Payroll taxes, firm entry and labour demand

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Highlights

- In 2016, Belgium permanently exempted all new employers from Social Security Contributions (SSC) for the first employee
- This policy allows us to study the relation between labour costs and the decision to hire a first employee
- The elasticity of the hiring decision with respect to the labour cost is -1.67, i.e. higher than the estimates of labour demand among existing employers typically reported in the literature
- The policy increased employment in small firms
- These findings validate the "free entry condition" commonly made in search and matching and firm dynamics models

Abstract

Many theoretical and empirical papers define 'firm birth' as the point in time when a firm hires its first employee. But few papers have explored to which extent labour costs determine the decision to hire a first employee. This paper exploits a unique and generous Belgian policy introduced in 2016 which permanently exempts new employers of Social Security Contributions (SSC) when hiring a first employee–thereby decreasing the labour cost of the first employee by 18% – to establish the relation between labour costs and the decision to hire a first employee. Using a Regression Discontinuity in Time (RDiT), we find a large jump in the number of new, first-time employers entering the market immediately after the reform. The elasticity of the decision to hire a first employee with respect to the labour cost is estimated at -1.67, which is higher than the elasticity of labour demand among existing employers which is more frequently studied in the literature. We also show that the policy created employment in small firms. One contribution of the paper is that it validates the "free entry condition", commonly made in firm dynamics models as well as search and matching models, which assumes that new employers can and will (immediately) enter the market so that the expected value of profits is always equal to a fixed entry cost.

1. Introduction

Numerous papers have estimated the elasticity of labour demand (Lichter et al., 2015). Theoretically, one can distinguish between the intensive margin, i.e. to which extent do existing employers hire more workers when labour costs drop, and the extensive margin, i.e. to which extent do labour costs affect the decision to hire a first worker and become an employer (Hamermesh, 1993).[1] Most empirical papers focus on the intensive margin or estimate a (weighted) average of the intensive and extensive margin, but do not consider to which extent labour costs affect the decision to become an employer.

The relation between labour costs and the decision to employ a first worker is nevertheless important. Young employers account for a disproportionate share of job creation (Decker et al., 2014; Haltiwanger et al., 2013) Even a temporary dip in the number of new employers – often referred to as start-ups – can have long-lasting effects on employment (Sedláček, 2020; Sedlacek & Sterk, 2020). Many governments support start-ups and offer (wage) subsidies to promote entrepreneurship and to encourage the solo self-employed to hire (Acs et al., 2016). Whether these policies are effective depends on the extent to which the decision to hire a first worker is sensitive to the cost of labour.

Hamermesh suggests that the elasticity of labour demand at the intensive and the extensive margin is of similar magnitude, but notes that little empirical evidence exists to back up this claim (Hamermesh, 2021). Typical features of (potential) employers including liquidity and credit constraints[2] (Fairlie, 1999; Holtz-Eakin et al., 1994), high recruitment and training costs (Muehlemann & Leiser, 2018)[3] and high compliance costs (Harju et al., 2019) offer arguments to expect that the decision to hire a first worker is more sensitive to the cost of labour than the decision of existing employers to hire an additional worker. Many studies indeed report larger effects on employment and investment of business support targeted at smaller firms.[4]

To establish the relation between the labour cost and the decision to hire a first employee, we exploit a unique and unanticipated policy in Belgium in place since 2016 which permanently exempts new employers from Social Security Contributions (SSC) for their first employee, thereby decreasing labour costs by approximately 18%. Using administrative micro-level panel data of the population of Belgian firms from the Crossroads Banks of Enterprises (CBE), we exploit that potential employers quickly adjusted labour demand to the policy-induced reduction in labour costs. This quick adjustment allows us to estimate a Regression Discontinuity in Time (RDiT). At the aggregate level, the number of firms hiring a first employee jumps up by roughly 30% immediately after the reform. In a similar vein, the probability to hire among nonemployer firms also increases by 30%. We find no evidence of strategic firm behaviour in which existing employers exit the market and establish new firms to benefit from the SSC exemption. Assuming that, in the short run, the SSC exemption is fully incident on the employer, our estimates indicate that the elasticity of the decision to hire a first employee with respect to the labour cost equals -1.67, which is higher than the elasticity of labour demand among existing employers which is typically in the range of minus one and zero (Lichter et al., 2015).

