer. The estimate is statistically significant for sole proprietors at 0.9 ppt, for revenue over €40K firms at 1.3 ppt and 0.6 ppt for lower predicted probability firms. In addition, the estimate on employment is statistically significant for the lower probability firms at 0.3 employees, and on wage costs for sole proprietors and lower probability firms at about €300. It should be noted, that the dummy for being a full-time employer is based on wage costs, consequently, they move in parallel. These results show that there may be a small positive effect on employment for firm groups that are more likely to use the subsidy given that they become employers. However, the estimation sample includes firms that may not even be eligible for the subsidy so the results should be interpreted with caution. Other coefficients are not statistically distinguishable from zero.

[24] See discussion in 4.2 and Appendix C

6 Conclusion

This paper studies the effects of a sizable subsidy for hiring the first employee. Overall, the results show very low take-up of the subsidy and, as a consequence, zero effects on hiring and other firm outcomes. A relative effect larger than 7% on the probability of becoming an employer by the first year can be ruled at a 95% confidence level.

I document a low 2% take-up of the subsidy among firms that became employers. While the take-up was reduced by restricting the subsidy to full-time employees, take-up increases only to 6% for new full-time employers. The overall conclusion is that a large majority of firms that become full-time employers seem to forgo substantial amounts of money when becoming an employer – €6,000 to €20,000, or close to one quarter of their wage costs – by not using the subsidy. While the low take-up for firms with low subsidy benefits is consistent with rational firm responses to compliance costs, the result of many firms leaving significant amount of money on the table is puzzling. Even if this seems to be explained by a lack of awareness, awareness is not exogenous and profit-maximizing firms should have an incentive to find out about relevant subsidies.

The low observed take-up has important implications for both the effectiveness and the efficiency of the subsidy. First, low awareness reduces the effectiveness of the subsidy. In the theoretical model of the paper, I derive how to calculate awareness rate based on observables using the rather strong assumptions that take-up is not correlated with labor productivity for firms that would become employers if they were aware of the subsidy and expected benefits of the subsidy surpass compliance costs for firms with wage costs above €12,000. Using the calculated awareness rate of 24% to scale the estimated effect on the targeted firms provides the estimated effect on the firms aware of the subsidy. The upper bound of the 95% confidence interval is 3.4 ppt, which is 37% of the baseline probability. For some firm groups, the upper bounds of the effect are much larger. Hence, the statistical precision does not permit detection of a substantial effect on the treated firms given the low awareness rate. Potential violations to the assumptions, in the light of the data, bias the calculated awareness rate downwards, implying that the estimated effect on the firms aware of the subsidy is an upper bound. While the calculations are based on strong assumptions, they can help to assess what the effect could be if all firms knew about the subsidy. In particular, the calculations illustrate that even an economically significant effect on the firms aware of the subsidy is consistent with the empirical results.

Second, the full-time restriction or compliance costs reduce the efficiency of the subsidy by reducing the treatment effect on the firms aware of the subsidy. This is because some of the potential subsidy-takers seem to have self-selected out of using the subsidy because of the full-time restriction or compliance costs. Only about 32% of non-employer firms that became employers during the subsidy period fulfilled the full-time restriction. However, the extent to which this reduces the effect cannot be evaluated using observable quantities.

There are four noteworthy points regarding the external validity of the results. First, a direct, salient incentive, e.g. reduction in payroll taxes, with lower compliance costs and higher awareness rate may have very different effect on the probability of becoming an employer than the subsidy studied here. Second, the subsidy was regional with decentralized implementation. This may have been a factor in explaining the low take-up and awareness of the subsidy. National coverage or central implementation may have had different effects on the subsidy take-up. Third, non-employer firms are a unique firm group with a significant share of solo self-employed persons who may prefer to simply employ themselves, and not expand their business. They may react differently to employment incentives than other types of firms. Lastly, the subsidy period coincided with the 2009 financial crisis, which was one of the largest economic depressions in Finland. Hence, the results reflect firm responses during a severe depression. However, it is not clear whether the economic situation might have increased or decreased the take-up and effectiveness of the subsidy.

My results have important implications for policy and research. Take-up of business subsidies, especially targeted at small firms, is a crucial factor in the effectiveness of subsidies. Policy design can matter for awareness and compliance costs and is therefore important. It is also important to distinguish between the reasons for low take-up as they have different implications on the interpretation for the ITT (intent to treat) effect: low awareness reduces only the ITT, but compliance costs reduce even the ATT, i.e., the effect on those who know about the subsidy. Awareness is particularly important if the subsidy has a fiscal motive, as it directly affects the extent of the fiscal stimulus. On the other hand, low take-up because of compliance costs could even reduce the efficiency of the subsidy, as it directs the subsidies to firms that are more likely to hire even without the subsidy.

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Appendix

A Employment protection

The Employment Contracts Act (55/2001) sets out minimum employment protection rules. In addition, there are industry-specific collective agreements that regulate employment contracts. In international comparison the Finnish employment protection regulations governing individual dismissals are modest: protection is more lenient than in other Nordic or some European countries but stricter than in e.g. UK and the US. The COED index (OECD, 2019) (ranging from 0 to 5 with 0 indicating low protection) for employment protection for individual dismissals was 2.17 in 2006. For example, Sweden has 2.61, and Norway 2.33, with other EU countries such as Germany (2.68) or France (2.47) even higher. However, the Finnish regulation is significantly stricter than in the UK (1.26) or US (0.26).

In general, the dismissal costs for new employees are lower than for older employees for two reasons: trial period and layoff costs that increase in tenure. First, during the trial period the employment contract can be terminated immediately for any personal reason that is not discriminatory. A trial period that is at most six months for permanent contracts, implying low dismissal costs. Second, tenure increases notice period, that can be considered as a proxy for direct financial costs of layoffs. This implies lower costs for shorter tenured workers. The notice period for terminating an employment contract is 14 days for contracts under one year, one month for contracts of one to four years, and gradually increasing up to six months after 12 years. Consequently, the direct firing costs in the first year amount to about 4% of yearly wage costs and 8% in the second year.

After the trial period, an employment contract can only be terminated for financial or production reasons or for a proper and weighty personal reason. First, dismissals for financial and productive reasons allow entrepreneurs to dismiss employees if the firm no longer has suitable work to offer the employees. In addition, temporary lay-offs are possible when the work available has decreased temporarily.

Second, possibility to terminate the employment contract for personal, employee specific reason may may be especially important for small businesses where the entrepreneur may work with one or two employees. This, however, requires that the worker has to be given notice and a chance to remedy the issue before termination of the contract. When evaluating what counts as a proper and weighty reason, the overall situation of the employer and employee are evaluated to account for situation-specific factors.[25]

[25] In 2019 a clause was added that the firm size needs to be taken into consideration.

B Theoretical Framework

B.1 Proofs

Derivation of $\bar{p_1}$ with compliance costs

The probability of becoming an employer at wage w is $p(w) = P(f'(0) \geq w) = 1 - P(w \leq f'_0) = 1 - D_0(w)$. From properties of $f_i(l)$ defined in Assumption 1, $f'_i \geq 0$ and $f''_i \leq 0$ we know that $f'_i(0) \geq f'_i(l)$ for all $l > 0$. Then, it follows that

$$P(f'_0 \geq w | f'_{\bar{l}} \geq w) = 1. \tag{13}$$

Define the joint cumulative density function $D_{f'_0 f'_{\bar{l}}}(f'_0, f'_{\bar{l}})$ of labor productivities evaluated at zero and the full-time threshold with marginal densities $D_{f'_0}(f'_0) = D_0(.)$ and $D_{f'_{\bar{l}}}(f'_{\bar{l}}) = D_{\bar{l}}(.)$, where the production functions $f_i()$ define the joint distribution.

Using the properties of probability and equation 13, the probability of becoming an employer can now be can be written using the the sum of probability of becoming an employer but not full time and becoming a full-time employer:

$$\begin{aligned}
p(w) &= P(\text{hire and full-time employer}) + P(\text{hire and not full-time employer}) \\
1 - D_0(w) &= P(f'_0 \geq w, f'_{\bar{l}} \geq w) + P(f'_0 \geq w, f'_{\bar{l}} < w) \\
1 - D_0(w) &= P(f'_0 \geq w | f'_{\bar{l}} \geq w) P(f'_{\bar{l}} \geq w) + P(f'_0 \geq w | f'_{\bar{l}} < w) P(f'_{\bar{l}} < w) \\
1 - D_0(w) &= P(f'_{\bar{l}} \geq w) + P(f'_0 \geq w | f'_{\bar{l}} < w) P(f'_{\bar{l}} < w) \\
1 - D_0(w) &= (1 - D_{\bar{l}}(w)) + (1 - D_{f'_o | f_{\bar{l}} < w}(w)) D_{\bar{l}}(w) \\
1 - D_0(w) &= 1 - D_{f'_o | f_{\bar{l}} < w}(w) D_{\bar{l}}(w)
\end{aligned}$$

where the fourth row follows from equation 13. After rearranging we get:

$$D_{f'_o | f_{\bar{l}} < w}(w) = \frac{D_0(w)}{D_{\bar{l}}(w)}. \tag{14}$$

Now, we can write the probability of becoming an employer with the subsidy with

compliance costs as

$$
\begin{aligned}
\bar{p_1} &= P(\text{hire and full-time employer}) + P(\text{hire and not full-time employer}) \\
&= P(f_0' \geq sw, f_{\bar{l}}' \geq sw) + P(f_0' \geq w, f_{\bar{l}}' < sw) \\
&= P(f_{\bar{l}}' \geq sw) + P(f_0' \geq w | f_{\bar{l}}' < sw) P(f_{\bar{l}}' < sw) \\
&= 1 - D_{\bar{l}}(sw) + (1 - D_{f_o'|f_{\bar{l}}<sw}(w)) D_{\bar{l}}(sw) \\
&= 1 - D_{f_o'|f_{\bar{l}}<sw}(w) D_{\bar{l}}(sw) \\
&= 1 - D_{\bar{l}}(sw)(D_{f_o'|f_{\bar{l}}<sw}(sw) + \int_{sw}^{w} d_{f_o'|f_{\bar{l}}<sw}(u)du) \\
&= 1 - D_{\bar{l}}(sw)(\frac{D_0(sw)}{D_{\bar{l}}(sw)} + \int_{sw}^{w} d_{f_o'|f_{\bar{l}}<sw}(u)du) \\
&= 1 - D_0(sw) - D_{\bar{l}}(sw) \int_{sw}^{w} d_{f_o'|f_{\bar{l}}<sw}(u)du. \\
\bar{p_1} &= p_1 - (1-b) \int_{sw}^{w} d_{f_o'|f_{\bar{l}}<sw}(u)du
\end{aligned}
$$

where third row uses equation 13 and definition of conditional probability, seventh row uses equation 14, $b = E(b_i) = 1 - D_{\bar{l}}(sw)$ i.e. the fraction of firms that satisfy the full-time requirement under the subsidy, and $p_1 = 1 - D_0(sw)$ is equal to the probability of becoming an employer with the subsidy without compliance costs.

