Effects of subsidizing the first employee – Empirical evidence from Finland
Ensimmäisen työntekijän palkkaustuen vaikutukset – Empiirinen analyysi Suomesta

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24th January 2017

A backgroud report for Report 2016 of the Economic Policy Council

Talouspolitiikan arviointineuvoston 2016 raportin taustaraportti

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Abstract

This[1] paper studies the effects of the first employee employment subsidy that was in force in Finland from 2007 to 2011. Businesses that had no employees in the previous 12 months could receive the subsidy to hire the first employee. The subsidy was granted by the ELY center from application and amounted to 30% of the wage costs for the first year and 15% for the second year excluding payroll taxes. The employment contract had to be permanent and for at least 25 hours per week. The subsidy was first available in a few high unemployment municipalities in northern and eastern Finland, but was extended to cover a large share of municipalities in 2008 and small areas were added in 2009 and 2010. The areas added to the subsidy area from 2008 onward included economically average areas in addition to worse performing areas.

The subsidy was supposed to encourage non-employer firms to become employers. Becoming an employer is not a trivial step for a firm: there are fixed costs in becoming an employer for the first time both naturally and due to bureaucracy (such as learning the legal responsibilities of an employer), and it generally implies a relatively large increase in business activity that may require large investment and increase in risk. Actually, more than half of Finnish companies are non-employer firms. Consequently, political discussion has suggested that there is large employment potential in non-employer firms. However, the special case of non-employer firms and the determinants of becoming an employer have not yet received much attention in economic literature. Therefore, there is no evidence on the effectivity of such subsidies yet.

Using tax register data of all Finnish companies from 2000 to 2013, I study the effects of the subsidy on the probability of becoming an employer using a Difference-in-Differences framework. The analysis is restricted to the border of the subsidy area to exclude the largest regional differences in economic trends. The change in the rate of becoming an employer in the subsidy area is compared to the change in the control area. Therefore, the treatment area is the municipalities on the border of the subsidy area where the subsidy is available. Correspondingly, the control area is the municipalities on the border of the subsidy where the subsidy is not available i.e. the neighboring municipalities of the treatment area. Comparisons of pre-treatment trends suggest that the common trends assumption is not violated and difference-in-differences can be used. Placebo regressions are also conducted and they do not raise serious concerns about the validity of the method.

To identify the effect on non-employer firms, I restrict the analysis to firms that have zero employees in the year before the subsidy period and follow whether they became employers in the subsidy period. Similarly, in the pre-subsidy period, i.e. the four years before the subsidy, the firms that had zero employees in the previous year are included. Here, only the areas where the subsidy became available in 2008 and the corresponding control area are included (i.e. neighboring municipalities of the 2008 treatment municipalities where the subsidy is never available). Therefore, in the subsidy period i.e. 2008–2011 firms that have zero employees in 2007 are included and in 2004–2007 firms that have zero employees in 2003 are included. Then, I do simple Diff-in-Diff regressions using a Dummy for being an employer by the first, second, third or fourth year as the outcome variable. In short, I study whether the subsidy affected the rate of becoming an employer in a span of four years.

The results suggest that there is no significant effect on the hiring probability. This result does not change when the analysis is restricted to groups of firms that are more likely to hire: firms excluding sole proprietorships, new firms or VAT-liable firms (firms that have a turnover of more than 8,500 euros). In addition, I estimate the effect on hiring probability using a proportional

[1] Finnish abstract below.

hazards model where firms at risk of hiring the first employee are defined as those that had zero employees in the previous year. This approach enables me to account for the time in which a firm has been a non-employer. These results support the conclusions from the simple Diff-in-Diff regressions that the subsidy did not increase hirings.

In addition to the effect on non-employer firms, the subsidy can affect the overall probability to become an employer firm as it encourages starting a business, or may discourage exiting, as well as encourages existing non-employer firms to hire. However, this effect is much more difficult to study because there is no clear target group in the data. This question is studied here using data aggregated to the municipality*industry level where the outcome variable of interest is the number of firms. These results do not suggest that the subsidy significantly affected the number of firms. Using, for example, the number of employer or non-employer firms as the outcome variables does not change the results. Therefore, no effect at the exit/entry margin is found.

The results, therefore, suggest that the first employee subsidy did not have a significant effect on existing non-employer firms or at the firm exit/entry margin. The estimates are also precise, except in some subgroups, implying that labor demand elasticity of non-employer firms is very small. The results are robust across different firm groups, so the elasticity is not very different between, for example, all firms and new firms. The results can be due to non-employer firms consisting mainly of self-employed people who are not responsive to hiring incentives so that the affected group of firms is really small. If there are large constraints in becoming an employer that may cause many firms to remain non-employers and the subsidy can help them overcome these constraints, the subsidy should increase the probability of becoming an employer significantly. Thus, the results imply that either the constraints were not significant or the subsidy was not effective in abolishing them. The ineffectiveness of the subsidy can be due to poor implementation and design – the payment and application process included some bureaucracy and firms were not well informed about the subsidy, which may be a reason for the low participation rate.

Tiivistelmä

Tässä artikkelissa tutkin ensimmäisen työntekijän palkkaustukea, joka oli Suomessa käytössä vuodesta 2007 vuoden 2011 loppuun. Tuki haettiin ELY-keskuksesta ja se myönnettiin ensimmäisen työntekijän palkkaamiseen yrityksille, joilla ei ollut ollut työntekijöitä edeltävien 12 kuukauden aikana. Tuki oli 30% työntekijän palkasta ensimmäisen ja 15% toisen vuoden ajan lukuunottamatta työnantajan sosiaaliturva- ja eläkemaksuja. Työsopimuksen tuli olla toistaiseksi voimassa ja kokopäiväinen eli vähintään 25 tuntia työtä viikossa. Tuki oli aluksi voimassa vain muutamissa Pohjois- ja Itä-Suomen kunnissa, mutta laajennettiin suureen osaan Suomen kuntia 2008 ja pieniä lisäyksiä tehtiin 2009 sekä 2010. Tukialueeseen vuoden 2007 jälkeen liitetyt kunnat eivät ole pelkästään taloudellisesti heikosti menestyviä, vaan niihin kuuluu paljon myös keskimääräisiä suomalaisia kuntia.

Tuen tarkoitus oli rohkaista yksinyrittäjiä ryhtymään työnantajiksi. Työnantajaksi ryhtyminen ei ole triviaali askel yritykselle: se sisältää kiinteitä kustannuksia sekä luonnostaan että byrokratian vuoksi (kuten työnantajan juridisten velvollisuuksien opettelu) ja se yleensä vaatii suurta liiketoiminnan kasvatusta, mikä voi tarkoittaa suurta investointia ja riskin nousua. Yli puolet suomalaisista yrityksistä on itse asiassa yksinyrittäjiä, minkä vuoksi poliittisessa keskustelussa niissä ajatellaan olevan paljon työllistämispotentiaalia. Taloustieteessä ei kuitenkaan ole juuri tutkittu, miten eri tekijät vaikuttavat työnantajaksi ryhtymiseen, joten tietoa tällaisten tukien vaikutuksista tai yksinyrittäjien työllistämispotentiaalista ei vielä ole.

Tutkin ensimmäisen työntekijän palkkaustuen vaikutuksia työnantajaksi ryhtymisen todennäköisyyteen Difference-in-Differences metodologialla. Aineistona on kaikkien suomalaisten yritysten verotustiedot vuosilta 2000–2013. Jotta suurimmat alueelliset erot taloudessa eivät vaikuta tuloksiin, tutkimus on rajoitettu vain tuen raja-alueelle. Muutosta työnantajaksi ryhtymisen todennäköisyydessä tukialueella verrataan kontrollialueen muutokseen. Tässä tukialue viitaa kuntiin, jotka ovat tukialueen rajalla ja joissa tukea voi saada, ja kontrollialue näiden naapurikuntiin. Trendien tarkastelusta näkyy, ettei trendeissä tuki- ja kontrollialueiden välillä ole suuria eroja, joten yhteisen trendin oletus pitää. Oletusta tarkasteltiin myös placeboregressioilla, jotka myöskään eivät aiheuta epäilyksiä metodin validisuudesta.

Identifioidakseni vaikutuksen yksinyrittäjiin rajaan analyysin yrityksiin, joilla ei ollut yhtään työntekijää tukiperiodia edeltävänä vuotena ja seuraan, palkkasivatko ne tukiperiodin aikana ensimmäisen työntekijän ensimmäiseen, toiseen, kolmanteen tai neljänteen vuoteen mennessä. Samoin tukiperiodia edeltävällä periodilla, joka on tukiperiodia edeltävät neljä vuotta, mukana ovat ne yritykset, joilla oli nolla työntekijää ennen tätä periodia. Tukialueen rajalla vain kunnat, joissa tuki tuli voimaan vuonna 2008, ja kontrollialueella niiden naapurikunnat ovat mukana tässä analyysissa. Tukiperiodilla eli 2008–2011 ovat siis mukana yritykset, joilla ei ollut yhtään työntekijää 2007, ja vertailuaikana eli 2004–2007 mukana ovat yritykset, joilla ei ollut yhtään työntekijää 2003. Tällä otoksella teen yksinkertaisia Diff-in-Diff regressioita, joissa selitettävänä muuttujana on Dummy-muuttuja sille, onko yrityksessä työntekijöitä vai ei ensimmäiseen, toiseen, kolmanteen tai neljänteen vuoteen mennessä. Lyhyesti sanottuna tutkin sitä, vaikuttiko tuki työnantajaksi ryhtymisen todennäköisyyteen neljän vuoden aikajänteellä.

Tulosten mukaan tuella ei ollut vaikutusta yksinyrittäjien palkkaamistodennäköisyyteen. Otoksen rajoittaminen yrityksiin, jotka palkkaavat keskimäärin todennäköisemmin ei muuta tuloksia. Näitä ryhmiä ovat muut kuin elinkeinonharjoittajat, uudet yritykset tai arvonlisäverovelvolliset yritykset (yritykset, joiden liikevaihto ylittää 8500 euroa). Estimoin vaikutuksen myös käyttämällä "proportional hazards" mallia, jossa yritykset, joilla oli edeltävänä vuonna nolla työntekijää, ovat mukana. Tällä lähestymistavalla voidaan ottaa huomioon, kuinka kauan yritys on ollut yk-

sinyrittäjänä. Tulosten mukaan tuella ei ollut vaikutusta palkkaamistodennäköisyyteen ja siten nämä tulokset tukevat ensimmäisen lähestymistavan tuloksia.

Yksinyrittäjiin vaikuttamisen lisäksi tuki voi vaikuttaa työnantajaksi ryhtymisen kokonaistodennäköisyyteen rohkaisemalla yrityksen perustamista tai se voi vähentää markkinoilta poistumista. Vaikutusta markkinoille tuloon tai poistumiseen on kuitenkin vaikeampi tutkia, koska datassa ei ole selkeää kohderyhmää. Tutkin tätä kysymystä käyttämällä kunta*toimiala-tasolle aggregoitua dataa yritysten määrän ollessa selitettävä muuttuja. Näiden tulosten mukaan tuella ei ollut vaikutusta yritysten määrään eikä esimerkiksi työnantajayritysten määrään. Tuki siis ei tulosten mukaan vaikuttanut yritysten markkinoille tuloon tai markkinoilta poistumiseen eikä toisaalta työnantaja- ja yksinyrittäjäyritysten suhteeseen.

Tutkimusten tulosten mukaan siis tuella ei ollut merkittävää vaikutusta olemassa olevien yksinyrittäjien palkkaustodennäköisyyteen tai markkinoiden tulo/poistumis-tasolla. Tulokset ovat myös tarkkoja lukuun ottamatta joitain alaryhmiä, joten yksinyrittäjien työvoiman kysyntä ei vaikuta olevan kovin joustavaa. Tulokset eivät myöskään muutu erilaisten yritysryhmien välillä, joten jousto ei ole erilaista esimerkiksi kaikkien ja uusien yritysten välillä. Tulokset voivat johtua siitä, että suurin osa yksinyrittäjistä ovat itsensä työllistäjiä, jotka eivät reagoi palkkaustukiin, joten tuen vaikuttama ryhmä on todella pieni. Jos työnantajaksi ryhtymiseen liittyy suuria esteitä, jotka saavat monet yrittäjät jäämään yksinyrittäjiksi ja tuki auttaa yrityksiä ylittämään nämä esteet, tuen pitäisi nostaa työnantajaksi ryhtymisen todennäköisyyttä merkittävästi. Tutkimuksen tulokset siis tarkoittavat, että joko työnantajaksi ryhtymisen esteet eivät ole merkittäviä tai tuki ei onnistunut esteiden purkamisessa. Tuen vaikuttamattomuus voi johtua huonosta toimeenpanosta ja suunnittelusta – tuen maksatus- ja hakuprosessi oli jokseenkin byrokraattinen ja yritykset eivät olleet kovin hyvin tietoisia tuen olemassaolosta, mikä voi olla syynä alhaiseen osallistumisprosenttiin.