[1] It is more common to define the 'intensive' margin as the response in number of hours worked with respect to the labour costs and the 'extensive' margins as the response in terms of the number of workers hired. What we define as the '*elasticity of labour demand at the extensive margin'* is called the *'quasi elasticity of labor demand through plant openings'* by Hamermesh (1993).

[2] An extensive literature focuses on the potential of reducing financial constraints among solo self-employed micro-entrepreneurs in developing countries to create jobs (e.g. Banerjee & Duflo (2014); Kersten et al. (2017) for a review).

[3] Using unusually rich Swiss data, Muehlemann and Leiser (2018) show that recruitment costs amount to 16 weeks of wage payments. These costs are 50% higher in large (>50 employees) than small firms (<50 employees). But, given the limited number of employees in small firms, recruitment in small firms is a considerable long-term investment.

[4] E.g. Criscuolo et al. (2019) for the UK; Zwick et al. (2017) for the US; Decramer and Vanormelingen (2016) for Belgium.

We complement this evidence by exploring the impact of the SSC exemption on the number of employers and on employment in small firms using repeated cross-sectional aggregate data on the number of employers and employees grouped by firm size-sector-province cells obtained from the National Social Security Office (NSSO). Reassuringly, the jump observed in the number of new employers immediately after the 2016 reform is almost identical in the CBE and NSSO datasets. Using a difference-in-differences set-up contrasting employment in small versus slightly larger firms, we show that the SSC exemption increased the number of firms with exactly one worker and employment in those firms by 6.2% and 8.5%, respectively. Although we do not capture general equilibrium effects which may have caused job losses in larger firms (e.g. Cahuc et al. (2022)), our framework nevertheless provides suggestive evidence that, at least in the short run, the SSC exemption created additional employment.

This paper is among the first to provide causal evidence on the relation between labour costs and the hiring decision, and, subsequently, employment. Previous studies compared the characteristics of the self-employed with and without employees, in both the US (Fairlie & Miranda, 2017) and Europe (Dvouletý, 2018), showing that most solo self-employed entrepreneurs never hire and that most of the hiring occurs in the first years of existence. Both observations also hold in Belgium. Surprisingly, these studies did not consider the labour cost as a determinant of hiring decisions. Our paper is most closely related to the study of De Mel et al. (2019) who conducted an experiment in Sri Lanka offering wage subsidies to microenterprises of which most only employed the owner-manager. The wage subsidy amounted to approximately half the minimum wage and was offered for six months. The subsidy convinced microenterprises to hire a first worker, thereby increasing the total number of workers employed by microenterprises in the control group by 52%. These results appear to be in line with our findings.[5]

We also contribute to three other strands of literature. First, we contribute to the literature on payroll taxes, a topic that continues to attract considerable attention. While several older papers reported no positive employment effects of payroll tax cuts (Gruber, 1997), our paper is in line with more recent evidence which finds positive employment effects (Benzarti & Harju, 2021; Ku et al., 2020; Saez et al., 2012; Saez et al., 2019).[6] These findings challenge the canonical tax incidence model which predicts that, due to the low elasticity of labour supply, payroll taxes are fully passed on to the employee, with no effect on employment.

Second, our findings validate an important assumption in both search and matching models (e.g. ; Moen (1997); Mortensen and Pissarides (2001)) and firm dynamics models (e.g. Hopenhayn (1992); Melitz (2003); Sterk et al. (2021)). These models typically rely on a free entry condition to close the model, thereby assuming that new employers will (immediately) enter the market if labour costs decrease until the expected net value of profits over the firm's lifetime is equal to a fixed entry cost. Note that, with the notable exception of Bento and Restuccia (2019), these models do not make a distinction between employer and nonemployer firms, but simply assume that all firms employ at least one worker. Our findings support the free entry condition in that we observe an instantaneous increase in the number of employers when labour costs drop. One implication of firm

[5] Although the authors mention that the wage subsidy amounts to half the minimum wage in Sri Lanka, wages of the employees in the microenterprises are not reported nor is the impact of the subsidy on wages offered to the workers analysed. This prevents us from computing the elasticity of the hiring decision with respect to the labour cost. The elasticity is approximately -1 if we assume that the subsidy reduced wages by 50%.

