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(No) Effects of subsidizing the first employee – Evidence of a Low Take-up Puzzle Among Firms in Finland

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ABSTRACT

This paper studies the effects of a large regional wage subsidy for hiring the first employee in Finland, using data on the universe of Finnish firms. By comparing firms in the eligible area to firms in the neighboring area, I find a precisely estimated zero effect on the probability of becoming an employer as well as other outcomes for the targeted firms. As a reason for the zero effect, I document the low take-up of the subsidy by only 2% of the firms that became employers in the eligible area. The take-up is not completely explained by the low benefits of using the subsidy: The take-up increases to 12% at the most when focusing on firms with larger gains from the subsidy based on their ex post wage costs. As a result, a large majority of firms that became employers seem to be leaving thousands of euros on the table by not applying for the subsidy. Descriptive evidence on the use of the subsidy suggests restricting the subsidy to full-time employment and a lack of awareness as a potential reason for the low take-up.

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

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

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1 Introduction

Employment and business subsidies are often used and suggested 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.[2] Accordingly, there is a wide amount of literature studying the effects of employment subsidies but the evidence is quite diverse. For example, there are studies that show firm growth in response to subsidies (Saez *et al.* , 2019; Lombardi *et al.* , 2018) but some studies find negligible employment effects on the targeted firms (Bennmarker *et al.* , 2009), even in the same country. However, there is lack of evidence on how entrepreneurs *use* the subsidies and what is the role of take-up in the effectiveness of subsidies. This is despite the obvious fact that using a subsidy (when eligible) is a necessary requisite for the entrepreneur to respond to the subsidy, and the take-up related to, for example, social transfers is recognized as an important factor.[3]

In this paper, I study the effects and take-up of a sizable first employee subsidy in Finland, using register data on the full population of Finnish firms and the use of the subsidy. The first employee subsidy was targeted to non-employer firms i.e. entrepreneurs with no external employees and was put into place in parts of Finland between 2007–2011. Firms that had no employees for at least 12 months were eligible to the subsidy that amounted to 30% of the wage costs of the first employee in the first year and 15% in the second year. In order to qualify for the subsidy, the firm had to hire an employee for a permanent employment contract with at least 25 hours of work per week. In addition, the firms had to apply for the subsidy at the regional administrative agency before hiring the employee.

This is the first paper, to my knowledge, to provide empirical evidence on the effects of employment subsidies on the decision to become an employer; this decision is an important for two reasons. First, non-employer firms form a major share of the firm population and many firms start as non-employers, making the decision to hire the first employee relevant for a large proportion of firms. In Finland, the target group of non-employer firms accounts for about 55 per cent of the firm population in Finland before the subsidy. Second, non-employer firms may face constraints and costs when becoming an employer, thus causing an inefficiently low number of employer firms. In this case, subsidizing the first employee can improve efficiency if it stimulates firms to become permanent employers. In this paper, I consider a simple model of becoming an employer motivated by Lucas (1978) and Evans & Jovanovic (1989). The model demonstrates that fixed costs of becoming an employer can cause firms to remain smaller than optimal. These costs may be, for example, related to hiring, training and managing employees, the compliance costs of taxation and labor laws, or a result of financial and labor market frictions.[4]

[2] E.g. European Commission (2013); Neumark (2013); Kritikos (2014). Wage subsidies have been suggested since (Kaldor, 1936)to decrease unemployment. The COVID-19 crisis has lead to many countries to adopt different business subsidies, including employment subsidies, to decrease the shock to businesses and employment such as the paycheck protection program in the US studied by Neilson *et al.* (2020).

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

[4] Similar intuitions of a wage subsidy for hiring for the first time are presented in Mckenzie *et al.* (2019). In addition, multiple studies provide evidence on firm growth hindered by credit constraints e.g.(Brown & Earle, 2017; Banerjee & Duflo, 2014) and, accordingly, that especially small firms may benefit from subsidies that help overcome the financial market frictions (e.g. Criscuolo *et al.* 2019). Harju *et al.* (2019) provide evidence of significant compliance costs of VAT in Finland. Labor market friction such as search costs and information asymmetries are integral elements in labor

The Finnish first employee subsidy program is well suited to the study of the effects of employment incentives on the targeted firms primarily, because it provides geographical variation in the eligibility criteria, and also, because it is a sizable subsidy with minimal restrictions and discretion in granting. For example, many employment subsidies are restricted to low wage jobs, or targeted to certain types of employees such as the long term unemployed. This makes using them less attractive for a firm, as using the subsidy restricts the employee selection.[5] In addition, employment and business subsidies are often granted with discretion, which can make it difficult for a firm (and the researcher) to know *ex ante* whether it qualifies for the subsidy. This, again, makes the subsidy less attractive due to uncertainty and potential bureaucratic hassle.

To identify the effects of the subsidy on the targeted firms, I use a standard difference-in-difference strategy exploiting the regional eligibility criteria, focusing on the border of the subsidy area to improve the comparability between the subsidy and control areas. The subsidy was mainly targeted to disadvantaged areas but areas closer to the Finnish average were included, as there were intentions to extend the subsidy to all of Finland. The municipalities on the border of the subsidy area are closer to the Finnish average, providing a better counterfactual. Using the border area is warranted, because the subsidy is not likely to have important spillover effects, as it only affects a small fraction of jobs in the labor market. I provide trend comparisons and descriptive statistics of the firms and the municipalities before the subsidy period to show that the neighboring municipalities in the border areas indeed develop similarly and are similar prior to the subsidy period. Because the subsidy area was specifically defined for the subsidy, there are no other institutions and, especially policy changes, with the same geographical criteria.

The estimated effect of the subsidy on the probability of becoming an employer by existing non-employer firms is estimated to be precisely zero except for new firms that have a larger estimated effect that is less precise. Using a simple linear probability model the estimated effect on becoming an employer in first year after the subsidy is -0.2 percentage points, 0.5 by the second year and -0.05 by the fourth year, amounting to a maximum of 6% difference in the baseline probability of becoming an employer. The estimated effect is, in order of magnitude, larger for new firms at 4.8 percentage points by the fourth year, but still not statistically significant as the number of new firms is smaller, decreasing the precision of the estimates. The subsidy does not seem to have an effect on alternative firm outcomes such as employment and turnover or firm entry and exit decision. Hence, my results are in contrast to Saez *et al.* (2019) that find large employment effects in firms due to a payroll tax reduction for young workers and more in line with the moderate or insignificant employment effects in response to payroll tax reductions found by Korkeamäki & Uusitalo (2009) and Bennmarker *et al.* (2009).

Because of the zero estimated effect, despite the quite substantial monetary gains from the subsidy, I study the take-up of the subsidy as a potential reason for ineffectiveness. Studying the take-up of the subsidy is enabled by access to data on the subsidy use that can be combined with the firm population data. This allows me to observe which firms became employers while eligible for the subsidy and which actually used the subsidy. In addition, I construct a measure for the *ex post* subsidy amount for the eligible firms based on their wage costs after becoming an employer.

market literature.

[5] Examples of studies using subsidies restricted to types of employment include Kangasharju (2007), Huttunen *et al.* (2013) and Lombardi *et al.* (2018).

I find a low overall take-up of only 2% among the eligible firms that became employers and a higher, but still low, take-up rates of 12, at the most, among firms with a higher *ex post* calculated subsidy amoun oft between 6,000 to 20,000 euros. This evidence suggests that a large majority of new employer firms left money on the table by applyinf for the subsidy. For example, among new effectively full-time employers, defined as having total wage costs above the median wage cost per employee, the take-up rate was only 6% and the average calculated subsidy close to about 8,000€ which is about 25% of the average total wage costs in the first year.

The low take-up rate among new employer firms raises a puzzle as to why firms do not use the subsidy despite the relatively large financial gains. I consider the costs of using the subsidy and imperfect awareness as potential causes for the low take-up in the theoretical model. To analyze the importance of these reasons, I use the predictions of the model combined with anecdotal descriptive evidence on the use of the subsidy. First, as the take-up rate is still considerably low among firms with a high calculated subsidy, the costs of using the subsidy alone do not seem to explain the lack of take-up. Significant regional differences in take-up rates, consistent with an information channel through the regional authorities that administer the subsidy, further supports a lack of awareness as a reason for the low take-up. Low awareness of the subsidy among small firms is consistent with the results in Neilson *et al.* (2020). Second, the restriction to full-time employment contracts seems to be an important factor in decreasing the overall take-up while, at the same time, targeting the subsidy to principally larger firms. The full-time restriction increases the costs of using the subsidy for small firms hiring employees on temporary or short term employment contracts, thus decreasing the efficiency of the subsidy. Third, firms facing higher frictions in becoming an employer or liquidity constraints may be more likely to use the subsidy. For example, new firms are more likely to take-up the subsidy and these firms have a larger estimated effect. Lastly, I find no evidence that the administrative costs of the subsidy can explain differences in take-up, beyond the costs implied by the full-time restriction.

The low take-up can explain the difference in my results from Saez *et al.* (2019), where there was full, immediate take-up due to the institution. In fact, if the 12% upper bound of the take-up rate is considered as an awareness rate, then the average effect of 0.5 percentage point is consistent with a 4.2 percentage point effect on the treated firms. These results highlight the importance of institutional design on the effectiveness of business incentives; suggesting that immediately salient tax reductions may be more effective than subsidies that can suffer from incomplete awareness and costs of using them. In addition to awareness and the full-time restriction, the timing of the subsidy may be an issue. The first employee subsidy was only paid half-yearly after becoming an employer. For example, Zwick & Mahon (2017) provide evidence that firms have a stonger response to immediate rather future cash flows. The importance of these institutional issues in take-up may be emphasized for small firms, as observed by Neilson *et al.* (2020). In addition, prior studies in Finland (Korkeamäki & Uusitalo, 2009; Huttunen *et al.* , 2013) mention low take-up of employment incentives among small firms. Thus, small firms may be especially difficult to target by business subsidies.

Finally, I provide descriptive evidence that the subsidy seemed to be beneficial for the subsidized firms by describing the development of the subsidized firms relative to similar firms that did not use the subsidy. The subsidized firms were more likely to remain as employers, had higher employment and turnover after the subsidy period. This may indicate that firms do suffer from frictions in hiring and temporary subsidies can help their growth. This is contrary to Mckenzie *et al.* (2019) that find

subsidies to increase employment but only temporarily, however it is closer to Neilson *et al.* (2020) who find that subsidized firms perform better. However, the differences in development can also reflect higher propensity of the subsidized firms to hire employees on a full-time employment contract and associated higher productivity.

The study contributes to strands of literature on the effects of employment incentives on firms, and the take-up of business subsidies and tax benefits. First, this paper is one of the few studies on the take-up of business subsidies. The take-up rate of 2–12% that I find is significantly lower than in Zwick, (Forthcoming) who finds that only 37% of eligible firms claim a tax refund. Neilson *et al.* (2020) study how firms apply for paycheck protection loans, aimed to help small businesses during the COVID-19 crisis, and findthat smaller firms are slower to learn about the subsidies, and less likely and slower to apply for them. Additionally, Zwick, (Forthcoming) finds that take-up is higher among larger firms, which is also documented in Korkeamäki & Uusitalo (2009) and Huttunen *et al.* (2013). 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 similar to Zwick, (Forthcoming) that a low take-up does not seem to be completely explained by the costs and benefits of the take-up. Consequently, my results are more in line with novel evidence[6] that firms can make mistakes contrary to the canonical assumption of profit maximizing firms.

Second, this paper is the first, to my knowledge, to provide empirical evidence on how entrepreneurs respond to incentives to become an employer. Previously, Mckenzie *et al.* (2019); Mel *et al.* (2010) study the effects of a temporary wage subsidy offered to micro enterprises in a randomized experiment in Sri Lanka. The dynamics of becoming an employer are studied in Fairlie & Miranda (2016) in the US and Lechmann & Wunder (2017) in Germany also demonstrating that non-employer firms form a major share of the firm population in other countries similar to Finland. This discussion on the firm size and 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).

Third, my paper also relates to the more general, growing body of literature on how firms respond to employment incentives. There is a strand of research that studies the effects of payroll tax reduction on firm employment: Saez *et al.* (2019) study a youth payroll tax reduction in Sweden, Korkeamäki & Uusitalo (2009) investigate a regional payroll tax reduction 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 to the unemployed and find positive effects on firm performance. Conversely, Lechner *et al.* (2013) finds adverse effects of active labor market policies on firms.[7]

Finally, my paper relates to the literature on subsidies targeted to firm growth. Betcherman *et al.* (2010) and Cahuc *et al.* (2019) 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.[8]

[6] 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 in developing countries of non-profit maximizing behavior of firms.

[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); Card *et al.* (2010); Brown *et al.* (2011). However, the firm responses have received less attention.

[8] Small firms, however, may refer to firms with up to 50 employees which may differ substantially from entrepreneurs considering hiring the first employee.

The rest of this paper is organized as follows. Section 2 describes the institutional setting and the data and Section 3 sets out the theoretical model on the effects of the subsidy on firm behavior and the decision to take-up the subsidy. Section 4 describes the identification method and provides estimates of the effect of the subsidy on the targeted firms. Section 5 studies the subsidy take-up and the behavior of the subsidized firms. Finally, Section 6 concludes.

2 Institutional Setting and Data

The regional first employee subsidy was specifically introduced to support the hiring of the first employee; initially it was launched in some areas in 2007 but expanded extensively in 2008–2010 to cover a large proportion of Finnish municipalities, as there were plans to extend it to all of Finland. The subsidy was granted to firms that applied for it and fulfilled the requirements of the non-employership status and the planned employment contract; thus there was minimal discretion in granting the subsidy. This paper uses register data on the full population of firms in Finland and the subsidy decisions. The subsidy was targeted at a large group of firms as firms in Finland are very small on average – about 55% of the firm population had no employees and, consequently, were in the target group of the subsidy. However, the number of firms that used the subsidy was 1,349 which is quite low.

Section 2.1 describes the subsidy program and the institutional setting. Then, section 2.2 describes the data used and presents descriptive statistics on the targeted firm population and the first employee subsidy decisions.

2.1 Institutional Setting

The First Employee Subsidy

The first employee subsidy was in force from 2007 to 2011 when firms in the eligible municipalities were able to apply for the subsidy. The subsidy amounted to 30% of the wage costs of the first employee in the first year and 15% in the second not including payroll taxes.

The subsidy had two motivations: To encourage entrepreneurs to hire their first employee and encourage business growth, and to support employment in scarcely populated regions that were losing population. Because the subsidy had the motive of encouraging business in addition to its regional support motive, extending the subsidy to all of Finland was suggested; the subsidy was to function as an experiment on the effectiveness of subsidizing the first employee as a policy tool to increase employment.

To qualify for the subsidy the firm had to: i) have no external employees (other than the initial entrepreneur) for at least 12 months and ii) hire an employee on a permanent employment contract with at least 25 hours of work per week. Minimal discretion was used when granting the subsidy so that each firm that fulfilleds the qualifications should be granted the subsidy.[9] As the subsidy was meant to support the growth of firms, there were no restrictions on the type of employee hired. Due to EU regulations, the subsidy could not be granted to businesses in the industry of fishery, agriculture, forestry and the processing or marketing of agriculture products.

[9]The firm could be denied the subsidy if it did not have requisites for a profitable business or subsidizing was evaluated to distort local competition or markets considerably. In practice, these reasons were not often used for a denial of the subsidy.

To receive the subsidy, the firms had to apply to the regional ELY Centre before hiring the first employee. After hiring, the firms had to apply to receive the subsidy, which was paid semi-annually.

The subsidy was administered and granted by the regional ELY Centre (The Centre for Economic Development, Transport and the Environment) that were thought to be a natural source of information for firms on issues of employment growth. There are 15 ELY Centres in Finland that are responsible for the regional implementation of central government policy in areas of business and industry, labor force, competence, and cultural activities. They also steer and supervise the local Employment and Economic Development Offices.

Geographical variation

Because of the dual motivation of the subsidy, the subsidy area includes the economically most disadvantaged areas in Finland, but there are municipalities close to the national average both inside and outside the subsidy areas. Importantly for the empirical strategy, the areas seem to have similar economic trends, and there are no other institutions that follow the same geographic criteria as the area that was specifically defined for this subsidy. Some municipalities were chosen with the motivation to support certain areas but others more economically advantaged municipalities were included, as the government wanted the subsidy to function as an experiment on whether it should be extended to all of Finland. For the causal estimation below, I exploit the availability of the subsidy in similar municipalities by focusing on neighboring municipalities on the border of the subsidy area. These municipalities are less likely selected due to worse economic conditions and closer to the Finnish average. Moreover, the neighboring municipalities likely share the local economy. Section 4 discusses the identification strategy in more detail.

Figure 1 depicts the subsidized area and how it developed. 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 Finland. Small areas were added in 2009 and 2010.

The subsidy area was specifically chosen for the subsidy so that it did does not follow, for example, regional borders. Consequently, some regions belong fully to the subsidy area or are outside of it, however, some regions have municipalities both in and outside of the subsidy area. Because of this some of the ELY Centres administer the subsidy to their full area of responsibility but some ELY Centres only have a few municipalities where the subsidy is available, possibly resulting in regional differences in treatment intensity – e.g. information availability – through the operations of the ELY Centres. In addition, a firm's location relative to the regional ELY Centre may affect its exposure to the subsidy.

The first employee subsidy area included remote areas considered the most economically disadvantaged in Finland as well as municipalities close to the national average. In general, Northern and Eastern Finland are losing population, have higher unemployment, and businesses are increasingly centralizing in the capital area and major cities in Southern and Western Finland. Regarding business subsidies, Finland is divided into three assisted areas according to the EU criteria on population density (Einiö, 2014, 712). A Type I assisted area receives the most business subsidies and Type III the least in terms of absolute amounts. Briefly, the assisted areas can be thought of as a classification

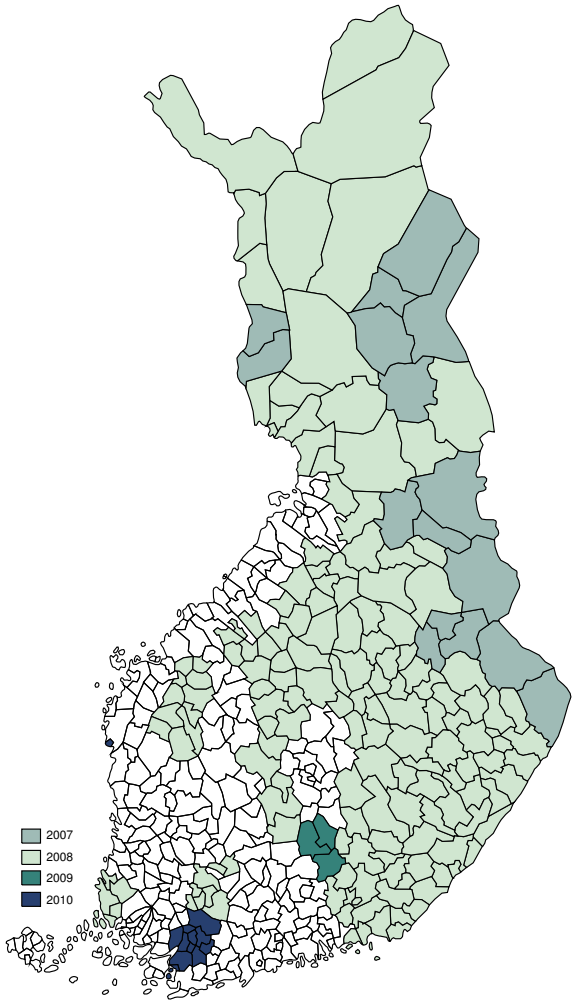

Figure 1: Development of the subsidy area

Notes: Map drawn using municipality borders from 2007

of how economically disadvantaged an area is, Type I being the worst. First, in 2007 only a few municipalities in the I assisted area were given the first employee subsidy. From 2008 onwards the whole Type I assisted area belonged to the subsidy area as well as a minority of municipalities in the Type II and III assisted areas.