1 Introduction

Considering job creation, the first decision for a firm is whether to become an employer, in contrast to how many employees to hire. However, the decision to become an employer has largely been neglected in the economic literature. As becoming an employer includes fixed costs, a high relative increase in risk and often large investments, it is an essential question. In addition, hiring employees after the first one may become easier as the firm can learn about hiring and being an employer. The observation that a large share of firms are non-employers supports the idea that becoming an employer is not a trivial issue.

Governments are often interested in encouraging job creation especially in small firms, so the step to becoming an employer should also be of interest for them. In Finland lowering the threshold of hiring the first employee has received special interest in political discussion and different subsidies have been proposed. The need for such a subsidy is usually argued by the large number of non-employer firms in Finland – more than half of all firms – that is supposedly caused by too large a threshold to hire the first employee. Therefore, there is assumed to be large employment potential in non-employer firms. However, empirical evidence on the effects of such policies is still missing.

In this paper I study the effects of a regional first employee employment subsidy – the first employee subsidy from here on – that was in force in Finland in 2007–2011 using the difference-in-differences method. Non-employer firms located in the subsidy area were eligible for a subsidy amounting to 30% of wage costs in the first year and 15% in the second (not including payroll taxes) when they hired the first employee. In addition to the regional aspects, the subsidy was to function as a trial to extend such a subsidy to all of Finland.

This paper provides the first empirical evidence on the effects of the first employee subsidy and, to my knowledge, on subsidies to hire the first employee in general. Furthermore, it raises an important, though often overlooked, perspective that firms, as well as employees, decide whether to participate in the labor market or not. Consequently, this paper provides evidence on labor demand elasticity at the binary employ or not margin.

This paper contributes to the existing literature on employment and business subsidies, by providing new empirical evidence on how non-employer firms react to employment subsidies. As targeted employment subsidies can correct market inefficiencies, they are of special interest in public policy. In contrast to popular employment or hiring subsidies that are targeted to certain groups of employees, this paper studies the effects of employment subsidies targeted according to firm characteristics. The first employee subsidy is argued to decrease the threshold of becoming an employer, which is thought to be a market inefficiency. Although the efficiency of the subsidy is not studied here, efficiency implies effectiveness that is the subject of interest here.

The results suggest that the subsidy did not have a significant effect on the probability of becoming an employer. The results are robust to model specifications and between different groups of firms. Moreover, the subsidy did not seem to increase the number of firms or employer firms in the subsidy area. Therefore, no effect on the exit/entry margin of firms is found.

This paper is organized as follows. Section 2 describes the first employee subsidy scheme and presents the analytical framework. Section 3 briefly reviews the existing relevant literature. Section 4 describes the data, descriptive statistics and methodological strategy. Section 5 presents the main results and section 6 provides some additional results and robustness checks. Section 7 concludes.

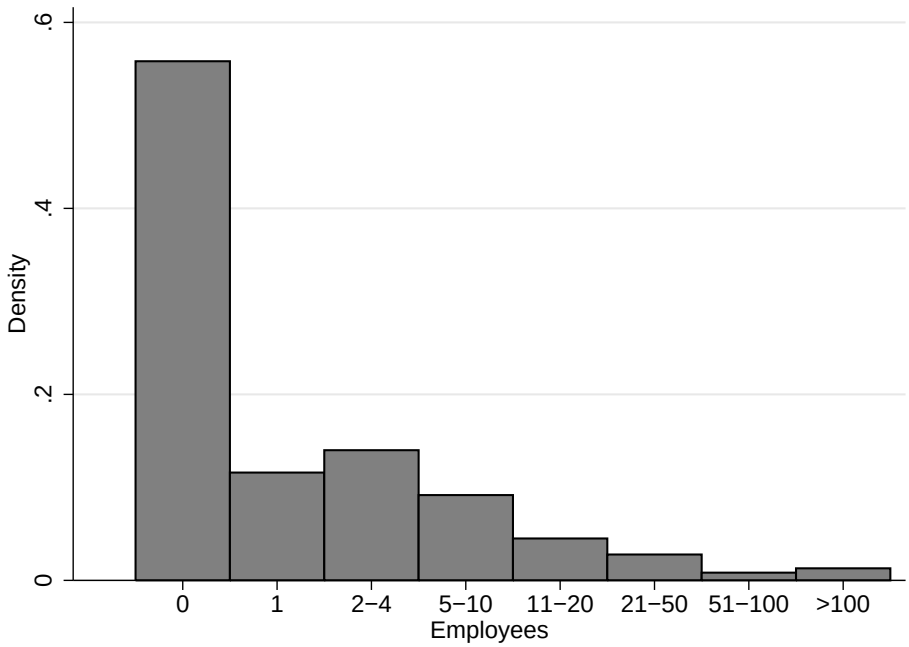


Figure 1: Size distribution of firms in 2006 by employment

2 Background and subsidy description

2.1 Description of the subsidy

Finland introduced an employment subsidy for hiring the first employee – "the first employee subsidy" – in 2007 as a regional subsidy. The subsidy was in force until 2011. The subsidy was meant to act as an encouragement for non-employer firms to become employers and, therefore, create jobs and grow. In addition to supporting entrepreneurship, the subsidy had a regional aspect as well. It was supposed to support employment in scarcely populated areas that were losing population. However, extending the subsidy to the whole of Finland was proposed and the initial subsidy was to function as an experiment of a first employee subsidy more generally.

The subsidy is motivated by the large share on non-employer firms in Finland. The idea is that many firms refrain from becoming an employer even though there would be large potential for employment in those firms. Figure 1 depicts the size distribution of firms by employment in 2006. About 55% of firms have zero employees and the share of firms declines with the number of employees. Therefore, becoming an employer is clearly not a trivial issue even though it has not received much attention in economic research. The target group of the subsidy is very large and, therefore, the firms affected by it is potentially significant. However, the reasons why firms are non-employers determine the effectiveness of the subsidy.

Firms in the eligible municipalities were able to apply for the subsidy to hire the first employee. Not having employees in the previous 12 months was a requisite for the firm to receive the subsidy. To qualify for the subsidy the firm would have to hire an employee for a permanent employment contract with at least 25 hours of work per week. The employment subsidy covered 30% of the wage

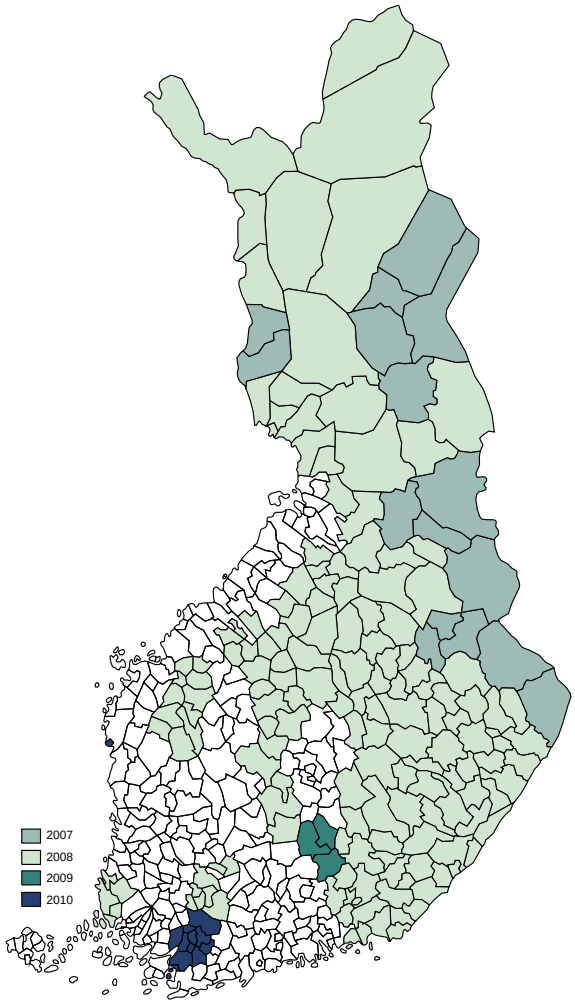
costs of the first employee in the first year and 15% in the second, not including payroll taxes. The subsidy was granted by the regional ELY Centre (The Centre for Economic Development, Transport and the Environment). In addition, the firm's requisites for a profitable business were evaluated and the subsidy was granted only if it was estimated not to distort competition or markets considerably. After being granted the subsidy, the firms had to apply to get the subsidy paid twice per year. Due to EU regulations, the subsidy could not be granted to businesses in fishery, agriculture, forestry or processing or marketing agriculture products.

According to a report by Aaltonen *et al.* (2011), there were a few problems with the subsidy. First and foremost, participation in the program was a lot lower than expected, which led to a budget surplus. A specific problem causing this could be the low awareness of the subsidy. According to a survey by Aaltonen *et al.* (2011), only about a third of companies had heard of the first employee subsidy.(Aaltonen *et al.*, 2011, 21–23, 11.) However, since the admittance was through ELY centers firms that are planning on growing their business are more likely to know about the subsidy.Another problem, according to Aaltonen *et al.* (2011), was the long payment cycle and the bureaucratic costs of applying to get the payment. This has direct effects on the participation in and the effectiveness of the subsidy. Half-yearly payments may not help enough firms that refrain from hiring due to slow or small income flows. Also, applying for the payment decreases the net benefit of the subsidy. As noted earlier, some firms that were granted the subsidy and continued to have positive employment were not paid the full subsidy so some firms did not bother to apply for the payment of the subsidy. This indicates that the costs of applying for the payment were not insignificantly small.

Aaltonen *et al.* (2011) also surveyed the subsidized entrepreneurs on the effectiveness of the subsidy. According to the survey, less than a fifth of thr companies felt that the subsidy was necessary to hire the first employee and less than half reported that the subsidy encouraged them to hire a full-time employee. Actually, about a third reported that they would have hired the employee even without the subsidy. Taking into account that firms have an incentive to lie in favor of the subsidy, the share of firms that report finding the subsidy superfluous is quite high. Also, Aaltonen *et al.* (2011) compare the average employment in firms that received the subsidy to those whose application was rejected and find no difference. Their evaluation suggests that the subsidy was most essential for old non-employer firms that had no hiring history. This tentative analysis already suggests that the subsidy was not very effective, but cannot yet provide any causal evidence.

Figure 2 is a map of Finland that depicts the subsidized area and how it developed. The subsidy programme started in June 2007 in a few municipalities in Northern and Eastern Finland. In 2008 the subsidy area covered most of Finland including all of Lapland and Eastern Finland, most of Northern and Central Finland as well as some municipalities in Southern Finland. Small areas were added in 2009 and 2010. The subsidy experiment continued until the end of 2011. The subsidy area was defined specifically for this subsidy and no other subsidies are targeted according to the same geographic criteria.

In general, Northern and Eastern Finland are losing population, and businesses are centralising more and more in the capital area and major cities in Southern and Western Finland. For business subsidies Finland is divided into three assisted areas according to the EU criteria on population density (Einiö, 2014, 712). The I assisted area receives the most business subsidies and III the least in absolute amount. In short, the assisted areas can be thought of as a classification of how economically



Notes: Map drawn using municipality borders from 2007

Figure 2: Development of the subsidy area

disadvantaged an area is, the I being the worst. First, only some municipalities in the I assisted area were given the first employee subsidy. From 2008 onwards the whole I assisted area belonged to the subsidy area as well as minorities of municipalities in the II and III assisted areas. In conclusion, the first employee subsidy area included the area considered economically most disadvantaged in Finland but also municipalities close to the national average.

There are some partially overlapping business and employment subsidies in Finland. For example, there is a discretionary subsidy for developing business that can be granted for the labour costs of a small start-up or a growing incumbent company. There is also a business starting subsidy targeted to companies in rural areas for enlarging or starting a business amounting to 50% maximum for two years of the labour costs of the first employee.[2] Even though these subsidies are clearly substitutes for the first employee subsidy, the first employee subsidy is most extensive and simple, as not much discretionary power can be used in granting the subsidy. (Aaltonen *et al.*, 2011, 25–27.) Other

[2]In addition to the general start-up business subsidy that is available in all of Finland.

employment subsidies include, for example, a hiring voucher for the unemployed, exemption from payroll taxes in some municipalities and a wage subsidy for over 54-year-old low-wage employees. These differ quite a lot from the first employee subsidy and do not directly affect the incentives of becoming an employer, except for the exemption from payroll taxes that affects all businesses to a threshold similarly. These other subsidies will be considered below in section 3.