[6] Bozio et al. (2019) conduct a meta-analysis and attempts to reconcile the contradicting findings reported in the literature. They show that payroll tax cuts are only (fully) passed on to the worker if there is a clear linkage between the SSC and (future) benefits. This finding suggests that the Belgian payroll tax cut, which has no impact on (future) benefits, will have positive employment effects.

dynamics models relevant to our particular setting is that the SSC exemption will induce low-productivity firms – which were not sufficiently productive to employ workers without the subsidy – to become employers (Bento & Restuccia, 2019).

Finally, the paper contributes to the large literature discussing whether and which policies supporting start-ups have the potential to create jobs (Acs et al., 2016; Dvouletý et al., 2021). Fackler et al. (2019), for instance, show that start-ups may provide opportunities for disadvantaged workers. We contribute to this literature by showing that entrepreneurs are indeed more sensitive to labour costs than existing firms so that supporting start-ups might be an effective instrument to support employment, but also warn that policies reducing labour costs for small firms may create and sustain low-productivity firms.[7]

The remainder of this paper is structured as follows. In the next section, we discuss the 2016 policy reform and present some descriptive statistics. Section 3 presents the data of the Crossroads Bank of Enterprises (CBE) used to examine the impact of the policy on the hiring decision and the data of the National Social Security Office used to examine the impact of the policy on employment. Sector 4 discusses the Regression Discontinuity in Time used in section 5 to examine to which extent labour costs affect the hiring decision. Section 6 exploits the NSSO data to evaluate whether the 2016 reform created new employers and employment, thereby reinforcing and extending our main findings presented in section 5. The final section concludes.

2. The policy: subsidizing the first employee

Labour costs consist of the gross wage paid to the employee and Social Security Contributions (SSC) paid by the employer. For most employers in the private sector, the nominal SSC rate currently amounts to 25% of gross wages. SSC are lower for low-wage workers.

On October 10, 2015, the government unexpectedly[8] announced that firms which hired the first employee after January 1, 2016, would be permanently exempt from SSC for the first employee.[9] Private sector firms are eligible for the SSC exemption if they did not employ a worker subject to SSC for at least four subsequent quarters. The law forbids firms to split into smaller units or to establish a new firm to benefit from the subsidy, a condition which is (imperfectly) enforced by the National Social Security Office (Court of Audit, 2021).

The SSC exemption has some remarkable and unique features. First, the SSC exemption is not time-limited, which makes it a very generous hiring subsidy. Second, firms keep the SSC exemption even if they continue to expand. Third, the SSC exemption is not linked to a specific individual so that firms keep the SSC exemption if the 'first employee' leaves and is replaced by a new worker. Fourth, firms with more than one employee can designate the employee for whom the exemption is claimed so that employers have an incentive to claim the SSC exemption for the employee with the highest gross wage to maximize the wage subsidy.

The 2016 reform replaced previously existing temporary hiring subsidies for the first employee which had been in place with some modifications since 2004 (see Appendix A for details). New employers

[7] Caliendo et al. (2020) advance a similar argument. They show that start-up subsidies for the unemployed are very effective in improving labour market prospects of the participants, but the subsidised start-ups are less productive than start-ups created without subsidies.

[8] We conducted a thorough search of the Belgian newspapers, and did not find a single article mentioning this policy before October 2015. The coalition agreement of the Michel I government (2014-18) did not mention this policy either.

[9] The law initially stipulated that the SSC exemption would only be granted for entrepreneurs hiring a first employee before December 31, 2020. In 2020, the new government extended the policy until the end of 2021. From 2022 onwards, the SSC reduction is capped to €4,000 per quarter, but remains permanent.

hiring a first employee in 2015 could claim a temporary quarterly SSC reduction of €1,550 during the first 5 quarters, followed by a SSC reduction of €1,050 in the subsequent 4 quarters and €450 in the last 4 quarters.