Figure 2 depicts the trends of GDP and Employment percentage in the subsidy area and the rest of Finland (excluding the capital area) before the subsidy period. The levels are clearly higher in the no subsidy area, but the trends seem similar. This is crucial for the difference-in-differences identification strategy discussed below – even though a smaller area is used in the estimation.

Related subsidies

There are some partially overlapping business and employment subsidies in Finland. For example, there 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. There is also a business starting subsidy targeted

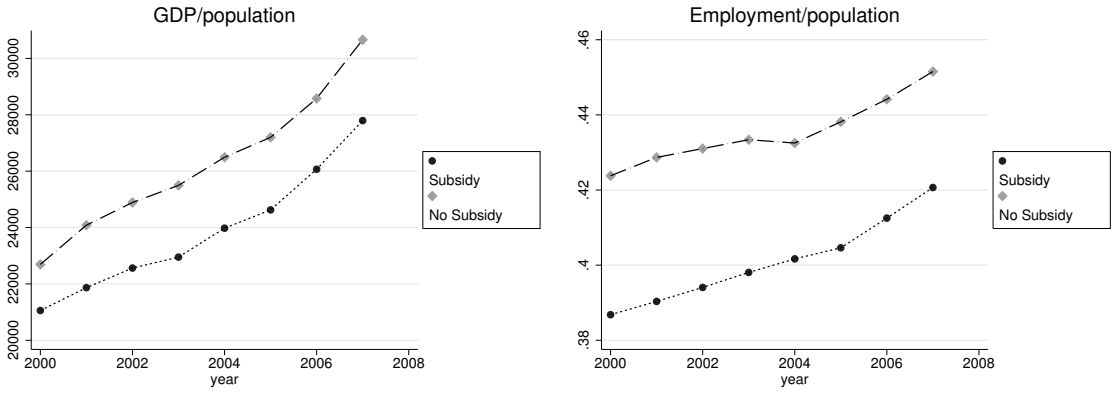

Figure 2: Trend comparison of GDP and employment

Data: Statistics Finland, Regional accounts, Transactions by NUTS 2, NUTS 3 and LAU 1 regions 2000–2007
Notes: GDP (in market price) and total employment divided by population, excluding capital area

at companies in rural areas for enlarging or starting a business amounting to a maximum of 50% of the labor costs of the first employee for two years. These subsidies are clearly substitutes for the first employee subsidy, but less extensive and simple because discretionary power is always used in granting them.

Other employment subsidies include, for example, a hiring voucher for the unemployed, exemption from payroll taxes up to a threshold in some rural municipalities in 2003–2011, and a wage subsidy for over 54-year-old low-wage employees. These differ quite considerably from the first employee subsidy 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 entrepreneur, but that has no component to incentivise job creation.

Employment protection

To qualify for the first employee subsidy the firms had to hire an employee on a permanent employment contract which together with employment protection can introduce additional costs in the subsidy program. This is because when hiring, the firms have to take into account the costs of dismissing employees in the case where the business does not grow as intended or the worker is not a good match for the job. Consequently, the employment protection laws related to permanent employment contracts are relevant when considering the effects of the subsidy.

The Finnish employment protection regulation related to individual dismissals is modest in comparison with international regulations – higher than in the UK or US but lower than in comparable EU countries. The OECD index (OECD, 2019) (ranging from 0 to 5 with 0 indicating low protection) for employment protection for individual dismissals was 2.17 in 2006. This is lower than other Nordic countries with, for example, Sweden with 2.61, and Norway with 2.33, which is higher than the UK (1.26) or US (0.26) but lower than some EU countries such as Germany with 2.68 or France with 2.47. Consequently, employment protection is quite high, but for a small firm with new employees the associated costs of dismissals are not very large.

According to the regulation, the costs of dismissals are not very large for small firms with new

employees but the costs increase with tenure and firm size. The regulation is different depending on whether the employee is dismissed for economic or personal reasons. The Employment Contract Act (55/2001) sets the minimum employment protection regulation. In addition, there are industry specific Collective Agreements that regulate employment contracts.

During the trial period an employee can be dismissed for any reason except for discriminatory reasons, but after that the employment contract can only be terminated due to a proper and weighty personal reason, or financial and productive reasons. According to the Employment Contract Act, an employee has a trial period of 6 months when hired for a permanent contract. When a worker is dismissed due to a personal reason, a notice and a chance to remedy the issue has to be given to the worker before the termination of the contract. When evaluating what counts as a proper and weighty cause, the entire situation of the employer and employee are evaluated to account for situation specific factors..[10]

The employment contract can be terminated due to financial or productive reasons if the firm no longer has suitable work to offer the employee. However, a notice period, that can be considered as a proxy for the financial costs of the dismissal, is required and it increases in the tenure of the employee. In addition, temporary lay-offs are possible when the work available has decreased temporarily. The notice period for terminating an employment contract is 14 days for contracts under one year, one month for a contract of one to four years, two months after four years, four months after eight years, and six months after 12 years.

2.2 Data and Descriptive Statistics

I use two register data sets in the research. The main data set is full population yearly firm tax returns from 2000 to 2013 provided by the Finnish Tax Administration. I exclude non-business company forms, and firms in agriculture that are not relevant for the first employee subsidy. In addition, I exclude firms in the capital area to exclude the area that deviates most from the subsidy area. After these restrictions there are 3,642,506 observations of 596,740 different firms.

All active firms are required to file a tax return yearly including information needed for taxation as well as key variables about the firm. The variables include, for example, the number of employees, wage costs, turnover and assets as well as detailed information on the firm's characteristics such as home municipality, company form and industry at the 5-digit level. The number of employees used throughout this paper is the total number of employees that worked in the company during the year – excluding the entrepreneur. Accordingly, the wage costs exclude the wages paid to the entrepreneur. [11] This data is used for the estimation and description of the firms.

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

[11] The data includes the number of total employees and employees in categories of entrepreneur employees (the entrepreneurs that receive wage income from the firm), full-time employees, and part-time employees but the categories have many missing observations as they are not compulsory to report. Especially, the number of entrepreneur employees is coded as missing when it is zero and for sole proprietors it is always zero as they do not pay wage income to themselves. Other company forms can, but do not necessarily, pay wage income to the entrepreneur. The number of entrepreneur workers is never missing when there are reported wage costs for the entrepreneur. Consequently, I assume the number of entrepreneur employees to be zero when the observation is missing and define the number of external employees as the number of total employees minus the number of entrepreneur employees when it is not missing. This means that in some cases the number of external employees may include the entrepreneur workers if there are some inconsistencies in reporting. However, using the total number of employees includes the entrepreneur worker when it is not missing and only using the non-missing number of entrepreneur workers would omit a large portion of the data that have zero (entrepreneur) employees. Additionally, using the number of external versus number of total employees does not change

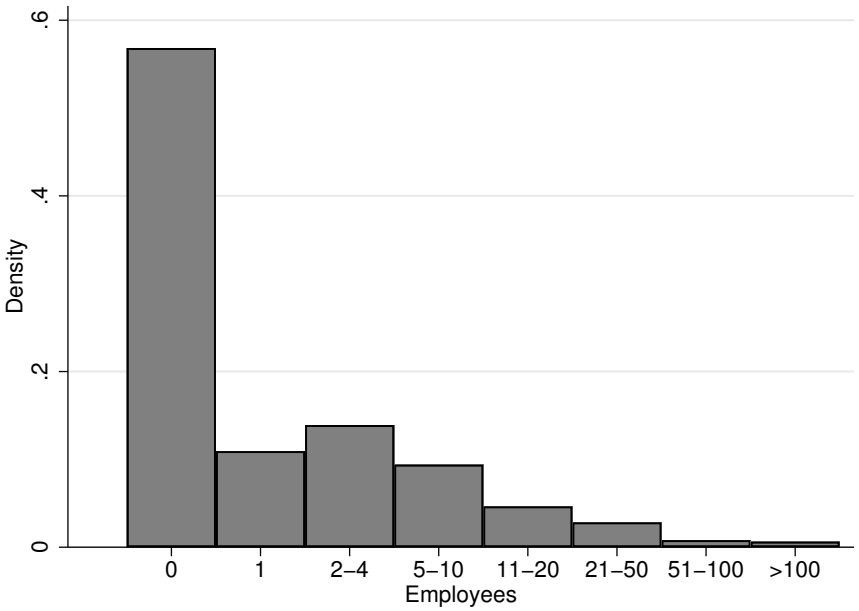

Figure 3: Size distribution of firms in 2006 by employment

Notes: Sample includes all firms with non-zero turnover 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.

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 describe the subsidy used. The data consists of the 1,351 positive decisions on the subsidy granted including a firm identifier, the amount of the subsidy granted and paid to the firm, as well as some background information about the firms in the year of their application. The subsidy was granted to 1,349 different firms as two firms applied for the subsidy twice.

The number of firms using the subsidy is small but not negligible compared to all non-employer firms in the area. For example, there were 77,327 active firms in the subsidy area in 2006 of which 40,241 were non-employers. 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 with most of the negative decisions being firms that did not qualify for the subsidy. For example, hiring the first employee before applying for the subsidy was the most common reason for a negative decision.

As both data include the firm identifiers, I match the data to identify the subsidized firms in the tax return panel. Some of the firms in the subsidy data do not appear in the tax return data so they cannot be matched; There are 1,020 subsidy decisions matched to the tax return data. This could either be due to mistakes in the firm ids or that the firms have not filed their taxes.

The size distribution of the firms is highly concentrated at zero employees with almost 60% of the firms having no 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 percentage of firms declines in each size category.

much. For example, the fraction of non-employer firms is 59% using the number of external employees and 55% using the total number of employees.

Table 1 presents descriptive statistics in 2006–2007 on all firms, non-employer firms and firms that were non-employers in the previous year. Firms are small on average, as already indicated by the size distribution. The average number of employees is 4.9 and turnover 641,000€. Non-employer firms are, by construct, smaller and have on average a turnover of 65,000€. Of firms that were non-employers in the previous year 7.8 per cent have employees, with an average of 0.14 employees and a turnover of 77,000€ indicating small growth of the non-employer firms on average.

Most firms are sole proprietors with 53% proportion of all firms and 73% of non-employers and 70% of non-employers in the previous year. The second largest company form is corporations that form a third of all firms but only about 17% of non-employer firms. Partnerships account for about 10% of firms in all groups.

Largest industries are wholesale and retail trade, construction, and manufacturing with 19, 15 and 11 per cent shares respectively. The industry distribution differences between all and non-employer firms are quite small, but the share of firms in the largest sectors is smaller in the non-employer samples. Human health and social work, and other service activities are more common among non-employer firms than all firms.

3 Theoretical Model

This section develops a theoretical model to analyze the effects of the first employee subsidy, making two contributions. First, the model allows for fixed costs of becoming an employer, or hiring costs that are larger with fewer number of employees, that may make subsidizing the first employee efficiency increasing. Second, the model allows for imperfect take-up of the subsidy that may reduce both the effectiveness and efficiency of the subsidy. Moreover, imperfect take-up reduces the number of the subsidized firms which may complicate predicting the spending on the policy.[12] The model produces some empirically observable predictions that can be tested using the data.

First, section 3.1.1 develops a model of becoming an employer where I introduce management costs – costs related to having and hiring employees – to the labor costs in addition to the wage costs. Second, section 3.1.2 sets out the benchmark model of the effect of the first employee subsidy on becoming an employer, and the costs and benefits of the subsidy. Third, section 3.1.3 introduces imperfect take-up of the subsidy to the benchmark model, in order to analyze the effects of imperfect take-up. Section 3.1.4 summarizes the empirical implications of the theoretical model. Lastly, section 3.2 discusses potential effects beyond the hiring effects and unintended effects of the subsidy.

3.1 Model on Becoming an Employer

3.1.1 Hiring Decision

I consider a simple model of firm hiring. I introduce management costs, that I define as all non-wage costs related to having or hiring employees, to a flexible firm production model with entreneur specific ability and labor as the factor of production. If the marginal management costs are decreasing, small firms, and non-employer firms especially, have a higher threshold for hiring which may be amplified if the firms face liquidity constraints. This may lead to inefficiently low hiring rates of non-employer

[12] The model idea is close to the tax system view, presented in Gillitzer & Slemrod (2014), where agents face compliance costs of taxation but here firms have compliance costs of using a subsidy.

Table 1: Descriptive statistics of firms in 2006–2007

	(1) All	(2) Non employer	(3) Non employer in t-1
Turnover	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)
Net Assets	234,734	36,773	40,526
	(4,287,196)	(859,317)	(1,072,667)
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)
Other	.22	.24	.17
	(.41)	(.43)	(.38)
New	.26	.34	.21
	(.44)	(.47)	(.41)
Entry firm	.082	.12	0
	(.27)	(.32)	(0)
Previous employer	.58	.34	.29
	(.49)	(.47)	(.45)
Observations	428,564	251,731	192,539

Notes: Table presents descriptive statistics: mean and standard errors in parentheses. The sample includes firms with nonzero turnover. Firms in the capital area and agriculture are excluded. Standard Industrial Classification according to Statistics Finland 2008. Largest industries are included in the table. The Other industry category includes: 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.

and very small firms, as they remain small while it could be optimal for them to have employees in the long run.

A firm produces output with a firm specific production function dependent on the entrepreneur ability $f_i(l)$ using labor and the entrepreneur's effort. The production costs include wage costs with exogenous wage level w and firm specific management costs $m_i(l)$ so that the firm's profit can be written:

$$\pi_i(l) = f_i(l) - m_i(l) - wl. \tag{1}$$

This implies the following conditions for optimal employment in the firm:

$$f_i'(l) \geq w + m_i'(l), \tag{2}$$

$$f_i(l) - m_i(l) - wl \geq 0 \tag{3}$$

so that $f_i(0) = 0$ is the entrepreneur's production without employees normalized to zero. Consequently, the marginal management costs can be defined as a wedge between the marginal productivity of labor and the wage evaluated at the optimal level of labor, or formally $m_i'(l^*) = f_i'(l^*) - w$. As the entrepreneur might only learn the true f_i and m_i after hiring, these should strictly be considered as expectations of production and management costs. Note, that the canonical case is when $m_i(l) = 0$ so that the management costs are implicitly assumed to be zero or embedded in $f_i(l)$.

The firm specific management costs $m_i(l)$ include all real and opportunity costs related to having or hiring employees. The opportunity costs can bethought of as the reduction in the entrepreneur's own output due to having labor. This can include, for example, resources the entrepreneur needs to use for recruitment, training and managing the employees as well as compliance costs of labor legislation and taxation. The management costs can depend on the level of employment or the firm's experience in managing employees.

Separating the management costs from the production function $f_i(l)$, that includes only the increase of output due to labor, allows me to analyze how the functional form of the management costs affects labor demand. This is because both production and management costs are weakly increasing by assumption. In other words, it makes visible how labor demand and firm size are associated.

For example, consider $m_i(l) = m_i^c I(l > 0) + m_i^l * l$ so that there is a fixed cost of having employees m_i^c and a constant cost per employee m_i^l. In this case, the management costs have a larger impact on the hiring decision of small firms that are considering hiring one or two employees. It is easy to imagine that there are fixed costs in having employees including, for instance, learning how to hire employees and bureaucratic responsibilities such as payroll and tax remittance, and setting up a management system for the employees. In this case the first order condition for the number of employees for a non-employer firm is

$$f_i'(l^*) = w + m_i^c + m_i^l$$

but the first order condition for the optimal labor for employer firms is

$$f_i'(l^*) = w + m_i^l.$$

This implies that the threshold of hiring the first employee is higher than the second employee, thus,

the labor demand is dependent on the firm size. Especially, if the costs are one off costs, such as learning, it can be optimal for the firm to have employees after hiring but not to hire due to the fixed costs if $w + m_i^c + m_i^l \geq f_i'(l^*) \geq w + m_i^l$.

The probability of becoming an employer is determined by the distribution of the marginal productivity of labor and the form of the management costs. Let $d(.)$ and $D(.)$ be the probability density and cumulative distribution functions associated with the marginal productivity of labor $f_i'(l)$. Then, the probability of becoming an employer is

$$P(w + m_i \leq f_i'(l)) = 1 - P(f_i(l) \leq w + m_i) = 1 - D(w + m_i) \tag{4}$$

where $m_i = m_i^c + m_i^l$ is the marginal management cost of the first employee.

Of importance for efficiency concerns is, whether the management costs arise from or cause distortions which may call for corrective policies. Some of the sources of management costs are not necessarily efficiency decreasing or distorting. For example, recruiting, managing or training costs may not be distortive themselves. Also, management costs may arise from the entrepreneur's preferences. For example, if an entrepreneur has a clear preference for solo self-employment, the management costs can be considered as infinite.

Some management costs can, however, arise due to or be attenuated by market frictions, causing distorted hiring decisions. For example, public policies such as labor legislation and taxation can cause compliance costs for employers, and their effects may be pronounced on small firms.

For example, imperfections in the capital markets may cause or attenuate the management costs due to several issues. First, the one off costs may be more difficult to overcome if the firm is liquidity constrained. Second, the financing costs may increase even without fixed costs of becoming an employer if the firm needs to borrow cash to pay wages during a lower productivity training period, as the labor costs often realize before an increase in revenue. Third, departing from the model without capital in the production function, the firm may need to adjust its capital when increasing labor, which can imply further adjustment costs.

Additionally, management costs may arise due to risk associated with hiring. This decreases hirings by small firms to inefficiently low levels from the society's perspective, especially if entrepreneurs are risk averse. The risk associated with hiring can be related to the quality of an employee not observable prior to hiring, the employee becoming sick or leaving the firm, or productivity shocks to the firm. If the risk is employee or firm specific, the aggregate risk is smaller than the risk for any single entrepreneur or hire. Furthermore, small firms suffer more from employee related risk, whether it is due to quality of an employee or sickness, because they have less ability to diversify risk than large firms.

Relatedly, Arellano *et al.* (2019) develop a model showing that just the fact that hiring labor is risky can introduce a wedge between marginal labor productivity and wage, that I call management costs here. They present a model where firms hire risky inputs in incomplete financial markets, which results an increase in the costs of labor due to higher risk, thus, increasing the wedge between marginal productivity of labor and the wage.

Lastly, an entrepreneur that has never had employees may not perceive the true management costs but has to rely on perceived management costs when deciding whether or not to hire additional workforce. Consequently, under- or overestimation of these costs can affect the hiring decision. Firms with experience from being an employer are, as a result, better informed in making the hiring decision.

Mckenzie *et al.* (2019) discuss the effect of this and financial constraints in hiring labor.

3.1.2 Effects of the First Employee Subsidy

The first employee subsidy increases the probability of becoming an employer, by decreasing the marginal cost of becoming an employer, but does not directly affect labor demand after the first employee. However, if there are significant marginal management costs or firms suffer from liquidity constraints in hiring, the subsidy can cause firms to employ more than without the subsidy. In this case the benefits of the policy increase surpass the benefits of the increase in the probability of becoming an employer.