2.2 Analytical framework

The first employee subsidy is a special instance of employment subsidies that are a form of business subsidies. Other business subsidies include investment, and research and development (R&D) subsidies. Employment subsidies decrease the price of labor and increasing the demand for labor. Generally, the effects of employment subsidies are evaluated in a framework of labor markets where the aggregate labor demand and supply determine wages and employment. Employment subsidies increase both wages and employment and the effect is determined by the elasticities of labor demand and supply. The more elastic labor demand is the larger is the effect on employment and the larger the labor supply elasticity is the smaller is the effect on wages. Several labor market aspects and imperfections affect the extent and timing of the effects of employment subsidies. For example, frictions in labor markets can delay and diminish the effect and the large negotiation power of labor unions increases wages more.

The first employee subsidy differs from the general employment subsidies in various respects, which is why the basic labor market model is not well equipped to analyze the effects. First, the subsidy is marginal, which means that it is conditional on increasing employment in the firm. Therefore, a smaller amount of jobs are subsidized i.e. the average price of labor is less affected, which emphasizes the effect on increasing labor instead of increasing wages. This also implies that the deadweight effect is smaller, because less money is spent on subsidizing jobs that would exist without the subsidy (Brown *et al.*, 2011). Second, the first employee subsidy affects only a small part of companies and employees that form only a small part of the labor market. Therefore, a microeconomic framework is more suitable to analyze the subsidy. Labor supply is here, in practice, perfectly elastic because the available jobs in the non-employer firms are so small that the employees always have an option to work for some other companies. Therefore, the non-employer firms can be treated as price takers. In conclusion, the first employee subsidy is not likely to affect wages and the effect on hiring is determined by the elasticity of labor demand of non-employer firms.

The government targets a specific problem, i.e. hiring the first employee, with the subsidy. This issue has not been studied theoretically, although it is not trivial. Hiring the first employee is a substantial leap and may require a lot of investment compared to hiring the second, tenth or hundredth employee. This is also why it is a large risk for the entrepreneur and may be relatively difficult to finance. In addition, becoming an employer requires some bureaucracy, as the employer needs to learn the regulations and to register to pay the payroll taxes. In other words, there are fixed costs of becoming an employer. The fixed costs and bureaucratic hassle may be too large a step for the company so that it never hires, even though it may have the potential to employ one or more employees.

Employment subsidies are often argued to increase employment but, economically, subsidies should be justified by efficiency concerns and not solely by the potential to increase employment. Subsidies can be efficient if they can reduce the effects of existing market imperfections. For example, there

are arguments in favor of R&D subsidies due to positive externalities and the financial difficulties of R&D projects. Also, labour markets suffer from information asymmetry, which can cause inefficient employment levels (See e.g. Brown *et al.* (2011)). Consequently, there are some targeted employment subsidies for the unemployed, for example, that can enhance efficiency and overcome the problems caused by imperfect information.

The first employee subsidy can decrease the impact of the burden of legal responsibilities that becoming an employer brings. The first employee subsidy can also be seen as reducing the costs of entering markets, which can increase competition. The first employee subsidy can, therefore, have efficiency gains. Also, information asymmetry in labour and capital markets can affect small and non-employer firms more than larger firms. For example, managing risk and imperfect information of potential employees may become easier and less costly as a firm employs more people (e.g.. learning by doing, risk diversification) . Also, small firms may need to get external financing for the necessary investments to hire the first employee but financiers tend to view them as more risky because there is a lack of reliable information. The subsidy can decrease the downward distortion in probability of becoming an employer caused by these imperfections.

It should also be taken into account that non-employer firms are basically the entrepreneurs themselves. They should be rather thought of as individuals with their preferences rather than a traditional profit maximizing firm. Therefore, the plans and goals of the entrepreneurs are a key factor determining the future of the firm. As Decker *et al.* (2014) point out, entrepreneurs can be divided into subsistence and transformational entrepreneurs. Subsistence entrepreneurs merely aim to employ themselves and maybe a few people and are not growth-seeking but employ themselves for other reasons. Transformational entrepreneurs, on the other hand, plan to innovate and grow the business and create jobs as a result. These high risk firms are essential for the job growth of the economy. (Decker *et al.*, 2014, 4–6.) Many non-employer firms are subsistence entrepreneurs with no intentions to grow their business to employ an external workforce. If an entrepreneur is simply planning to employ herself or himself, a subsidy for hiring the first employee is not likely to have an effect. Also, for firms seeking high growth a subsidy for the first employee does not affect the average employment costs (if they plan to hire, for example, 20 employees) enough to have significant effects. Therefore, the subsidy is only relevant to those non-employer firms that are open to growing their business to employ some external workforce and the effect of the subsidy depends on the share of these companies and their reaction.

Subsidies can also have substitution or distortive effects, in addition to increasing employment and business, as firms can change their behavior to qualify for the subsidy. For example, the first employee subsidy may delay hiring the first employee if a firm decides to wait until a year has passed after the last employee or until the subsidy period. At the extreme, a firm may lay off the last employee to qualify for the subsidy later. Also, the subsidy encourages founding multiple companies or splitting up companies to qualify for the subsidy or hiring one person with a full contract instead of two with fewer hours. In addition, formal and real effects should be distinguished. The subsidy encourages the formalization of first jobs as the price of a formal worker decreases with respect to informal workers. Consequently, the subsidy can decrease tax evasion in small firms.

To conclude, the effect of the first employee subsidy depends on the elasticity of labor demand of non-employer firms and on the severity of market imperfections in hiring the first employee. Specifically, the size of the effect depends on the relation of non-employer firms that are open to becoming an

employer to those who only plan to employ themselves or high growth seeking entrepreneurs. The effect and, especially, efficiency also depend on the extent to which market imperfections decrease the number of firms that become employers. Taking into account the fact that there are market imperfections that may cause too many firms to abstain from becoming an employer the subsidy may be quite efficient, since it has relatively little deadweight costs and does not have large adverse effects on other companies.

3 Previous empirical literature

3.1 Employment and business subsidies

There is a vast empirical literature on employment and business subsidies. The most reliable evidence relies on natural experiments. Natural experiments provide exogenous variation that can be exploited to estimate the effect of a policy – e.g. employment subsidy – directly. Usually there is a change, or some other discontinuity in policy, that affects some group while there exists a group not affected by the policy that can be used as a counterfactual. The difference-in-Differences (Diff-in-Diff) method, that is also exploited in this study, is one of the most used. If there is a suitable control group that does not experience a change in policy while the treatment group does, the change in the difference between these groups equals the treatment effect i.e. the effect of the policy. For example, if an employment subsidy is given to firms in a certain area, comparing employment in this area and some other before and after the subsidy shows the effect of the subsidy given that the areas are comparable.

In Finland in 2003 payroll taxes were reduced temporarily (by removing the employer contribution to the national pension insurance and the national health insurance) in some target municipalities. The exemption was first planned to last 2003–2005 but was continued later with more municipalities. Korkeamäki & Uusitalo (2009) use this change as a natural experiment to study the effects of employment subsidies, as a decrease in payroll taxes is equivalent to an employment subsidy. The average payroll tax reduction was about 4% while the average payroll taxes are about 24%. The reduction for a firm was at maximum 30,000 per year. Firms were eligible for exemption by simply filing a starting declaration. All firms with over 50 employees filed, while smaller percentages of smaller firms filed for exemption. Of firms with 2–4 employees 75% of firms filed for exemption. Interestingly, some small firms that would have received the full benefit of the exemption did not use it. Korkeamäki & Uusitalo (2009, 755-757.)

Korkeamäki & Uusitalo use the Diff-in-Diff method to estimate the effects of the subsidy. They form the control group by matching treated firms to similar ones in the control area to ensure the validity of the control group. According to their results, the payroll tax decrease did not have a significant effect in employment but half of the subsidy went into higher wages. However, the decrease in labor costs is quite small and the research only includes a small duration (2003–2004) so the time may not be long enough for the firms to react to small changes in wages. According to their point estimate labor demand elasticity is about 0.6 but it is not precise enough to guide policy. (Korkeamäki & Uusitalo, 2009, 771.)

Bennmarker *et al.* (2009) conduct a similar study using a regional payroll tax reduction in Sweden in 2002. The payroll tax reduction in the eligible municipalities was 10 percentage points – clearly larger than in Sweden – but the restriction was for an annual gross wage bill up to SEK 852,000, which

is a lot smaller than in the Finland case. However, their results are similar to those of Korkeamäki & Uusitalo (2009), as they find no significant effect on employment and a small effect on wages. They also review previous literature on payroll tax changes in Nordic countries, which mostly have the same conclusions. In addition, Bennmarker *et al.* (2009) study the effect on firm entry and exit as decreasing labor costs can encourage entrepreneurs to start businesses. The above results hold for existing companies but they estimate effects that take into account firm exit and entry using aggregate level regressions. They find evidence of positive effects on the number of companies but these results are not precise. (Bennmarker *et al.*, 2009, 488.)

A marginal employment subsidy in Turkey has been studied by Betcherman *et al.* (2010). As mentioned above, marginal employment subsidies are more likely to be effective with less dead weight spending, which is why these are of special interest. Moreover, the first employee subsidy is a marginal employment subsidy. The legal framework of the subsidy in Turkey is complex, but, in short, firms that increased their registered employment by a determined amount were eligible for different sizable subsidies. The empirical results show a large positive effect on employment, which implies large labor demand elasticity. However, the authors find evidence that this may be mainly due to job formalization rather than an increase of economic activity. (Betcherman *et al.*, 2010)

Other studies on employment subsidies in Finland include Huttunen *et al.* (2013) on low wage subsidies and Kangasharju (2007) on employment subsidies or hiring vouchers targeted to employees with difficulties to find jobs. Huttunen *et al.* (2013) find evidence that a low-wage subsidy for old workers increased employment of the target group but mostly at the intensive margin i.e. it mainly increased the working hours of employees already employed. An interesting point in the study is that smaller businesses were less likely to apply for the subsidies, which is in line with the study by Korkeamäki & Uusitalo. Kangasharju (2007) studies the firm level effects of targeted wage subsidies in Finland. There is a wage subsidy for the unemployed that have difficulties finding jobs themselves: the long term unemployed, those facing a threat of unemployment and people aged 25 or younger. The subsidy is marginal and granted to the firm on the basis of application. Kangasharju (2007) finds positive effects on firm employment, but the results are inconclusive due to the lack of exogenous variation in the data. Firms that used the subsidy are compared to those that did not, and while the endogeneity of the "treatment" group is taken into account, there is no guarantee that the control group is valid.

Employment subsidies can be considered as a form of business subsidies as in Koski & Pajarinen (2013, 2015). Moreover, marginal employment subsidy is analogous to an investment subsidy. as they are both conditional on growing business. Investment subsidies are usually found quite ineffective in the literature. For example, Bronzini & de Blasio (2006) study investment subsidies in Italy and find a positive effect on investment, that, however, seems to be mostly due to the effect on timing the investments and substitution, so that firms make investments on business opportunities other firms would have invested in without the subsidy, rather than increasing total investment. However, there are exceptions where investment subsidies may be more efficient. Tokila *et al.* (2008) evaluate the probability that the deadweight of an investment subsidy is zero in Finland. They find that the probability is larger for small companies, and companies in more distant areas or firms that have lower investment bearing capacity i.e. investment costs to turnover ratio. (Tokila & Haapanen, 2012)

Koski & Pajarinen (2013) study the effect of several business subsidies on employment growth in

Finland in different types of firms. They find strong positive effects on incumbents and start-ups in general but it seems that young, fast-growing businesses i.e. gazelles are not affected. However, the authors note that the unbelievably large effect may be caused by endogeneity bias that is not sufficiently accounted for. Therefore, their results are inconclusive. In a 2015 paper Koski & Pajarinen study the effect of business subsidies on firm exit and labor productivity. They find no effect on labor productivity but the evidence suggests that subsidies decrease the probability of firm exit. This suggests that subsidies may enable unproductive firms to stay in markets for too long, which decreases the efficiency of economy. This is one reason business subsidies can be counterproductive for the economy. (Koski & Pajarinen, 2015)

The empirical literature also gives many lessons in the case of the first employee subsidy but the evidence is a slightly mixed. Empirical literature on the effectiveness of employment subsidies in Nordic countries suggests that the subsidies do not have a significant effect on employment. This means the elasticity of labor demand – at least in the short run – is small. If this holds also for non-employer companies, the first employee subsidy is not effective. However, some studies suggest that hiring vouchers and business subsidies may, in fact, increase employment in companies. For example, considering the first employee subsidy as an investment subsidy suggests the subsidy might have positive effects, as investment subsidies are more likely to be effective for small companies.