Proof that $\tilde{\delta_f} \geq \overline{\delta}$

Rewrite equation 3:

$$
\begin{aligned}
\tilde{\delta} =& D_0(w) - D_0(sw) = \int_{sw}^{w} d_{f_o'}(u)du \\
\tilde{\delta} =& \int_{sw}^{w} d_{f_o'|f_{\bar{l}}'<sw}(u)du * D_{\bar{l}}(sw) + \int_{sw}^{w} d_{f_o'|sw \leq f_{\bar{l}}'<w}(u)du * (D_{\bar{l}}(w) - D_{\bar{l}}(sw)) \\
&+ \int_{sw}^{w} d_{f_o'|f_{\bar{l}}'>w}(u)du * (1 - D_{\bar{l}}(w)) \\
\tilde{\delta} =& \int_{sw}^{w} d_{f_o'|f_{\bar{l}}'<sw}(u)du * D_{\bar{l}}(sw) + \int_{sw}^{w} d_{f_o'|sw \leq f_{\bar{l}}'<w}(u)du * (D_{\bar{l}}(w) - D_{\bar{l}}(sw))
\end{aligned}
$$

where the last line follows because $\int_{sw}^{w} d_{f_o'|f_{\bar{l}}'>w}(u)du * (1 - D_{\bar{l}}(w)) = 0$ for $f_0' \geq f_{\bar{l}}'$. Then we can rewrite equation 5

$$
\begin{aligned}
\overline{\delta} =& \tilde{\delta} - D_{\bar{l}}(sw) \int_{sw}^{w} d_{f'_o|f_{\bar{l}}<sw}(u)du \\
\overline{\delta} =& \int_{sw}^{w} d_{f'_o|f'_{\bar{l}}<sw}(u)du * D_{\bar{l}}(sw) + \int_{sw}^{w} d_{f'_o|sw\leq f'_{\bar{l}}<w}(u)du * (D_{\bar{l}}(w) - D_{\bar{l}}(sw)) \\
& - D_{\bar{l}}(sw) \int_{sw}^{w} d_{f'_o|f_{\bar{l}}<sw}(u)du \\
\overline{\delta} =& \int_{sw}^{w} d_{f'_o|sw\leq f'_{\bar{l}}<w}(u)du * (D_{\bar{l}}(w) - D_{\bar{l}}(sw)).
\end{aligned}
$$

Now the inequality follows:

$$
\begin{aligned}
D_{\bar{l}}(w) - D_{\bar{l}}(sw) &\geq \int_{sw}^{w} d_{f'_o|sw\leq f'_{\bar{l}}<w}(u)du * (D_{\bar{l}}(w) - D_{\bar{l}}(sw)) \\
\tilde{\delta_f} &\geq \overline{\delta}
\end{aligned}
$$

because $\int_{sw}^{w} d_{f'_o|sw\leq f'_{\bar{l}}<w}(u)du \in [0,1]$.

B.2 Deriving Variance of Awareness Rate and ATT

Awareness $a_i \in \{1,0\}$ is independent of f'_i by assumption. Equation 10 defines how awareness can be calculated using estimable quantities. Hence, the estimate for awareness $\hat{a}$ is a function g of $\hat{\hat{a}}$, $\hat{\hat{b_1}}$ and $I\hat{T}T_f$: $\hat{a} = g(\hat{\hat{a}}, \hat{\hat{b_1}}, I\hat{T}T_f) = \frac{\hat{\hat{a}}\hat{\hat{b_1}} - I\hat{T}T_f}{\hat{\hat{b_1}} - I\hat{T}T_f}$. Now, the variance of $\hat{a}$ can be calculated using the delta method:

$$
\mathrm{Var}(\hat{a}) = \nabla g^{\top} \sum \nabla g \tag{15}
$$

where ∇g is the gradient of g and $\sum$ is the covariance matrix of $\hat{\hat{a}}$, $\hat{\hat{b_1}}$ and $I\hat{T}T_f$. The gradient evaluated at the estimates is:

$$
\nabla g^{\top} = (1 + \frac{I\hat{T}T_f}{\hat{\hat{b_1}} - I\hat{T}T_f}, -\frac{(\hat{\hat{a}} - 1)I\hat{T}T_f}{(\hat{\hat{b_1}} - I\hat{T}T_f)^2}, \frac{(\hat{\hat{a}} - 1)\hat{\hat{b_1}}}{(\hat{\hat{b_1}} - I\hat{T}T_f)^2})
$$

The sample variances of $\hat{\hat{a}}$, $\hat{\hat{b_1}}$ and $I\hat{T}T_f$ can be calculated from the data. The covariances can be derived using the model assumptions.

$$
\begin{aligned}
\text{Cov}(\hat{\bar{a}}, \hat{\bar{b}_1}) &= \frac{1}{N}\text{Cov}(\bar{a}, \bar{b}_1) = \frac{1}{N}[\text{E}(\frac{ab_1}{b_0 + ITT_f}(b_0 + ITT)) - \text{E}(\frac{ab_1}{b_0 + ITT_f})\text{E}(b_0 + ITT_f] \\
&= \frac{1}{N}[\text{E}(ab_1) - \text{E}(\frac{ab_1}{b_0 + ITT_f})\text{E}(\text{E}(b_0 + ITT_f | b_0 + ITT_f))] \\
&= \frac{1}{N}[\text{E}(ab_1) - \text{E}(\frac{ab_1}{b_0 + ITT_f} * \text{E}(b_0 + ITT_f | b_0 + ITT_f))] \\
&= \frac{1}{N}[\text{E}(ab_1) - \text{E}(\frac{ab_1}{b_0 + ITT_f} * (b_0 + ITT_f))] \\
&= 0
\end{aligned}
$$

where the first row uses the definitions of covariance and $\bar{a}$ and $\bar{b}_1$, second row LIE and third row properties of expectation.

$$
\begin{aligned}
\text{Cov}(\hat{\bar{a}}, \hat{ITT_f}) &= \frac{1}{N}\text{Cov}(\bar{a}, ITT_f) = \frac{1}{N}[\text{E}(\bar{a}ITT_f) - \text{E}(\bar{a})\text{E}(ITT_f)] \\
&= \frac{1}{N}[\text{E}(\bar{a}a\delta) - \text{E}(\bar{a})\text{E}(ITT_f)] = \frac{1}{N}[\text{E}(a\delta) - \text{E}(\bar{a})\text{E}(ITT_f)] \\
&= \frac{1}{N}[\text{E}(ITT_f) - \text{E}(\bar{a})\text{E}(ITT_f)] = \frac{1}{N}\text{E}(ITT_f)(1 - \text{E}(\bar{a}))
\end{aligned}
$$

where the second row equality follows because $\overline{a} = 1$ if $a\delta = 1$.

$$
\begin{aligned}
\text{Cov}(\hat{\bar{b}_1}, \hat{ITT_f}) &= \frac{1}{N}\text{Cov}(\bar{b}_1, ITT_f) = \frac{1}{N}[\text{E}((b_0 + a\delta)a\delta) - \text{E}(\bar{b}_1)\text{E}(ITT_f)] \\
&= \frac{1}{N}[\text{E}(b_0 a\delta) + \text{E}(a\delta * a\delta) - \text{E}(\bar{b}_1)\text{E}(ITT_f)] \\
&= \frac{1}{N}[\text{E}(a\delta) - \text{E}(\bar{b}_1)\text{E}(ITT_f)] = \frac{1}{N}\text{E}(ITT_f)(1 - \text{E}(\bar{b}_1))
\end{aligned}
$$

where the last row follows because if $b_0 = 1$ $\delta = 0$ and vice versa, and $a\delta = \{0, 1\}$.

Again, the delta method can be used for calculating the variance of the estimate for ATT as $\text{ATT} = \frac{\hat{\text{ITT}}}{\hat{a}} = g(\hat{\text{ITT}}, \hat{a})$ and $\nabla g^\top = (1, -\frac{\hat{\text{ITT}}}{\hat{a}^2})$. Here, covariance between a and ITT can be derived using the assumptions of the model as follows:

$$
\begin{aligned}
\text{Cov}(\hat{a}, \hat{ITT}) &= \frac{1}{N}\text{Cov}(a, a\tilde{\delta}) = \text{E}(a^2\tilde{\delta}) - \text{E}(a)\text{E}(a\tilde{\delta}) \\
&= \frac{1}{N}\text{E}(a\tilde{\delta}) - \text{E}(a)\text{E}(\text{ITT}) \\
&= \frac{1}{N}\text{E}(\text{ITT})(1 - (\text{E}(a))
\end{aligned}
$$

where the first line is just definition of covariance, second uses the fact that $a^2 = a$ as a

is binary, and $E(ITT) = E(a\tilde{\delta})$. The variance of ATT is:

$$\text{Var}(\tilde{\delta}) = \text{Var}(\frac{\hat{\text{ITT}}}{\hat{a}}) = \frac{\text{Var}(\hat{\text{ITT}})}{\hat{a}^2} - \frac{2 * \text{E(ITT)Cov}(\hat{a}, \hat{\text{ITT}})}{\hat{a}^3} + \frac{\text{E(ITT)}^2\text{Var}(\hat{a})}{\hat{a}^4}.$$

C Identifying Eligible Firms and Calculated Subsidy in Data

There are problems in defining the eligibility for the subsidy in the data with respect to i) defining the target group (i.e. those that would get the subsidy if they hired for a full-time contract) and ii) defining the eligible based on their labor choice (i.e. those that should get the subsidy if they applied for it). Here I discuss the issues they raise and whether the results are robust to different specifications. I also relate the defined eligibility to observed eligibility, i.e., how many of the subsidized firms fulfill the different criteria.

First, the target group is firms with no employees for at least 12 months. Using the yearly data does not allow to detect non-employer spells from e.g. June 2007 to June 2008 even though the firm would be eligible for the subsidy. This could drop out firms that are non-employers for a shorter duration and, hence, more likely to become employers again and but still eligible. It is also possible that the "no employees" rule may be interpreted as no full-time employees. Additionally for identifying the target group, there could be measurement error in firm location: the home municipality in the tax returns may not be the updated location. As there are only 23 subsidized firms outside of the subsidy region, this is not likely a major concern.

Second, to qualify for the subsidy the firm had to hire on a permanent employment contract with at least 25 hours per week but I do not observe the employment contract in the data – only number of employees and total wage costs. I use wage costs as proxy for working hours as the total wage costs are wh. Of course, wages differ between firms and employees but with no additional information this is the only proxy available. However, it could be that some of the firms that do not use the subsidy do not qualify for the 25 hour contract but have relatively high wage costs because of higher wage level. These firms would not be eligible. Without information on hours, I cannot evaluate this problem.

To assess the 12 month non-employer rule, I use monthly (and quarterly) data on payroll tax reports that allows to detect 12 month long non-employer spells. The problem with payroll tax data is that the wage costs also include wage costs for a subset of the owners that are subject to the employee payroll tax regulation. Hence, the payroll tax data excludes some eligible firms if their wage costs are to the owner. I also use 12 month long cumulative wage costs to assess becoming a "full-time" employer as measured by total wage costs. This accounts for having low yearly wage costs because of hiring the employee late in a year.