The first employee subsidy reduces the labor costs of the first employee by introducing a subsidy as a proportion s of wage costs. The firm profit is now $\pi_i(l) = f_i(l) - m_i(l) - wl + sw * I(l > 0)$ implying a first order condition for hiring the first employee:

$$f_i'(l) \geq (1-s)w + m_i \tag{5}$$

with a probability to become an employer of $1 - D((1-s)w + m_i)$ correspondingly. Consequently, using the baseline probability of become an employer in equation 4, the increase in the probability of becoming an employer is

$$D(w + m_i) - D((1-s)w + m_i). \tag{6}$$

The increase in the probability of becoming an employer depends on how many firms there are just below the margin of becoming an employer. On the other hand, the welfare effects depend on the effect on the number of hired employees and the increas in the income of the entrepreneur that, in turn, depend on the form of the management costs. This is because the social welfare depends on the value created for the employees and the entrepreneur and their respective welfare weights. Let the society's welfare function be a weighted sum of the profit of the entrepreneur and the incomes of the employees, and α_e and α_l the general welfare weights of the entrepreneurs and the workers, respectively [13]. The social welfare is defined as $W(\pi_i, l) = \alpha_e * \pi_i(l) + \alpha_l * wl = \alpha_e * [f_i(l) - m_i(l) - wl] + \alpha_l * wl = \alpha_e * [f_i(l) - m_i(l)] + (\alpha_l - \alpha_e) * wl$. Denote by l^* the labor choice of a firm. Now, the subsidy increases the social welfare by:

$$[\alpha_e * [f_i(l^*) - m_i(l^*)] + (\alpha_l - \alpha_e) * wl^*] * [D(w + m) - D((1-s)w + m)]. \tag{7}$$

As l^* and $f_i(l^*) - m_i(l^*)$ depend on the form of the management cost, the welfare gains depend on them. For example, large fixed costs in becoming an employer but smaller costs of hiring additional employees can cause firms to increase employement by more than the one subsidized employee.

Marginal management costs that are large initially can lead to higher welfare gains of the first employee subsidy In addition, especially if the management costs are, to a large extent, caused by or intensified by market frictions, setting $sw = m$ can be a second best policy. In this case, the welfare gains are larger than stated in equation 7 as the subsidy improves the allocation of resources. Therefore, the welfare gains stated in 7 should be considered as a lower bound of the welfare gains, if

[13] The welfare weights can be considered as general welfare weights in Saez & Stantcheva (2016). The welfare function is linear in profits and labor income, but the general welfare weights can depend on income, thus, including dependence on income of the group.

there are significant frictions in becoming an employer.

Notice that because the subsidy only changes the incentives to become an employer for the duration of the subsidy, any persistent effect on employment or an effect surpassing the first employee are due to hiring frictions or management costs. In other words, the first order condition for hiring *after* the first employee is the same as without the subsidy in equation (2). This is also noted in Mckenzie *et al.* (2019). For example, Saez *et al.* (2019) document evidence that firms use the extra cash flow from a payroll tax reduction to increase the size of the business. Similarly, the firms receiving the first employee subsidy may use the money for growth, which would increase the welfare gains of the policy.

As the subsidy causes a transfer of money to all firms that hire but some of them would have hired without the subsidy i.e. have $f_i'(l) \geq w + m_i'(l)$ there is a deadweight cost equal to

$$(1 - D(w + m)) * sw \tag{8}$$

calculated using the baseline probability of becoming an employer as defined in equation 4 and the subsidy amount.

3.1.3 Imperfect Take-up of the Subsidy

Here, I consider a model with imperfect take-up of the subsidy that can result from administrative costs related to applying for or using the subsidy or lack of information of the subsidy. The take-up decision essentially differentiates the subsidy from a reduction in wages or labor taxes. Administrative costs and low awareness of the subsidy both lead to lower effectiveness of the subsidy but have different implications on its efficiency and which firms take-up the subsidy. At the minimum, low take-up reduces the government spending on the policy that can be problematic if not known *ex ante,* especially if there are fiscal policy concerns related to a subsidy.

I define subsidy take-up as the fraction of the firms that used the subsidy of those that were eligible for the subsidy, i.e., non-employers that hired the first employee. This is to separate the take-up decision from the observed hiring decision. In the model above, the subsidy was considered as an automatic reduction in wage costs. However, in reality the subsidy is only paid to firms that apply for it, thus, there is an additional decision to take-up the subsidy. Consequently, with imperfect take-up the subsidy is not equivalent to a price decrease which has implications for the effects of the subsidy.

To analyze the effects of imperfect take-up, I model the hiring decision by introducing i) fixed administrative costs c^s of applying for and using the subsidy common to all firms and ii) individual awareness $a_i \in \{0, 1\}$ or "proneness" to use the subsidy to the model presented above in section 3.1.2. I consider administrative costs common to firms to address the bureucratic costs of the subsidy program that consist of two parts: (1) The process of applying for and getting the subsidy paid, and (2) the alternative costs of hiring with the subsidy that are introduced by the restrictions on the employment contract. These costs depend on the policy and can be affected by design of the policy. Awareness or proneness to use the subsidy is firm specific. At the simplest, the awareness a_i can be considered to equal 1 for firms aware of the subsidy and 0 for those not aware. However, there can be more nuanced differences including partial knowledge or endogenous learning of the program, firm specific costs of applying, or even entrepreneur's attitude to using subsidies, but for simplicity I do not consider them in the model.

While there can be a firm specific component in costs of using the subsidy, I model the administrative costs common to all firms for two main reasons. First, this allows differentiatint the implications of costs versus awareness for empirical assessment. However, this means that the empirical differences in awareness should not be strictly interpreted as awareness but rather a "proneness" to use the subsidy including firm specific costs. Second, the subsidy specific costs are more relevant for policy, as they can be directly affected by the design of the subsidy. Firm specific costs can be more difficult to identify empirically or change by policy design. It is also quite natural to consider firm specific costs to be related to information or awareness of the subsidy, for example, if firms have to search for information on the subsidy and the time spent on searching has opportunity costs.

The introduction of administrative costs and awareness changes the benchmark model of hiring in two ways. First, the subsidy only affects the decision of the firms aware of the subsidy with no effect on the incentives of the firms unaware. Second, the affected firms have now three options: i) hire with the subsidy with lower wage costs but fixed administrative costs, ii) hire without the subsidy or iii) not hire. Dropping the management costs for simplicity of notation, the labor costs with the subsidy are $wl - a_i(swl + c^s)$. The firm uses the subsidy for hiring if $sw > c^s$. Formally, the firm profit can now be written:

$$\pi_i^*(l) = f_i(l) - wl + a_i(sw - c^s)b_i$$

where $a_i b_i$ defines take-up and $b_i = I(sw > c^s)$ is an indicator for the subsidy being larger than the costs. This translates to the conditions for hiring dependent on wage costs and the administrative costs:

$$\begin{cases} f_i'(l) \geq (1-s)w + c^s & \text{if } sw > c^s \text{ and } a_i = 1 \\ f_i'(l) \geq w & \text{if } sw \leq c^s \text{ or } a_i = 0. \end{cases} \tag{9}$$

Next, I first consider the implications of the administrative costs by assuming $a_i = 1$ and, second, the implications of imperfect awareness by assuming $c^s = 0$. Finally, I conclude with a short note on the budget considerations related to the take-up.

First, the administrative costs reduce the effectiveness of the subsidy by reducing hirings by firms further from the margin of becoming an employer. The effects are clear from the conditions in 9 by assuming $a_i = 1$. Including the administrative costs to equation 6, the increase in hirings is $D(w) - D((1-s)w + c^s) < D(w) - D((1-s)w)$ so that firms closer to the margin of hiring become employers. However, the administrative costs cause firms further from the margin to abstain from hiring compared to the case without administrative costs. Restricting the subsidy to employment contracts with at least 25 hours per week further reduces the take-up of firms further from the margin of becoming an employer, if these firms consider hiring for a lower amount of weekly hours. This is because the costs of using the subsidy include the opportunity costs of hiring for at least 25 hours per week.

The simplified model implies either no effect with a zero take-up rate or full take-up with reduced effectiveness and efficiency of the subsidy. Firm differences in take-up, however, may arise if there are differences between firms in wage levels, planned work hours or hiring frictions. First, higher wage or work hours increase the paid subsidy directly, increasing the likelihood of take-up. Second, if the subsidy helps the firms with frictions in hiring to have employment closer to the optimum, their

benefits of the subsidy exceed the amount of paid subsidy. Consequently, the take-up rates should be for firms with higher hiring frictions.

The administrative costs decrease efficiency more than effectiveness because they decrease the effect on becoming an employer but do not reduce the deadweight spending. That is unless the costs of using the subsidy are large enough so that mainly firms with hiring frictions take-up the subsidy. This is because at the baseline model without frictions, only hirings by firms below the margin of becoming an employer are reduced, thus, the deadweight spending does not change. However, the efficiency may improve if the take-up of firms above the margin of becoming an employer without the subsidy is reduced, but firms with hiring frictions use the subsidy.

Secondly, the effect of lack of awareness or proneness to use the subsidy depends on the joint distribution of a_i and $f_i'(l)$: Positive (negative) correlation between the awareness and firm productivity reduces (increases) the efficiency of the subsidy compared to the case of full awareness. Let $\bar{a} = E(a_i)$ be the probability that $a_i = 1$, $\bar{a}_h$ stand for the awareness rate (or take-up rate without administrative costs) for those above the margin of becomine an employer without the subsidy i.e. $f_i'(l) \geq w$, and $\bar{a}_l$ for those below the margin i.e. those with $f_i'(l) < w$. Then, the average effect of the subsidy on the probability of becoming an employer is

$$\bar{a}_l(D(w) - D((1-s)w) \tag{10}$$

and the deadweight spending is

$$\bar{a}_h(1 - D(w)) * sw. \tag{11}$$

Consider randomly distributed awareness of the subsidy so that a_i and $f_i'(l)$ are independent, and $\bar{a}_h = \bar{a}_l = \bar{a}$. In this case, imperfect awareness simply reduces effectiveness and deadweight spending similarly by $(1-\bar{a})$. This happens if awareness is randomly distributed so that a flat fraction $\bar{a} = E(a_i)$ of firms are affected by the subsidy. If awareness of the subsidy is positively correlated with labor productivity $f_i'(l)$ $\bar{a}_h > \bar{a} > \bar{a}_l$ the effectiveness is reduced by imperfect take-up more than the deadweight spending. This results in lower efficiency and take-up that is correlated with the probability of becoming an employer. The opposite holds for negative correlation with awareness and labor productivity.

There are thee important differences in the implications of low take-up due to lack of awareness not associated with firm characteristics rather than due to administrative costs. First, lower take-up reduces only effectiveness but not efficiency of the subsidy. Second, the empirical prediction of the subsidy take-up is different, as there should be no differences related to firm characteristics in using the subsidy, unless the characteristics are related to the awareness of the subsidy. Third, the effect on the probability of becoming an employer under the assumption of full take-up of the subsidy can be calculated using the estimated average treatment effect ATE , that is the effect on the probability of becoming an employer, and equation 10 so that the average effect on the *treated* $ATET$ is:

$$ATET = \frac{ATE}{\bar{a}} \tag{12}$$

where $\bar{a}(D(w) - D((1-s)) = ATE$ and $D(w) - D((1-s) = ATE$.

However, it is plausible that the awareness is associated with labor productivity and, hence, the

probability of hiring without the subsidy. In this case, the differences in the take-up are explained by differences in the baseline probability to hire. For example, positive correlation may arise if the awareness of the subsidy is correlated with entrepreneur ability or higher productivity firms are more familiar with government programs, or information of the subsidy is endogenous so that firms that are willing to become employers search for information on hiring subsidies. On the other hand, negative correlation may arise if firms below the margin of hiring are more likely to search for information on subsidies or the government officials target information to firms they consider to need it more.

Lastly, while not directly relevant for effectiveness or efficiency, imperfect take-up reduces the total spending on the policy which can make it difficult to predict government spending on the subsidy program. This is of special concern if there are fiscal policy concerns related to the policy, as reduced spending simply implies lower fiscal effects. In addition, in practice the government has a budget for a policy that cannot be used for other – potentially more beneficial – policies during the budget period. Consequently, problems can arise if the predicted spending i.e. the budget differs from the actual spending because the take-up differs from the predicted take-up. The total government spending on the subsidy is

$$n\{[\bar{a}_l(D(w) - D((1-s)w)sw + \bar{a}_h(1 - D(w)) * sw]\bar{b}\}$$

where $\bar{b}$ is the fraction of firms with $sw > c^s$ and $f_i(l) \geq (1-s)w + c^s$, total take-up fraction is $\bar{a}\bar{b}$ and n is the number of treated firms. If the government assumes full take-up but the take-up is, for example, half, the government overestimates the spending to be twice the actual spending. The difficulty arises due to predictability of take-up – if the take-up is known in advance, the problems do not arise.

3.1.4 Empirical Predictions

The model shows that costs of becoming an employer, and awareness and administrative costs of the subsidy affect the effectiveness and efficiency of the first employee subsidy. Here, I discuss the empirical predictions of the theoretical model and how to relate them to observed firm behavior. Of importance are the causal effect on the eligible and subsidized firms, and the take-up rate of the subsidy among the firms that became employers.

First, the subsidy causes an *increase in the average probability of becoming an employer.* The average effect is determined by the distribution of labor productivity or, more precisely, the mass of firms close to the margin of becoming an employer, the costs of becoming an employer, and the take-up rate. Formally it can be written:

$$\bar{ab}[D(w+m) - D((1-s)w + m + c^s)] = \text{increase in the probability to become an employer} \quad (13)$$

where $\bar{ab}$ is the take-up rate i.e. the fraction of firms that used the subsidy of the firms that became employers.

Second, the subsidy has a *possible increase in long-term employment on the subsidized firms* if there are frictions or costs in becoming an employer. As the subsidy only affects the incentives of the first employee in the first two years, long-term effects on employment or effects surpassing the first employee are a sign of management costs or frictions in becoming an employer. In this case, the subsidy can increase efficiency, as the welfare increase due to job creation can surpass the costs of the policy.

Third, *imperfect take-up* of the subsidy *among firms that become employers* is a sign of imperfect awareness or (administrative) costs of using the subsidy. Comparing the subsidized firms to firms that became employers without the subsidy can shed light on the reasons for imperfect take-up.[14]

If imperfect take-up is due to *costs of using the subsidy*, the *subsidized firms have larger wage costs* of the first employee than firms that hired without the subsidy. This is because the costs cause only firms closer to the margin of becoming an employer to use the subsidy compared to a subsidy without costs. Also, higher planned work hours increase the expected subsidy. The restriction of the subsidy to full-time employees with permanent contracts further emphasizes this. In addition, take-up is associated with higher wage levels as this increases the received subsidy. Consequently, positive association between wages reflected in, for example, industry wage levels and take-up implies costs of using the subsidy as a reason for imperfect take-up. The model also predicts full take-up of firms with the subsidy amount above the costs of using the subsidy. Therefore, imperfect take-up among firms that have sufficiently high wage costs[15] is an indication that costs of using the subsidy do not fully explain the imperfect take-up.

On the other hand, if the imperfect take-up is due to *lack of awareness of the subsidy* using the subsidy is related to the information – whether it is random between firms or associated with firm characteristics. If awareness is not related to the firm's probability of becoming an employer without the subsidy, the take-up rate can be considered as the awareness rate and the effect of the subsidy under the assumption of "full awareness", i.e., the average effect on the treated can be calculated as in equation 12. In this case, the efficiency of the subsidy is unaffected by the imperfect take-up. Awareness of the subsidy cannot be observed by the researcher, but the awareness can be associated with institutional aspects. For example, the ELY centres that administer the subsidy can have differences in how effectively they inform firms about the subsidy, creating geographical differences in awareness. Differences between ELY centres in practicalities related to the subsidy can arise, for example, due to different fractions of subsidy municipalities in their area. Firm location and characteristics can also be associated with firm interactions with the ELY centre that can bring about differences in the take-up between firms.

However, the awareness and, consequently, take-up of the subsidy can be associated with firm *productivity* through the ability of the entrepreneur. In this case take-up of the subsidy is associated with firm productivity, or characteristics associated with productivity, and the baseline probability of becoming an employer – rather than only wage costs or differences in awareness due to institutional aspects. This also reduces the efficiency of the subsidy, as it reduces the take-up by firms that would not have become employers without the subsidy. The opposite holds if awareness is negatively associated with firm productivity.

Finally, *take-up can be associated with firm specific frictions in becoming an employer* if there are costs of using the subsidy. This is because firms with larger frictions gain additional benefits by using the subsidy, if it allows them to hire labor, reducing the subsidy amount required to apply for the

[14] It should be noted that the "total" take-up of the subsidy as measured by the number of subsidized firms divided by the number of non-employer firms is the result of the probability of becoming an employer (also affected by the subsidy) and the take-up rate among those that become employers and as itself does not reflect the *compliance* of the treatment, as firms that do not become employers are treated in the sense that they could use the subsidy if they become employers i.e. their incentives are treated by the subsidy. Only imperfect take-up among new employers reflects non-compliance.

[15] Sufficiently high meaning that their subsidy amount (in expectation) is higher than the (expected) cost of using the subsidy.

subsidy. Endogenous costly awareness of the subsidy, where firms have opportunity costs of searching for information on employment subsidies, can cause similar association. In this case, the take-up should be associated with firm characteristics associated with hiring frictions. For example, new firms, firms with lower assets, and firms with no previous experience of being an employer could have higher frictions in becoming an employer. In this case, the subsidized firms have higher long-term growth than firms that become employers without the subsidy.

Especially, consider a model of rational firms with full awareness of the subsidy, costs of using the subsidy and no frictions in becoming an employer. In this benchmark case, the subsidy only increases employment in the subsidized firms temporarily – for the duration of the subsidy. In addition, the take-up of the subsidy can be below one but there should be a clear threshold such that firms with wage costs above that threshold take-up the subsidy, and the subsidy amount at these wage costs reflect the costs of using the subsidy.

3.2 Additional Incentives and Effects

In addition to affecting the decision to become an employer, the subsidy can affect firm entry and exit, job formalization or have unintended effects if firms strategically alter their behavior to qualify for the subsidy. The potential entry and exit decision should be accounted for in the empirical strategy but the strategic incentives mostly affect the interpretation of the results.

The first employee subsidy can increase entry of firms that plan to use labor, as it decreases the costs of becoming an employer. Naturally, it does not affect the incentives of potential entrepreneurs who never plan to use external labor. The subsidy can also decrease firm exits of those that use or plan to use the subsidy.

The improved incentives to become an employer can, then, increase the number of firms observed in the data that has two implications for the empirical analysis. First, the obvious implication is that the effect on the number of firms, employer firms an new firms is worth studying. Second, because the subsidy can change the composition of the non-employer firms, the estimation of the effect on the probability of becoming an employer should be restricted to the existing non-employer firms. For example, if the increase in hirings by the existing non-employers is offset by an increase in the number of new non-employers, there is no observed average effect on the probability of becoming an employer, even though some firms are affected. On the other hand, if the subsidy attracts new employer firms to enter the markets, the probability of being a new employer increases due to the new firms but the existing non-employer firms may not respond. Similarly, the effect is not precisely identified if the subsidy affects exit decisions of firms. Consequently, the identification of the effect on becoming an employer has to account for this composition change due to firms entry and exit incentives.

In addition to encouraging job creation, the subsidy encourages job formalization or creation of formal jobs at the expense of informal jobs, thus, the observed effect should be strictly interpreted as changes in formal job creation. This is because the subsidy decreases the costs of only formal employment. While Finland is a country with a highly developed tax system and a small informal economy, the self-employed are infamous for high tax evasion rates even in developed tax systems[16]. Therefore, job formalization is a possible effect of the subsidy, but distinguishing between real job creation and formalizing jobs is beyond the scope of this paper.