Considering the first employee subsidy, the empirical evidence on its effectiveness is somewhat mixed. Studies in Nordic countries suggest that employment subsidies are not very effective in increasing employment, implying that labor demand elasticity is small. However, these studies do not focus on becoming an employer and the reactions of non-employer firms may differ from other firms. Also, some of the literature on investment subsidies suggests that small firms may be more reactive to subsidies. On the other hand, studies in Finland suggest small firms are less likely to even apply for the subsidies they are eligible for.

3.2 Job creation and becoming an employer

There is some empirical literature on the role of businesses and entrepreneurs in job creation. Previously, the role of small businesses has been emphasized in job creation and economic growth. However, more recent empirical evidence suggests new businesses account for most of job creation. As new firms tend to be rather small, the new growing firms explain the role of small businesses in job creation. (Haltiwanger *et al.*, 2013, 347.) However, there is very little literature concerning job creation by non-employer firms.

Fairlie & Miranda (2016), however, is one of the few existing studies on hiring the first employee. Previous studies indicate that non-employer firms have a much higher start-up rate than all the existing companies and many new employer firms have started as non-employer firms (Fairlie & Miranda, 2016, 1). This suggests that, at least for some companies, being a non-employer is a starting point and they plan to grow their business, not just employ themselves. Fairlie & Miranda (2016) study the dynamics and determinants of becoming an employer in detail for the first time. They use large U.S. register and survey data as well as data from a random experiment of entrepreneurs.

Fairlie & Miranda (2016) study the dynamics of hiring among non-employer start-ups in the US using two different data sets: register data covering all non-employer firms and survey data including more growth-oriented companies. According to their analysis, both data sets show similar dynamics

of becoming an employer, the difference being that becoming an employer is, intuitively, higher in the survey data. In the register data most non-employer companies exited before hiring their first employee (84.8%) and a large percentage (12.7%) did not hire employees in the sample period. In the survey data the percentage of companies that went out of business is a lot smaller. Most of the firms that became employers did so in the first few years of operation with an emphasis on the first year. (Fairlie & Miranda, 2016, 13-16.)

Using the survey data, Fairlie & Miranda (2016) also study the differences in hiring probability between differing entrepreneur and firm characteristics. The legal form of the firms is very strongly associated with the hiring probability. Sole proprietorships, quite naturally, have the smallest probability of hiring the first employee. Compared to sole proprietorships incorporated businesses are 58.6 percent and partnerships 5.5 percent more likely to become employers. This is very intuitive since the firm's choice of legal form is affected by the entrepreneur's business plan and, therefore, plans on becoming an employer. Their analysis does not suggest that probability of hiring is strongly associated with attaining a certain level of revenues or assets. Industry differences are very important as firms in wholesale, transportation and manufacturing, for example, are more likely to hire. There are also differences in hiring probability between ethnic groups and gender. The owner's education does not seem to affect hiring probability, but industry work experience is associated with higher probability. (Fairlie & Miranda, 2016, 16-18.)

Fairlie & Miranda (2016) also use a random experiment to study the effects of entrepreneurship training on hiring the first employee. Entrepreneurship training supposedly increases the human capital of the entrepreneur and can, therefore, have an effect on hiring probability. They use data obtained from an entrepreneurship training experiment called GATE (Growing America through Entrepreneurship) conducted in the US. GATE is the largest randomized experiment on entrepreneurship training. The participants were people interested in starting or growing business and half of them were randomly chosen to receive free entrepreneurship training. Previous studies on the experiment found that the training had small or no effects on business outcomes. (Fairlie & Miranda, 2016, 8-11.) Fairlie & Miranda (2016) study the effects on non-employer firms only that are a small part of companies in the experiment. However, their results are in line with the previous results since they do not find a significant effect on the probability of becoming an employer. (Fairlie & Miranda, 2016, 27-31.)

In conclusion, the job creation patterns of all businesses and non-employer firms resemble each other: New businesses are more likely to create jobs and non-employer firms are more likely to become employers in the first years of operation. The study by Fairlie & Miranda (2016) does not suggest that hiring probability is strongly associated with attaining large revenues or assets, implying that there are not many frictions that keep firms that are too small from becoming employers. Also, entrepreneurship training does not seem to increase the probability of becoming an employer.

4 Data and empirical strategy

4.1 Data

I use two firm level register data sets in the research. The main data set is panel data from the Finnish Tax Administration including the years 2000-2013 of all the Finland-based companies. The data consists of tax declarations by firms including data on, for example, employment, wage expen-

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

Table 1: Yearly amounts of the first employee subsidy

diture, turnover and assets as well as detailed information on the firm characteristics such as home municipality, juridical form and industry at the 5-digit level. This data is used for estimation and description of firms.

The additional data, provided by the Ministry of Economic Affairs and Employment, contains the first employee subsidy decisions by firm. The data set includes the positive decisions of how much was granted and paid for the company, as well as background info on the firms on the year of application such as location, juridical form, grounding year, employment, turnover and industry (at the 4-digit level). This data is only used to describe the subsidy use and to identify the subsidized firms in the tax data for descriptive statistics.

Table 1 shows the yearly amounts of subsidy granted and used. In total 1,635 firms applied for the subsidy and 1,351 were granted it. This is a small but not negligible number 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 the report by Aaltonen *et al.* (2011), most of the negative decisions were due to the firm not being eligible. For example, hiring the first employee before applying for the subsidy was a usual reason for a negative decision. In total 14,9 million euros was granted for the firms and 10,2 millions paid for the companies. The budget for the subsidy was 30 millions, so less than half of it was used. A firm was granted on average 11,100 as the subsidy and used 7,540 of that. Consequently, the average difference between the granted and used subsidy was 3,540 euros. Most municipalities were added to the subsidy area in 2008 when there was also the bulk of granted subsidies but after that the amounts were quite steady. Otherwise, the average amount of subsidy per firm grew yearly as wages increased.

Since significantly less subsidy is used than granted, the reason for this should be examined, especially, whether it is due to a few companies not using the subsidy at all or many companies using less than the amount granted for them. Most companies, specifically 1,001, used less than the amount granted for them (by 100 euros). A small but significant number, 346 meaning 26%, used less than half of the subsidy granted for them, 245 i.e. 18% used less than 30% and, finally, 141 firms i.e. 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 hire an employee or the employee was laid off before two years had passed but some were not paid the subsidy even though they had an employee. As pointed out in section 2, this can be due to bureaucracy involved in getting paid the subsidy, so that some firms decided not to bother to get the subsidy paid.

Figure 3 depicts the distribution of subsidized firms by employment in the year they received the subsidy. It shows that most firms that were granted the subsidy hired one to four employees: less than

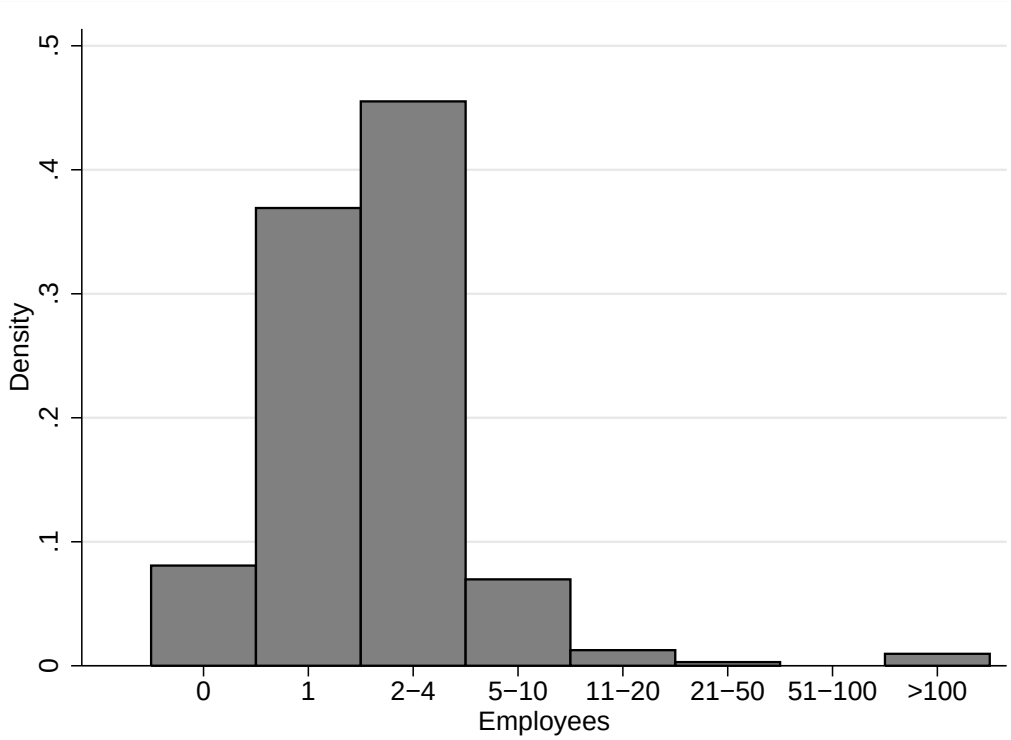
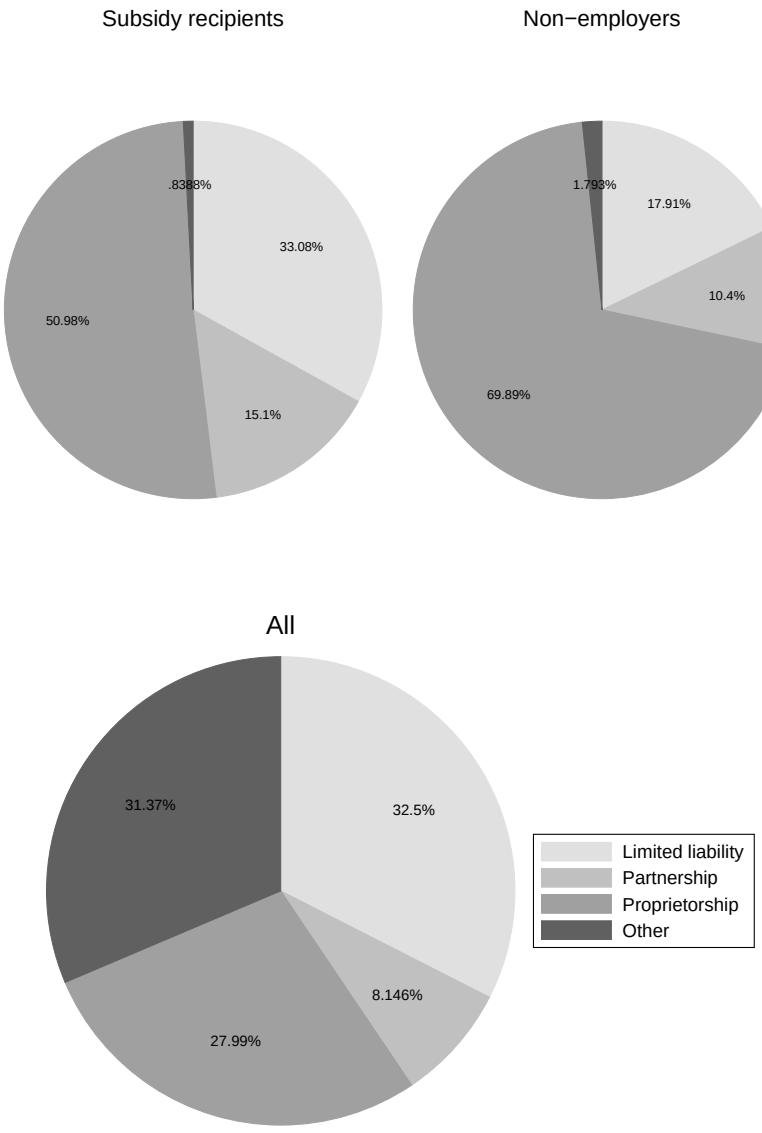


Figure 3: Firm distribution by employment of subsidized firms

40% had one employee while about 45% had 2–4 employees. Less than 10% still had zero employees and the shares decline with the number of employees with a small spike at more than 100 employees. Compared to figure 1 of the distribution of all firms in 2006, there is obviously not a large spike at zero employees but this mass seems to be mostly distributed between 1 and 2–4 employees, while the form of distribution of larger employment classes seems similar. This points out that most firms that were granted the subsidy hired 1–4 employees.