	Yearly data					Monthly data					Yearly or monthly				
	Target			Takers		Target			Takers		Target			Takers	
	% of firms	N of firms	Take-up rate	% of firms	N of firms	% of firms	N of firms	Take-up rate	% of firms	N of firms	% of firms	N of firms	Take-up rate	% of firms	N of firms
Non-employer:															
(1) Wage cost=0	0.73	101,792	0.004	0.72	723	0.24	33,349	0.013	0.73	737	0.79	11,0133	0.005	0.87	876
(2) Wage cost<12,000	0.47	64,715	0.003	0.59	592	0.52	72,700	0.003	0.43	432	0.55	76,332	0.003	0.62	623
(3) Wage cost<18,000	0.55	75,934	0.003	0.63	640	0.54	74,437	0.003	0.49	491	0.57	78,577	0.003	0.64	650
(4) Wage cost<6,000	0.50	69,583	0.003	0.55	559	0.51	71,038	0.002	0.35	354	0.54	74,867	0.003	0.59	596
(5) Wage cost per employee<12,000	0.60	83,587	0.003	0.65	656	0.44	61,682	0.001	0.22	223	0.61	85,338	0.003	0.66	672
Employer:															
(6) Wage cost>0	0.32	45,075	0.011	0.94	949	0.28	38,302	0.008	0.66	663	0.36	49,501	0.010	0.96	969
(7) Wage cost>12,000	0.24	32,638	0.008	0.48	489	0.25	34,784	0.009	0.68	687	0.28	38,894	0.010	0.76	768
(8) Wage cost>18,000	0.21	29,053	0.006	0.32	325	0.23	32,228	0.009	0.59	596	0.25	35,229	0.009	0.63	638
(9) Wage cost>6,000	0.27	37,102	0.010	0.72	723	0.27	37,488	0.009	0.75	759	0.31	43,225	0.011	0.88	891
(10) Wage cost per employee>12,000	0.15	20,382	0.007	0.26	261	0.38	52,445	0.006	0.60	604	0.39	54,556	0.006	0.66	667
Eligible:															
(1) + (6)	0.09	12,777	0.032	0.66	667	0.05	7,353	0.032	0.44	442	0.16	22,654	0.021	0.82	834
(1) + (7)	0.05	6,670	0.030	0.31	314	0.05	6,635	0.037	0.47	478	0.11	14,866	0.024	0.64	648
(1) + (8)	0.04	5,432	0.024	0.20	199	0.04	5,591	0.037	0.40	403	0.09	12,406	0.023	0.52	530
(1) + (9)	0.06	8,603	0.035	0.47	476	0.06	7,981	0.034	0.54	542	0.13	18,055	0.024	0.75	759
(1) + (10)	0.02	3,388	0.032	0.18	181	0.17	24,031	0.009	0.41	412	0.23	31,372	0.010	0.55	560
(2) + (7)	0.02	2,664	0.028	0.24	244	0.01	1,377	0.026	0.18	182	0.06	7,920	0.015	0.43	437
(2) + (8)	0.01	1,532	0.025	0.15	149	0.01	881	0.027	0.13	135	0.04	5,857	0.013	0.34	347
(3) + (7)	0.01	1,739	0.040	0.22	222	0.01	929	0.025	0.13	130	0.05	7,128	0.016	0.41	415
(3) + (8)	0.01	1,069	0.033	0.13	135	0.00	673	0.024	0.10	99	0.04	5,335	0.014	0.32	328
(3) + (9)	0.02	3,137	0.040	0.36	368	0.01	1,361	0.018	0.15	153	0.07	9,527	0.017	0.50	503
(5) + (10)	0.03	4,542	0.011	0.15	156	0.03	3,546	0.002	0.06	57	0.19	26,490	0.004	0.41	411

Table 10: Observed eligibility criteria and subsidy take-up in data

In table 10, I present how defining eligibility, both the non-employer and becoming a full-time employer, in different ways affects how many eligible firms there are in the target group (i.e. in the treatment area), what is the take-up rate and how many of the subsidy-takers fulfill the observed criteria. I include all firms in the treatment area when they are first observed during the treatment period, to not count the same firm many times. I use 5 definitions for being an employer: i) employer = positive wage costs, ii) full-time effective employer = wage costs above median wage cost per employee (€12,000), iii) 1.5 full-time effective = wage costs >18,000, iv) 0.5 full-time effective = wage costs >6,000 and v) full-time employee with wage costs per employee above 12,000. Target group is evaluated as being a non-employer in the previous year (employer=0) according to each of these definitions. I evaluate all these definitions using i) yearly data, ii) monthly data and iii) whether a firms fulfills the criteria according to either yearly or monthly criteria. The firm is defined as a previous year non-employer according to the monthly data if for any month of the year the cumulative wage sum from the previous 12 months is zero. For the full-time effective measures monthly criteria refers to cumulative sum of wage costs over 12 months from any month in the year. The full-time employee is monthly: if the firm has wage cost per employee over €1,000 in any month it is defined as being a full-time employer (in that month).

In summary, table 10 shows there are large differences in how many eligible firms there are according to the different criteria and how many of the subsidized firms fulfill the criteria. However, take-up rates do not differ vastly according to this criteria. Moreover, there is no single eligibility criteria that has both i) large share of subsidized firms and ii) high take-up rate. Hence, the take-up analysis is not sensitive to defining the eligibility criteria in data.

There are 12,777 eligible firms in the yearly data using the most general definition (1+6) and 7,353 in the payroll tax data, and 22,654 using either data for defining employer status. From the subsidized firms 66% are eligible according to yearly data, 44% according to payroll tax data and 82% according to one of these with take-up rates of 3%, 3% and 2% respectively.

Defining the calculated subsidy suffers from the same problems. Especially, the yearly data does not allow for precisely calculating the one year wage costs of the first employee. Hence, the measure supposes equal distribution of the first employee wage costs in the first and second year. Using the monthly payroll data removes this aggregation problem. Table 11 summarizes the paid subsidy, the calculated subsidy using the yearly and monthly data, and their difference and correlation for the subsidized firms that have both measures available. Both calculated subsidies are smaller than paid subsidy, yearly by €1,500 and monthly by €1,000. Hence the differences between the measures are quite small. The yearly calculated subsidy has 0.55 correlation with the paid subsidy while the monthly measure has 0.39. Hence, I use the measure based on yearly wage costs in the paper as it

	mean	sd
Paid subsidy	8,756.036	3,989.412
Calculated subsidy	6,873.449	4,390.388
Paid subsidy - calc. subsidy, payroll	1,493.018	3,945.724
Observations	526	
Correlation	0.549	
Calculated subsidy, payroll	7,803.749	6,582.398
Paid subsidy - calc. subsidy	952.2871	6,207.918
Observations	526	
Correlation	0.394	

Table 11: Paid subsidy and calculated subsidy based on yearly and monthly wage costs

Table 12: Subsidized vs not subsidized full-time employers

	(1) Subsidy takers	(2) Non-takers
Paid subsidy	8,940	.
	(3,373)	(.)
Calculated subsidy	8,792	8,328
	(4,021)	(6,059)
Employees	2.2	2.3
	(2.7)	(6.3)
Wage costs	25,230	34,703
	(17,945)	(216,003)
Observations	145	2,441

mean coefficients; sd in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Sample includes firms in the subsidy area during the subsidy period that had zero employees in the previous year and at least one effective employee, i.e. total wage costs above the median wage cost per employee. Calculated subsidy is defined as $0.3lc_t + 0.225lc_{t+1} + 0.075lc_{t+2}$ using the actualized wage costs per employee lc_t to proxy the wage costs of the first employee and the subsidy rule assuming that the first employee was hired in the middle of the first year. The means are calculated using only observations for which the variable is observed. For example, the subsidy paid is missing for the not subsidized firms.

has higher correlation and is observed for a larger number of firms.

D Predicted probability of becoming a full-time employer

I predict the probability of becoming an employer using a logistic regression with being a full-time employer as an outcome and data from 2006. The sample includes firms that were non-employer in 2005 in both treatment and control areas. The results are reported in table 13. The model one is chosen according to the smallest value of AIC.

Figure 10 plots the mean of becoming a full-time employer and the mean of the predicted probability by decile of the predicted probability out of prediction sample for target firms in the treatment area in 2008. For the 1-8 deciles the predictions and true probabilities are very similar and close to zero, but there is about 7 ppt difference in the 10th decile with a prediction of about 20% and true probability close to 28%. The predictions, thus, perform well except for the largest probabilities that are understated.

	(1)	(2)	(3)	(4)
Partnership	2.001***	2.010***	1.978***	1.999***
	(0.188)	(0.189)	(0.185)	(0.188)
Corporation	4.572***	4.545***	4.479***	4.550***
	(0.241)	(0.240)	(0.236)	(0.240)
Construction	1.299	1.249	1.291	1.283
	(0.384)	(0.369)	(0.378)	(0.379)
Wholesale and Retail Trade	0.787	0.761	0.779	0.775
	(0.233)	(0.225)	(0.229)	(0.229)
Transportation and storage	1.667	1.621	1.645	1.648
	(0.497)	(0.482)	(0.486)	(0.491)
Health and social services	0.857	0.820	0.847	0.841
	(0.261)	(0.250)	(0.256)	(0.257)
New	2.749***	2.731***	2.770***	2.748***
	(0.189)	(0.188)	(0.190)	(0.189)
Entry firm	31.94***	32.05***	32.26***	31.94***
	(5.794)	(5.818)	(5.851)	(5.794)
Previous Employer	2.431***	2.420***	2.445***	2.429***
	(0.147)	(0.147)	(0.148)	(0.147)
Revenue_25p	2.423***	2.434***	2.424***	2.423***
	(0.412)	(0.414)	(0.412)	(0.412)
Revenue_50p	7.280***	7.295***	7.324***	7.277***
	(1.208)	(1.211)	(1.215)	(1.207)
Revenue_75p	12.99***	13.03***	12.94***	12.98***
	(2.246)	(2.255)	(2.238)	(2.244)
Net asset quartile	Yes	Yes	Yes	Yes
Region FE	Yes	No	No	Yes
Subregion FE	No	Yes	No	No
Municipality statistics	No	No	Yes	Yes
N	120,302	120,302	120,302	120,302
pseudo R^2	0.250	0.251	0.246	0.250
AIC	24,775.3	24,822.3	24,872.1	24,779.4

Exponentiated coefficients; Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 13: Logistic regression results on becoming a full-time employer

Notes: Sample includes firms in 2006 that had zero employees in the previous year or are observed for the first time. Municipality statistics include: employment share, unemployment share and population. Only some of the key industry coefficients are reported on the table.

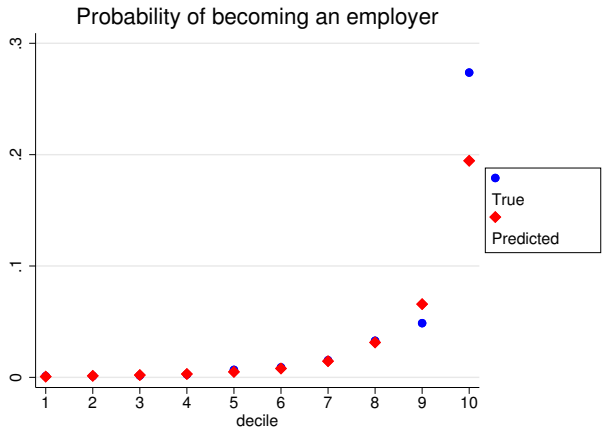

Figure 10: Predicted vs true probability of becoming a full-time employer

Notes: Sample includes firms in the treatment area in 2008 that had zero employees in the previous year or are observed for the first time. X axis is decile of predicted probability of becoming a full-time employer using the logistic regression results from model (1) in table 13. True probability is the mean of the outcome and predicted is the mean of the prediction.