[16] see e.g. Kleven *et al.* (2011)

The first employee subsidy could, of course, like any other subsidy, have some unintended effects if firms strategically or deceitfully take advantage of the subsidy. For example, firms may remain as non-employers longer to meet the criteria of 12 months of being an non-employer to qualify for the subsidy. It could be profitable to split up companies or create many firms instead of one to hire the first employee in many companies. A solo self-employed person may hire a family member just on paper to pocket the subsidy and keep the income in the same household. The subsidy could have spillovers to firms ineligible for the subsidy, but considering the size of the subsidized businesses, these are arguably negligible.

4 Effect of the Subsidy

This section estimates the average effect of the subsidy on the probability of becoming an employer defined in equation 13. According to the results, the subsidy had a precisely estimated zero effect on the probability of becoming an employer. To study the second question on whether the subsidy had an effect on the number of employees, growth of the affected firms or entry of new firms, I study additional outcomes such as employment and turnover and number of firms at the aggregate level again finding zero effect. Consequently, the subsidy did not on average seem to be effective at increasing employment or firm growth in non-employer firms. However, as discussed above, the average effectiveness depends on the take-up of the subsidy.

First, section 4.1 discusses the identification strategy. Then, section 4.2 presents the results of the baseline estimation of the effect on the existing non-employer firms. Lastly, section 5 summarizes the results of the further analysis on the effect on firm entry and additional outcomes, and robustness.

4.1 Identification Strategy

I use a standard difference-in-differences method to estimate the effect of the subsidy exploiting the regional criteria and the timing for the subsidy. Firms in the area without the subsidy are controls for firms in the subsidy area. The effect can be estimated through a regression model:

$$Y_{it} = \alpha + \gamma D_{it}^{AREA} + \lambda D_{it}^{PERIOD} + \delta(D_{it}^{AREA} * D_{it}^{PERIOD}) + X_{it}^{'}\beta + \epsilon_{it} \tag{14}$$

for firm i in period t, whereY_{it} is the dependent variable e.g. the indicator for being an employer or number of employees, 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 and X_{it} is a vector of additional covariates. Given that the difference-in-differences assumptions hold, δ estimates the average treatment effect of the subsidy. Standard errors are clustered at the municipality level, as that is the level of the treatment assignment.

The difference-in-differences estimator equals the change in the difference between the areas that corresponds to the effect of the subsidy if the parallel trends assumption holds. The parallel trends assumption means that the areas would have developed similarly without the subsidy. In other words, the firms outside of the subsidy area are a valid counterfactual for the firms in the subsidy area.

Because the subsidy was partly targeted at economically weaker areas, I improve the comparability between the subsidy and control areas by restricting the analysis to the border of the subsidy area.

Hence, the treatment area includes the municipalities with the subsidy that have a neighboring municipality without the subsidy, and the control area is the neighbor 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 and strongest areas. Because the subsidy only affects a small fraction of jobs in the labor market, significant spillover effects are not likely. Therefore, using the border area is feasible in this study. Figure 4 depicts the treatment and control areas as defined here.

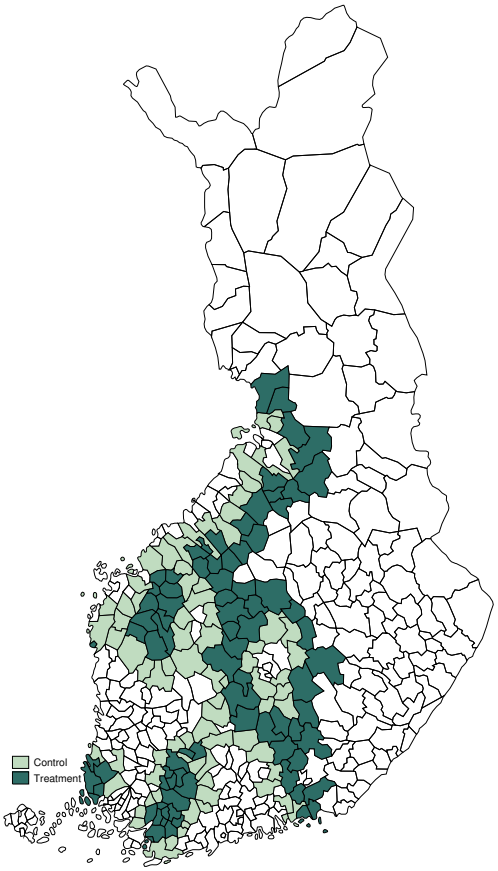

Figure 4: Treatment and control areas in the research

Notes: Map drawn using the municipality borders from 2007. The treatment area 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. Municipality mergers between 2007 and 2011 have changed the municipality borders in the treatment and control areas, in which case the prevailing municipality borders define the treatment and control areas while the map uses the 2007 municipality borders. Most of the treatment municipalities had the subsidy available from 2008 but some from 2009 and 2010. The treatment period for the control municipalities is defined according to the neighboring treatment municipality.

The parallel trends assumption seems plausible as the firms in the treatment and control areas have similar characteristics and pre-treatment trends as well as very few differences in the local labor market conditions. To assess the pre-treatment trends and describe the firms, I use a sample of the firms relevant for the subsidy by including only firms that are non-employers in some year. In addition, I exclude firms that have more than fifty employee because there are a few very large firms that can

Table 2: Descriptive statistics of firms and municipality labor market by area in 2006

	Subsidy	Treatment	No subsidy	Control	All Finland
Non-employer	.74	.75	.77	.76	.76
	(.44)	(.43)	(.42)	(.43)	(.43)
Turnover	91,291	92,867	101,839	87,343	98,299
	(433,446)	(453,887)	(1,409,740)	(552,165)	(1,153,059)
Employees	.73	.68	.66	.65	.68
	(2.4)	(2.4)	(2.4)	(2.2)	(2.4)
Wage Costs	6,213	6,061	6,382	5,548	6,306
	(35,030)	(33,397)	(36,515)	(28,899)	(35,928)
Net Assets	38,310	31,411	49,628	34,888	45,835
	(766,785)	(249,681)	(1,100,829)	(462,664)	(988,433)
Profit	16,334	16,066	17,826	17,207	17,292
	(62,623)	(38,800)	(99,991)	(61,288)	(88,584)
Observations	50,983	18,825	90,362	20,840	142,822
Municipality level statistics:					
Municipal Employment share	.86	.86	.84	.85	.85
	(.032)	(.033)	(.046)	(.046)	(.041)
Municipal unemployment rate	13	10	8.4	9	10
	(4.4)	(3.7)	(2.8)	(2.7)	(4.2)
Municipal 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 turnover 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.

have a considerable effect on averages and trends.

First, Table 2 shows that the firms and municipalities in the treatment and control areas are similar and that the restriction to the border areas improves the comparability between the areas. Table 2 presents the descriptive statistics of the firms and municipalities in 2006 in the subsidy area in the first column, the no subsidy area in the third, and the treatment and control areas as defined above in the second and fourth columns, and all of Finland (including the capital area) in the fifth column. The share of non-employer firms is similar in all of the areas but the firms tend to be, on average, larger in all of Finland and, the firms in the treatment and control areas more similar in size. There are larger differences in the municipal level statistics but, again, the control and treatment areas are similar. Employment rates are similar but unemployment was 13% in the subsidy area while 8.4 in the no subsidy area. Restriction to the border area improves this as the treatment area had an unemployment rate of 10% and the control area 9% – both close to the national average of 10%. It can be seen from Table 2 that the municipalities in the treatment and control areas are also of similar size with population of 6,700 compared to 8,600 in the subsidy area and 12,000 in the no subsidy area.

Second, Figure 5 shows that there are no significant differences in the firm trends between the areas before the subsidy period starting in 2008. The figure depicts the trends of employment, wage costs, (log of) turnover and share of new employers (defined as having at least one employee but zero employees in the previous year) by plotting the yearly changes compared to year 2006 by treatment status, and the change in the difference compared to 2006 with 95% confidence intervals.

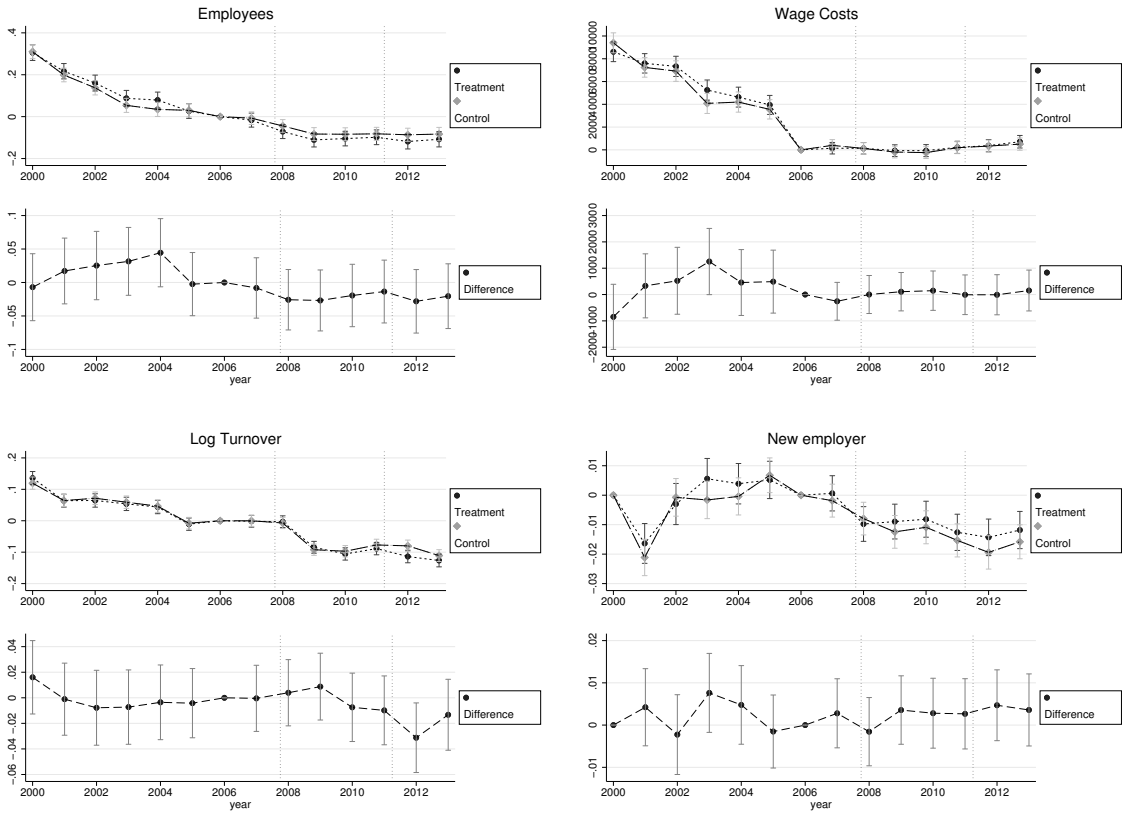

Figure 5: Firm trends in the research area

Notes: The figures plot the yearly dummy coefficients relative to 2006 separately for the treatment and control area in the upper panel and the annual difference-in-difference coefficients relative to 2006 in the lower panel for the outcomes (number of) employees, wage costs, log turnover 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 the most 50 employees with a non-zero turnover that have a zero number of employees in some years between 2000 and 2013. The treatment and control areas are as defined in Figure 4. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighbor municipalities.

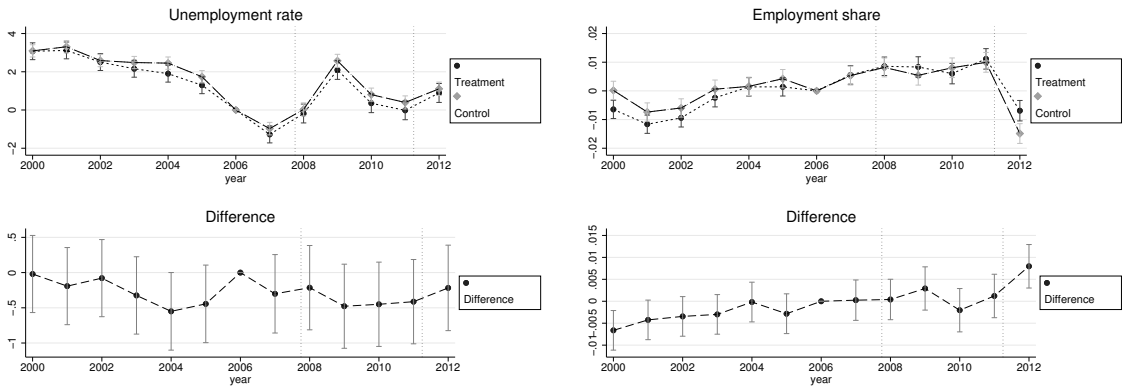

Figure 6: Labor market trends at the municipality level

Notes: The figures plot the year dummy coefficients relative to 2006 separately for the treatment and control area in the upper panel and the annual difference-in-difference coefficients relative to 2006 in the lower panel for the outcomes of the municipal employment rate and employment share. The treatment and control areas are as defined in Figure 4. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighboring municipalities.

Figure 6 plots the trends of the local labor market trends in the treatment and control area. There are no significant differences in the trends of unemployment but the employment percentage grows slightly faster in the treatment area through the whole observation period.

There are no other simultaneous policy changes that threaten the parallel trends assumption. There are no other important changes in policy related to hiring incentives during the time period and, as already mentioned in section 2.1, the subsidy area was defined separately for this subsidy so that there are no other policies targeted according to the same exact geographical area. The only related policy change was the payroll tax exemption from 2003 that affected some of the municipalities in the subsidy area. These municipalities are excluded from the analysis as a result of the restriction to the border areas and the reform started well before the subsidy period.

Additional assumptions for the difference-in-differences method are treatment exogeneity and lack of spillover effects that are not likely to cause problems. First, because the first employee subsidy affects only a small fraction of employees in the labor market, it is not likely to have an effect on the local labor market that could affect the hiring of competitive firms in the treatment or control area. Second, the treatment is not strictly exogenous as a firm can move to the treatment area to be eligible for the subsidy but this is not common according to the data. The number of firms that were in the no subsidy area in 2006 but had relocated to the subsidy area by 2009 is 1,274 and almost the same number of firms – 1,269 – did an opposite relocation away from the subsidy area. In the border area the numbers are 541 and 514 respectively showing a slightly larger difference. In addition, only 31 of the 1,349 subsidized firms moved to the subsidy area during the subsidy period – it is less than the number of subsidized firms (39) that moved to the subsidy area in the five years before the subsidy period, and in both periods the ratio of movers to and from the subsidy area is the same. Consequently, moving to the subsidy area does not seem to cause problems to the identification method.

4.2 Baseline Results

I estimate the effect of the subsidy on the probability of being an employer for the firms that had zero employees in the year before the subsidy, by using the four year period before the subsidy as a reference period. The effect is precisely estimated to be zero but the coefficient on new firms is larger and less precise. The results are robust between different ordinary least squares and fixed effects models.

I estimate the effect on the firms that had zero employees in the year before the subsidy, in year 2007, to restrict the analysis to firms that are at risk of becoming an employer and to avoid selection bias. The definition for being a non-employer has to be defined before the subsidy because including firms according to their employership status during the subsidy leads to a selection bias: Firms that become employers due to the subsidy leave the non-employer sample, leaving firms less likely to hire in the sample of non-employer firms in the susequent years in the subsidy area but not in the control area. Consequently, the population of non-employer firms between the treatment and control areas differ after the subsidy period starts if the subsidy has an effect on the probability of becoming an employer. Fixing the target population before the start of the subsidy removes this problem.

To estimate the change in the probability, I define the pre-treatment period as the four years before the subsidy period i.e. 2004–2007, during this period the firms that had zero employees in the preceding year are included. To simplify, I only include the municipalities where the subsidy came into effect in 2008, that account for the majority of the treated municipalities.

The outcome variable is a dummy for being an employer in the first, or by the second, third and fourth year, to estimate separately the effect on becoming an employer over time by the time passed. I conduct regressions with five different specifications: OLS and fixed effects models with additional covariates. The standard errors are clustered at the municipality level, that is, at the level of the treatment assignment.

Table 3 summarizes the regression results. The results do not suggest the subsidy had an effect on becoming an employer during the time the subsidy was available. The estimated effect of the subsidy is close to zero with estimates ranging from -0.8 to 1.6 percentage points depending on the model specification and time after the subsidy, with small standard errors. In the preferred fixed effects model with additional covariates, the largest effect is on becoming an employer by the second year at 0.47 ppt and smallest by the fourth year at -0.5 ppt. None of the coefficients are statistically significant at the 5% level.

To test whether the result is driven by a large fraction of firms that never become employers, I estimate the effect separately for types of firms: Partnerships and corporations i.e. non sole proprietor firms, sole proprietors, new firms defined as appearing in the data for the first year, and firms that have at least 8,500€ in turnover that is the threshold for VAT reporting. These restrictions exclude about 60%, 40%, 93% and 33 of firms respectively. The non sole proprietors and new firms are selected due to empirical evidence by Fairlie & Miranda (2016) suggesting that new companies and non sole proprietorships are more likely to hire, which is also supported by the data. The VAT liability threshold simply removes the smallest firms with limited business activity that are unlikely to become employers. The firms are defined to belong to the category in the year before the subsidy so that, for example, exceeding the turnover threshold is only assessed in 2007 (or 2003 for the pre-period) and the firms is then included in the subsequent years.

Table 4 summarizes the results by subgroups. The presented estimates are estimated using the

Table 3: Estimated effect on the probability of becoming an employer

	(1) OLS	(2) OLS extended	(3) FE	(4) FE extended
By 1st year				
Treatment Effect	−0.00439	−0.00236	−0.00797	−0.00228
	(0.00474)	(0.00606)	(0.00518)	(0.00707)
Observations	49,141	36,490	49,141	36,490
Adjusted R^2	0.002	0.025	0.000	0.003
By 2nd year				
Treatment Effect	0.00657	0.00911	−0.00338	0.00470
	(0.00569)	(0.00753)	(0.00603)	(0.00862)
Observations	46,273	32,054	46,273	32,054
Adjusted R^2	0.003	0.039	0.000	0.002
By 3rd year				
Treatment Effect	0.0130	0.0156	−0.000581	−0.000270
	(0.00692)	(0.00820)	(0.00673)	(0.00869)
Observations	41,989	34,052	41,989	34,052
Adjusted R^2	0.004	0.050	0.000	0.004
By 4th year				
Treatment Effect	0.00519	0.0102	−0.00412	−0.00500
	(0.00816)	(0.00946)	(0.00734)	(0.00867)
Industry	No	Yes	No	Yes
Net assets	No	Yes	No	Yes
Company form	No	Yes	No	Yes
Municipality employment share	No	Yes	No	Yes
Observations	39,665	32,356	39,665	32,356
Adjusted R^2	0.004	0.058	0.007	0.012

Standard errors in parentheses

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

Notes: Table presents DID regression coefficients using 4 different models: (1) OLS with only the subsidy area and period dummies, (2) OLS extendedwith additional firm covariates, (3) FE with subsidy area and period dummies controlling for firm fixed effects and (4) FE extended with firm fixed effects and covariates. The dependent variable is a dummy for having positive number of employees in some year by the first, second, third and fourth year. The sample includes active (non-zero turnover) firms 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 treatment area is defined as in 4 but excluding municipalities that were added to the subsidy area in 2009 and 2010.