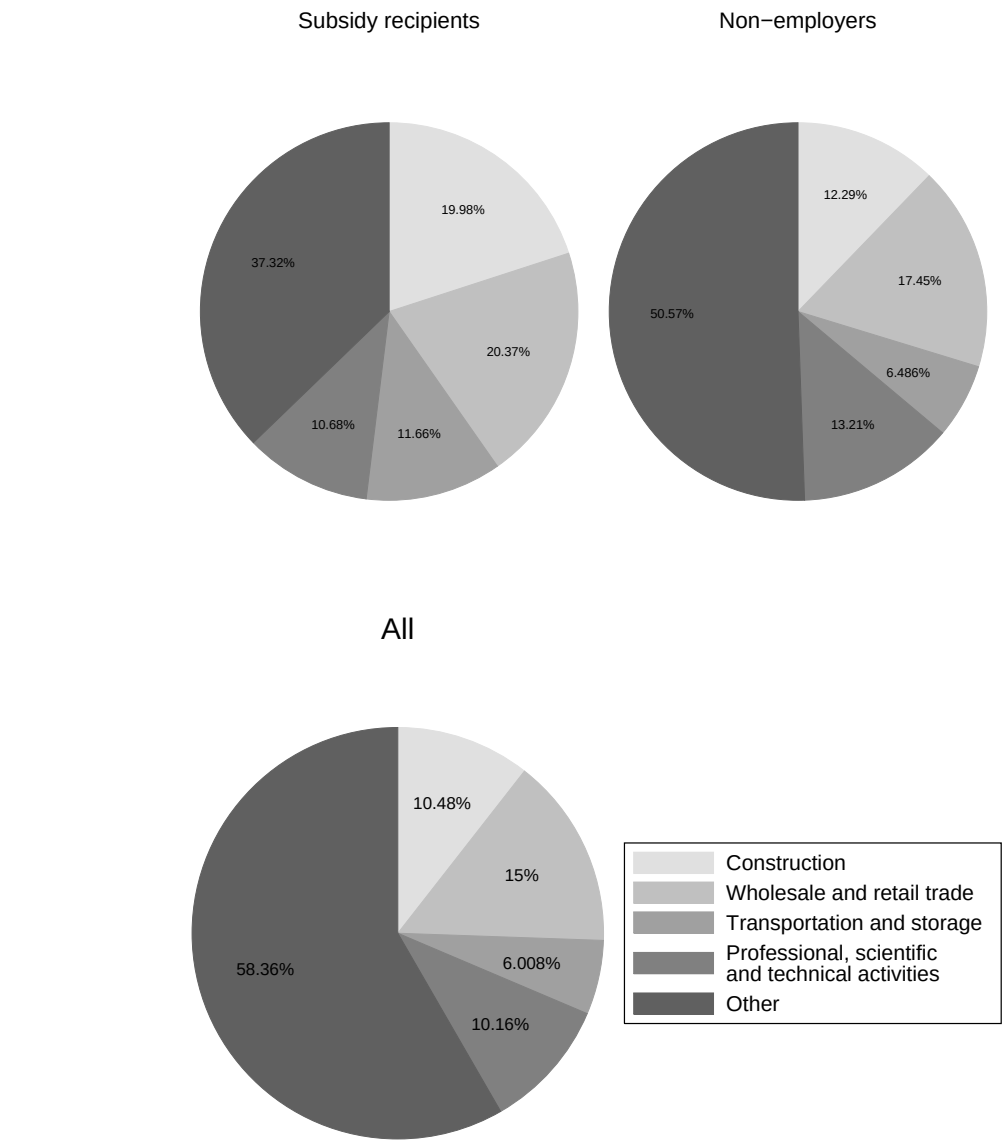
The pie diagrams in figure 4 depict the distributions of companies by juridical form. Non-employer firms are here defined as having zero employees while non-zero turnover (to exclude the large number of inactive companies). About 50% of the subsidized companies are sole proprietorships, which is a lot less than non-employers with about 70% and more than all firms with about 30% of sole proprietorships. Compared to non-employer firms, there are notably more limited liability firms and partnerships. Choice of juridical form is associated with the firms business plans and, therefore, employment. According to Fairlie & Miranda (2016) sole proprietorship firms are less likely to become an employer than other firms. Therefore, the firms that applied for the subsidy are more employment oriented on average according to the juridical form distribution.

Figure 5 has pie diagrams that depict the distribution of different industries within subsidized, non-employer and all firms. The largest industries within the subsidized firms are construction, wholesale and retail trade, transportation and storage, and professional, scientific and technical activities. These are also large industries within non-employer firms but construction, wholesale and retail trade, and transportation and storage are over-represented within subsidized firms in relation to non-employer firms while there is a slightly smaller fraction of firms in professional, scientific and technical activities.



All and non-employer firms in year 2006

Figure 4: Shares of firms by juridical firm



All and non-employer firms in year 2006

Figure 5: Shares of firms in different industries

	Subsidized	All	Non-employers	New employers
N	1,349	281,893	147,428	19,518
Share of:				
Has hired before	0.40	0.64	0.37	0.50
New	0.43	0.24	0.31	0.52
Non employers year before	0.30	0.49	0.84	0.73
Inactive year before	0.11	0.08	0.12	0.27

All, non-employer and new employer firms in year 2006

Table 2: Subsidized firms compared to other firms in the data

The fraction of companies in professional, scientific and technical activities is the same in subsidized and all firms, and there is the same fraction of firms in transportation and storage within all and non-employer firms. The shares of firms in construction and wholesale and retail trade are smaller in all firms than in non-employer and subsidized firms. Consequently, the subsidized firms differ with respect to industry distribution from other companies.

Table 2 shows shares of new firms, those that have had employees before and firms that were non-employers or inactive in the previous year within the group of subsidized, all, non-employer and new employer firms. New employer firm here means firms that have positive employment, and zero employment in the previous year or are observed for the first time in the data i.e. are at most 1 year old. These companies are those that would qualify for the first employee subsidy and, therefore, provide a natural comparison group to the subsidized companies. In 2006 there were 19,518 new employer firms. Compared to this number the yearly amount of firms that were granted the first employee – about 400 – is not very large, at about 2%. Of all 282,000 companies about 7% are new employers and 52% are non-employers.

The share of companies with a hiring history of subsidized firms is 40%, which is close to the share of non-employer firms with a hiring history but smaller than of new employers and all companies. The share of new firms at 43% (less than 3 years old) is clearly larger in subsidized firms than that in all firms and larger than in non-employer firms but, again, smaller than in new employers. Interestingly, only 30% of subsidized firms had zero employees in the previous year, while the fraction in all companies is 49%, in non-employer firms a large majority 84% and in new employers 73%. The share of firms, that were inactive in the previous year, is 11% in the subsidized firms which is roughly equal to non-employers. In all companies the share is a bit smaller but in new employers the number is clearly larger with 27%. Consequently, the group of subsidized firms is quite different from the comparison groups. The subsidized firms were much less likely non-employers or inactive in the previous year, and a bit less likely new or had hiring history than all new employers.

Table 3 summarizes descriptive statistics on employment, wage expenditures, turnover and net assets of subsidized firms when they were granted the subsidy and the year before (t-1), non-employer firms, new employers and all firms in 2006. The mean employment in subsidized companies is 2.4 and in the previous year 1.2, which are close to the median employment as well. New employers have a mean employment of 5.9 while the median is 2. Consequently, the median companies in these groups are quite similar but in new employers there are more firms at the tail of the distribution with large employment. The pattern is similar for other variables: the medians are much closer to each other than the means. Compared to non-employers the subsidized firms in the previous year have larger number of employees, wage expenditures, turnover and assets. Although the mean and median employment

VARIABLE		Subsidized (t-1)	Subsidized	Non-employers	New employers	All
	N	934	1,349	147,428	19,518	281,893
Employment	Mean	1.22	2.37	0	5.93	7.20
	Sd	(2.15)	(5.12)	0	(40.4)	(122)
	Min	0	0	0	1	0
	p10	0	1	0	1	0
	Median	1	2	0	2	0
	p90	3	4	0	9	10
	Max	37	161	0	2,400	34,500
Wages	Mean	8,530	22,400	211	69,000	138,000
	Sd	(17,500)	(26,800)	(1,33)	(670,000)	(3,480,000)
	Min	−4,460	0	−168,000	−4,870,000	−1.26e+07
	p10	0	1,670	0	0	0
	Median	250	14,700	0	6,700	0
	p90	30,000	51,100	0	93,300	146,000
	Max	234,000	295,000	15,000	5.42e+07	1.39e+09
Turnover	Mean	112,000	149,000	83,800	505,000	1,280,000
	Sd	(198,000)	(230,000)	(1.61e+06)	(6,410,000)	(6.75e+07)
	Min	0	0	−159,000	−25,200	−508,000
	p10	1	7,830	2,200	14,000	4,920
	Median	73,300	100,000	22,000	77,600	58,100
	p90	213,00	290,000	91,900	481,000	
	Max	3.18e+06	4.35e+06	3.30e+08	6.03e+08	3.22e+10
Net assets	Mean	25,000	22,000	68,200	171,000	636,000
	Sd	(130,000)	(112,000)	(1.79e+06)	(6,510,000)	(3.89e+07)
	Min	(−658,000)	(−687,000)	(−1.41e+07)	−5.19e+08	−5.19e+08
	p10	−9,530	−14,700	−5,840	−15,100	−8,200
	Median	6,370	5,580	3,750	8,610	10,300
	p90	54,400	60,800	50,100	92,400	225,000
	Max	2.54e+06	2.37e+06	3.78e+08	3.78e+08	1.02e+10

Table 3: Descriptive statistics of firms

VARIABLE	Simple	Covariates	1st year	2nd year	3rd year	4th year
Employment						
Difference	−3.342***	−1.695***	−1.897***	−1.589***	−1.561***	−1.457***
	(0.167)	(0.173)	(0.312)	(0.340)	(0.340)	(0.384)
Observations	464,103	406,840	113,751	108,601	103,907	94,964
R-squared	0.000	0.022	0.067	0.007	0.009	0.028
Wages						
Difference	−91,114***	−37,467***	−33,538***	-24,871*	−30,163***	−32,789***
	(11,729)	(3,429)	(5,083)	(12,928)	(7,391)	(6,541)
Observations	445,224	397,897	113,747	106,959	100,664	90,840
R-squared	0.000	0.003	0.013	0.001	0.128	0.175
Turnover						
Difference	0.265***	0.347***	0.301***	0.407***	0.410***	0.353***
	(0.0151)	(0.0150)	(0.0252)	(0.0234)	(0.0279)	(0.0330)
Observations	444,987	397,686	113,714	106,907	100,585	90,785
R-squared	0.000	0.101	0.086	0.105	0.115	0.114
Profit						
Difference	−25,614***	1,259	9,211	5,609	−7,858***	−612.1
	(4,119)	(3,234)	(6,946)	(3,725)	(2,605)	(4,099)
Observations	465,010	408,885	113,754	109,369	104,597	95,597
R-squared	0.000	0.437	0.631	0.726	0.023	0.186

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

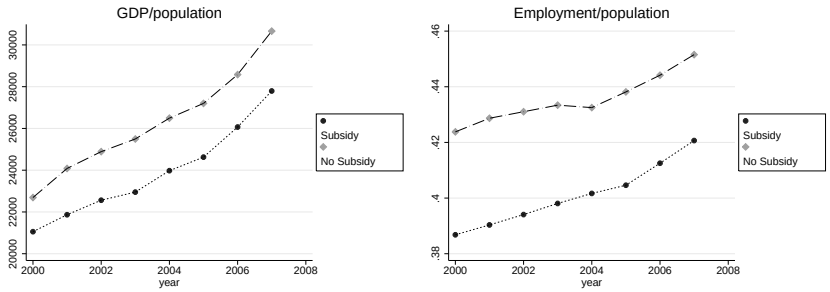
Table 4: Difference between subsidized firms and other new employers 4 years after hiring

in subsidized firms in the previous year is 1, the median wage expenditure is only 250. Therefore, the employment seems to be a bit biased and most of the companies do not really have employees. The median wage expenditure and turnover are greater in subsidized firms than in new employers. Therefore, it seems that the smallest firms, or those with smaller growth, that became employers were less likely to apply for the subsidy than the larger ones.

An essential question is how the subsidized firms perform afterwards compared to other new employer firms. Table 4 summarizes the regression estimates of the difference between the subsidized firms and new employers four years after hiring the first employee. This includes the average difference after four years as well as the yearly difference after the first, second, third and fourth year. The dependent variables used are employment, wage expenditure, log of turnover and profits.