E Robustness of main results

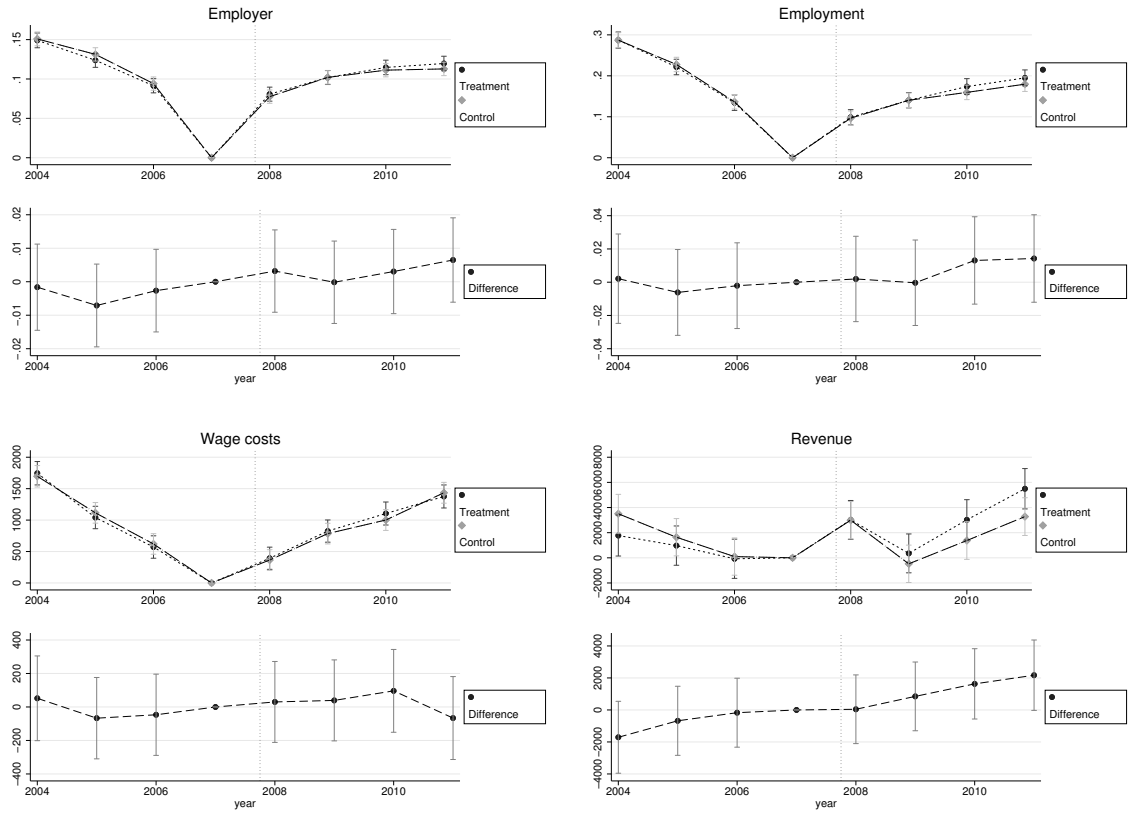

Figure 11: DD figures using the 2007 non-employer population

Notes: The upper panel plots the outcomes relative to year 2007 for the balanced panel of the population of non-employer firms in 2007. The figure plots the yearly coefficient plus the constant $\lambda_t + c$ from and the 95% confidence intervals from equation 12 with k=2007 as the reference year, estimated separately for the treatment and the control areas in the top panels. The lower panel plots the annual DD coefficients δ_t from equation 12 relative to year 2007. The treatment and control areas are as defined in Figure 1b. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighboring municipalities, to include only areas treated at the same time.

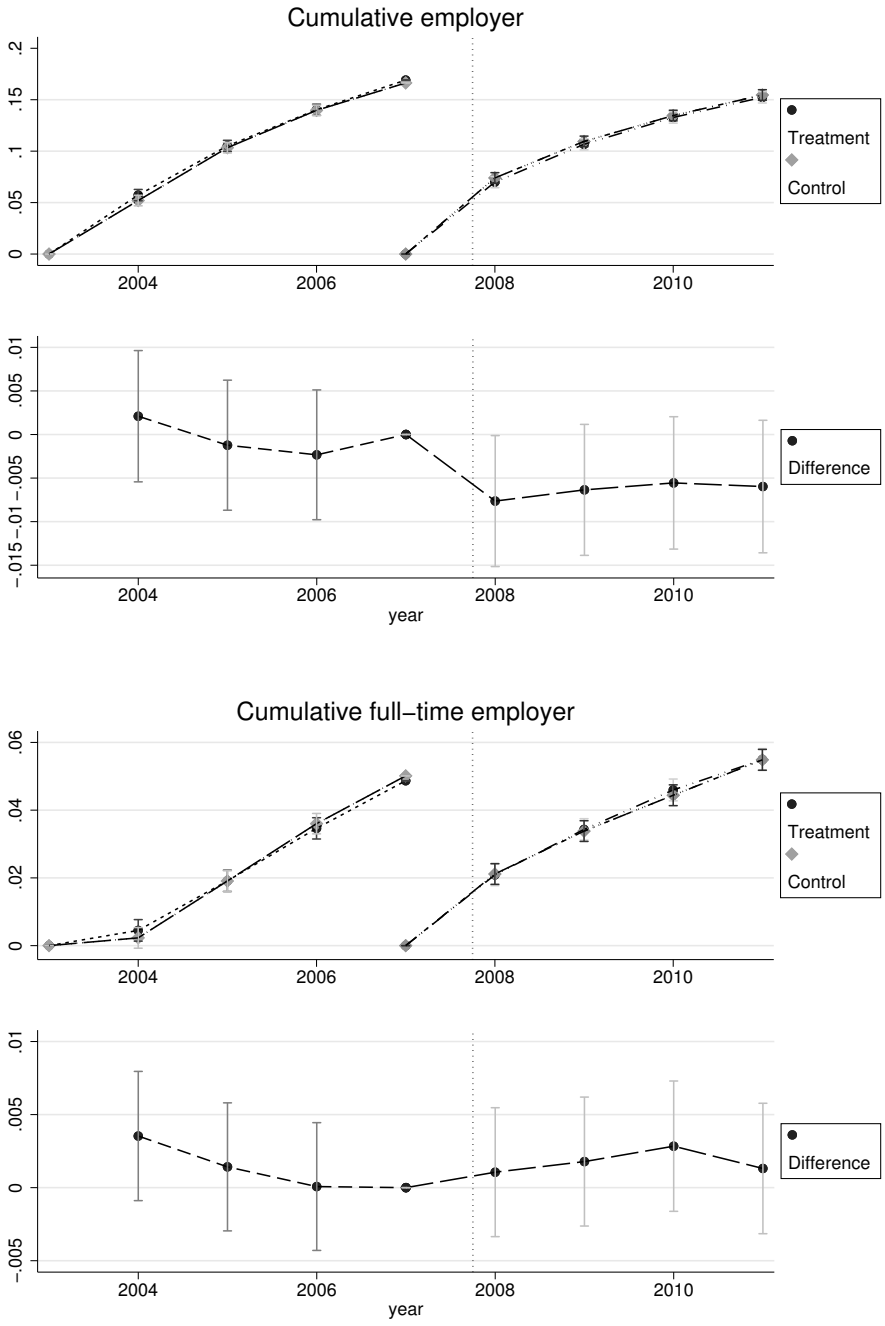

Figure 12: Effect on the cumulative probability of becoming an employer allowing for exits

Notes: The upper panel plots the cumulative probability of becoming an employer for the sample of non-employer firms in 2003 in years 2004-2007 and for the sample of non-employer firms in 2007 for years 2008–2011. The dummy is equal to zero in years the firm is not observed. The plotted estimates come from a single regression including both samples in years 2004–2011 and excluding the sample selection year. The figure plots the yearly coefficient plus the constant $\lambda_t + c$ from and the 95% confidence intervals from equation 12 with k=2007 as the reference year, estimated separately for the treatment and the control areas in the top panels. The lower panel plots the annual DD coefficients δ_t from equation 12 relative to year 2007. The treatment and control areas are as defined in Figure 1b. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighboring municipalities, to include only areas treated at the same time.

Table 14: Estimated effect on the probability of becoming an employer allowing for exits

	(1) OLS	(2) OLS with covariates	(3) FE	(4) FE with covariates
By 1st year				
Treatment Effect	−0.00384	−0.00587	−0.00805	−0.00807
	(0.00433)	(0.00504)	(0.00478)	(0.00600)
Observations	53,945	53,941	53,945	53,941
Adjusted R^2	0.002	0.029	0.000	0.003
95% CI upper bound	0.00472	0.00408	0.00138	0.00378
Outcome mean		0.0627		0.0627
By 4th year				
Treatment Effect	0.00110	−0.00950	−0.00203	−0.0106
	(0.00656)	(0.00736)	(0.00617)	(0.00805)
Controls	No	Yes	No	Yes
Observations	53,945	53,941	53,945	53,941
Adjusted R^2	0.005	0.063	0.002	0.007
95% CI upper bound	0.0140	0.00503	0.0102	0.00534
Outcome mean		0.158		0.158

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients δ using 4 different models: (1) OLS with only the subsidy area and period dummies, (2) OLS with controls, (3) firm FE with subsidy area and period dummies controlling for firm fixed effects and (4) firm FE with controls. The dependent variable is a dummy for having a positive number of employees in some year by the first, and fourth year. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region fixed effects and region*subsidy period fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period. The table includes the upper bound of the 95% confidence interval for the estimates. The sample includes firms that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period, excluding firms in agriculture. The treatment period is 2008–2011 and the treatment area is defined as in 1b, but excluding municipalities that were added to the subsidy area in 2009 and 2010.

Table 15: Estimated effect on the probability of becoming a full-time employer allowing for exits

	(1) OLS	(2) OLS with covariates	(3) FE	(4) FE with covariates
By 1st year				
Treatment Effect	−0.000608	−0.00134	0.000212	−0.00222
	(0.00218)	(0.00244)	(0.00219)	(0.00311)
Observations	53,945	53,941	53,945	53,941
Adjusted R^2	−0.000	0.017	0.002	0.014
95% CI upper bound	0.00370	0.00349	0.00453	0.00393
Outcome mean				0.0125
By 4th year				
Treatment Effect	0.00202	0.00144	−0.000193	−0.000200
	(0.00406)	(0.00477)	(0.00337)	(0.00410)
Controls	No	Yes	No	Yes
Observations	53,945	53,941	53,945	53,941
Adjusted R^2	0.000	0.047	0.010	0.018
95% CI upper bound	0.0100	0.0109	0.00647	0.00791
Outcome mean				0.0569

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients δ using 4 different models: (1) OLS with only the subsidy area and period dummies, (2) OLS with controls, (3) firm FE with subsidy area and period dummies controlling for firm fixed effects and (4) firm FE with controls. The dependent variable is a dummy for being a full-time employer in some year by the first, and fourth year. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region fixed effects and region*subsidy period fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period. The table includes the upper bound of the 95% confidence interval for the estimates. The sample includes firms that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period, excluding firms in agriculture. The treatment period is 2008–2011 and the treatment area is defined as in 1b, but excluding municipalities that were added to the subsidy area in 2009 and 2010.

Table 16: Estimated effect on the probability of becoming an employer in different subsamples

	(1) Sole proprietor	(2) Revenue over 40,000	(3) Decile 1-5	(4) Decile 6-10
By 1st year				
Treatment Effect	−0.0105	−0.00892	0.000870	0.00174
	(0.00715)	(0.0128)	(0.00602)	(0.0122)
Observations	26,085	15,602	18,646	13,464
Adjusted R^2	0.019	0.021	0.006	0.010
95% CI upper bound	0.00361	0.0163	0.0128	0.0259
Outcome mean	0.0548	0.131	0.0329	0.0982
By 4th year				
Treatment Effect	−0.0265*	−0.00590	−0.0246*	0.0228
	(0.0109)	(0.0159)	(0.0114)	(0.0188)
Controls	Yes	Yes	Yes	Yes
Observations	26,085	15,602	18,646	13,464
Adjusted R^2	0.061	0.048	0.023	0.024
95% CI upper bound	−0.00500	0.0256	−0.00201	0.0599
Outcome mean	0.161	0.312	0.117	0.267

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients δ estimated separately for subgroups of firms. New firms include firms that are at most 3 years old in 2003 or 2007. Higher subsidy availability means firms located in i) ELY Centre municipalities or ii) regions with above median share of subsidy municipalities. Decile refers to the decile of predicted probability of becoming a full-time employer estimated as described in section 4. The dependent variable is a dummy for having a positive number of employees in some year by the first and fourth year. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region fixed effects and region*subsidy period fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period. The table includes the upper bound of the 95% confidence interval for the estimates. The sample includes active (non-zero revenue) firms for the observation period excluding firms in agriculture that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period. The treatment period is 2008–2011 and the treatment area is defined as in 1b, but excluding municipalities that were added to the subsidy area in 2009 and 2010.