Table 4: Estimated effect on the probability of becoming an employer in different subsamples

	(1) All	(2) Partnership & Limited	(3) Sole proprietor	(4) New	(5) VAT liable
By 1st year					
Treatment Effect	−0.00228	−0.00378	−0.00190	0.0212	0.000376
	(0.00707)	(0.0148)	(0.00778)	(0.0284)	(0.00770)
Observations	36,490	13,161	23,329	2,642	29,167
Adjusted R^2	0.003	0.006	0.005	0.068	0.004
By 2nd year					
Treatment Effect	0.00470	0.0110	−0.000976	0.0532	0.00295
	(0.00862)	(0.0171)	(0.0105)	(0.0367)	(0.00999)
Observations	32,054	11,838	20,216	2,242	25,889
Adjusted R^2	0.002	0.006	0.006	0.069	0.004
By 3rd year					
Treatment Effect	−0.000270	0.00887	−0.00777	0.0508	0.00349
	(0.00869)	(0.0172)	(0.00937)	(0.0340)	(0.0100)
Observations	34,052	12,089	21,963	2,400	27,997
Adjusted R^2	0.004	0.008	0.005	0.102	0.005
By 4th year					
Treatment Effect	−0.00500	0.0211	−0.0203*	0.0485	−0.00127
	(0.00867)	(0.0174)	(0.00962)	(0.0366)	(0.0100)
Firm fe	Yes	Yes	Yes	No	Yes
Industry	Yes	Yes	Yes	Yes	Yes
Net assets	Yes	Yes	Yes	Yes	Yes
Company form	Yes	Yes	Yes	Yes	Yes
Municipality employment share	Yes	Yes	Yes	Yes	Yes
Observations	32,356	11,342	21,014	2,259	26,561
Adjusted R^2	0.012	0.014	0.013	0.108	0.012

Standard errors in parentheses

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

Notes: Table presents DID regression coefficients estimated separately for subgroups of firms. The dependent variable is a dummy for having positive number of employees in some year by the first, second, third and fourth year. The sample includes active (non-zero turnover) firms 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 treatment area is defined as in 4 but excluding municipalities that were added to the subsidy area in 2009 and 2010. Partnerships and limited includes firms with company form of partnerships and limited corporations. New firms include firms that are at most 1 year old i.e. appear in the data for the first time in 2003 or 2007, VAT-liable includes firms with turnover of more than 8,500€ in 2003 or 2007.

fixed effects model with firm covariates, except for the new firms that only have one observation for which effects are estimated using OLS with firm covariates. None of the estimates are, again, statistically significant at the five per cent significance level. The estimated effect on partnerships and corporations is larger than on all firms after the first year and mostly of similar magnitude. The effect on becoming an employer by the fourth year is larger at 2.1 percentage points. The estimated effect on sole proprietors is consistently negative, small and even decreasing so that the estimated effect by the fourth year is -2 percentage points. The estimates on VAT liable firms are close to zero with alternating signs and magnitudes less than 0.5 ppt.

However, the estimated effect on new firms is larger ranging from 2.1 ppt in the first year to 5.3 ppt by the second year, but the standard errors are larger due to the smaller number of new firms. The estimated effect of 5.3 percentage points implies an increase in hiring probability of 68% relative to the mean of becoming an employer so the estimates are consistent large effects. However, as the estimates are not statistically significant these results are inconclusive but may indicate a larger effect on new firms.

I also estimate the effect on additional firm outcomes: the probability of being an employer, employment, wage costs and turnover. These results depict how the subsidy affects the average employer share, employment, wage costs and turnover in the eligible firms during the treatment period. The results are summarized in Table 5. Again, the estimates are mostly small and none of them are statistically significant at the five per cent significance level. The estimated effect on average employer share is -0.3 ppt and on employment 0.01. The estimated effects on wage costs and turnover of new firms are quantitatively large at $6,880$€ and 17% respectively, but due to smaller sample size, the estimates are not precise.

Additionally, I estimate the effect on the probability of becoming an employer using a discrete time proportional hazards duration model. The approach can account for the duration of being a non-employer, an effect dependent on the duration, and can use more data, as firms that are non-employers in any year are included. However, the approach may suffer from the composition change after the subsidy period, as the subsidy can affect the type of entering new employer and non-employer firms differently between the treatment and control areas. The results from this approach are consistent with the results above – the estimated effect is precisely estimated and not statistically significantly different from zero. The estimated effect on the probability of becoming an employer is 2% for all firms that at the baseline of 7.8 probability corresponds to a 0.16 percentage point increase. These results are presented in the appendix.

4.3 Further Results And Robustness

Additionally, I estimate the effect on firm entry and exit, on a wider variety of firm outcomes, using a wider sample of firms and using the whole of Finland instead of the border areas, and find no effect on any of the studied outcomes. I also assess the parallel trends assumption using placebo regressions and the assumption does not seem to be violated. These results are included in the appendix.

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). There is no statistically significant effect on the number of firms, non-employer firms or employer firms.

I use a wider sample of firms to estimate the effect on multiple firm outcomes in addition to

Table 5: Estimated effect on additional firm outcomes

	(1) All	(2) Partnership & Limited	(3) Sole proprietor	(4) New	(5) VAT liable
Employer					
Treatment Effect	−0.00328	−0.00526	−0.00250	−0.0479	−0.00244
	(0.00375)	(0.00781)	(0.00416)	(0.106)	(0.00453)
Observations	134,952	48,430	86,522	14,918	109,614
Adjusted R^2	0.010	0.012	0.008	0.005	0.010
Employment					
Treatment Effect	0.00951	0.0179	0.000405	−0.591	0.00869
	(0.0136)	(0.0353)	(0.00668)	(0.571)	(0.0144)
Observations	128,003	43,189	84,814	14,114	106,105
Adjusted R^2	0.011	0.015	0.010	0.034	0.011
Wage Costs					
Treatment Effect	138.6	207.5	63.43	6,877.2	205.4
	(169.4)	(400.1)	(79.21)	(7,237.1)	(191.9)
Observations	114,394	40,154	74,240	13,389	92,692
Adjusted R^2	0.012	0.016	0.010	0.013	0.013
Log Turnover					
Treatment Effect	0.0285*	0.0344	0.0297*	0.171	0.0137
	(0.0139)	(0.0291)	(0.0141)	(0.170)	(0.0126)
Year fe	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes
Net assets	Yes	Yes	Yes	Yes	Yes
Company form	Yes	Yes	Yes	Yes	Yes
Municipality employment share	Yes	Yes	Yes	Yes	Yes
Observations	128,161	43,316	84,845	14,149	106,256
Adjusted R^2	0.009	0.014	0.006	0.014	0.013

Standard errors in parentheses

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

Notes: Table presents DID regression coefficients estimated separately for subgroups of firms. The dependent variable is a dummy for having positive number of employees in some year by the first, second, third and fourth year. The sample includes active (non-zero turnover) firms 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 treatment area is defined as in 4 but excluding municipalities that were added to the subsidy area in 2009 and 2010. Partnerships and limited includes firms with company form of partnerships and limited corporations. New firms include firms that are at most 3 years old in 2003 or 2007, VAT-liable includes firms with turnover of more than 8,500€ in 2003 or 2007.

becoming an employer. Because restricting the analysis to firms with no employees in the yearly tax return data may drop some firms with only part-time or temporary employees in the previous year, use a sample of firms that had zero employees in some year. I run regressions with multiple firm level outcomes: employer status, employment, labor costs and turnover, finding no effect on any of the studied outcomes. These regressions are a counterpart to the trend figures presented in section 4.1.

I run placebo regressions by defining a placebo subsidy period before the actual subsidy period, to estimate whether there are differences in hiring rates before the subsidy period that would violate the parallel trends assumption. I do not find that the parallel trends assumption is violated.

5 Take-up and Subsidized Firms

This section describes the take-up of the subsidy, how the subsidized firms differ from firms that became employers without the subsidy, and the development of the subsidized firms. I document a low take-up of 2% by firms that became employers that is a plausible reason for the zero average effect. The low take-up seems to be only partly explained by the costs of using the subsidy and the restriction to full-time employees: The take-up rate is 6% among new full-time employers and 10–12% among firms with an ex post estimated subsidy of 6,000 to 20,000 euros. This suggests low awareness as an additional reason for the low take-up, which is supported by regional differences in the probability of using the subsidy that coincide with the larger take-up in regions with a greater percentage of eligible municipalities. Descriptive evidence on which firms take-up the subsidy, conditional on becoming an employer, suggests that the full-time restriction is an important factor in reducing the effectiveness and efficiency of the subsidy: Take-up is highly associated with firm size among all firms that became employers but the association is weak among new full-time employers . Finally, I find that, compared to similar firms with similar observed hiring behavior, the subsidized firms remained employers for longer and had persistently higher employment. This can reflect either a persistent effect on firm employment, that implies frictions in becoming an employer, or unobservable differences in the propensity to hire for a full-time employment contract.

The results show a puzzle: Despite the first employee subsidy seeming to be beneficial for the subsidized firms, a large majority of the eligible firms did not use the subsidy leaving thousands of euros on the table. Descriptive evidence suggests low awareness of the subsidy and costs of using the subsidy implied by the full-time restriction as significant factors explaining the low take-up. The low take-up is quantitatively important, as an awareness rate of 12% is consistent with a 0.5 percentage average effect that would mean a 4.2 effect on the firms aware of the subsidy. The remainder of this chapter, firstly, summarizes the subsidy take-up in firm groups with different levels of the subsidy and the implications of the take-up on effectiveness in section 5.1. Secondly, section 5.2 describes the subsidized firms in comparison to the firms that were eligible for the subsidy i.e. those that became employers, in order to study how using the subsidy is related to firm characteristics. Lastly, section 5.3 studies how the subsidized firms developed compared to similar firms with similar observed hiring behavior that did not use the subsidy.

5.1 Take-up

Here I study the take-up of the subsidy among new employer firms and new full-time employers and calculate the ex post subsidy amount based on the observed wage costs per employee in the firm. The calculated subsidy helps to assess the direct benefits of using the subsidy to the firm versus not using it, which can shed light on the role of the costs of using the subsidy in the observed take-up. According to the theoretical predictions summarized in section 3.1.4, the differences in take-up of the subsidy without frictions in becoming an employer should only reflect differences in the expected subsidy amount, with firms below a threshold selecting not to use the subsidy and firms above it to use the subsidy.

I define take-up of the subsidy as the fraction of eligible firms that used the subsidy i.e. those that were non-employers in the previous period and hired an employee, to separate under-using of the subsidy from the probability of becoming an employer. This is because I want to disentangle the take-up decision from the decision to become an employer. The reason for this is that I consider the effect of the incentives created by the subsidy and also firms that did not become employers are treated in the sense that the subsidy changed their incentives. For firms that did not become employers, the take-up corresponds to the fraction of firms that would have used the subsidy if they had decided to become employers which cannot be observed in the data. Therefore, the take-up meaningful for the effect of the incentives of the subsidy is only observed for the eligible group i.e. those that became employers.

Some caution is needed in interpreting the observed take-up rate and the number of identified eligible firms, because the actual eligibility for the subsidy cannot be exactly identified in the data. In fact, the observed eligibility may underestimate the number of firms that fulfil the subsidy criteria. The problem in defining the eligible firms arises because the data does not have variables that exactly correspond to the subsidy criteria, so that some of the actually subsidized firms – and actually eligible – do not belong to the group of eligible firms identified in the data for a number of reasons. First, the yearly data does not allow the identification of non-employer periods that do not last for a full tax year, thus a firm can receive the subsidy after being a non-employer for 12 months while still having a positive number of employees in the yearly data. In fact, many of the subsidized firms had employees in the previous year. Second, the subsidy was also admitted to entering firms that have no prior observations in the data. I do not include entry firms as I cannot separate which entered as non-employers and would be eligible for the subsidy.[17] Third is the possibility of mistakes in granting the subsidy. This means that the number of actually eligible firms may be larger than the number of eligible firms identified in the data. However, the percentage of firms that use the subsidy within the group of eligible firms as defined in the data should be informative on the take-up rate. The downside is that the take-up rates of some groups of firms that were more likely to hire, especially firms entering the market and those that had recently had employees, could differ from the eligible firms identified in the data and are ignored here.

In order to evaluate the firms' financial gains of using the subsidy, I use the subsidy rule and actualized wage costs per employee to calculate the ex post subsidy amount. The wage costs per employee are meant to proxy the wage costs to the first employee. To account for the possibility that

[17] The take-up rates are not largely affected by including the entry firms – only take-up rates of entry, full-time employers is significantly lower than of new full-time employers.

the employee contract can start at any point during the year, I assume the subsidized two years of the first employee to start in the middle of the first year. Consequently, I define the calculated subsidy as $0.3lc_t + 0.225lc_{t+1} + 0.075lc_{t+2}$ where lc_t stands for the wage costs per employee in the year of becoming an employer. This calculated subsidy is smaller than, but close to, the actual amount of the subsidy paid to the subsidized firms so that it seems to slightly underestimate the real subsidy. This is reasonable, as the first employee has the potential to earn a wage longer in the firm than employees hired later, creating larger wage costs. It should be noted, that this is an imperfect proxy for the expected subsidy amount that the firm bases its take-up decision on. The calculated subsidy can still provide a sensible benchmark for the benefits of the subsidy.

Table 6 summarizes the take-up rate, the calculated and paid subsidy, employment and wage costs of the subsidized firms and three different groups of new employer firms i.e. firms the became employers in the subsidy area during the subsidy period. The first column simply presents the statistics of the firms that used the subsidy. The second column includes the new employer firms or firms with positive employment and zero employees in the previous year. These firms could have used the subsidy if they hired the first employee for a full-time, permanent contract. In order to assess the take-up rate among new full-time employers, the second and fourth columns include subgroups of the new employer firms that are more likely to fulfill the requisite of hiring for a full-time permanent employment contract. Because the data does not include information on the employment contracts, the exact requisite cannot be identified. New effective employers in the third column are those that hired at least one effective employee that I define as the total wage costs divided by the median wage cost per employee.[18] New full-time employers are those that have at least one full-time employee in the data. This is the group of firms that most certainly fulfills the criteria for the subsidy but there are a few problems with the definition. First, the number of full-time employers is small because the variable for full-time employees is missing for most firm due to data limitations. Second, the full-time definition in the data does not correspond to the definition in the subsidy, because the subsidy program defines the full-time requisite specifically for the subsidy. Notably, some of the subsidized firms do not belong to any category of the eligible firms as defined here due to the problems in defining eligibility in the data, as explained above.

According to Table 6, the total take-up rate among the firms that became employers is 1.9% but is higher at 5.6% and 6.4% among the new effective employers and new full-time employers, respectively. The new full-time employers also have higher calculated and paid subsidies, number of employees, and wage costs than the new employers and the subsidized firms on average. The new effective employers have an calculated subsidy of $8,361$€, higher than the average calculated subsidy of the subsidized firms. and the new full-time employers $13,372$€, while the calculated subsidy of the new employers is only $2,646$€. However, Table 7 shows that among the new effective employers the calculated subsidy is similar for the subsidized firms and non-takers.

The higher take-up rate and calculated subsidy of the new effective and full-time employer firms indicates, that the restriction to full-time employees causes significant costs of using the subsidy, leading to a 67% decrease in the total take-up rate. However, administrative costs of using the subsidy do not seem to explain the take-up because the calculated subsidy is similar among the subsidized and non-taker new employer firms. This may be explained by the large calculated subsidy of about $8,000$€ that is roughly 25% of the wage costs in the first year – a substantial amount of money for a small

[18] Median wage costs per employee is $12,000$€

Table 6: Take-up of the subsidy

	(1) Subsidy takers	(2) New employers	(3) New effective employers	(4) New full-time employers
Subsidy takers (%)	100	1.9	5.6	6.4
	(0)	(14)	(23)	(24)
Paid subsidy	8,131	7,673	8,940	11,970
	(3,846)	(3,682)	(3,373)	(3,418)
Calculated subsidy	6,136	2,646	8,361	13,372
	(4,176)	(4,106)	(5,936)	(7,555)
Employees	2.4	1.7	2.3	4.5
	(5.8)	(3.1)	(6.1)	(14)
Wage costs	16,854	5,735	34,135	93,647
	(22,276)	(80,427)	(209,428)	(533,793)
Observations	1,011	18,556	2,598	393

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 the subsidized firms. Some of the subsidized firms do not belong to the group of eligible firms (=new employers) in the data so that numbers of the eligible firms and the subsidized firms are not completely related. Subsidy takers are the firms that used the subsidy and are in the tax return data. New employers have positive number of (external) employees and had zero employees in the previous year. New effective employers have at least one effective employee i.e. total labor costs above the median labor cost per employee and had zero employees in the previous year. New full-time employers had positive number of full-time employees (a variable not compulsory to report and with many missing values) and had zero employees in the previous year. 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.

Table 7: Subsidized vs not subsidized new effective 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	34,662
	(17,945)	(215,476)
Observations	145	2,453

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.

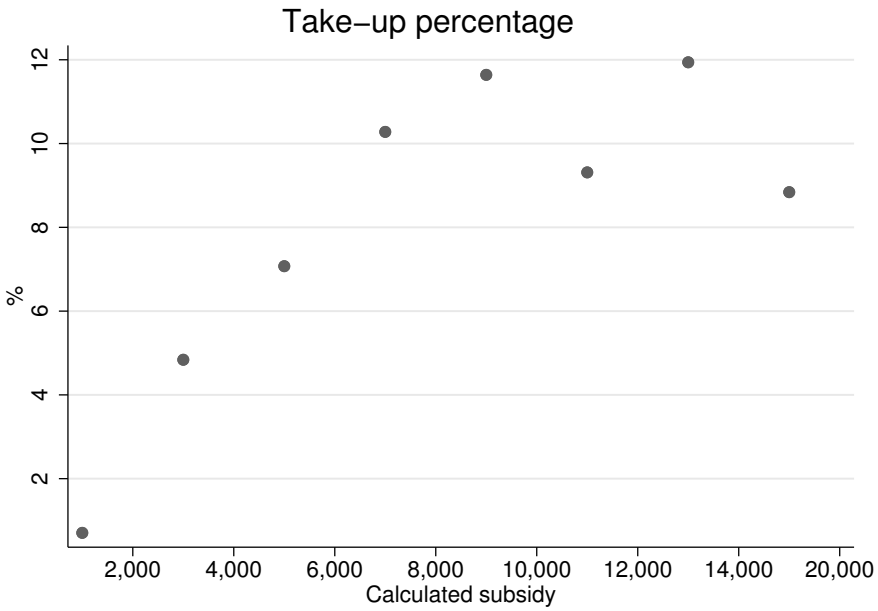

Figure 7: Take-up rate with respect to ex post calculated subsidy

Notes: Sample includes firms in the subsidy area during the subsidy period that had zero employees in the previous year, positive employment and calculated subsidy between 0 and 20,000 euros. Calculated subsidy is calculated 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.

firm.

Figure 7 further examines the relationship between the take-up and the subsidy amount, plotting the take-up percentage for intervals of the calculated subsidy amount. The take-up rate is close to zero for calculated subsidy less than $2,000€$ and increases in the subsidy amount until 6 to 10 thousand euros, after which the take-up rate remains fairly flat at 9 to 12 per cent. The positive association suggests that costs of using the subsidy are important for the lack of take-up for firms with smaller calculated subsidies. Nevertheless, the low, flat take-up of firms with calculated subsidy above $6,000€$ does not seem be solely explained by costs of using the subsidy.

As the take-up rate is low between 2 and 12 per cent in all the categories of new employer firms and even among firms with a large calculated subsidy, the evidence suggests that lack of information or awareness of the subsidy is an additional reason for the imperfect take-up. Evidence of a low awareness is also provided in a survey by Aaltonen *et al.* (2011) on the first employee subsidy that reports that only 30% of firms knew about the subsidy.

To put the take-up rates into context on how they affect the effectiveness of the subsidy, let us assume that the imperfect take-up is partly explained by randomly distributed awareness of the subsidy. First, the 2 take-up rate among the new employer firms can be considered as a lower bound on the awareness rate. Using equation 12 and the 0.5 percentage point effect on becoming an employer two years after the subsidy, the upper bound on the effect on the treated firms is 25 percentage points. Second, let us consider the 12% take-up rate as an upper bound on the awareness rates. This corresponds to assuming that the costs of using the subsidy are $8,000€$ and, consequently, explain

the lack of take-up among firms with a calculated subsidy lower than 8,000 euros, and the rest of the lack of take-up is due to low awareness of the subsidy. Then the implied lower bound on the effect on the treated firms is 4.2 percentage points. These naive calculations demonstrate, that 54% to 320% increases in the probability of becoming an employer are consistent with the estimated zero effect.