The first column depicts the four years average regression summary of a simple regression using only the dummy for receiving subsidy as explanatory variable and the second regression with additional covariates.[3] The rest of the columns show regression summaries of the yearly outcomes. The difference in employment and wage expenditure is statistically significant and negative. The absolute difference is smaller in the fourth year and largest in the first. The subsidized firms have on average 1.5 less employees which also shows in wage expenditures. The difference in (log of) turnover is also statistically significant but small and positive and there is no clear trend. The estimated difference in profits, however, is not that clearly significant and the sign of the coefficient varies. However, as noted above the average difference is heavily influenced by the large firms at the right tail of the distribution of new employer firms so the regression estimates are highly affected by the tail of the distribution.

[3] Additional covariates are net assets, and indicators for year, juridical form and industry.



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, excludes capital area

Figure 6: Trend comparison of GDP and employment

4.2 Methodology

I use the difference-in-differences (Diff-in-Diff) method to estimate the effect of the first employee subsidy. This is a natural experiment approach that exploits the exogenous variation in the incentives of non-employer firms. Given that the assumptions behind Diff-in-Diff hold, the difference between the treatment and control group in the change in the outcome variable the average effect of the subsidy. Here, the interest is whether or how the probability of hiring the first employee changes in the subsidy area compared to the unsubsidized area. The effect can be estimated through a regression model:

$$Y_{it} = \alpha + \gamma S_{it} + \lambda P_{it} + \delta(S_{it} * P_{it}) + X_{it}^{'}\beta + \epsilon_{it} \tag{1}$$

for individual i in period t,Y_{it} is the dependent variable, S_{it} is a subsidy Dummy that equals one in the subsidy area and zero in the control area, P_{it} is a Dummy for the subsidy period and X_{it} is a vector of additional covariates. Therefore, γ is an estimate for the fixed effect of the subsidy area, λ is the common trend and δ equals the change in the difference between the areas i.e. the subsidy effect. Additional covariates add explanatory power.

The suitability of the natural experiment approach relies on a valid counterfactual for the treatment group. In Diff-in-Diff this means that there has to be a control group that would have developed similarly as the treatment, or subsidy, group if there was no change in policy. This, usually called the common trends assumption, is the main assumption and is usually assessed by examining pre-treatment trends. Differing pre-treatment trends are taken as a violation of the common trends assumption but similar trends cannot, of course, guarantee similar trends in the future. In addition to similar pre-treatment trends, the groups need to be similar enough to each other so that they can be expected to follow the same trends.

Figure 6 depicts the trends of GDP and Employment share in the subsidy area and the rest of Finland (excluding the capital area). The levels are clearly higher in the no subsidy area while the trends seem similar. While the levels themselves are irrelevant for the Diff-in-Diff assumptions, they can affect the validity of the approach if the level is an indication of how the areas will develop. In fact, the subsidy was first directed to areas suffering from population loss and unemployment, which may mean that the firms in these areas face a different environment and, therefore, can have different trends.

To assess the suitability of the method, I only focus on firms relevant to the subsidy. First, firms that have more than 50 employees are excluded to exclude the largest, a small fraction of companies that would have a strong effect on the averages. Also, firms in the capital area are excluded, since the capital area has a greatly different economic development compared to the rest of the country and the subsidy was not available there. To restrict the data to those that may be affected by the subsidy, only firms that are non-employers in some year – i.e. firms that have at least one observation with zero employees, are included.

Table 5 summarizes the descriptive statistics of firms in the subsidy area compared to the rest of Finland and all of Finland including the capital area in 2006. The table shows that the subsidy area has a larger share of non-employer firms compared to the rest of Finland and all of Finland. The average number of employees is 0.71 in the subsidy area, a bit larger than the national average (includes capital area) 0.69 but clearly smaller than the 0.84 in the no subsidy area. The mean of wage expenditure, turnover and net assets are smaller than in the no subsidy area as well. However, the medians of all the variables are closer to each other. This indicates that distribution of the firm populations differs. The no subsidy area has firms further at the right tail of the distribution that affect the averages a lot. In other words, most of the firm population is similar in the areas but a few large firms in the no subsidy area affect the area average significantly. Although the differences between distributions is not commonly paid attention to in Diff-in-Diff studies, it can be problematic for the common trends assumption. The outliers that have a large effect on averages may have different dynamics to the rest of the population. For example, small and large firms may react very differently to economic shocks such as the depression of 2009.

To improve the natural experiment setting, the estimation is restricted to the area on the border of the subsidy area. Figure 7 is a map depicting the treatment and control areas for this research. Consequently, the treatment area refers to the subsidy municipalities at the border of the subsidy area and the control area includes the municipalities neighboring the treatment area. It is much more plausible that neighbouring municipalities have similar economic conditions and, indeed, it is not likely that the neighboring municipalities should have significantly different economic conditions for firms. Also, the border of the subsidy area excludes the economically worst and best areas, therefore, including municipalities closer to the Finnish average.

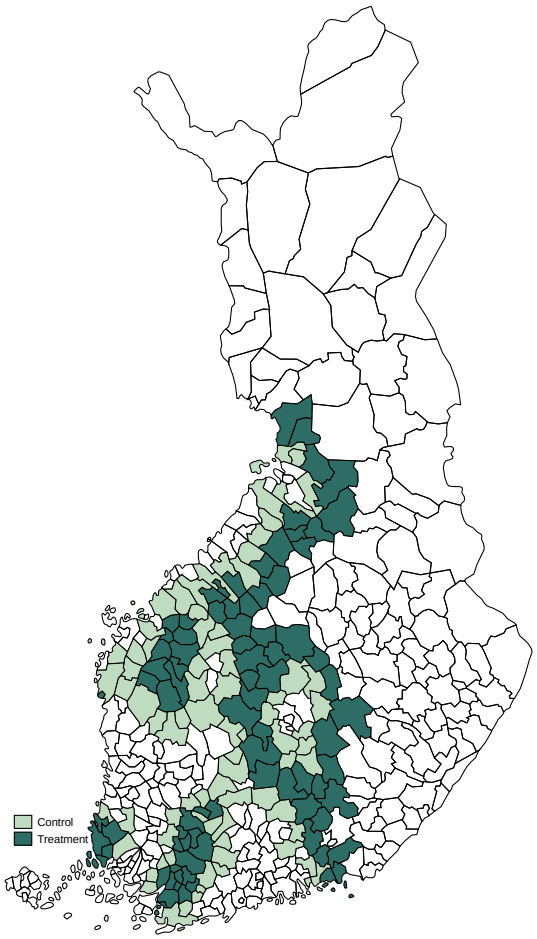
Table 5 also includes descriptive statistics of firms in the treatment and control areas. The statistics support the hypothesis that these firm populations resemble each other more. Mean employment is 0.67 in the treatment and 0.7 in the control area that are close to each other and the average of whole Finland 0.69. The medians and 90th percentiles are the same. Also means of wage expenditures, turnover and net assets are similar with all but net assets being slightly smaller in the control area. Also medians and percentiles are of the same magnitude, indicating that the firm distributions are similar in the areas. Consequently, restricting the estimation to the border area seems to make the counterfactual more plausible.

Figure 8 depicts the trends of average employment, wage expenditure, (log of) turnover per firm and share of new employers (defined as having at least one employee while zero employees in the previous year). The figures above plot the yearly changes compared to year 2000 and below the change in the difference compared to year 2000 with 95% confidence intervals. Both areas show similar trends. Employment declines quite sharply at the beginning of the 21st century and slowly after that. Wage

VARIABLE		AREA Subsidy	Treatment	No Subsidy	Control	All Finland
N firms		53,381	20,474	42,624	20,690	188,791
N non-employers		39,498	15,394	31,663	15,648	
Share of non-employers		0.79	0.75	0,74	0,75	0,75
Employment	Mean	0.71	0.67	0.84	0.7	0.69
	(sd)	(2.49)	(2.5)	(2.5)	(2.95)	(2.6)
	Min	0	0	0	0	0
	p10	0	0	0	0	0
	Median	0	0	0	0	0
	p90	2	2	2	2	2
	Max	50	50	50	49	50
Wages	Mean	6,850	6,800	11,400	6,200	8,100
	(sd)	(36,300)	(34,600)	(59,600)	(30,600)	(43,700)
	Min	−24,000	−24,000	−614,000	−41,200	−614 180
	p10	0	0	0	0	0
	Median	0	0	0	0	0
	p90	14,400	14,400	24,000	13,500	16,600
	Max	3,370,000	1,800,000	2,870,000	1,370,000	3,370,000
Turnover	Mean	91,600	95,200	282,000	87,500	140,000
	(sd)	(470,000)	(523,000)	(6,030,000)	(555,000)	(3,040,000)
	Min	−6,300	−2,230	−159,000	−2,600	−159,000
	p10	3,360	3,560	2,740	3,460	3,100
	Median	32,400	34,200	30,200	33,000	31,600
	p90	158,000	162,000	212,000	87,5,000	166,000
	Max	3.82e+07	3.82e+07	1.02e+09	5.81e+07	1.02e+09
Net assetts	Mean	39,200	33,500	192,000	35,900	79,500
	(sd)	751,000	285,000	3,320,000	464,000	1,790,000
	Min	−3,630,000	−1,410,000	−1.65e+07	−8,000,000	−1.65e+07
	p10	−8,460	−8,240	−7,000	−7,880	−7,800
	Median	5,020	5,160	6,200	4,820	5,040
	p90	62,300	61,700	112,000	61,600	71,500
	Max	1.37e+08	1.71e+07	3.78e+08	4.08e+07	3.78e+08

Includes firms with less than 50 employees in 2006, non-zero turnover and number of employees zero in some year between 2000 and 2013.

Table 5: Descriptive statistics of non-employer firms by area in 2006



Notes: Map drawn using municipality borders from 2007

Figure 7: Treatment and control areas in the research

expenditure declines first with employment but after that increases as wages increase in time. Turnover declines in time slowing in the mid 2000s but drops significantly in the depression of 2009 staying pretty much unchanged after that. Employment and wage expenditure in the treatment area increase a bit in comparison to the control group from 2004 to 2005 but the change is within confidence intervals. There is no difference in the trend of turnover or share of new employers. Consequently, the trend examination does not give reasons to reject the common trends assumption.

The second assumption is treatment exogeneity or, in other words, no composition change. If firms that plan to become employers move to the subsidy area, the estimated effect would include selection bias that results from the subsidy area having more employment-oriented companies than the unsubsidized area. However, few firms change their location in 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 with a slightly larger difference. Consequently, the selection bias is not likely to be a major problem in this study.

In addition, there should be no simultaneous changes in policy that affect the treatment and control

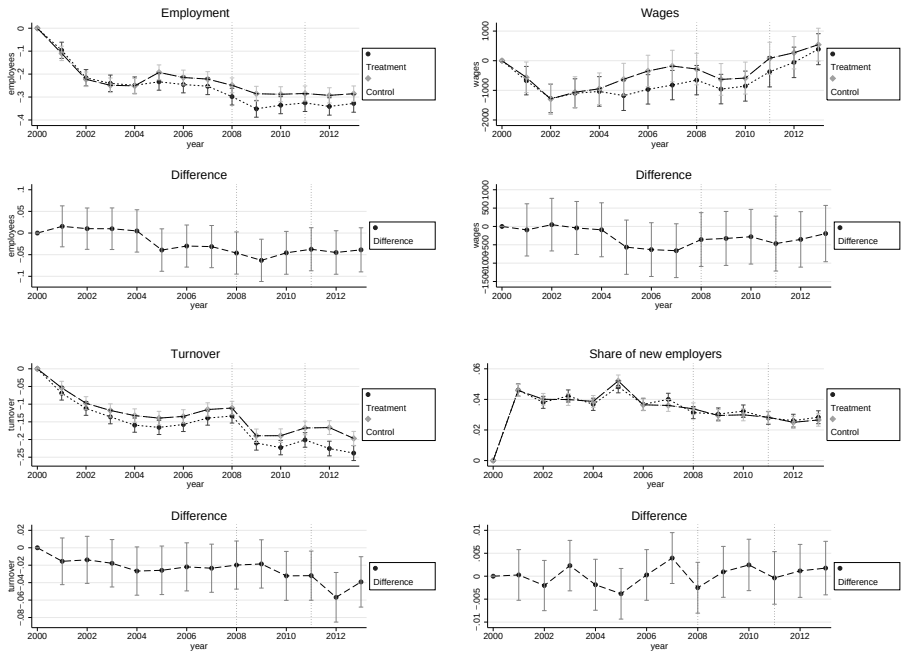


Figure 8: Firm trends in the research area

groups differently and are relevant for the outcome, because then the effect would include the effect of many policy changes. As the subsidy area is defined especially for this subsidy, there are no changes along the same regional criteria. Also, there should be no spillover effects between the groups, since this would lead to over- or under-estimation of the effect. Because the first employee subsidy affects only small shares of markets and it is not granted if it is evaluated to distort competition, it is not likely to have spillover effects.