E.1 Proportional hazards estimation of the effect on the probability of becoming an employer

The correct model to study the effect on the probability of becoming an employer is a duration model, that models the probability of becoming an employer conditional on being a non-employer. I use a discrete time proportional hazards model to estimate the ITT effect. To identify the effect of the first-employee subsidy, I use the difference-in-differences method to estimate the baseline hazard. Therefore,

$$h_o(t) = h(t)\exp(\alpha + \gamma D_{it}^{AREA} + \lambda D_{it}^{PERIOD} + \delta(D_{it}^{AREA} * D_{it}^{PERIOD})). \tag{16}$$

I allow for a flexible time-dependent hazard by letting it vary depending on time at risk or $h(t) = \exp(\sum_{j=1}^{13} I((j-1) \leq t \leq j)$ where t is the duration at risk, i.e. duration of the non-employer ship spell. The discrete time version of the proportional hazards model can be estimated with complementary log-logs allowing for time-variant individual regressors.

I use data from 2005–2013 for the estimation including all firms that were non-employers in the previous year. [26]Here the treatment period dummy is defined according to whether the subsidy was available in the home municipality of the firm at that year. In addition, there is a dummy for the post-treatment period, i.e. for the years 2012–2013.

In contrast to the simple difference-in-differences approach above, the proportional hazards model uses more information and can account for the effect dependent on the duration of the non-employer spell. Also, areas where the subsidy was introduced in 2009 and 2010 are included, and the "post-treatment" effect, or the effect of stopping the subsidy, can be estimated.

Table 17 summarizes the results from the model with covariates by subgroups. None of the coefficients are statistically significant and effects larger than 9.3% can be ruled out with a 95% confidence level. Interestingly, the post-treatment effect is also positive, although it should be negative if the subsidy increases the probability of hiring the first employee. As the estimated effect is small and the post treatment effect is positive, the results do not suggest that the subsidy increased the average probability of hiring.

Because the effect of the subsidy may differ by how long the firm has been a non-employer, I allow for the estimated effect to depend on the duration of the non-employer spell by interacting the treatment dummy with the duration indicators. In this case the baseline hazard is written:

$$h_o(t) = \exp(\sum_{j=1}^{13} h_j * I((j-1) \leq t \leq j) \tag{17}$$

[26]Before that the data has a lot of missing observations for employment that should be zero, making it difficult to differentiate between non-employer firms and firms with missing employment observations in many cases.

Table 17: Effect in different subsamples estimated using proportional hazards model

	(1) coef	(2) RH	(3) coef	(4) RH	(5) coef	(6) RH
Employer						
Treatment Effect	0.0566	1.058	0.0604	1.062	0.0459	1.047
	(0.0325)	(0.0343)	(0.0311)	(0.0330)	(0.0275)	(0.0288)
Post Treatment Effect	0.0835	1.087	0.0866	1.090	0.0566	1.058
	(0.0447)	(0.0486)	(0.0448)	(0.0488)	(0.0368)	(0.0389)
Time at risk	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	No	No	Yes	Yes	Yes	Yes
Controls	No	No	No	No	Yes	Yes
Observations	284,268	284,268	284,268	284,268	284,216	284,216

Standard errors clustered at the firm level in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression treatment effect on becoming an employer as coefficients and relative hazards (RH) and the post-treatment effect that corresponds to the effect of removing the subsidy in the period 2012–2013. The effect is estimated using the cloglog link function. The dependent variable is a dummy for a having positive number of employees and only firms at risk of becoming an employer, i.e. had zero employees in the previous year or appear in the data for the first time, are included. The sample includes active (non-zero revenue) firms in 2005–2013 excluding firms in agriculture. The treatment period is 2008–2011 and the treatment area is defined as in 1b. Controls include: industry and company form fixed effects, net assets, duration of non-employership spell, municipality employment share, and region*year fixed effects.

where $h_j = \alpha + \gamma D_{it}^{AREA} + \lambda D_{it}^{PERIOD} + \delta(D_{it}^{AREA} * D_{it}^{PERIOD})$.

Figure 13 plots the results. Again, none of the estimates suggest a statistically significant effect and there is no clear pattern of time dependent effect.

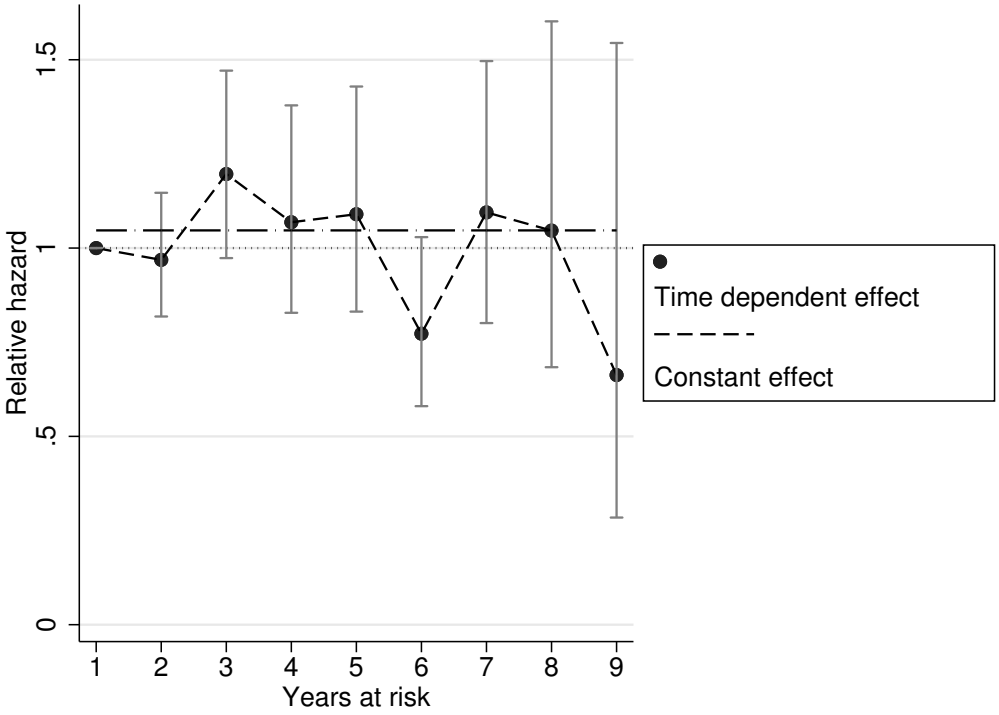

Figure 13: Estimated effect dependent on duration on non-employer spell

Notes: Figure depicts the effect of the subsidy on the probability of becoming an employer depending on the duration of being a non-employer with 95% confidence intervals. The effect is estimated using discrete time proportional hazards model estimated using the cloglog link function. The dependent variable is a dummy for having a positive number of employees and only firms at risk of becoming an employer, i.e. had zero employees in the previous year, are included.
The sample includes active (non-zero revenue) firms excluding firms in agriculture in 2005–2013. The treatment period is 2008–2011 and the treatment area is defined as in 1b.

F Placebo regressions and additional area trends

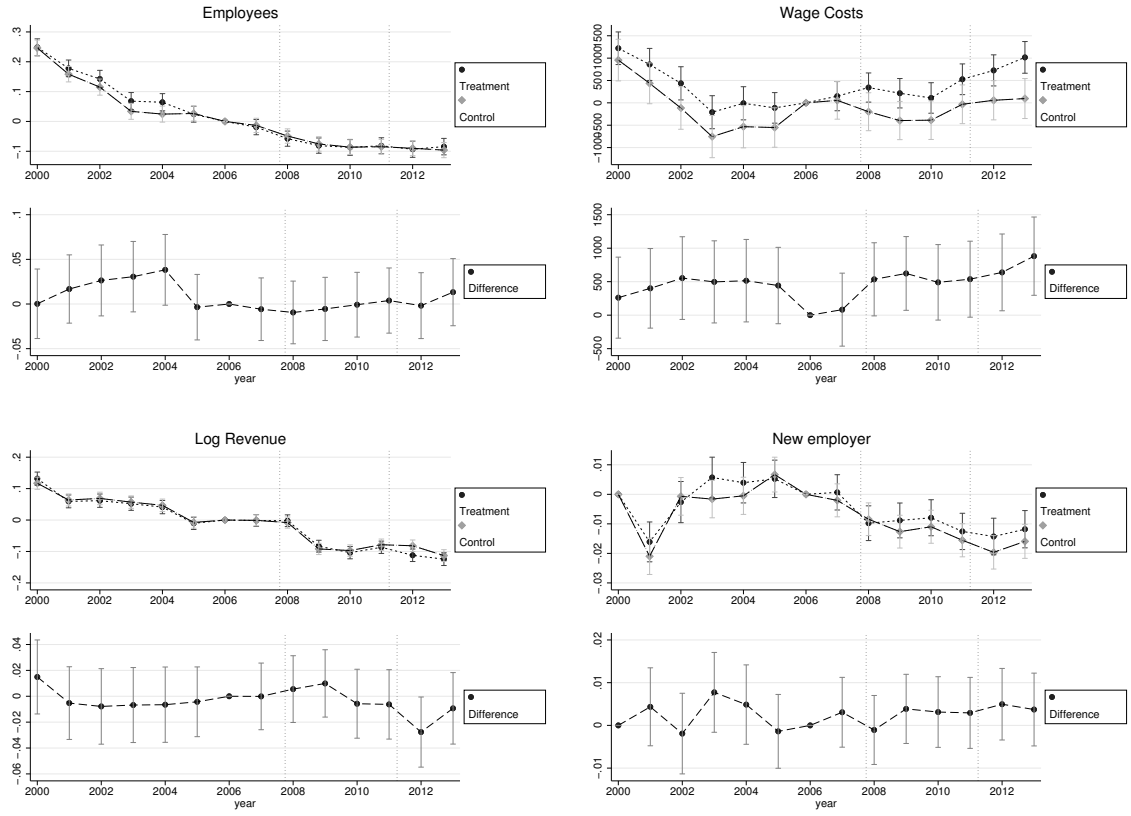

Figure 14: Firm trends in the research area, at most 20 employees

Notes:The figures plot the estimated yearly coefficients λ_t and the 95% confidence intervals from equation 12 with k=2006 as the reference year, estimated separately for the treatment and the control areas in the top panels, and the coefficients δ_t in the lower panels that correspond to the annual difference-in-differences estimates. The specification includes firm fixed effects. The outcome variables are: (number of) employees, wage costs, log of revenue and a dummy for being a new employer that equals one for firms that have positive employment and had zero employees in the previous year. The sample includes firms with at most 20 employees with a non-zero revenue that have zero number of employees in some year between 2000 and 2013. The treatment and control areas are as defined in Figure 1b. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighbor municipalities, to only include areas treated at the same time.