Economic theory predicts that the marginal entrepreneur hires a first worker if the expected profits of employing a worker are equal to the expected labour cost over the lifetime of the firm. Table 1 shows the change in the expected labour costs for entrepreneurs hiring a first employee with a gross monthly wage of €2,050 (which corresponds to the median gross wage of the first employee) before and after the 2016 reform, taking into account both the temporary SSC reduction in place before 2016 and the permanent SSC exemption in place since 2016. Whether a permanent SCC exemption is more generous than a temporary SSC reduction depends to a large extent on the time horizon considered by the entrepreneur when making hiring decisions. We compare the expected reduction in labour costs induced by the SSC exemption for entrepreneurs with an infinite time horizon and for entrepreneurs with a time horizon of three years. We assume that entrepreneurs have rational expectations about the probability of continuing to employ workers in the future.[10] The introduction of the permanent SSC exemption reduces expected labour costs by 18% for entrepreneurs with an infinite time horizon and by 5% for entrepreneurs with a time horizon of 3 years.

Table 1: The expected reduction in labour costs for the first employee after the reform

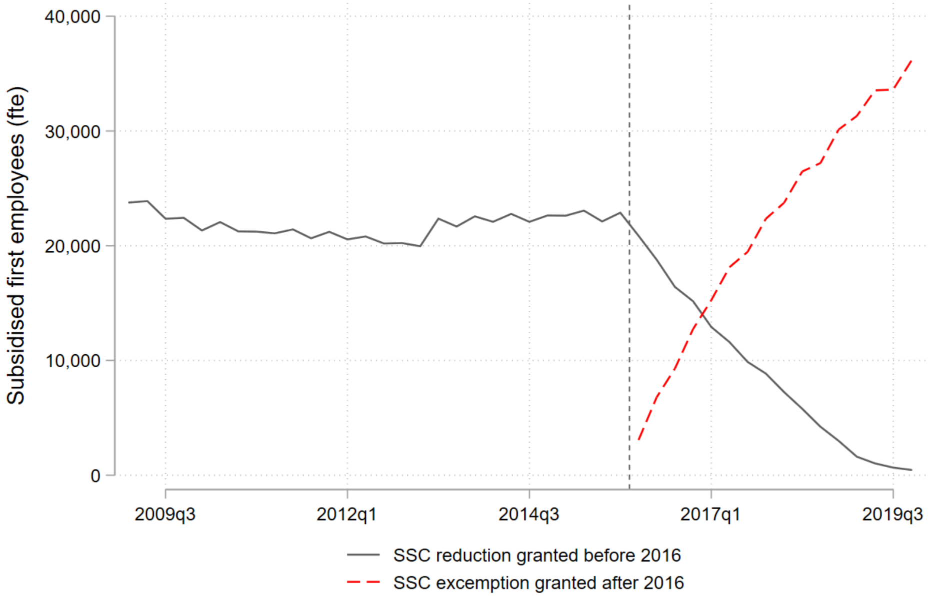
Type of entrepreneur	**Reduction labour costs**
Time horizon of 3 years	-5%
Infinite time horizon	-18%

Note: Assuming that SSC reductions are fully incident on the employer, the expected labour cost over the firm's lifetime can be computed as $E(labour\ cost) = \sum_{t=0}^{k} \frac{w(1+\tau_t)E(s_t)}{(1+r)^t}$, where w represents the gross wage; τ_t the SSC rate t quarters after having hired the first employee; $E(s_t)$ the expected probability that the firm still employs workers a quarter t; r the discount rate; and k the time horizon of the entrepreneur. The SSC reduction and exemption alter τ_t. After the reform, τ_t equals zero in all quarters; before the reform, τ_t depends on the quarter t after hiring and the gross wage w. The temporary SSC reduction in 2015 (just before the reform) amounted to € 1,550 during the first 5 quarters; € 1,050 in the subsequent 4 quarters; and €450 in the last 4 quarters. We assume a gross monthly wage of €2,050. The probability of continuing to employ workers in the first seven years after hiring a first employee were computed by the Federal Planning Bureau for the cohort of firms which hired a first employee in 2012 (Novella (2021), Figure 1). We assume that all firms continue employing workers after having done so for 7 years. The quarterly interest rate is set at 0.5%. An entrepreneur with a time horizon of 3 years only takes into account the labour costs in the first three years after hiring when making hiring decisions ($k = 12$); an employer with an infinite time horizon considers all quarters over the firm's lifetime ($k = \infty$).