5 Empirical analysis

5.1 Simple Diff-in-Diff

To estimate the effect on the probability of becoming an employer, the analysis needs to be restricted to firms that are *at risk* of becoming an employer i.e. non-employers. Consequently, the interest is on whether a firm is an employer conditional on being a non-employer in the previous year (since the data is yearly this cannot be identified more precisely). However, simply including firms that had zero employees in the previous year leads to selection bias after the subsidy period begins: Firms that become employers exit the data, leaving a selected group of firms for the next year that includes companies less likely to hire. The areas are no longer comparable after the first year of the subsidy because there is no selection in the control area.

To address this issue, only companies that have zero employees in the year before the subsidy period are included. This way there is no selection bias since the sample is formed before the subsidy. Only the area where the subsidy became available in 2008 is included for consistency. Correspondingly in the control area, only the municipalities that are neighbors to the 2008 treatment municipalities

By	(1) Simple	(2) Simple + year	(3) Extended	(4) FE + year	(5) FE extended
2008	0.000172	0.000172	0.00501	−0.000103	0.00601
	(0.00411)	(0.00411)	(0.00552)	(0.00538)	(0.00524)
Observations	124,526	124,526	42,224	124,526	42,224
R-squared	0.004	0.004	0.018	0.002	0.004
Number of firms				87,673	32,451
2009	0.00975*	0.00975*	0.0101	0.00651	0.0105
	(0.00566)	(0.00566)	(0.00650)	(0.00705)	(0.00658)
Observations	106,935	106,935	41,668	106,935	41,668
R-squared	0.000	0.000	0.032	0.001	0.004
Number of firms				71,380	31,485
2010	0.0109	0.0109	0.0107	0.00523	0.00300
	(0.00670)	(0.00670)	(0.00719)	(0.00718)	(0.00740)
Observations	100,143	100,143	43,373	100,143	43,373
R-squared	0.000	0.000	0.040	0.001	0.007
Number of firms				66,252	31,515
2011	0.00692	0.00692	0.00847	0.00184	−0.000594
	(0.00644)	(0.00644)	(0.00840)	(0.00520)	(0.00728)
Observations	73,335	73,335	40,399	73,335	40,399
R-squared	0.000	0.000	0.049	0.010	0.018
Number of firms				56,517	29,442

Robust standard errors clustered at the municipality level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Estimated effect using simple Diff-in-Diff

are included. In short, the treatment and control areas are as defined in figure 7 but excluding municipalities where the subsidy became available in 2009 and 2010 and their neighbors in the control area. To estimate the change in the probability also pre-treatment observations are needed. The pre-treatment period is defined as the four years before the subsidy period i.e. 2004–2007 when firms that have zero employees in the preceding year are included. To conclude, firms that have zero employees in 2003 are included in 2004–2007 and firms that have zero employees in 2007 are included in 2008–2011. If a firm is a non-employer in 2003 and 2007 it is included in both periods.

The outcome variable is a Dummy for being an employer that equals zero when the firm has zero employees and one when the firm has a positive number of employees. I do regressions with five different specifications: The first, denoted as simple, includes only treatment area and period Dummies as regressors. The second, simple + year, has year dummies as additional explanatory variables to account for fixed year effects and the third, i.e. the extended model, has additional individual covariates: industry, net assets, juridical form, and employment level in the municipality. The fourth and fifth models include firm fixed effects (fe) to account for firm specific unobserved effects and are otherwise the same as the second and third, respectively. The effect on the probability that the firms hired their first employee by the first, second, third and fourth year are estimated separately.

Table 6 summarizes the regression results. The estimated subsidy effect is negative in the first year in the models that do not take any firm characteristics into account but positive, although really small, in all the others. The estimated effect is really small in all the cases with at most 1.5% and

By	All	Partnerships & Limited	New firms	VAT liable
2008	0.00601	0.00728	0.0107	0.00607
	(0.00447)	(0.00843))	(0.0334)	(0.00769)
Observations	42,224	19,017	2,399	27,198
R-squared	0.004	0.008	0.094	0.006
Number of firms	32,451	13,954		21,409
2009	0.0105	0.0121	0.0159	0.00894
	(0.00658)	(0.0109)	(0.0292)	(0.0106)
Observations	41,668	18,531	2,502	26,141
R-squared	0.004	0.010	0.159	0.006
Number of tunnus	31,485	13,683		20,484
2010	0.00300	0.00881	0.0165	0.00787
	(0.00740)	(0.0124)	(0.0285)	(0.0102)
Observations	43,373	17,951	2,695	28,549
R-squared	0.007	0.016	0.142	0.005
Number of tunnus	31,515	12,907		21,421
2011	−0.000594	0.00678	0.0313	0.00182
	(0.00728)	(0.0137)	(0.0338)	(0.0100)
Observations	40,399	15,518	2,550	27,066
R-squared	0.018	0.036	0.145	0.015
Number of tunnus	29,442	11,517		20,298

Robust standard errors clustered at the municipality level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Estimated effect from simple Diff-in-Diff for different subsamples

mostly between 0.3 and 0.7 percent. Only one estimate, the effect on hirings by the second year in the FE extended model, is significant at the 10 percent significance level. There does not seem to be any clear difference between the estimated effect in different time periods. All in all, the standard errors are small and, thus, the estimates are quite precise.

As most non-employer companies never hire, the results may be driven by this majority even though there would be an effect on companies that are more open to hiring. Also, the effect on different firms may be different. Therefore, the estimation is also done for subgroups that are more likely to hire. According to analysis by Fairlie & Miranda (2016) new companies and those that have some other juridical form than sole proprietorships are more likely to hire. Therefore, the analysis is done for new firms, here defined as at most 3 years old, and for companies that are not sole proprietorships. Also, to exclude really small companies, those under the limit for value added tax (VAT) liability are excluded. The limit for VAT liability is a turnover of 8,500 euros.

Table 7 summarizes the results of the relevant subsamples. Here new firms are those that are new, at most 3 years old, in the reference year. For example, new firms that have zero employees in 2007 are included in 2008–2011 in the new firms' subsample. The same goes for VAT-liability. Only the estimates of the fe extended model are presented, except for new firms theestimates from the extended model is presented (for the sole reason that new firms in 2004 and 2007 are different and, therefore, firm fixed effects do not add anything).

By the first year the estimates are of similar size for all the subgroups as for all companies ranging from 0.6% to 1.1% with a slightly larger estimated effect on new firms. However, by the third and

fourth year the estimated effect on all companies is about zero on all and VAT-liable firms but a slightly larger on other than sole proprietorships at 0.7% and a much larger estimated effect on new firms at 3.1%. However, the estimated effects are still within confidence intervals and the standard error for new firms is so large that not much can be inferred from the estimates, as the sample size is much smaller.

In conclusion, the results do not suggest that the subsidy had a significant effect on the probability of becoming an employer. However, the problem with this approach is that it does not use information very efficiently since it excludes a lot of data. For example, a firm that is a non-employer in 2008 but not in 2007 and, therefore, can use the subsidy in 2009, is excluded from the analysis.

5.2 Proportional Hazards

The probability of becoming an employer can also be estimated using a Cox proportional hazards model. The proportional hazards model defines the probability of an event at time t after the onset of risk conditional on being at risk until t. Here, as above, a firm is defined as being at risk when it is a non-employer. Moreover, this approach enables us to take into account how long the firm has been a non-employer. As non-employer firms are less likely to hire the longer they have been non-employer firms, this is a good feature in a model estimating the probability of hiring the first employee.

The proportional hazard model defines the hazard function as

$$h(t, x_i) = h_o(t) \exp(x_i \beta), \tag{2}$$

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. Here the hazard is assumed to be the same within an interval of a year but can vary between years. In this case the baseline hazard is written:

$$h_o(t) = \exp(\sum_{i=1}^{13} h_{io}(t) * I((i-1) \leq t \leq i) \tag{3}$$

To identify the effect of the first employee subsidy, the Diff-in-Diff method is used to estimate the baseline hazard. Therefore,

$$h_o(t) = \exp(\alpha + \gamma S_{it} + \lambda P_{it} + \delta(S_{it} * P_{it})). \tag{4}$$

Data from 2005–2013 is used for estimation.[4]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 Diff-in-Diff approach above, the proportional hazards model uses more information and can account for 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 suffers from selection bias by

[4]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.

VARIABLES	All		Partnership & limited		New		VAT liable	
	Coef.	RH	Coef.	RH	Coef.	RH	Coef.	RH
Treatment	0.0287	1.029	0.0500	1.051	0.0542	1.056	0.0458	1.047
Effect	(0.0398)	(0.0410)	(0.0493)	(0.0519)	(0.0769)	(0.0812)	(0.0390)	(0.0408)
Post	0.0413	1.042	0.0213	1.022	0.0380	1.039	0.0544	1.056
Effect	(0.0572)	(0.0597)	(0.0785)	(0.0802)	(0.125)	(0.130)	(0.0545)	(0.0575)
Observations	191,490	191,490	53,059	53,059	28,118	28,118	148,010	148,010

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

Table 8: Effect in different subsamples estimated using Cox proportional hazards model

construction, so it is less reliable. Nonetheless, this approach can be used as an addition to the simple approach.

Three model specifications are used: simple, simple + year and extended as above in section 5.1, but since the results from the alternative models are consistent only the results from the extended model are reported. Table 8 summarizes the results for the same subgroups as above. These estimates are slightly larger as in the simple Diff-in-Diff approach but so are the standard errors so that none of the estimates are statistically significant. The estimated effect on all companies is 2.9% and the largest estimated effect is on new companies at 5.4%. Consequently, the point estimates are small and not statistically significant. 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.

Firms that have been non-employers for a shorter time are more likely to hire. 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 growing the business at all and the subsidy may have zero effect on them.

Table 9 summarizes the estimated effect that accounts for time at risk from the extended model. The effect, again, is estimated to be small in most of the cases and not statistically significant. The effect increases with longer duration at first but drops to below zero at 6 years at risk. After that the estimated effect increases again and the estimated effect for companies at risk for 9 years is very large: firms with 9 years of being a non-employer are almost 4 times more likely to become employers with the subsidy. However, the estimated effect is very large only for one duration at risk that has a smaller number of observations than other durations and there is no consistent pattern to how the effect changes with duration at risk. Consequently, these results do not provide conclusive evidence on how the effect differs between different times at risk.

Duration of time at risk	Coef.	RH
1	−0.0287	0.972
	(0.0582)	(0.0566)
2	0.00888	1.009
	(0.0884)	(0.0892)
3	0.0490	1.050
	(0.114)	(0.120)
4	0.171	1.186
	(0.132)	(0.157)
5	0.185	1.203
	(0.119)	(0.144)
6	−0.0915	0.913
	(0.148)	(0.135)
7	0.000149	1.000
	(0.161)	(0.161)
8	0.357	1.429
	(0.285)	(0.407)
9	1.314***	3.719***
	(0.391)	(1.453)
Observations	164,244	164,244

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

Table 9: Estimated effect varying with respect to duration of non-employer spell

5.3 Effect on firm entry and exit

The first employee subsidy can also increase incentives to start a business, as well as grow an existing business. This effect on firm entry is difficult to asses for the sole reason that potential business ideas do not exist in any data before the firm enters the markets. Thus, there is no target group in the data. 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. Also, the subsidy encourages splitting companies or founding many companies instead of one to employ the first employee in several companies. This should also be seen as an increase in the number of firms. I follow the method used in Bennmarker *et al.* (2009) that aggregates data on the municipality*industry level. I use the two code industry level to aggregate data which gives 8,244 different municipality*industry observations in total.

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. The regressions are done separately for each year in the subsidy period and for the whole period. The number of firms in 2008–2011 is compared to the average of 2006–2007.