5.2 Subsidized Firms

Here I describe the subsidized firms relative to eligible firms that did not take-up the subsidy using a logistic regression, in order to address the reasons for the low take-up and how the take-up affects the efficiency of the subsidy. Although the evidence is descriptive and the small number of subsidized firms limits the analysis, the association between take-up and firm characteristics may shed light on the reasons for the low take-up, as predicted by the theory.

I study the association of take-up among the new employer firms with firm characteristics observed in the data: company form, firm age, size measured as turnover and net asset quartile in the previous year, an indicator for previous employer experience, industry and location. To specifically address the role of costs of using the subsidy and awareness in take-up, I consider differences in average wage costs per employee in a firm and regional differences in access to information.

First, take-up associated with higher wage costs per employee are an indication of costs of using the subsidy decreasing the take-up. I include firm level mean wage costs per employee (when the firm has positive employment) to proxy the expected subsidy benefit to the firm. Secondly, industry differences in the take-up can arise due to differences in wage levels, as industry specific minimum wages are centrally negotiated and generally binding in Finland. Third, alternatively to the industry fixed effects, I consider industry level mean wage costs per employee. The mean wage costs per employee reflects wage differences as well as amount of labor per employee, which both affect the subsidy benefits to a firm.

Second, differences in take-up rates according to firm location may reflect differences in awareness of the subsidy, or access to information, created by the institutional design. I consider two location measures: region and an indicator for the firm being located in an ELY centre municipality. First, the ELY centres that administer the subsidy are regional, thus, the differences between how well the agencies inform the firms about the subsidy can result in regional differences in the take-up rate. Regional differences in take-up rates can naturally result from many other reasons as the awareness rate. Consequently, I consider a more direct measure of regionalal differences awareness rate: the fraction of subsidy municipalities of the municipalities in the region. The regional subsidy municipality rate can result in regional differences in firm awareness, because the incentives and ability of the regional authorities in informing the firms about the subsidy can depend on it. In fact, there is substantial variation in the subsidy municipality rate from 3.7% to 100%. Second, being located in the ELY centre municipality may increase the firm's exposure to the subsidy, as it may be more likely to, for example, visit the centre. However, the ELY centre municipalities are regional centers so firms in these municipalities may differ in many respects from the firms in more rural areas.

Third, I address the role of the restriction to full-time employees on the take-up, by comparing the results using different groups of eligible firms: new employer firms and new "full-time" employer firms. The idea is, that if the full-time restriction is crucial in explaining the take-up, the take-up should be strongly associated with the wage costs per employee or firm size in the group of new employer firms

but not in the group of new full-time employers. The new employers would be eligible if they hired the first employee for a full-time contract, so the probability to take-up the subsidy is also associated with the probability of hiring on a full-time contract. The new full-time employers, on the other hand, should be eligible and get the subsidy if they just applied for it. Among them, the take-up should not be correlated with the probability of hiring on a full-time contract. As the probability of hiring on a full-time contract means higher wage costs per employee, the association of take-up with the wage costs should also be lower among the full-time employers.

I define the eligible firm groups in the data in three alternative ways. First, I use the new employers as above in section 5.1, to describe the take-up of the firms that became employers with any size of the employment contract. Of the firms that used the subsidy only 357 i.e. 36% belong to this group with a 1.9% take-up rate. This group of firms suffers from excluding many of the actually subsidized firms, leading to lower number of observations used in the estimation. As many firms that used the subsidy are not strictly non-employers in the previous year, I use the number of effective employees to measure becoming an employer. Consequently, the second eligible group includes firms that had less than 0.5 effective employees in the previous year and at least 1.5 effective employees in the following two years (defined as having the sum of wage costs in the two years above 1.5 times median wage cost per employee). Of the subsidized firms 43% or 437 belong to this group with a take-up rate of 3.9%. Third, the last group of eligible firms adds the entering firms (i.e. firms observed for the first time) that have at least 1.5 effective employees in the first two years to the second group of firms. This group includes the maximum number of the subsidized firms at 659 i.e. 70% and a take-up rate of 3.8%.

Table 8 presents the logistic regression results of the subsidy take-up in the three alternative eligibility groups. The table includes only selected region and industry coefficients depicting the largest and most relevant differences. In addition to the presented firm characteristics, year fixed effects and prior reported use of subsidies (as reported in the tax returns) are included in the regression but do not seem to be associated with take-up.

There are three key findings. First, the full-time restriction seems to be important in decreasing the take-up, which implies reduced effectiveness as well as efficiency. The efficiency is decreased because the full-time restriction targets the subsidy to firms larger and more likely to become employers at the baseline. This is supported by a strong association with the firm mean wage cost per employee and turnover quartile among the new employers, presented in the first column, but a much weaker association among the new effective employers, presented in the second to fourth columns. For example, the odds ratio of having mean wage cost per employee above $10,000$ decreases from about 9 to about 2 when changing the eligibility criteria to effective new employers and the odds ratio of having turnover in the 75th percentile decreases from 7.4 to 1.4 that is no longer statistically significant. Consequently, these results support the conclusion in section 5.1 above.

Second, there are no significant differences in the take-up between industries or industry mean wage costs per employee. Therefore, the results do not support that purely administrative costs among the firms that hire on a full-time contract area significant factor in explaining the take-up. This does not necessarily mean that administrative costs are not important – just that the subsidy is large enough to exceed the administrative costs that are not due to the full-time restriction.

Third, significant regional differences may point to a lack of awareness as an additional reason for the imperfect take-up, that reduces the average effectiveness. For example, the odds ratio among

Table 8: Logistic regression results of subsidy take-up

	(1)	(2)	(3)	(4)
Partnership	0.214**	0.166***	0.118***	0.122***
	(0.102)	(0.0715)	(0.0461)	(0.0469)
Corporation	0.401***	0.385***	0.376***	0.375***
	(0.0665)	(0.0545)	(0.0409)	(0.0388)
Construction	0.968	0.958	1.421	
	(0.722)	(0.583)	(0.845)	
Wholesale and Retail Trade	1.583	1.730	2.168	
	(1.189)	(1.059)	(1.293)	
Other service activities	2.315	3.072	4.076*	
	(1.751)	(1.911)	(2.474)	
ELY Center municipality	1.275	1.107	1.073	1.247*
	(0.173)	(0.133)	(0.103)	(0.110)
Pirkanmaa	1.133	1.436	1.359	
	(0.527)	(0.629)	(0.507)	
Central Finland	0.724	0.646	0.748	
	(0.307)	(0.287)	(0.268)	
South Ostrobothnia	1.716	3.056***	3.002***	
	(0.527)	(0.933)	(0.770)	
Central Ostrobothnia	0.702	3.572*	3.967***	
	(0.534)	(1.786)	(1.506)	
North Ostrobothnia	1.062	1.704	1.445	
	(0.403)	(0.614)	(0.436)	
New	1.601***	1.706***	1.670***	1.614***
	(0.221)	(0.203)	(0.195)	(0.187)
Previous Employer	0.584***	0.433***	0.446***	0.455***
	(0.0764)	(0.0489)	(0.0493)	(0.0495)
Net assets_25p	0.824	0.931	0.948	0.972
	(0.124)	(0.128)	(0.129)	(0.131)
Net asset_50p	0.828	0.970	0.988	0.987
	(0.123)	(0.130)	(0.129)	(0.127)
Net asset_75p	0.492**	0.460***	0.485***	0.483***
	(0.116)	(0.0989)	(0.102)	(0.101)
Turnover_25p	3.604***	0.993	0.984	1.089
	(1.076)	(0.239)	(0.234)	(0.256)
Turnover_50p	6.743***	1.372	1.320	1.335
	(2.064)	(0.317)	(0.299)	(0.297)
Turnover_75p	7.404***	1.405	1.418	1.467
	(2.501)	(0.369)	(0.364)	(0.369)
Mean wage cost per employee 4,000-10,000	5.413***	2.033***	1.818***	
	(0.796)	(0.259)	(0.189)	
Mean wage cost per employee 10,000-18,000	8.855***	2.186***	1.671***	
	(1.508)	(0.312)	(0.197)	
Mean wage cost per employee >18,000	7.549***	2.040***	1.401*	
	(1.776)	(0.392)	(0.215)	
Industry mean wage cost per employee _50p				1.053
				(0.0877)
Regional subsidy municipality rate_50p				1.692**
				(0.292)
N	18,437	10,967	17,117	17,179
pseudo R^2	0.203	0.122	0.090	0.064

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

Notes: The sample includes firms eligible for the subsidy in the subsidy area and the period. The sample in (1) includes new employers i.e. firms with zero employees in the previous year and positive number of employees, (2) firms that effectively hired the first employee or had below 0.5 effective employees in the previous year and at least 1.5 effective employees in the two following years, and (3)–(4) as in (2) with the addition of entry firms that have at least 1.5 effective employees in the two following years. Effective employees is defined as wage costs divided by the median of wage costs per employee. Net asset and turnover percentiles according to the previous year relative to full population. Standard industrial classification according to Statistics Finland, only reporting some industries. Region defined according to home municipality of firm and ELY Center 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 in some prior year had positive number of employees.

the new effective employers in South or Central Ostrobothnia is around 3 or 4 but 1 or 1.5 in the neighboring regions Central Finland and North Ostrobothnia. In addition, firms in regions with above median fraction of subsidy municipalities have about 70% higher odds of using the subsidy. This further supports that the regional differences may, indeed, be related to institutional differences in awareness. Interestingly, between the take-up and a region depends on the eligibility group, which may indicate differences in the administration, information targeting or interpretation of the eligibility criteria between the ELY Centres. In addition, being located in an ELY Centre municipality has a weak positive association with the take-up. These differences suggest that a seemingly simple subsidy institution can suffer from complexity in regional administration which, in turn, affects the salience of the subsidy.

Additionally, the association with the firm level characteristics in the group of new effective employers are in line with firm characteristics related to lower baseline probability of becoming an employer or frictions in becoming an employer. If that is the case, the selection to take-up can have an improved (or at least not decreased) effect on the efficiency of the subsidy. This could result from firms with higher frictions in becoming an employer having benefits of using the subsidy additional to the financial gains causing firms to be more willing to use the subsidy. In addition, there can be endogenous search for information, where firms with frictions in becoming an employer search more for information on subsidies for hiring. For instance, firms with lower net assets, that may have more liquidity concerns, are more likely to take-up the subsidy.

Higher take-up rates do not coincide with a higher estimated effect. For example, new firms are more likely to use the subsidy, have a higher basline probability of becoming and employer and have a larger estimated effect. This can be because new firms can experience financial constraints, leading to higher subsidy benefits, or be more likely to contact the ELY centres, leading to higher awareness rates. However, sole proprietors have a higher take-up rate but, if something, a smaller estimated effect. In addition to sole proprietors, firms with no employer experience are more likely to take-up the subsidy – these characteristics are generally associated with a lower baseline probability of becoming an employer.

5.3 Development Of The Subsidized Firms

Here, I describe the development of the subsidized firms relative to similar firms with similar observed hiring behavior that did not use the subsidy despite being eligible, using a matching event study approach. The differences in the persistence of employment and growth can reflect the effect of the subsidy on the firms that used it. However, the differences can be due to the selection of firms into using the subsidy that is not accounted for by the matching.

I compare the development of the subsidized firms to matched, similar firms that did not use the subsidy despite being eligible. The idea is to compare firms that differ only in the observed take-up of the subsidy. I compare the subsidized firms to firms that fulfill the observed eligibility criteria of hiring by matching according to employer status, employment and wage costs to avoid the problem of defining the eligibility in the data.[19] I include firms in the subsidy area during the subsidy period and define a randomly selected year as the subsidy year for the comparison group, in order to compare the development of the firms relative to a fixed point in time. I exclude firms that have more than 50

[19]See above section 5.1 and 5.2 on the difficulty of defining eligibility.

employees in the subsidy year, to exclude the largest firms not affected by the subsidy that could have a large effect on the averages.

It should be noted that, by construct, the comparison group includes firms that are more likely to hire or have higher labor productivity as they have similar observed hiring as the subsidized firms but their employment decision is unaffected by the subsidy. This holds especially when the non-take-up is due to lack of awareness instead of costs of using the subsidy.

However, the full-time restriction can imply that out of firms with similar observed hiring firms more willing to hire on a permanent full-time contract take-up the subsidy. The matching can account for the take-up decision related to the observable characteristics and, especially, selection into using the subsidy according to actualized wage costs and number of employees. Among the matched firms, then, the differences in direct subsidy benefits do not explain the take-up, but unobservable propensity to hire on a full-time contract cannot be accounted for.

I use coarsened exact matching (CEM) method introduced in Iacus *et al.* (2008). Shortly described, CEM is a matching method that directly restricts the sample to common support of data so that firms similar in observed variables are matched with each other and the analysis is only conducted on the matched firms. The method coarsens or categorizes continuous variables either using conventional histogram methods or categories manually defined by the user, and divides the data into strata according to the coarsened variables. Then, matching status is defined as one for observations in strata that have both treated and untreated observations. This way, the method automatically accounts for interactions between variables, and non-linearities in selection. In other words, the method drops observations that are not in the multidimensional neighborhood of (un)treated observations with neighborhood defined as a stratum formed by categorizing the continuous variables. This way, there is no need to specify and estimate a model for selection, as in, for example, propensity score matching, and then test for similarity on observables. To account for different number of matches for the treated firms between different strata, CEM weights are calculated for the untreated matched firms so that the differences can be estimated using a weighted regression.[20]

To match the firms, I use employer status (overall and full-time), employment and wage cost category in the subsidy year and the previous year to define the observed eligibility. I define the wage costs and number of employees categories so that they reflect the eligibility to the subsidy i.e. distinguish non-employer firms from full-time employers, and that there is variation in the categories among the subsidized firms. In addition, I use a turnover and net assets in the previous year, firm age category, company form, industry and hiring history to account for the differences between the subsidized and the comparison firms.

Table 9 presents descriptive statistics for the matched and all firms. There are 990 subsidized firms and the matching results in 562 matched subsidized and 13,455 matched unsubsidized firms. The subsidized firms are smaller, on average, than the firms not subsidized but with a higher fraction of employer firms. This means that there is greater variation in the size of the not subsidized firms, and the subsidized firms are larger than most firms in the population but there are fewer very large firms among the subsidized firms. The matched subsidized firms do not significantly differ in the key characteristics from all subsidized firms. For the unsubsidized firms, the matching reduces considerably the average firm size from 2.8 to 0.48 with 20% employer firms. Using the CEM weights to equalize

[20] See **?** for explanation of the weights.

Table 9: Descriptives on the matched firms

	All		Matched		
	(1)	(2)	(3)	(4)	(5)
	Subsi-dized	Not subsidized	Subsi-dized	Not subsidized	Not subsidized weighted
Turnover	156,916	233,759	142,033	52,367	130,800
	(233,726)	(759,881)	(180,955)	(120,057)	(186,559)
Employees	2.3	2.8	2.1	.48	2.1
	(3.4)	(7.6)	(3)	(1.4)	(2.7)
Wage costs	16,878	61,125	16,436	4,088	17,515
	(22,434)	(628,714)	(22,243)	(13,119)	(23,275)
Employer	.94	.32	.93	.2	.93
	(.24)	(.47)	(.26)	(.4)	(.26)
Net assets	24,502	90,079	17,230	8,719	15,999
	(113,707)	(332,049)	(57,760)	(49,990)	(63,509)
Profit	20,813	19,171	19,048	10,158	17,053
	(38,259)	(51,756)	(33,387)	(20,342)	(31,026)
Sole Proprietor	.51	.53	.53	.83	.53
	(.5)	(.5)	(.5)	(.37)	(.5)
New	.53	.32	.59	.62	.59
	(.5)	(.47)	(.49)	(.49)	(.49)
Imbalance	.97		.44		
Observations	990	119,830	562	13,455	13,455

mean coefficients; sd in parentheses

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

Notes: Descriptives of the subsidized and not subsidized and matched firms. The sample used for matching includes firms in the subsidy area during the subsidy period. The matching is designed to match on eligibility status including the hiring decision and firm differences. Matching results: L1 imbalance measure before matching: 0.967; after matching: 0.440. Number of strata: 23,442; number of matched strata: 433. Number of subsidized firms matched: 562; not matched: 399; number of controls matched: 13,455; not matched: 89,840. Variables used for matching: employer status, lag of employer status, full-time employer status, lag of full-time employer status, wage cost category, lag of wage cost category, lag of turnover category, lag of net assets, number of employees category, lag of number of employees category, company form, industry at the two digit level, age category (1, 3 or more years), and a dummy for previous employer history. Wage cost category is defined to capture the eligibility criteria for being a full-time employer.

number of matches for each subsidized firm, the differences between the subsidized and unsubsidized firms disappear. I tried different matching specifications including different size categorizations and eligibility definitions and the results are similar between the specifications.

Figures 8 and 9 plot the development of the subsidized firms compared to the matched firms that did not use the subsidy from 2 years before to 5 years after applying for the subsidy.[21] The firms have a similar trend in the number of employees until receiving the subsidy, but one year after the subsidy the subsidized firms have persistently about one employee more. At the extensive margin, the subsidized firms are more likely to remain as employers so that each year after the subsidy they are about 20% more likely to be employers than the unsubsidized firms. Similar pattern arises in the wage costs. The subsidized firms have about 50% higher wage costs after the subsidy year.

The subsidized firms also grow their turnover more, but there is already higher growth of turnover from the one year before the subsidy to the subsidy year. The subsidized firms also seem to have about 2 percentage points higher likelihood of reporting positive turnover. In addition, the subsidized firms seem to have higher growth of profits until 3 years after the subsidy.[22]

These comparisons of firm development may imply that the subsidy had persistent effects on the subsidized firms' likelihood of remaining as employers and the number of employees. This would imply frictions in becoming an employer that the subsidy is able to alleviate. For example, the subsidy may loosen the firms' liquidity constraints, making them are able to hire more employees. Alternatively, the subsidized firms may be more likely to remain as employers if theycan better accommodate the employee specific risk with a higher number of employees. The results can, however, reflect differences in firm growth due to unobservable differences between the subsidized firms and not subsidized firms in their willingness to hire on a full-time employment contract.

6 Conclusion

This paper studies the effects of a sizable subsidy for hiring the first employee. Generally/ Overall, the results do not show any effect on the probability of existing non-employer firms becoming an employer, the creation of new employer firms, or other firm outcomes such as employment or turnover. Consequently, the subsidy was not effective at increasing aggregate employment even though the subsidy targeted over half of the firm population.

I document a low 2 % take-up of the subsidy among new employer firms as a reason for ineffectiveness. This raises a puzzle of why firms do not use a subsidy they are eligible for. The descriptive evidence supports the full-time restriction of the subsidy as an important factor in decreasing the take-up. However, the take-up is still very low at only 12% among firms that had an ex post calculated subsidy close to the typical amount of subsidy paid to the subsidized firms , which suggests low awareness as an additional explanation for the low take-up. Descriptive evidence further supports this explanation as there are regional differences in using the subsidy consistent with an information channel through the regional authorities. The overall conclusion is that a large majority of firms seem to leave substantial amounts of money – 6,000 to 20,000 euros close to one quarter of their wage costs

[21] I only plot the trend for two years before the subsidy because a large fraction of the firms that used the subsidy are new. Hence, for many firms there are not that many observation years to go back in time.