The SSC exemption proved popular. Figure 1 shows the number of full-time equivalent (fte) subsidised "first" employees for whom a SSC reduction was granted under the rules in place before and after the 2016 reform. Until 2016 the number of fte subsidised first employees for whom the employer claimed a temporary SSC reduction fluctuated around 22,000. Since 2016 the number of first employees for whom employers were exempted from SSC exemption has gradually increased, surpassing 30,000 full-time equivalents by 2019. This implies that approximately 1% of the employees in Belgium is a subsidised first worker. The total cost of the policy amounted to 250 million euros by 2019 and continues to increase.

[10] The results are not sensitive to this assumption. Results are nearly identical when considering "optimistic" entrepreneurs who assume that their firm will always continue to employ workers.

Figure 1: Evolution of the number of subsidised first employees (fte)

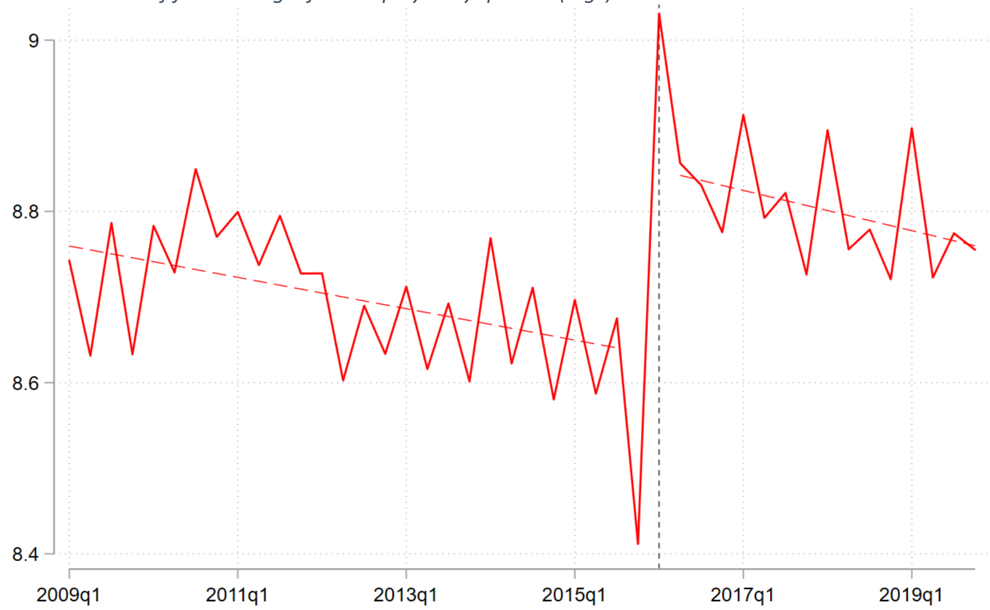


Note: Source: Quarterly data of the NSSO. Own compilation. The permanent SSC exemption replaced the temporary SSC reduction for the first employee for new employers from 2016 onwards. Employers who received temporary SSC reductions before 2016 continued to receive these reductions after the reform.

The increase in the number of subsidised employees after the reform is driven by two factors. The most important factor is that the permanent SSC exemption replaced the temporary SSC reductions which lasted at most 13 quarters. The switch from temporary to permanent SSC reductions mechanically increases the number of subsidised employees even if everything else had remained unaltered.