To study the parallel trends assumption, I evaluate the similarity of the pre-treatment trends using a placebo treatment period before the subsidy. The estimated placebo treatment effect indicates if there is a change between the trends of the groups before the subsidy period. Here, the placebo regressions use the time period 2004–2007, where 2006–2007 is defined as the placebo treatment period. the firms included in 2004–2005 are non-employers in 2003 and firms included in 2006–2007 are non-employers in 2005. There are, consequently, estimates for the change in hiring an employee by the first and second year. Table 19 summarizes these results. I conducted the placebo tests for all of the approaches used in the paper. Overall, the results do not suggest that the parallel

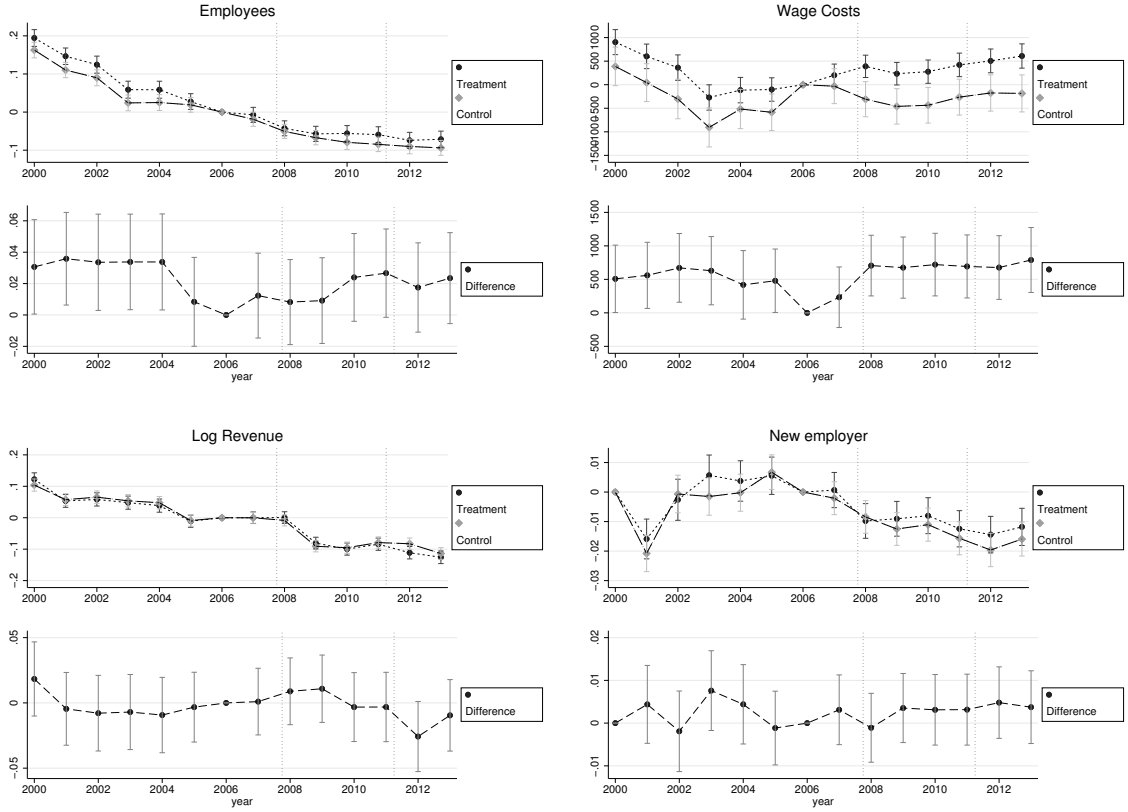

Figure 15: Firm trends in the research area, at most 10 employees

Notes: The figures plot the estimated yearly coefficients λ_t and the 95% confidence intervals from equation 12 with k=2006 as the reference year, estimated separately for the treatment and the control areas in the top panels, and the coefficients δ_t in the lower panels that correspond to the annual difference-in-differences estimates. The specification includes firm fixed effects. The outcome variables are: (number of) employees, wage costs, log of revenue and a dummy for being a new employer that equals one for firms that have positive employment and had zero employees in the previous year. The sample includes firms with at most 10 employees with a non-zero revenue that have zero number of employees in some year between 2000 and 2013. The treatment and control areas are as defined in Figure 1b. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighbor municipalities, to only include areas treated at the same time.

trends assumption is violated.

Table 20 summarizes the estimates of the placebo effect using the proportional hazards model for all of the subsamples used. The placebo treatment effects are again small and statistically insignificant.

G Additional outcomes, alternative estimation sample and entry and exit

While the simple difference-in-differences approach and the duration model used above are conceptually correct ways to estimate the effect on becoming an employer, in practice they do not make full use of the data. Restricting the analysis only to firms with zero employment according to the data can ignore many firms that actually would be eligible for the subsidy. This is because the data does not allow for strictly identifying which firms would be eligible for the subsidy. To address these concerns, I conduct regressions on a wider sample and include additional dependent variables that may respond to the subsidy.

In fact, according to the data some of the subsidized firms were not non-employers in the year previous to receiving the subsidy, as already observed in section 4. This can be due to a number of factors. First, some of the subsidized firms were new so that they did not exist in the data in the previous year. Second, the data is yearly so that non-employer periods less than a full year long may not be included. Third, the employment variable in the data does not correspond to the same criteria as used in the subsidy eligibility criteria. In the data, number of employees refers to all employees employed by a firm while the firm was eligible for the subsidy if it did not have full-time employees. For example, some of the firms with a positive number of employees have very small wage costs that cannot correspond to full-time employees.

To include more potentially eligible firms in the analysis while ignoring irrelevant firms, I restrict the sample to firms with zero employees in some year and at most 50 employees. This is the sample that was used for the descriptive statistics and trend comparisons above. In addition to the dummy for being a new employer I use many different dependent variables to measure the effect of the subsidy on other margins: employment, new employment, labor costs and (log of) revenue. The difference-in-differences estimates from these regressions cannot be directly interpreted as the causal effect of the subsidy on the eligible firms. However, they can serve as indicators of the effect.

Table 18: Placebo regression results

	(1) OLS	(2) OLS with covariates	(3) FE	(4) FE with covariates
By 1st year				
Placebo Treatment Effect	−0.00541	−0.00870	−0.00593	−0.00503
	(0.00447)	(0.00552)	(0.00455)	(0.00599)
Observations	46,406	46,402	46,406	46,402
Adjusted R^2	−0.000	0.024	0.004	0.006
95% CI upper bound	0.00342	0.00221	0.00306	0.00681
Outcome mean				0.0733
By 2nd year				
Placebo Treatment Effect	−0.00000713	−0.00253	−0.00203	0.00219
	(0.00540)	(0.00679)	(0.00554)	(0.00691)
Controls	No	Yes	No	Yes
Observations	46,406	46,402	46,406	46,402
Adjusted R^2	0.000	0.037	0.031	0.034
95% CI upper bound	0.0107	0.0109	0.00892	0.0158
Outcome mean		0.122		0.122

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients δ using 4 different models: (1) OLS with only the subsidy area and period dummies, (2) OLS with controls, (3) firm FE with subsidy area and period dummies controlling for firm fixed effects and (4) firm FE with controls. The dependent variable is a dummy for having a positive number of employees in some year by the first, and fourth year. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region fixed effects and region*subsidy period fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period. The table includes the upper bound of the 95% confidence interval for the estimates. The sample includes active (non-zero revenue) firms for the observation period excluding firms in agriculture that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period. The treatment period is 2008–2011 and the treatment area is defined as in 1b, but excluding municipalities that were added to the subsidy area in 2009 and 2010.

Table 19: Placebo regression results by subgroups

	(1) All	(2) Partnership & Corporation	(3) Sole proprietor	(4) New	(5) Revenue >40K	(6) Higher Subsidy Availability	(7) Deciles 2-5	(8) Deciles 6-9	(9) Deciles 1-5	(10) Deciles 6-10
By 1st year										
Placebo Treatment Effect	−0.00870	−0.0141	−0.00749	−0.0279*	−0.00843	−0.0116	−0.00231	−0.000576	−0.0175	−0.0105
	(0.00552)	(0.0111)	(0.00643)	(0.0109)	(0.0124)	(0.00691)	(0.00695)	(0.00587)	(0.0115)	(0.0114)
Observations	46,402	15,209	31,193	13,692	16,938	27,344	18,269	23,688	12,958	14,578
Adjusted R^2	0.024	0.020	0.016	0.038	0.018	0.024	0.006	0.006	0.008	0.009
95% CI upper bound	0.00221	0.00790	0.00521	−0.00635	0.0161	0.00214	0.0114	0.0110	0.00526	0.0119
Outcome mean	0.0733	0.105	0.0571	0.0751	0.123	0.0744	0.0446	0.0376	0.0930	0.0988
Full-time Employer										
Placebo Treatment Effect	−0.00253	−0.00921	−0.000856	−0.0142	−0.00147	−0.000247	0.00115	0.00344	−0.00702	0.00133
	(0.00679)	(0.0131)	(0.00809)	(0.0114)	(0.0158)	(0.00822)	(0.00952)	(0.00849)	(0.0139)	(0.0125)
Observations	46,402	15,209	31,193	13,692	16,938	27,344	18,269	23,688	12,958	14,578
Adjusted R^2	0.037	0.026	0.029	0.056	0.028	0.037	0.009	0.011	0.013	0.016
95% CI upper bound	0.0109	0.0166	0.0151	0.00829	0.0297	0.0161	0.0200	0.0202	0.0204	0.0261
Outcome mean	0.122	0.170	0.0980	0.121	0.200	0.124	0.0830	0.0703	0.155	0.164

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID placebo regression coefficients estimated for all firms and separately for subgroups of firms. The dependent variable is a dummy for having a positive number of employees by the first, and by the second year. The sample includes active (non-zero revenue) firms that have zero employees in 2003 or 2005, i.e. in the year before the placebo treatment or comparison period. The placebo treatment period is 2006–2007 and the treatment area is defined as in Figure 1b but excluding municipalities that were added to the subsidy area in 2009 and 2010. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region*year fixed effects.

Table 20: Placebo regression in the Cox proportional hazards model

	(1) coef	(2) RH	(3) coef	(4) RH	(5) coef	(6) RH
Employer						
Placebo Treatment	0.0213	1.022	0.0206	1.021	0.0459	1.047
Effect						
	(0.0357)	(0.0365)	(0.0354)	(0.0361)	(0.0365)	(0.0382)
Time at risk	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	No	No	Yes	Yes	Yes	Yes
Controls	No	No	No	No	Yes	Yes
Observations	103,695	103,695	103,695	103,695	103,693	103,693
103,693	103,693					

Standard errors clustered at the firm level in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID placebo regression coefficients and relative hazards (RH) of becoming an employer. The effect is estimated separately for subgroups of firms using discrete time proportional hazards model estimated using the cloglog link function. The dependent variable is a dummy for having positive number of employees and only firms at risk of becoming an employer, i.e. had zero employees in the previous year, are included. The sample includes active (non-zero revenue) firms excluding firms in agriculture in 2004–2007. The placebo treatment period is 2006–2007 and the treatment area is defined as in 1b. Partnerships and limited includes firms with the company form of partnerships and limited corporations. New firms include firms that are at most 3 years old, VAT-liable includes firms with revenue of more than €8,500. Control variables include: industry (at the two-digit level), company form, duration of non-employership spell, net asset level, municipal employment share, and region*year fixed effects.