Table 10 depicts the effect on the number of firms estimated in different models. The estimated effect is negative except for a few estimates and always in the preferred model. On average in 2008–2011 the number of firms in the subsidy area decreased about 4% compared to the control area. Most estimates are very small and not statistically significant except that the estimates in 2008 are weakly statistically significant. Therefore, the results do not suggest that the subsidy had a significant effect on the number of firms. However, the standard errors are quite large so that the effect would have to

Post reform year	(1) Simple	(2) Simple + year	(3) Extended	(4) FE + year	(5) FE extended
2008	−0.0738*	−0.0738*	−0.0871***	−0.0333	−0.0400*
	(0.0397)	(0.0397)	(0.0316)	(0.0292)	(0.0222)
Observations	7,207	7,207	7,016	7,207	7,016
R-squared	0.004	0.004	0.666	0.078	0.751
Number of fe				2,634	2,591
2009	−0.0630	−0.0630	0.0336	−0.0539	−0.0373
	(0.0441)	(0.0441)	(0.0680)	(0.0331)	(0.0252)
Observations	7,304	7,304	6,778	7,304	6,778
R-squared	0.003	0.003	0.672	0.107	0.700
Number of fe				2,639	2,614
2010	−0.00721	−0.00721	0.00413	−0.0180	−0.0127
	(0.0700)	(0.0700)	(0.0723)	(0.0404)	(0.0309)
Observations	6,923	6,923	6,761	6,923	6,761
R-squared	0.007	0.007	0.670	0.146	0.690
Number of fe				2,617	2,609
2011	−0.0509	−0.0509	−0.0326	−0.0573	−0.0543
	(0.0686)	(0.0686)	(0.0738)	(0.0420)	(0.0350)
Observations	6,850	6,850	6,677	6,850	6,677
R-squared	0.003	0.003	0.657	0.034	0.644
Number of fe				2,596	2,575
2008–2011	−0.0521	−0.0501	−0.0224	−0.0413	−0.0375
	(0.0449)	(0.0453)	(0.0535)	(0.0300)	(0.0245)
Observations	13,794	13,794	13,153	13,794	13,153
R-squared	0.004	0.005	0.674	0.079	0.659
Number of fe				2,689	2,677

Robust standard errors clustered at the municipality level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Effect on number of firms

Number of	Firms	Non-employer firms	Employer firms	New firms	Founded firms
2008	−0.0400*	−0.0535*	−0.0496*	−0.0398	−0.0205
	(0.0222)	(0.0280)	(0.0269)	(0.0330)	(0.0649)
Observations	7,016	6,060	5,866	5,151	3,079
R-squared	0.751	0.508	0.588	0.421	0.484
Number of fe	2,591	2,303	2,213	2,108	1,701
2009	−0.0373	−0.0543	−0.0472	−0.0520	−0.0135
	(0.0252)	(0.0355)	(0.0301)	(0.0359)	(0.0750)
Observations	6,778	5,844	5,686	4,970	2,923
R-squared	0.700	0.475	0.570	0.332	0.418
Number of fe	2,614	2,323	2,253	2,122	1,633
2010	−0.0127	−0.0212	0.00842	−0.00636	0.0243
	(0.0309)	(0.0383)	(0.0352))	(0.0437)	(0.0748)
Observations	6,761	5,693	12,755	4,970	2,906
R-squared	0.690	0.549	0.349	0.349	0.310
Number of fe	2,609	2,263	5,451	2,147	1,644
2011	−0.0543	−0.0357	−0.0196	−0.0184	0.0570
	(0.0350)	(0.0408)	(0.0391)	(0.0508)	(0.0690)
Observations	6,677	5,807	5,648	4,871	2,808
R-squared	0.644	0.484	0.546	0.301	0.409
Number of fe	2,575	2,336	2,240	2,102	1,582
2008–2011	−0.0375	−0.0468	−0.0293	−0.0300	−0.00413
	(0.0245)	(0.0304)	(0.0272)	(0.0326)	(0.0586)
Observations	5,838	11,504	11,034	9,888	6,676
R-squared	0.494	0.435	0.512	0.323	0.320
Number of fe	2,677	2,436	2,328	2,286	2,041

Robust standard errors clustered at the municipality level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Effect on number of different firms

be unreasonably high for it to show in the analysis so not much can be concluded. Nevertheless, the effect should not be negative.

Further study shows whether the subsidy has an effect on the number of some firms. For example, if the subsidy increases the number of employer firms at the expense of non-employer firms the effects could crowd out each other and not show in the aggregate number of firms. Table 11 depicts the effect for the number of firms in total and separately for non-employer firms, employer firms, new firms (at most 3 years old) and newly founded firms (at most 1 year old). The estimates do not differ by group. The estimated effect is really small and not statistically significant at the 5% significance level. Also, the yearly differences between the changes in the number are greater than the differences between subsets of firms. Therefore, the results do not support the hypothesis that the subsidy had an impact at the firm entry/exit margin.

6 Robustness of results

6.1 Additional regressions

The simple Diff-in-Diff approach used above ignores a lot of data. Furthermore, since the data is yearly it is impossible to identify all firms that have been non-employers for 12 months. In addition, the employment info is not very precise in the data: some firms with positive employment have wage expenditures so small that they cannot have any full-time employees. Therefore, to avoid excluding eligible firms the analysis is also done for a sample that is defined less strictly. On the other hand, the sample should not include large firms or those clearly not non-employers. Therefore, this analysis is restricted to companies that are non-employers in some year and have at most 50 employees. In other words, if a firm has zero employees at least once in the data, it is included throughout its existence.

The regression results from this approach do not, strictly speaking, provide an estimate of the subsidy effect on the eligible firms. However, the size and significance of the Diff-in-Diff estimate can provide additional evidence provided that employer firms in the subsidy and control area develop similarly. Also, this approach makes it straight-forward to extend the analysis to additional variables. Although there is no effect on hiring, maybe the subsidized firms increase their employment more, either by number the of employees or hours worked, pay more wages or grow their business more. In addition, defining whether a firm became an employer is not precise due to the data limitations discussed above. These effects should be seen as an increase in employment, wage expenditures or turnover. The subsidy can also have an effect on the probability of being a non-employer. Therefore, employment, new employment (defined as the employees in the firm for at most 9 months), wage expenditure, turnover and a Dummy for being a non-employer are used as additional dependent variables in addition to a Dummy for being a new employer.

Table 12 summarizes the estimate of the effect on different variables. The results from this analysis do not change the conclusion that the subsidy did not have any effects on hirings. In addition, there does not seem to be any effect on the additional variables as none of the regression coefficient 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 that were affected by the subsidy.

6.2 Placebo regressions

To study the parallel trends assumption, similarity of pre-treatment trends is evaluated using placebo regressions. Placebo regressions are essentially the same as Diff-in-Diff regressions but instead of the subsidy period a placebo period is used in the regression. Consequently, the placebo interaction coefficient indicates if there is change between the trends of the groups before the subsidy.

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 Diff-in-Diff 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 effect on hiring an employee by the first and second year. Table 13 summarizes these results. All

VARIABLES	All	Partnership & limited	New	VAT liable
D(New employer)	9.20e-05	0.00151	−0.00726	
	(0.00221)	(0.00310)	(0.0137)	
Observations	291,265	115,089	49,528	
R-squared	0.002	0.002	0.018	
Number of firms	68,126	23,952	27,661	
D(Non-employer)	0.000410	0.00175	0.00332	
	(0.00441)	(0.00762)	(0.0152)	
Observations	271,061	96,956	49,436	
R-squared	0.004	0.010	0.009	
Number of firms	65,841	22,021	27,637	
Employment	−0.0395	−0.0764	−0.123	
	(0.0277)	(0.0625)	(0.0742)	
Observations	271,061	96,956	49,436	
R-squared	0.010	0.017	0.006	
Number of firms	65,841	22,021	27,637	
New employment	−0.0268	−0.0487	−0.113	
	(0.0204)	(0.0392)	(0.0710)	
Observations	297,815	119,475	49,558	
R-squared	0.009	0.014	0.005	
Number of firms	68,505	24,143	27,662	
Wages	252.7	992.8	9,140	
	(403.0)	(1,012)	(6,378)	
Observations	270,393	96,263	49,558	
R-squared	0.007	0.010	0.017	
Number of firms	65,675	21,867	27,662	
log(Turnover)	−0.00111	−0.00599	0.0135	
	(0.0135)	(0.0254)	(0.0398)	
Observations	270,270	96,156	49,542	
R-squared	0.012	0.025	0.035	
Number of firms	65,657	21,852	27,659	

Robust standard errors clustered at the municipality level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Includes firms that have zero employees at least once at each year they have non-zero turnover

Table 12: Effect on alternative variables in a population of non-employer firms

	All	Partnerships & Limited	New firms	VAT liable
2006	0.00319	0.00772	0.0143	−0.00723
	(0.00533)	(0.00801)	(0.0322)	(0.00703)
Observations	39,692	18,687	3,099	26,081
R-squared	0.014	0.023	0.105	0.016
Number of firms	28,399	12,492		19,042
2007	0.00244	−0.00611	−0.038	0.00411
	(0.00619)	(0.00852)	(0.0251)	(0.00868)
Observations	41,977	18,582	3,598	26,131
R-squared	0.047	0.053	0.199	0.056
Number of tunnus	29,207	12,614		18,578

Robust standard errors clustered at the municipality level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13: Basic Diff-in-Diff placebo regressions

Group	Simple	RH	Simple+year	RH	Extended	RH
All	0.0982*	1.103*	0.100**	1.105**	0.121***	1.129***
	(0.0513)	(0.0566)	(0.0510)	(0.0563)	(0.0410)	(0.0463)
Observations	280,493	280,493	280,493	280,493	100,121	100,121
Partnership	0.140*	1.151*	0.138*	1.148*	0.159***	1.172***
& limited	(0.0752)	(0.0865)	(0.0744)	(0.0855)	(0.0578)	(0.0678)
Observations	165,068	165,068	165,068	165,068	45,339	45,339
New	−0.0114	0.989	−0.00392	0.996	0.0416	1.042
	(0.0667)	(0.0660)	(0.0674)	(0.0671)	(0.0851)	(0.0888)
Observations	61,503	61,503	61,503	61,503	14,500	14,500
VAT liable	0.0768**	1.080**	0.0804**	1.084**	0.0851*	1.089*
	(0.0390)	(0.0421)	(0.0391)	(0.0424)	(0.0446)	(0.0486)
Observations	89,953	89,953	89,953	89,953	64,323	64,323

Robust standard errors clustered at the municipality level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 14: Placebo regression in the Cox proportional hazards model

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 in the Cox proportional hazards model for all of the subsamples used. For new firms the placebo effect is small and statistically insignificant in all of the models. However, the placebo effect on other groups is large and statistically significant in all of the models: in the preferred model at the 1% significant level for all firms and other than sole proprietorships and at the 10% significance level for VAT liable firms. The estimated placebo effect is about 12% for all firms and 16% for other than sole proprietorships. These estimates are very large, almost unbelievably so. These placebo regressions suggest that the parallel trends assumption does not hold.

However, these regressions include only two years for each period and are, consequently, quite sensitive to large deviations in one year. Actually, figure 8, which depicts the development of the share of new employers, shows that the difference changes most between 2004 and 2005. In fact, there is a

spike in 2005 but after that the trends look more even. The data has a lot of missing observations for employment until 2007 and the problem almost vanishes after 2005. This may affect the large spike in the share of new employer firms because defining new employer firms uses two observations that may be violated. I check the robustness of placebo regressions, by excluding one year at a time. The placebo results are not robust to this. For example, excluding year 2005 from the regressions reduces the estimates considerably and they are no longer statistically significant except in the extended model at the 5% significance level. Therefore, the year 2005 alone is essential in explaining the change in difference between the areas in 2004–2007. Therefore, these results are not conclusive evidence that the parallel trends assumption is violated.

7 Conclusion

This is the first research to study the effects of subsidizing the first employee empirically. I use the Finnish first employee subsidy regime in 2007–2011 as a natural experiment to estimate the effects. The results do not suggest that the subsidy had any effect on the probability of becoming an employer. Consequently, labor demand of non-employer firms is not estimated to be very elastic. I also studied the effect on starting a business or leaving markets and did not find any significant effects. These results are in line with more general studies on employment subsidies by Korkeamäki & Uusitalo (2009)and Bennmarker *et al.* (2009) in Finland and Sweden, concluding that employment subsidies did not increase employment in companies. Consequently, non-employer firms do not seemt to react more to employment subsidies than firms in general.

One reason behind the results can be that non-employer firms consist mainly of self-employed entrepreneurs that are not planning on growing their business and, therefore, not responsive to hiring incentives. A natural worry is that there is a group of firms that reacts to the subsidy but the large share of not-growth-oriented businesses hides the effect. This is considered by estimating the effect on some subgroups of firms that are known to be more growth oriented, but it is possible that there is still some reactive group of firms that is not identified. However, the group of reacting firms is, regardless, very small so that the aggregate effect is insignificant. The subsidy did not have the large employment effects that it was argued to have.

Becoming an employer can be a difficult step for a firm due to market imperfections. Since subsidizing the first employee did not have any effect, either the constraints to become an employer are not that significant or the subsidy was not successful at mitigating the constraints. In fact, firms were not well informed about the subsidy and the payment included bureaucracy so that the subsidy was not very well organized. This has implications for the generalizability of the results. It cannot be concluded that a first employee subsidy with a different institutional setting would not be effective.

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