The second factor, which is the focus of this paper, is that the policy-induced reduction in labour costs convinced entrepreneurs to hire a first employee. The sharp jump in the number of firms hiring a first employee immediately after the 2016 reform is striking (Figure 2) and strongly hints at a positive effect of the SSC exemption on the hiring decision. Note that the figure also reveals anticipation and catch-up effects. The reform was announced in early October 2015, leading to a drop in the number of first-time employers in 2015Q4, followed by a strong increase in 2016Q1. The latter effect consists of a 'real' effect – i.e. entrepreneurs hiring a first employee who would not have hired in the absence of the reform – and a 'catch-up' effect – i.e. entrepreneurs hiring in 2016Q1 after postponing hiring in 2015Q4. The empirical strategy, discussed in the next section, will take the anticipation and catch-up effects into account.

Figure 2: Number of firms hiring a first employee by quarter (logs)



Note: Source: CBE. The population consists of all Belgian firms. Own compilation. Raw data: no adjustment for seasonal patterns.

In addition to the SSC exemption for the first employee, firms are also eligible for SSC reductions for the 2nd to the 6th employee. While the SSC exemption for the first employee is permanent, SSC reductions for the 2nd to the 6th employee are temporary, lasting at most 13 quarters, and are capped at a fixed amount which decreases with firm size and employment duration (see Appendix A for details). These SSC reductions can only be claimed when the firm is expanding, and are not granted when an employee is replaced.[11] Importantly, the 2016 reform introduced the permanent SSC exemption for the first employee, reinforced the SSC reductions for the 2nd to 5th employee and introduced a SSC reduction for the 6th employee. The SSC reductions for the 3rd to the 6th employee were further reinforced in 2017.

We argue that the switch from a temporary SSC reduction to a permanent SSC exemption for the first employee affects the entrepreneur's decision to hire a first employee, while the reinforcement of the SSC reductions for the 2nd to 6th employee can only marginally affect this decision. The SSC exemption for the first employee is far more generous than the SSC reductions for the next five employees, particularly for entrepreneurs with a long time horizon. Moreover, only a minority of new employers hire a second employee within a few years so that SSC reductions for the second employee are unlikely to be a decisive factor when deciding whether or not to hire a first employee.

3. Data

3.1. The CBE dataset

The starting point is an administrative, micro-level firm dataset of the population of Belgian firms from the Crossroads Bank for Enterprises (CBE). The CBE administers a comprehensive database of all companies and businesses in Belgium. By law, all firms have to register at the CBE before they can start their activities. This obligation holds for legal entities as well as sole proprietors and for non-profit as

[11] For instance, the subsidy is not granted if the 'third employee' leaves the firm and is immediately replaced by a new worker. When deciding whether or not to grant the subsidy, the NSSO determines the maximum number of workers in the last four quarters.

well as for-profit companies. In the second quarter of 2015, 970,172 firms were registered at the CBE of which 514,191 are nonemployers.

The CBE collects a limited set of firm characteristics such as the sector,[12] the legal form, the address of the firm and whether the firm is liable to VAT. Importantly, the CBE is automatically notified when a firm hires an employee subject to SSC and registers the exact date at which this event occurs. In addition, it registers when a firm no longer employs workers or leaves the market. The main limitation of the CBE data is that it only contains a dichotomous indicator indicating whether a firm employs workers subject to SSC, but does not contain information on the total number of workers employed by the firm.

We obtained micro-level data on the population of all firms active on June 30 for years from 2009 to 2019. The data selection procedures imply that firms that start and stop their activities between June 30 of year X and June 30 of year X+1, i.e. firms which are active for less than a year, are not included. Firms that die within a year are unlikely to hire employees.

A potential concern with this dataset is that sole proprietors, which account for approximately 60% of nonemployer firms at any point in time, obtain a new unique firm identifier when transforming their business to a firm with a legal entity such as a private limited liability company.[13] As a result, these firms are wrongly classified as 'new firms' and cannot be tracked over time. Sole proprietors have an incentive to re-register as a private limited liability company when hiring a first employee to limit their personal risk in case of bankruptcy. This issue does not affect our baseline RDiT specification because this specification does not exploit the panel dimension of the CBE data. It may bias, however, the specification which examines the impact of the reform on the number of new firms established each month and the robustness check which exploits the panel dimension to investigate how firms established before the reform was announced responded to the policy.