[22] Interestingly, the results are qualitatively and quantitatively similar when matching to firms without the hiring variables so firm with similar size before the subsidy. Only the subsidized firms grew between -1 and 0 as they hired the first employee.

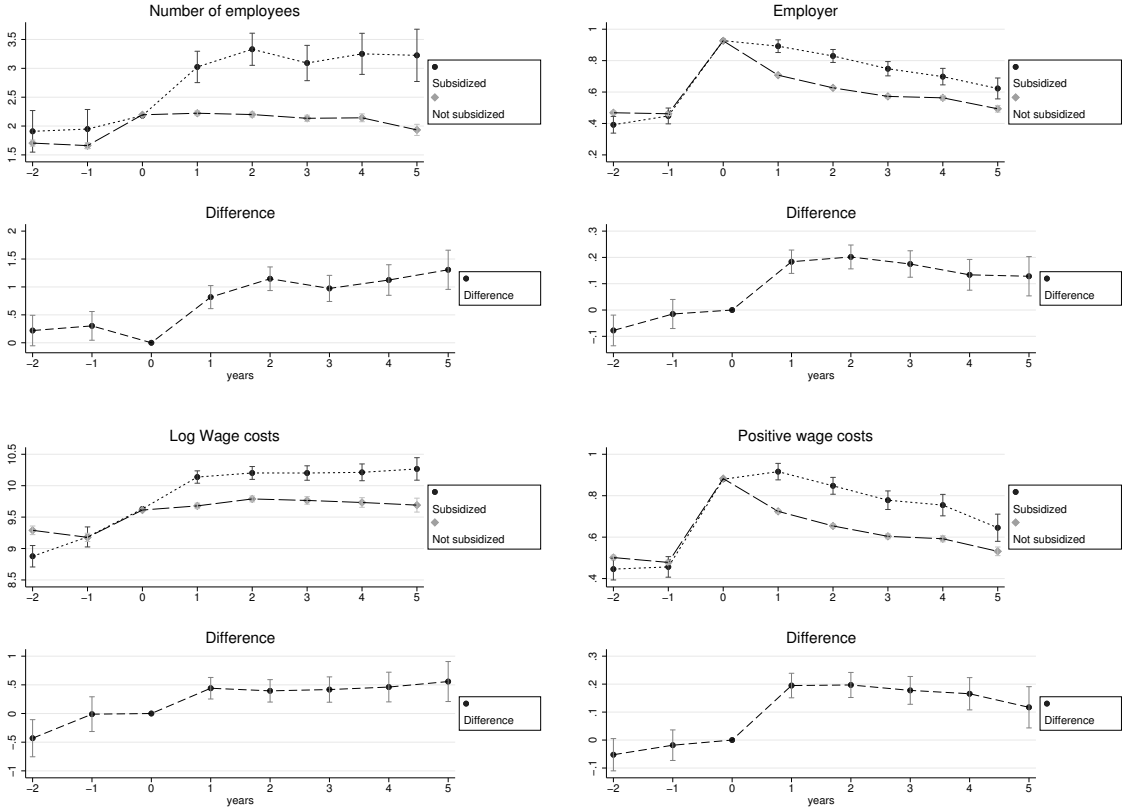

Figure 8: Development employment of subsidized firms compared to matched firms

Notes: The figures plot the annual firm development coefficients separately for the subsidized and non-subsidized firms in the upper panel and the difference-in-differences coefficients in the lower panel estimated by fixed effects regression on matched firms using CEM weights. The year 0 means the year of receiving the subsidy for the subsidized firms and for the unsubsidized firms the year of becoming an employer or the first observation year during the subsidy period if the firm remained as a non-employer. The sample used for matching includes firms in the subsidy area during the subsidy period. The matching is designed to match on eligibility status including the hiring decision, and firm characteristics. Matching results: L1 imbalance measure before matching: 0.967; after matching: 0.440. Number of strata: 23,442; number of matched strata: 433. Number of subsidized firms matched: 562; not matched: 399; number of controls matched: 13,455; not matched: 89,840. Variables used for matching: employer status, lag of employer status, full-time employer status, lag of full-time employer status, wage cost category, lag of wage cost category, lag of turnover category, lag of net assets, number of employees category, lag of number of employees category, company form, industry at the two digit level, age category (1, 3 or more years), and a dummy for previous employer history. Wage cost category is defined to capture the eligibility criteria for being a full-time employer.

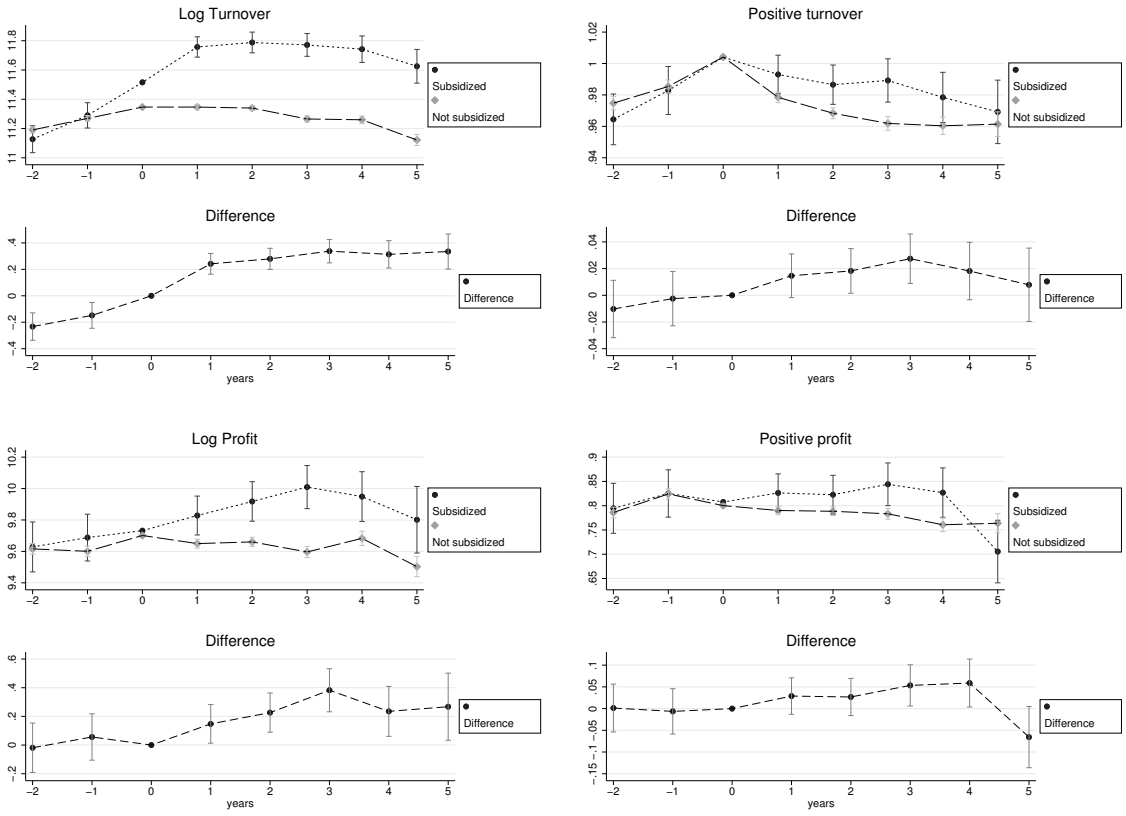

Figure 9: Development of turnover and profit of subsidized firms compared to matched firms

Notes: The figures plot the annual firm development coefficients separately for the subsidized and non-subsidized firms in the upper panel and the difference-in-differences coefficients in the lowe panel estimated by fixed effects regression on matched firms using CEM weights. The sample used for matching includes firms in the subsidy area during the subsidy period. The matching is designed to match on eligibility status including the hiring decision, and firm characteristics. Matching results: L1 imbalance measure before matching: 0.967; after matching: 0.440. Number of strata: 23,442; number of matched strata: 433. Number of subsidized firms matched: 562; not matched: 399; number of controls matched: 13,455; not matched: 89,840. Variables used for matching: employer status, lag of employer status, full-time employer status, lag of full-time employer status, wage cost category, lag of wage cost category, lag of turnover category, lag of net assets, number of employees category, lag of number of employees category, company form, industry at the two digit level, age category (1, 3 or more years), and a dummy for previous employer history. Wage cost category is defined to capture the eligibility criteria for being a full-time employer.

when becoming an employer – on the table by not using the subsidy, and even a larger proportion of firms do not use the subsidy due to the opportunity costs implied by the full-time restrictions.

The findings of the subsidy take-up have important policy implications. In general, it may be difficult to incentivize small firms using employment subsidies if they are not likely to take-up the subsidies. This emphasizes the importance of the policy design of incentives targeted at small firms. For example, the full-time restriction in the policy seems to do more harm than good by reducing effectiveness and inefficiently targeting the subsidy to firms that were larger to begin with. For simplicity and effectiveness, there should be minimal restrictions. In addition, more direct forms of finance such as tax reductions could be more effective if they do not require applications. Lastly, information on the incentives is a key concern and authorities need to consider how to reach firms and inform them about subsidy programs.

The observation of an imperfect take-up has important implications for the literature on business subsidies. First, it has implications for the interpretation, and especially external validity, of this and other studies using variations, such as regional criteria, in the eligibility to a subsidy in order to estimate the effects of business subsidies or tax benefits. In the context of my paper, this seemingly simple subsidy may have a complexity in its salience due to the regional administration and design of the policy, which may have reduced the effects of the subsidy. The results, consequently, identify the effects of the policy and provide little evidence as regards how responsive non-employers are in general to incentives to become an employer. For example, if the 12% take-up rate is considered as an upper bound for the awareness rate and the 2% as a lower bound, a 0.05 percentage point increase in the probability of becoming an employer is consistent with an elasticity between 1.2 and 7.1 of becoming an employer. While the do not support even a small positive effect, these calculations demonstrate the extent of how important take-up is on the interpretation of the results.

Second, the results call for attention to be paid to the take-up of business subsidies in future research. For example, the fiscal consequences of policies for firms is hugely dependent on the take-up, as the take-up reduces the money used, thus reducing the potential stimulus effect. Furthermore, even if there are grounds for an efficiency enhancing policy, such as market frictions, take-up may reduce or improve the efficiency depending on which firms take-up the subsidies. Consequently, taking into account possible imperfect take-up can change the optimal policy.

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Appendix A: Robustness and placebo regressions

Proportional hazards estimation of the effect on the probability of becoming an employer

Using a duration model, that estimates the probability of an event given time "at risk", can account for the selection bias described above in 4.2. In this case, the probability of becoming an employer is estimated conditional on being at risk of becoming an employer i.e. being a non-employer until that period. Consequently, the mechanical composition change introduced by increased hiring incentives is accounted for in a duration model.

Here, I use a discrete time proportional hazards model to estimate the effect on the probability of becoming on employer. The proportional hazards model is a semi-parametric duration model that models the probability of an event , here becoming an employer, for a firm given the duration of being at risk, here being an non-employer, relative to the baseline hazard. The relative hazard is independent of the duration at risk.

Using the proportional hazards model allows me to estimate how the subsidy changes the relative likelihood of becoming an employer while accounting for the duration of non-employership. Hence, I can include all non-employer firms in the estimation. The proportional hazard model defines the hazard function as

$$h(t, x_i) = h_o(t) \exp(x_i \beta), \tag{15}$$

where $h(t, x_i)$ is the hazard at time t conditional on x_i, $h_0(t)$ is the time dependent baseline hazard and x_i is a vector of individual covariates. The baseline hazard, consequently, equals the hazard with additional regressors set to 0. The discrete time version of the proportional hazards model can be estimated with complementary log-logs allowing for time-variant individual regressors.

To identify the effect of the first employee subsidy, the difference-in-differences method is used to estimate the baseline hazard as in Uusitalo & Verho (2010). 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 $h(t) = \exp(\sum_{j=1}^{13} I((i-1) \leq t \leq t)$ where t is the duration at risk i.e. duration of non-employer spell.

I use data from 2005–2013 for the estimation.[23]Here the treatment period dummy is defined according to whether the subsidy was available at the home municipality of the firm at that year. In addition, there is a dummy for post treatment period i.e. for 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 non-employer spell. Also, areas where the subsidy was introduced in 2009 and 2010 can be included and the "post-treatment" effect or the effect of stopping the subsidy can be estimated. However, it may suffer from composition change due to firms entering the sample of non-employers if the subsidy attracts new non-employers, as discussed in 4.2. Nonetheless, this approach can be used as an addition to the simple

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

Table 10: Effect in different subsamples estimated using proportional hazards model

	(1) All	(2) Partnerships and limited	(3) Sole proprietor	(4) New	(5) VAT liable
Employer					
Treatment Effect	1.019	1.003	1.014	0.988	1.020
	(0.0308)	(0.0384)	(0.0453)	(0.0418)	(0.0302)
Post Treatment Effect	1.008	1.000	0.994	0.978	1.005
	(0.0426)	(0.0560)	(0.0720)	(0.0636)	(0.0432)
Time at risk	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes
Net assets	Yes	Yes	Yes	Yes	Yes
Company form	Yes	Yes	Yes	Yes	Yes
Municipality employment share	Yes	Yes	Yes	Yes	Yes
Observations	216,272	842,35	131,997	61,999	152,568

Exponentiated coefficients; 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 relative hazards (RH) and the post treatment effect that corresponds to the effect of removing the subsidy and to the period 2012–2013. The effect estimated separately for subgroups of firms using discrete time proportional hazards model estimated through 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 or appear in the data for the first time are included. The sample includes active (non-zero turnover) firms in 2005–2013 excluding firms in agriculture. The treatment period is 2008–2011 and treatment area is defined as in 4. Partnerships and limited includes firms with company form of partnerships and limited corporations. New firms include firms that are at most 3 years old, VAT-liable includes firms with turnover of more than 8,500€.

approach.

Three model specifications are estimated: simple, simple + year and extended as above in section 4.2, but only the results from the extended model are reported since the results from the alternative models are consistent. Table 10 summarizes the results for the same subgroups as above. The coefficient for all firms is about 0.018, 0.008 for other than sole proprietors, 0.17 for new and 0.04 for VAT liable firms translating to relative hazard of 1.02, 1.01, 1.18 and 1.04 respectively. Consequently, the subsidy is estimated to increase the probability of becoming an employer by about 2 per cent, but much more, about 18 per cent, for new firms. None of the coefficients are statistically significant, so that zero effect cannot be rejected. Interestingly, the post-treatment effect is also positive, although it should be negative if the subsidy increases the probability to hire the first employee. As the estimated effect is small and the post treatment effect is positive, the results do not suggest that the subsidy was effective at increasing the average probability of hiring.

Firms that have been non-employers for a shorter time are more likely to become employers (Fairlie & Miranda, 2016; Lechmann & Wunder, 2017). Consequently, the effect of the subsidy may differ according to the duration of the non-employer spell. For example, the subsidy may have more effect on firms that have difficulties to hire and have, therefore, remained non-employers for longer time. On the other hand, firms that have been non-employers for a long time may not be interested in hiring or

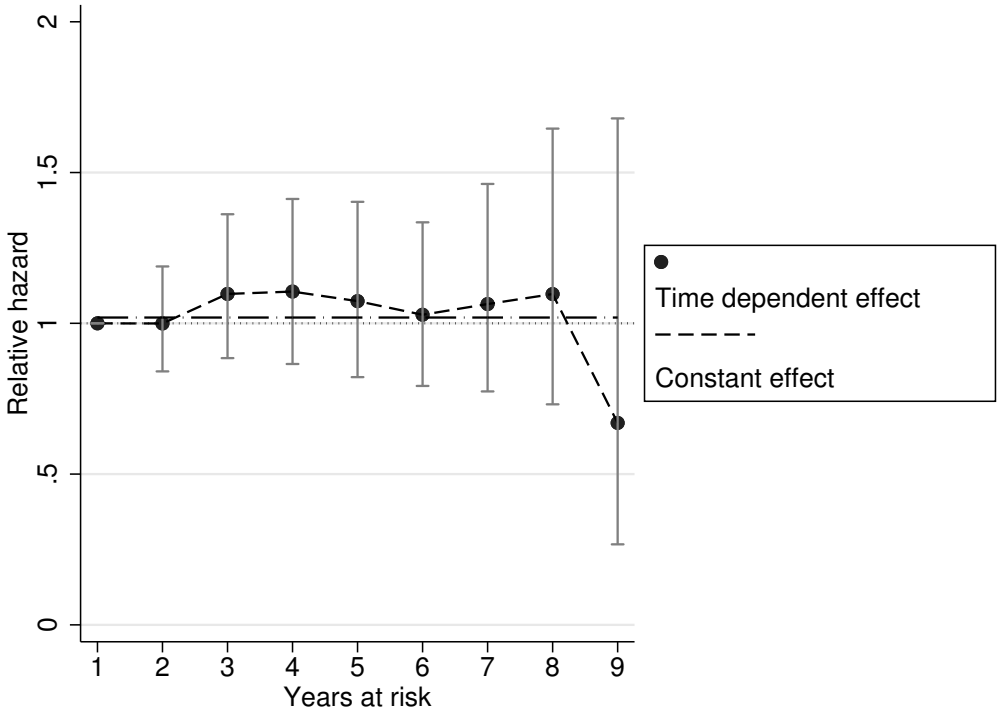

Figure 10: Estimated effect dependent on duration on non-employer spell

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 per cent confidence intervals. The effect is estimated using discrete time proportional hazards model estimated through 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 turnover) firms excluding firms in agriculture in 2005–2013. The treatment period is 2008–2011 and treatment area is defined as in 4.

growing the business at all and the subsidy may have zero effect on them.

Consequently, I allow for the estimated effect to depend on the duration of the non-employer spell by interacting the indicators for different durations and the treatment dummy. In this case the baseline hazard is written:

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

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

Figure 10 plots the estimated time varying effect of the subsidy. Again, none of the estimates suggest a statistically significant effect. The effect seems to decrease a little with duration, with a curious spike at 9 years of non-employership, but so do the confidence intervals. Consequently, the effect does not seem to significantly differ depending on how long the firm has been a non-employer.

Effect on firm entry and exit

The first employee subsidy can also increase incentives to start a business or stay in business, as well as hiring of existing firms. This effect on firm entry is difficult to asses for the sole reason that there

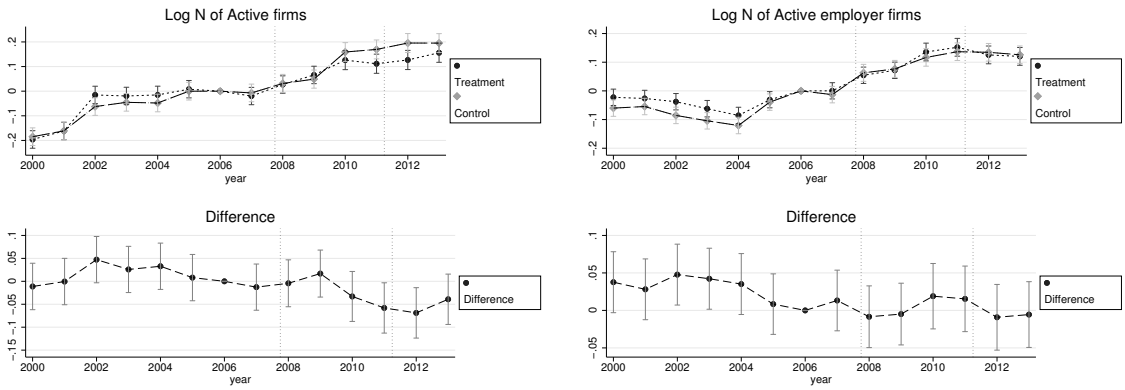

Figure 11: Trends of number of firms in the treatment and subsidy area

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 turnover excluding firms in agriculture. The treatment and control areas are as defined in figure 4. The sample only includes the treatment municipalities added to the treatment area in 2008 and their neighbor municipalities.

is no data on potential entrants before the firm entry. One way to assess the effect on firm entry or exit is to study the effect on aggregates, for example, the aggregate number of firms. The number of firms could also increase, if the subsidy encourages splitting up companies of founding multiple firms instead of one to take advantage of the subsidy.