Table 21: Estimated effect using full eligible and ineligible area

	(1) Sole proprietor	(2) Revenue over 40K	(3) Decile 1-5	(4) Decile 6-10
By 1st year				
Treatment Effect	−0.0105	−0.00892	0.000870	0.00174
	(0.00715)	(0.0128)	(0.00602)	(0.0122)
Observations	26,085	15,602	18,646	13,464
Adjusted R^2	0.019	0.021	0.006	0.010
95% CI upper bound	0.00361	0.0163	0.0128	0.0259
Outcome mean	0.0548	0.131	0.0329	0.0982
By 4th year				
Treatment Effect	−0.0265*	−0.00590	−0.0246*	0.0228
	(0.0109)	(0.0159)	(0.0114)	(0.0188)
Observations	26,085	15,602	18,646	13,464
Adjusted R^2	0.061	0.048	0.023	0.024
95% CI upper bound	−0.00500	0.0256	−0.00201	0.0599
Outcome mean	0.161	0.312	0.117	0.267

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients δ estimated separately for subgroups of firms. New firms include firms that are at most 3 years old in 2003 or 2007. Higher subsidy availability means firms located in i) ELY Centre municipalities or ii) regions with above median share of subsidy municipalities. Decile refers to the decile of predicted probability of becoming a full-time employer estimated as described in section 4. The dependent variable is a dummy for having a positive number of employees in some year by the first and fourth year. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region fixed effects and region*subsidy period fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period. The table includes the upper bound of the 95% confidence interval for the estimates. The sample includes active (non-zero revenue) firms for the observation period excluding firms in agriculture that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period. The treatment period is 2008–2011 and the treatment area is defined as in 1a, but excluding municipalities that were added to the subsidy area in 2007, 2009 and 2010.

Table 22: Estimated effect using full eligible and ineligible area

	(1) All	(2) Partnership & Limited	(3) New	(4) Local subsidy availability	(5) Decile 2-5	(6) Decile 6-9
By 1st year						
Treatment Effect	−0.000563	0.00445	−0.0166	0.00175	−0.00123	0.00206
	(0.00439)	(0.00999)	(0.0108)	(0.00537)	(0.00540)	(0.00808)
Observations	149,437	53,573	38,787	90,258	53,836	46,612
Adjusted R^2	0.029	0.013	0.049	0.031	0.006	0.009
95% CI upper bound	0.00807	0.0241	0.00458	0.0123	0.00938	0.0180
Outcome mean	0.0919	0.125	0.111	0.0923	0.0461	0.102
By 4th year						
Treatment Effect	0.00779	0.0248	0.0102	0.00893	−0.00400	0.0224
	(0.00690)	(0.0127)	(0.0139)	(0.00807)	(0.00894)	(0.0134)
Observations	149,437	53,573	38,787	90,258	53,836	46,612
Adjusted R^2	0.066	0.027	0.094	0.068	0.022	0.026
95% CI upper bound	0.0214	0.0499	0.0376	0.0248	0.0136	0.0488
Outcome mean	0.243	0.294	0.271	0.245	0.165	0.289

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients δ estimated separately for subgroups of firms. New firms include firms that are at most 3 years old in 2003 or 2007. Higher subsidy availability means firms located in i) ELY Centre municipalities or ii) regions with above median share of subsidy municipalities. Decile refers to the decile of predicted probability of becoming a full-time employer estimated as described in section 4. The dependent variable is a dummy for having a positive number of employees in some year by the first and fourth year. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region fixed effects and region*subsidy period fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period. The table includes the upper bound of the 95% confidence interval for the estimates. The sample includes active (non-zero revenue) firms for the observation period excluding firms in agriculture that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period. The treatment period is 2008–2011 and the treatment area is defined as in 1a, but excluding municipalities that were added to the subsidy area in 2007, 2009 and 2010.

Table 23: Estimated effect on the probability of becoming an employer in different subsamples

	(1) Sole proprietor	(2) Revenue over 40K	(3) Decile 1-5	(4) Decile 6-10
By 1st year				
Treatment Effect	−0.0105	−0.00892	0.000870	0.00174
	(0.00715)	(0.0128)	(0.00602)	(0.0122)
Observations	26,085	15,602	18,646	13,464
Adjusted R^2	0.019	0.021	0.006	0.010
95% CI upper bound	0.00361	0.0163	0.0128	0.0259
Outcome mean	0.0548	0.131	0.0329	0.0982
By 4th year				
Treatment Effect	−0.0265*	−0.00590	−0.0246*	0.0228
	(0.0109)	(0.0159)	(0.0114)	(0.0188)
Observations	26,085	15,602	18,646	13,464
Adjusted R^2	0.061	0.048	0.023	0.024
95% CI upper bound	−0.00500	0.0256	−0.00201	0.0599
Outcome mean	0.161	0.312	0.117	0.267

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients δ estimated separately for subgroups of firms. New firms include firms that are at most 3 years old in 2003 or 2007. Higher subsidy availability means firms located in i) ELY Centre municipalities or ii) regions with above median share of subsidy municipalities. Decile refers to the decile of predicted probability of becoming a full-time employer estimated as described in section 4. The dependent variable is a dummy for having a positive number of employees in some year by the first and fourth year. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region fixed effects and region*subsidy period fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period. The table includes the upper bound of the 95% confidence interval for the estimates. The sample includes active (non-zero revenue) firms for the observation period excluding firms in agriculture that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period. The treatment period is 2008–2011 and the treatment area is defined as in 1b, but excluding municipalities that were added to the subsidy area in 2009 and 2010.

Table 24: Estimated effect on additional firm outcomes

	(1) All	(2) Partnership & Corporation	(3) Sole proprietor	(4) New	(5) Revenue >40K	(6) Higher Subsidy Availability	(7) Deciles 2-5	(8) Deciles 6-9	(9) Deciles 1-5	(10) Deciles 6-10
Employer										
Treatment Effect	−0.00642	−0.0106	−0.00594	0.0405	−0.00581	−0.0106	−0.00245	−0.000719	0.000176	−0.00371
	(0.00513)	(0.0119)	(0.00488)	(0.0295)	(0.0109)	(0.00663)	(0.00733)	(0.0158)	(0.00516)	(0.0154)
Observations	145,852	44,285	101,567	36,857	59,798	84,745	56,093	44,513	71,565	50,562
Adjusted R^2	0.011	0.017	0.009	0.014	0.016	0.011	0.012	0.019	0.011	0.020
Outcome mean	0.132	0.176	0.104	0.165	0.186	0.133	0.0786	0.163	0.0670	0.172
Full-time Employer										
Treatment Effect	−0.00231	−0.00955	−0.000701	0.0308	−0.00153	−0.00184	0.000145	−0.00599	0.00104	−0.00203
	(0.00302)	(0.00833)	(0.00219)	(0.0322)	(0.00691)	(0.00399)	(0.00332)	(0.00978)	(0.00218)	(0.0106)
Observations	145,852	44,285	101,567	36,857	59,798	84,745	56,093	44,513	71,565	50,562
Adjusted R^2	0.016	0.026	0.012	0.024	0.024	0.019	0.015	0.026	0.014	0.027
Outcome mean	0.0358	0.0592	0.0210	0.0524	0.0549	0.0357	0.0165	0.0441	0.0139	0.0502
Employment										
Treatment Effect	−0.00624	−0.00683	−0.0101	−0.434	0.000631	−0.0107	−0.0109	0.00416	0.000922	0.0264
	(0.0137)	(0.0358)	(0.00862)	(0.393)	(0.0314)	(0.0183)	(0.0168)	(0.0559)	(0.0138)	(0.0726)
Observations	145,852	44,285	101,567	36,857	59,798	84,745	56,093	44,513	71,565	50,562
Adjusted R^2	0.011	0.020	0.010	0.025	0.015	0.013	0.039	0.015	0.032	0.016
Outcome mean	0.265	0.412	0.173	0.372	0.354	0.275	0.136	0.366	0.114	0.385
Wage Costs										
Treatment Effect	51.85	65.74	−10.73	−2,910.5	257.9	92.17	−35.53	−107.9	41.01	378.2
	(163.3)	(456.0)	(72.20)	(3646.9)	(440.7)	(224.8)	(159.8)	(556.1)	(138.3)	(937.3)
Observations	145,852	44,285	101,567	36,857	59,798	84,745	56,093	44,513	71,565	50,562
Adjusted R^2	0.014	0.026	0.012	0.021	0.019	0.017	0.030	0.026	0.030	0.021
Outcome mean	1,615.1	2,854.2	834.1	2,293.9	2,292.6	1,640.1	811.1	2,242.2	665.0	2,438.6

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients δ estimated separately for subgroups of firms. Employer is a dummy equal to one when a firms has positive employment, Full-time employer is one when a firm has wage costs>€12,000, Employment is the number of employees that worked in the firms during the tax year. The sample includes active (non-zero revenue) firms for the observation period excluding firms in agriculture that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period. The treatment period is 2008–2011 and the treatment area is defined as in 1b, but excluding municipalities that were added to the subsidy area in 2009 and 2010. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, and region* year fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period.

Table 25: Estimated effect on additional firm outcomes

	(1) All	(2) Partnership & Corporation	(3) Sole proprietor	(4) New	(5) Revenue >40K	(6) Higher Subsidy Availability	(7) Deciles 2-5	(8) Deciles 6-9	(9) Deciles 1-5	(10) Deciles 6-10
Log Revenue										
Treatment Effect	−0.000453	−0.0334	0.0122	0.151	−0.0228	−0.0133	0.0398	−0.0620	0.00630	−0.0536
	(0.0132)	(0.0288)	(0.0147)	(0.119)	(0.0223)	(0.0153)	(0.0229)	(0.0385)	(0.0174)	(0.0381)
Observations	145,852	44,285	101,567	36,857	59,798	84,745	56,093	44,513	71,565	50,562
Adjusted R^2	0.014	0.018	0.016	0.021	0.026	0.013	0.017	0.017	0.016	0.016
Outcome mean	10.40	10.28	10.47	10.39	11.26	10.38	10.23	10.72	10.12	10.75
D[Business subsidy]										
Treatment Effect	0.000469	−0.00469	0.00246	0.0559	0.000123	−0.0000415	−0.00277	0.000854	−0.00115	0.00129
	(0.00270)	(0.00676)	(0.00298)	(0.0355)	(0.00511)	(0.00356)	(0.00449)	(0.00935)	(0.00321)	(0.00907)
Observations	145,852	44,285	101,567	36,857	59,798	84,745	56,093	44,513	71,565	50,562
Adjusted R^2	0.002	0.006	0.002	0.008	0.003	0.004	0.003	0.005	0.003	0.005
Outcome mean	0.0280	0.0377	0.0219	0.0421	0.0375	0.0301	0.0210	0.0336	0.0183	0.0367

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients δ estimated separately for subgroups of firms. Employer is a dummy equal to one when a firms has positive employment, Full-time employer is one when a firm has wage costs>€12,000, Employment is the number of employees that worked in the firms during the tax year. The sample includes active (non-zero revenue) firms for the observation period excluding firms in agriculture that have zero employees in 2003 or 2007 i.e. in the year before the treatment or comparison period. The treatment period is 2008–2011 and the treatment area is defined as in 1b, but excluding municipalities that were added to the subsidy area in 2009 and 2010. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, and region* year fixed effects. Outcome mean is the mean in the treatment group in the pre-treatment period.