3.2. The NSSO dataset

The micro-level CBE data allows us to study the effect of the reform on the decision to hire a first employee. The absence of information on the number of workers employed by a firm prevents us from using the CBE data to investigate the effect of the reform on employment.

We, therefore, complemented the CBE data with aggregate, repeated cross-sectional quarterly data from the National Social Security Office (NSSO) for the period 2009Q1 to 2019Q4. The NSSO administers SSC paid by employers and employees. We obtained data on the number of firms and the number of full-time equivalent (fte) employees disaggregated by firm size, sector and province. The population was restricted to firms in the private sector as firms in the public sector are not eligible for the SSC exemption. This dataset allows us to compute the total number of employers and fte-employment by firm size-sector (15)-province (11) cells in each quarter from 2009Q1 to 2019Q4.

Disaggregation by firm size is crucial for the analyses. As we explain in section 6, we will estimate differences-in-differences regression contrasting growth in the number of employers and employees in province-sector cells between firms with one employee and firms with 7 to 10 employees, before and after the reform. Following the convention of the NSSO, firm size is defined as the total number

[12] The sector is reported at the NACE 2-digit or 3-digit level. We excluded five very specific sectors with few nonemployers 'Electricity, gas, steam and air conditioning supply', 'Public administration and defence; compulsory social security'; 'Activities of households as employers of domestic personnel'; 'Undifferentiated goods- and services-producing activities of private households for own use'; and 'Activities of extraterritorial organisations and bodies'.

[13] The unique firm identifier does not change when firms switch to a different legal form (e.g. from a private limited liability company to a public limited company).

of employees subject to SSC employed by the firm on the last day of each quarter. Firm size is determined exactly for firms with 1 to 10 employees.

4. Regression Discontinuity in Time

The immediate adjustment of labour demand to the (announcement) of the policy, as evidenced by Figure 2, leads us to adopt a Regression Discontinuity in Time (RDiT) to determine the relation between labour costs and the hiring decision (Anderson, 2014; Cui et al., 2021; Hausman & Rapson, 2018). We start by applying the canonical continuity-based sharp RD design, which assumes continuity of the potential outcomes in the neighbourhood of the cutoff. This approach allows us to quantify the 'jump' in several outcomes such as the number of firms hiring a first employee and the probability to hire among firms without employees. We then turn to local randomization methods, which is sometimes considered a more natural approach in settings with a discrete running variable (Cattaneo et al., 2019), but which only allows us to examine the effect of the SSC exemption on the probability to hire among firms without employees. We will show that the results of the continuity-based and the local randomization method are very similar.

In its most basic set-up, a sharp continuity-based RDiT consists in estimating the following regression (e.g. Anderson (2014)):

$$y_t = \alpha + \beta I_t + f(day_t) + \varepsilon_t$$

where y_t denotes the outcome at day t (e.g. the number of firms hiring a first employee on day t or the probability to hire on day t among all firms without employees on day $t-1$); I_t is an indicator equal to 1 after the reform is implemented, and zero otherwise; $date_t$ is measured as the number of days since the start of the reform on January 1st, 2016.

The identifying assumption of the RDiT is that, in the absence of the 2016 reform, the relation between the potential outcomes and calendar time – which is the running variable in a RDiT – would have been continuous in the neighbourhood of the cutoff and is entirely captured by the flexible function $f(day_t)$. Under these assumptions, the parameter β identifies the impact of the reform on the outcome. It is nowadays standard practice in the RDD literature to approximate the function $f(day_t)$ by a linear function in a small window around the cutoff. We follow the procedure developed by Calonico et al. (2014) to determine the optimal bandwidth on either side of the cutoff.[14] To validate the empirical approach, we follow standard practice by presenting placebo tests which pretend that the reform took place in a different year and by examining whether the firm characteristics of the population of nonemployers evolved continuously around the cutoff.