To estimate the model on the number of firms, I follow the approach used in Bennmarker *et al.* (2009) that aggregates data on the municipality*industry level. I use the two code industry level according to Statistics Finland classification to aggregate the data. This results with 8,244 different municipality*industry observations in total with an average of 31 firms in a unit.

Figure 11 plots the difference in the number of firms and number of employer firms in the treatment and control areas. There does not seem to be any difference in the trend of number of firms except for a small decline in the treatment area after the subsidy period. The number of employer firms decreases in the treatment area a little compared to the control area between 2004 and 2005 but after that there is no difference in the trend. These figures, consequently, demonstrate a lack of pre-treatment trends and effect of the subsidy.

The models are essentially the same as previously with different additional covariates in the extended models. The additional covariates include industry dummies interacted with year dummies i.e. industry-specific time trends and municipality-level employment share. I only report the results using the fixed effects extended model. The number of firms in 2008–2011 is compared to the average of 2006–2007.

Table 11 summarizes the results separately for all firms, entry firms, new firms, employer firms and non-employer firms. The between years variations in the estimates is larger than the variation between the groups of firms. The estimated effect is negative ranging from -5.2 per cent on the number of new firms to -0.45 on the number of employer firms. None of the estimates are, again, statistically significant. Consequently, there does not seem to be a significant effect at the firm entry and exit

Table 11: Effect on (log of) number of different firms

	(1) Firms	(2) Entry firms	(3) New firms	(4) Employer firms	(5) Non-employer firms
Diff-in-Diff	−.017	−.04	−.052	−.0045	−.037
	(.022)	(.041)	(.028)	(.024)	(.022)
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	13,195	7,004	10,308	10,657	12,566
Adjusted R^2	0.602	0.441	0.377	0.188	0.534

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 turnover excluding firms in agriculture. Uses data from 2006–2011.

margin.

Additional regressions

While the simple difference-in-differences approach and duration model used above are conceptually correct ways to estimate the effect on becoming an employer, practically they do not make full use of the data. Restricting the analysis to only 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 the firms would be eligible for the subsidy.

In addition, firms can respond on other margins as becoming an employer. Specifically, firms may grow their business more with the subsidy including hiring more employees, employees with longer working hours or higher wage, or make other investments with the extra income. To address these concerns, I conduct regressions on a wider sample and include additional dependent variables that may respond to the subsidy.

In fact, some of the subsidized firms were not non-employers, in the light of the data, in the previous year of receiving the subsidy, as already observed in section 5.1. This can depend on 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 less than a full year long non-employer periods 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 at firm while a firm was eligible for the subsidy if it did not have full-time employees. For example, some of the firms with positive number of employees have very small wage expenditures 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 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) turnover. The difference-

Table 12: Effect on additional outcomes estimated using a wider sample of firms

	(1) New employer	(2) Non-employer	(3) Employment	(4) Wage costs	(5) Log Turnover
All					
Treatment Effect	−0.0000642	0.000897	−0.0300	40.41	−0.000831
	(0.00226)	(0.00418)	(0.0273)	(363.6)	(0.0139)
Observations	268,927	268,927	268,927	251,272	268,804
Adjusted R^2	0.002	0.005	0.008	0.007	0.018
Partnership and limited					
Treatment Effect	0.00269	0.000705	−0.0769	314.4	−0.00464
	(0.00387)	(0.00741)	(0.0632)	(931.3)	(0.0262)
Observations	90,090	90,090	90,090	84,603	89,983
Adjusted R^2	0.001	0.013	0.016	0.012	0.028
Sole proprietor					
Treatment Effect	−0.00168	−0.000699	0.00754	−8.363	0.00872
	(0.00247)	(0.00454)	(0.0145)	(141.7)	(0.0130)
Observations	178,837	178,837	178,837	166,669	178,821
Adjusted R^2	0.003	0.003	0.004	0.005	0.021
New					
Treatment Effect	0.00581	−0.00198	−0.0843*	206.3	0.00106
	(0.00861)	(0.0101)	(0.0409)	(529.8)	(0.0293)
Observations	58,435	58,435	58,435	53,569	58,420
Adjusted R^2	0.022	0.010	0.009	0.006	0.070

Standard errors in parentheses

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

Notes: Table presents DID regression coefficients for the stated outcomes. New employer is a dummy that equals one for firms with positive employment that had zero employees in the previous year, non-employer is a dummy equal to one when the firm has zero employees, Employees is number of employees that worked in the firms during the tax year. The sample includes firms with at most 50 employees, non-zero turnover in 2005–2011 and number of employees zero in some year between 2000 and 2013.

in-differences estimate 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 12 summarizes the results. The results from this analysis are consistent with the above results and do not suggest a significant effect on any of the variables examined: None of the regression coefficients are statistically significant. Some variables, including employment, new employment and turnover, actually have negative signs. All in all, the coefficients are really small and do not suggest that the subsidy had any effect. In conclusion, these results do not suggest that the previous results were driven by excluding companies or margins of response.

Placebo regressions

To study the parallel trends assumption, I evaluate the similarity of pre-treatment trends using placebo regressions. Placebo regressions are essentially the same as difference-in-differences regressions but instead of the subsidy period a placebo period before the subsidy period is used in the regression. Consequently, the estimated placebo treatment effect indicates if there is change between the trends of the groups before the subsidy period.

Table 13: Basic difference-in-differences placebo regressions

By 1st year	(1) All	(2) Partnership & Limited	(3) Sole Proprietor	(4) New	(5) VAT liable
Placebo Treatment Effect	−0.00668	−0.00666	−0.00741	0.0178	−0.00873
	(0.00555)	(0.00971)	(0.00759)	(0.0274)	(0.00656)
Observations	33,066	12,937	20,129	2,652	27,286
Adjusted R^2	0.010	0.015	0.007	0.052	0.012
By 2nd year					
Placebo Treatment Effect	−0.000475	−0.0172	0.0106	0.0228	0.00310
	(0.00730)	(0.0130)	(0.00873)	(0.0329)	(0.00861)
Year fe	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes
Net assets	Yes	Yes	Yes	Yes	Yes
Company form	Yes	Yes	Yes	Yes	Yes
Municipality employment share	Yes	Yes	Yes	Yes	Yes
N	31,007	12,066	18,941	2,458	25,712
adj. R^2	0.040	0.047	0.040	0.081	0.043

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 positive number of employees by the first, and by the second year. The sample includes active (non-zero turnover) 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 treatment area is defined as in Figure 4 but excluding municipalities that were added to the subsidy area in 2009 and 2010. Partnerships and limited includes firms with company form of partnerships and limited corporations. New firms include firms that are at most 3 years old in 2003 or 2005, VAT-liable includes firms with turnover of more than 8,500€ in 2003 or 2005.

Here, the placebo regressions use the time period 2004–2007 where 2006–2007 is defined as the placebo period. The placebo tests are done for all approaches used earlier. Overall, the results do not raise concern that the parallel trends assumption is violated.

In the simple difference-in-differences approach, 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 13 summarizes these results. All the coefficients are small with varying signs and statistically insignificant. Therefore, these placebo regressions do not raise concerns regarding the parallel trends assumption.

Table 14 summarizes the estimates of the placebo effect using the proportional hazards model for all of the subsamples used. The placebo effect is statistically insignificant.

Appendix B: Subsidized Firms

Subsidy decisions by year

Table 15 shows the yearly amounts of subsidy granted and used. In total 14.9 million euros was granted, and 10.2 millions paid for the companies. The budget for the subsidy was 30 million €, so

Table 14: Placebo regression in the Cox proportional hazards model

	(1) All	(2) Partnerships and limited	(3) Sole proprietor	(4) New	(5) VAT liable
Placebo Treatment Effect	1.059	1.036	1.053	1.071	1.018
	(0.0414)	(0.0548)	(0.0603)	(0.0609)	(0.0434)
Time at risk	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes
Net assets	Yes	Yes	Yes	Yes	Yes
Company form	Yes	Yes	Yes	Yes	Yes
Municipality employment share	Yes	Yes	Yes	Yes	Yes
Observations	91531	37377	54136	26857	65947

Exponentiated coefficients; Standard errors in parentheses

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

Notes: Table presents DID placebo regression coefficients and relative hazards (RH) on becoming an employer. The effect estimated separately for subgroups of firms using discrete time proportional hazards model estimated through 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 turnover) firms excluding firms in agriculture in 2004–2007. The placebo treatment period is 2006–2007 and treatment area is defined as in 4. Partnerships and limited includes firms with company form of partnerships and limited corporations. New firms include firms that are at most 3 years old, VAT-liable includes firms with turnover of more than 8,500€.

Table 15: Yearly amounts of the first employee subsidy

Year	Decisions	Granted	Granted /firm	Used	Used /firm	Difference	Difference /firm
Total	1,349	14,900,000	11,078	10,200,000	7,541	4,771,858	3,537
2007	18	168,625	9,368	147,139	8,174	21,486	1,194
2008	444	4,648,363	10,469	3,320,974	7,480	1,327,389	2,990
2009	293	3,103,918	10,594	2,094,632	7,149	1,009,286	3,445
2010	295	3,422,909	11,603	2,305,072	4,453	1,117,837	3,789
2011	299	3,600,777	12,043	2,304,917	4,887	1,295,860	4,333

Note: Decisions denotes the number of subsidy decisions done, Granted the total amount of the subsidy granted in €, Granted/firm the average amount of the subsidy per subsidized firm (€), Used the total amount of subsidy used by firms, Used/firm the average amount of subsidy used per subsidized firm, and Difference the difference between the amount of subsidy granted and used.

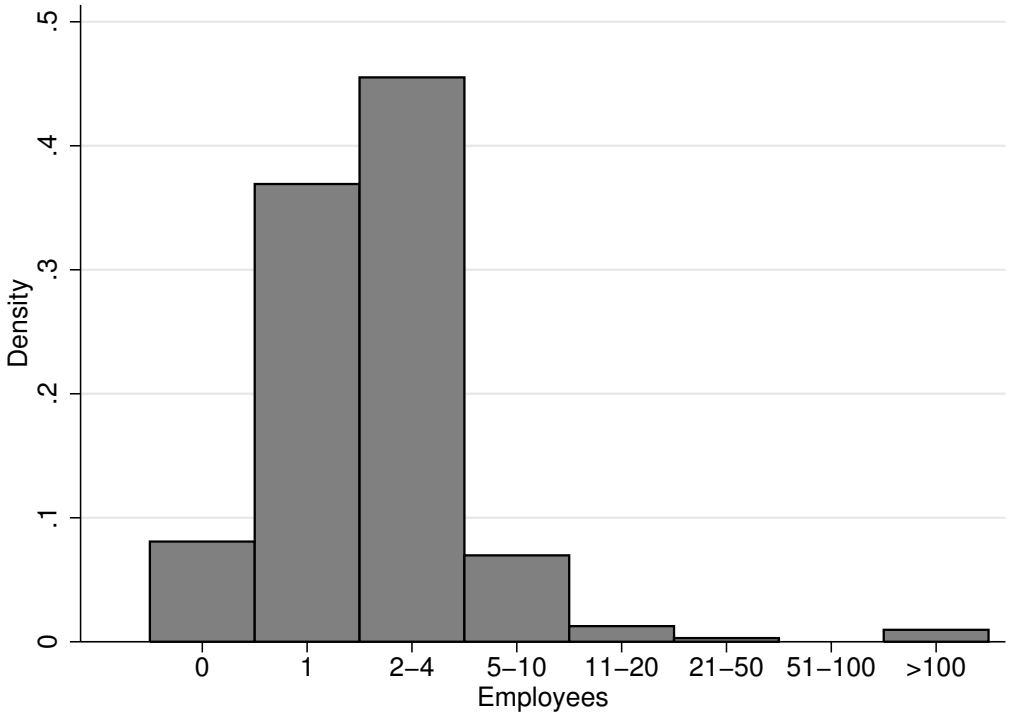

Figure 12: Firm distribution by employment of the subsidized firms

Notes: The sample includes the firms that used the subsidy and are matched with the tax return data. Number of employees in the year of application for the subsidy refers to number of all employees in the firm during the tax year.

less than half of it was used. A firm was granted on average 11,100€ as the subsidy and used 7,540€. Most municipalities were added to the subsidy area in 2008 when there was also the bulk of granted subsidy decisions but after that the amounts were quite steady.

Significantly less subsidy was used than granted that seems to be mostly due to lower actualized wage costs than the subsidy was granted for. However, some firms that did not use the subsidy had a positive number of employees later, so they applied for the subsidy but did not apply to get it paid in full. A quarter of firms used less than half of the subsidy granted for them and 10% did not use the subsidy at all. Some of the firms that did not use all or almost all of the subsidy did not have any employees after getting the subsidy and some returned to non-employer status before the end of the subsidy period.

Descriptives of the subsidized firms

Matching subsidized firms to targeted firms

Table 16: Descriptive statistics of the subsidized firms

	(1) Subsidized	(2) Subsidized(t-1)
Turnover	156,935	111,053
	(234,036)	(173,445)
Employees	2.4	1
	(5.9)	(2.3)
Employer	.94	.41
	(.24)	(.49)
Wage costs	16,854	4,435
	(22,496)	(11,576)
Net Assets	21,408	22,121
	(87,780)	(91,251)
Profit	21,102	20,238
	(43,559)	(34,355)
Sole Proprietor	.51	.56
	(.5)	(.5)
Partnership	.16	.15
	(.36)	(.36)
Corporation	.33	.28
	(.47)	(.45)
Manufacturing	.055	.068
	(.23)	(.25)
Construction	.2	.19
	(.4)	(.39)
Wholesale and retail trade	.2	.2
	(.4)	(.4)
Transportation and storage	.12	.11
	(.32)	(.31)
Professional, scientific and technical activities	.1	.12
	(.3)	(.32)
Human health and social work activities	.049	.054
	(.22)	(.23)
Other service activities	.073	.079
	(.26)	(.27)
Other	.2	.19
	(.4)	(.39)
New	.53	.44
	(.5)	(.5)
Entry firm	.28	.18
	(.45)	(.38)
Previous employer	.44	.5
	(.5)	(.5)
Observations	984	680
Observations	984	680

Notes: Table presents descriptive statistics: mean and standard errors in parentheses. The sample includes the firms that used the subsidy and are matched with the tax return data. Subsidized are the firms that used the subsidy in the year of application for the subsidy and subsidized(t-1) are the subsidized firms in the year before application. Standard Industrial Classification according to Statistics Finland. New firms are at most 3 years old, entry firms appear for the first time in the data i.e. are at most one year old. Previous employer means firms that had positive number of employees in some prior year.

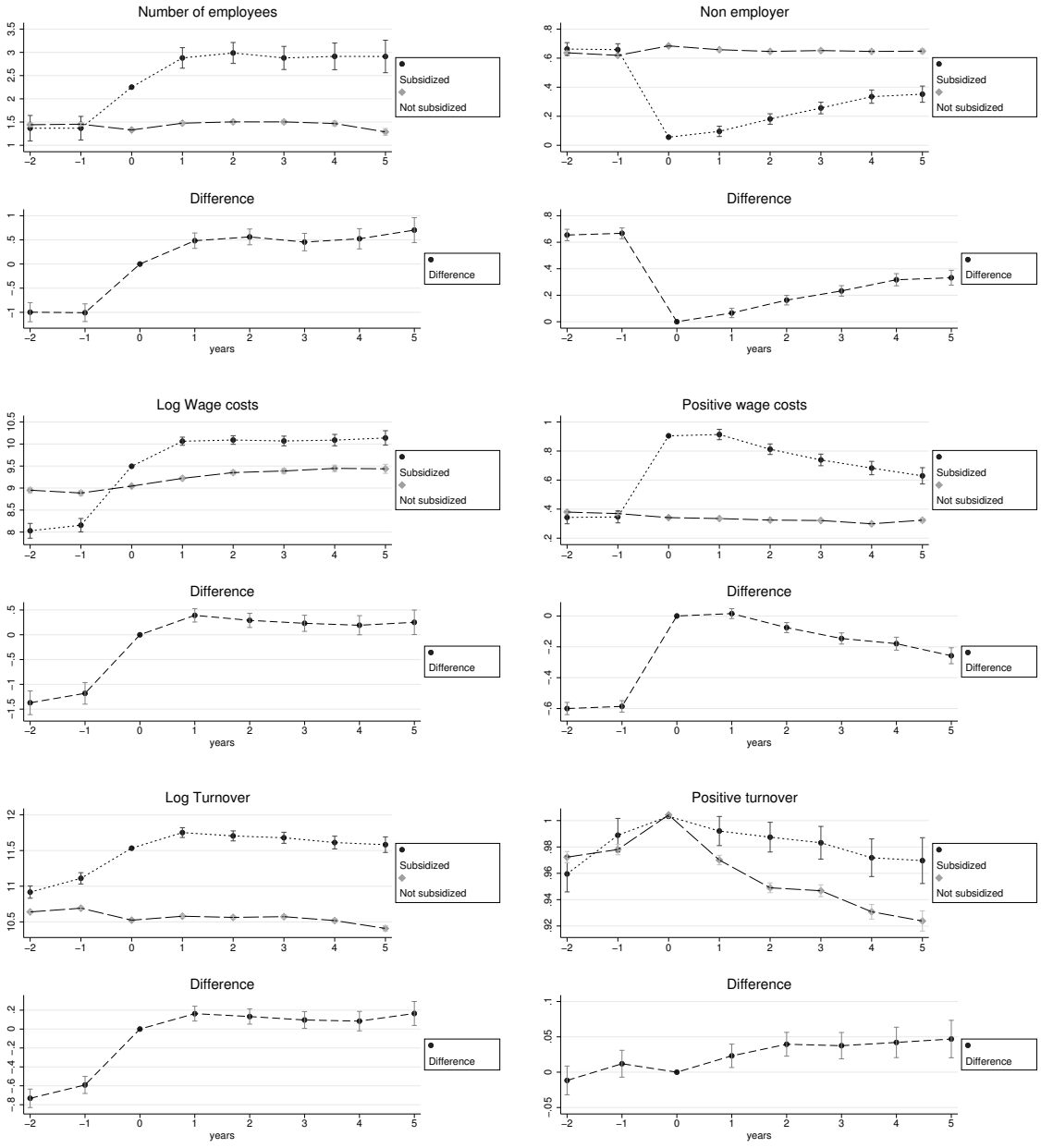

Figure 13: Behavior of subsidized firms compared to matched subsidy target group

Notes: The figures plot fixed effects regression coefficients of firm timeline since using the subsidy using CEM weights in the regression. The sample used for matching includes firms that could have been eligible for the subsidy if they hired (including both firms that hired and did not hire): firms that were effectively non-employers in the previous year (labor costs below median of labor costs per employee), in the subsidy area during the subsidy period. Matching results: L1 imbalance measure before matching: 0.993; after matching: 0.639. Number of strata: 7,241; number of matched strata: 597. Number of subsidized firms matched: 767; not matched: 31; number of controls matched: 33,568; not matched: 81,793. Variables used for matching: continuous lag of turnover, labor costs and profits; categorical company form, industry, region, business center municipality, previous employer, previous non employer, new, entry. Only active firms included in the matching year. Matching done including firms that were non-employers in the previous year, and in the subsidy area during the subsidy period.

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