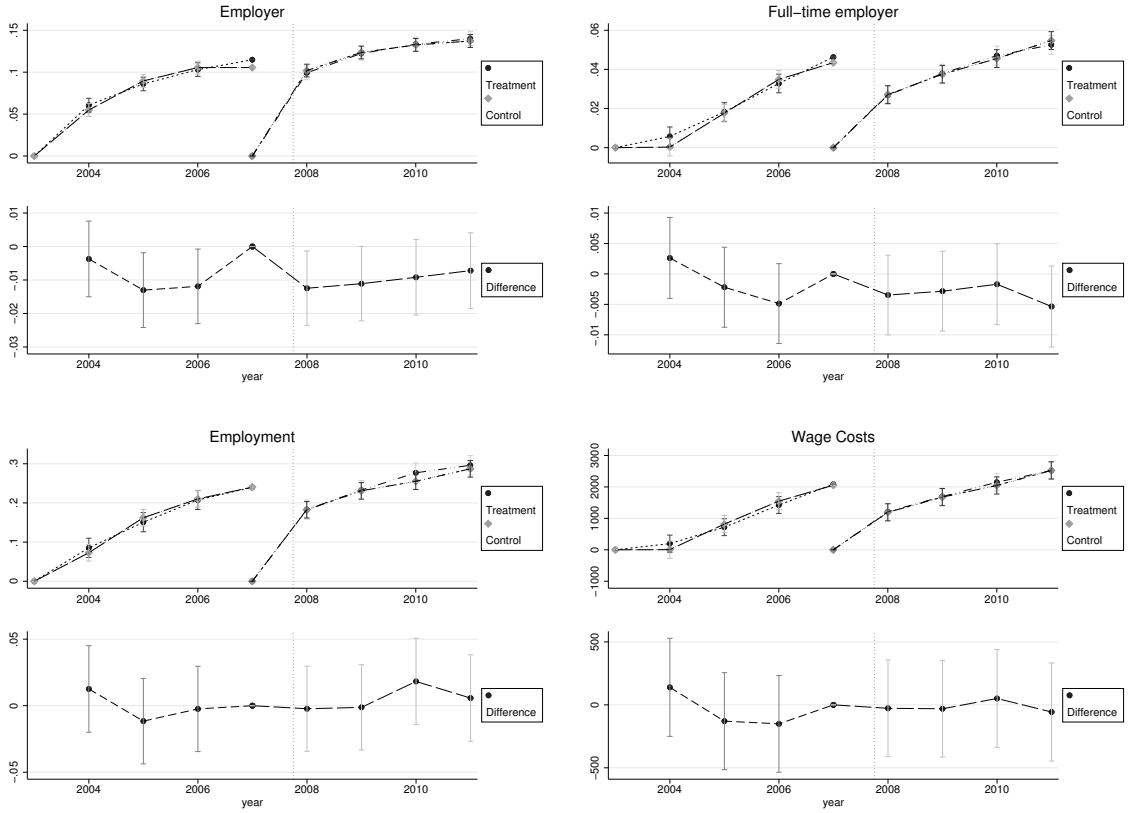

Figure 16: Effect on employership, employment and wage costs for the target group

Notes: The upper panel plots the cumulative probability of becoming a (full-time) employer for the sample of non-employer firms in 2003 in years 2004-2007 and for the sample of non-employer firms in 2007 for years 2008–2011. The plotted estimates come from a single regression including both samples in years 2004–2011 and excluding the sample selection year. The figure plots the yearly coefficient plus the constant $\lambda_k + c$ from and the 95% confidence intervals from equation:
$Y_{it} = \alpha_i + \sum_{k=2000}^{2013} \lambda_k D_{it}(t = k) + \epsilon_{it}$
with k=2007 as the reference year, estimated separately for the treatment and the control areas in the top panels. The lower panel includes the difference relative to year 2007 i.e. the yearly DD coefficients δ_k from equation:
$Y_{it} = \alpha_i + \gamma D_{it}^{AREA} + \sum_{k=2000}^{2013} \lambda_k D_{it}(t = k) + \sum_{k=2000}^{2013} \delta_k (D_{it}^{AREA} * D_{it}(t = k)) + \epsilon_{it}$. The treatment and control areas are as defined in Figure 1b. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighboring municipalities, to include only areas treated at the same time.

Table 26: Effect on additional outcomes estimated using a wider sample of firms

	(1) All	(2) Partnership & Corporation	(3) Sole proprietor	(4) New	(5) Revenue >40K	(6) Higher Subsidy Availability	(7) Deciles 2-5	(8) Deciles 6-9	(9) Deciles 1-5	(10) Deciles 6-10
Employer										
Treatment Effect	0.00265	0.00860	0.000730	0.00260	0.00537	−0.00203	0.00365	0.00143	0.00152	−0.000214
	(0.00487)	(0.00864)	(0.00496)	(0.00782)	(0.00733)	(0.00618)	(0.00475)	(0.00781)	(0.00398)	(0.00687)
Observations	276,921	80,481	196,440	65,729	129,832	163,690	87,053	71,727	111,123	87,645
Adjusted R^2	0.010	0.024	0.006	0.010	0.021	0.010	0.019	0.043	0.017	0.048
Outcome mean	0.230	0.379	0.161	0.119	0.396	0.234	0.0266	0.0766	0.0222	0.0917
Full-time Employer										
Treatment Effect	0.00756*	0.00446	0.00909***	0.00587	0.0132*	0.00541	0.00575*	−0.000701	0.00421*	0.000816
	(0.00294)	(0.00811)	(0.00273)	(0.00509)	(0.00518)	(0.00311)	(0.00232)	(0.00482)	(0.00184)	(0.00485)
Observations	276,921	80,481	196,440	65,729	129,832	163,690	87,053	71,727	111,123	87,645
Adjusted R^2	0.005	0.012	0.006	0.014	0.011	0.005	0.018	0.035	0.015	0.035
outcomemean	0.104	0.214	0.0528	0.0411	0.204	0.105	0.00290	0.0116	0.00237	0.0199
Employment										
Treatment Effect	0.00682	−0.0283	0.0243	−0.0242	0.0260	−0.0129	0.0228*	0.00978	0.00487	0.0119
	(0.0197)	(0.0510)	(0.0155)	(0.0314)	(0.0330)	(0.0247)	(0.0113)	(0.0198)	(0.0156)	(0.0217)
Observations	276,225	80,002	196,223	65,639	129,248	163,303	87,022	71,617	111,091	87,454
Adjusted R^2	0.008	0.020	0.005	0.007	0.016	0.010	0.019	0.027	0.022	0.027
Outcome mean	0.698	1.465	0.347	0.318	1.312	0.726	0.0401	0.127	0.0333	0.162
Wage Costs										
Treatment Effect	258.7	160.9	317.3*	322.1	462.0	140.3	313.9*	253.2	242.6*	305.8
	(295.7)	(958.2)	(134.9)	(372.9)	(596.9)	(379.6)	(122.2)	(213.9)	(98.92)	(276.9)
Observations	276,921	80,481	196,440	65,729	129,832	163,690	87,053	71,727	111,123	87,645
Adjusted R^2	0.005	0.012	0.005	0.007	0.010	0.011	0.019	0.019	0.016	0.020
Outcome mean	6,313.3	15,463.2	2,100.8	2,121.6	12,661.4	6,381.2	166.8	697.4	134.2	1051.3

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DD regression coefficients δ estimated separately for subgroups of firms. Employer is a dummy equal to one when a firms has positive employment, Full-time employer is one when a firm has wage costs>€12,000, Employment is the number of employees that worked in the firms during the tax year. The sample includes firms with at most 50 employees, non-zero revenue in 2005–2011 and number of employees zero in some year between 2000 and 2013. Groups are assigned in year 2007. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region*year fixed effects.

Table 27: Effect on additional outcomes estimated using a wider sample of firms

	(1) All	(2) Partnership & Corporation	(3) Sole proprietor	(4) New	(5) Revenue >40K	(6) Higher Subsidy Availability	(7) Deciles 2-5	(8) Deciles 6-9	(9) Deciles 1-5	(10) Deciles 6-10
Log Revenue										
Treatment Effect	0.00676	−0.00579	0.0140	0.0175	0.00153	0.000376	0.0188	−0.0139	0.0137	−0.00280
	(0.0120)	(0.0241)	(0.0116)	(0.0246)	(0.0150)	(0.0148)	(0.0218)	(0.0211)	(0.0182)	(0.0207)
Observations	276,812	80,395	196,417	65,705	129,786	163,623	87,035	71,703	111,104	87,608
Adjusted R^2	0.043	0.048	0.051	0.070	0.068	0.043	0.044	0.048	0.041	0.045
Outcome mean	10.23	10.77	9.978	9.295	11.44	10.22	9.729	10.33	9.574	10.48

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DD regression coefficients δ estimated separately for subgroups of firms. Employer is a dummy equal to one when a firms has positive employment, Full-time employer is one when a firm has wage costs>€12,000, Employment is the number of employees that worked in the firms during the tax year. The sample includes firms with at most 50 employees, non-zero revenue in 2005–2011 and number of employees zero in some year between 2000 and 2013. Groups are assigned in year 2007. Control variables include: industry (at the two-digit level), company form, net asset level, municipal employment share, region*year fixed effects.

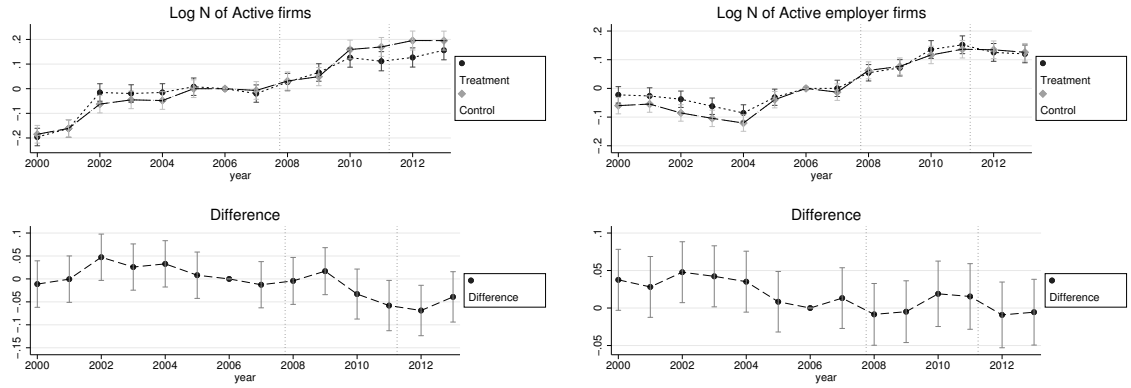

Figure 17: Trends of number of firms in the treatment and subsidy area

Note: The figures plot the year dummy coefficients relative to year 2006 separately for the treatment and control area in the upper panel and the annual difference-in-difference coefficients relative to year 2006 in the lower panel for the outcomes (log of) number of active firms and number of active employer firms. Number of firms is aggregated to the municipality*industry level with two code industries according to Statistics Finland classification. There are 8,244 municipality*industry units with an average of 31 firms in a unit. The sample includes firms with non-zero revenue excluding firms in agriculture. The treatment and control areas are as defined in figure 1b. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighbor municipalities.

Table 28: Effect on (log of) number of different firms

	(1) Firms	(2) Entry firms	(3) New firms	(4) Employer firms	(5) Non-employer firms
Treatment effect	−.02	.027	−.064	.012	−.051
	(.03)	(.053)	(.037)	(.031)	(.026)
Year, and industry*year fe	Yes	Yes	Yes	Yes	Yes
Municipality employment share	Yes	Yes	Yes	Yes	Yes
Municipality population	Yes	Yes	Yes	Yes	Yes
Observations	9,349	5,019	7,313	7,618	8,899
Adjusted R^2	0.631	0.449	0.377	0.211	0.550
Outcome mean	2.2	.81	1.3	1.5	1.9

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents DID regression coefficients on the number of firms. Number of firms is aggregated to the municipality*industry level with two code industries according to Statistics Finland classification. 8,244 municipality*industry units with an average of 31 firms in a unit. The sample includes firms with non-zero revenue excluding firms in agriculture. Uses data from 2006–2011. Control variables include: year*industry fixed effects, municipality employment share, municipality population, and region*year fixed effects.