While a RDiT resembles a Regression Discontinuity Design (RDD) with calendar time as the running variable, a RDiT does not share all the attractive features of a RDD (Hausman & Rapson, 2018). A RDD relies on the assumption that the outcome would have evolved continuously in the neighbourhood of the cutoff absent the policy reform. As a result, including covariates in a RDD may increase the precision of the estimate, but is not required to obtain unbiased estimates (Calonico et al., 2019). This property does not hold in a RDiT: one needs to control for seasonal patterns to obtain unbiased estimates. For instance, employers are much more likely to hire on the first than on the last day of the month. This "first day of the month" effect creates discontinuities in the outcome, thereby violating the identifying assumption of the RDD.

[14] We use the Stata command "rdrobust" which automatically selects the optimal bandwidth using a data-driven approach.

We deal with this issue in two ways. Following the recommendations of Hausman and Rapson (2018), we follow a two-step procedure. We first correct flexibly for seasonal effects by regressing the outcome on dummies indicating each day of the year (365 dummies)[15] and dummies indicating the day of the week (i.e. Monday to Sunday)[16] using the entire time series, except for observations within the "donut", i.e. the time period characterized by anticipation and catch-up effects, discussed in detail below. The residuals of this regression are then used as the outcome in the RDiT. We use a bootstrapping procedure to obtain correct standard errors. Alternatively, we aggregate the data by month which eliminates "day" effects. This aggregation is our preferred approach because, as we will show, it substantially reduces the noise in the data. Nevertheless, even the monthly data exhibit seasonal patterns since employers are more likely to hire in the first month of each quarter. We again use the aforementioned two-step procedure to remove seasonal effects in the monthly data.

One of the attractive features of a RDD is that, as the sample size increases, the observations used in the regressions get closer and closer to the cutoff. A RDiT does not share this feature. The running variable has discrete mass points: each time period (e.g. day or month) constitutes a distinct mass point. As Cattaneo et al. (2019) show, when the running variable has mass points, estimating a RDD using all observations is essentially similar to estimating a RDD on data aggregated by mass points. Hence, even with a very large sample size, observations relatively far from the cutoff are needed to extrapolate on either side of the cutoff.[17] This is an inherent limitation of a RDiT. It is the main reason why we collapsed the data by time period and use the average outcome at a given point in time in the continuity-based RDiT.[18] The fact that we have access to population data ensures that the average outcome is precisely measured, implying that sampling error is not a concern. The only uncertainty that remains is related to the functional form of the function $f(day_t)$ and the interpolation towards the cutoff.

A discrete running variable is not uncommon in RDD but poses additional challenges (Kolesár & Rothe, 2018; Lee & Card, 2008).[19] Cattaneo et al. (2019) advocate using the local randomization framework rather than the continuity-based framework when the running variable is discrete. In contrast to the continuity-based approach, the local randomization framework does not rely on assuming continuity of the outcome around the cutoff. Instead, this approach assumes that, within a small window around the cutoff, the running variable is independent of the potential outcome. Within this window, the effect of the intervention is simply the difference in the mean outcome before and after the reform. Following Cattaneo et al. (2019), we will first apply this method using the smallest possible window given the monthly data and we will then gradually expand the window until nonemployer firms within the window no longer have the same characteristics. In this analysis, the outcome variable is the probability to hire in month m among all firms without employees on the last day of the previous

[15] In leap years, February 29th is coded as February 28th.

[16] It is not uncommon that employment starts in the weekend or on a public holiday because the NSSO registers the start of the contract, not of the first day worked.

[17] For this reason, (Hausman and Rapson, 2018) argue that, with only one observation per period, a RDiT is essentially the same as a simple before/after comparison or an interrupted time series analysis.

[18] Collapsing the data by time period also reduces the computational burden considerably.

[19] Lee, D. S., & Card, D. (2008). Regression discontinuity inference with specification error. *Journal of Econometrics*, *142*(2), 655-674. suggest clustering the standard errors by mass point; Kolesár, M., & Rothe, C. (2018). Inference in regression discontinuity designs with a discrete running variable. *American Economic Review*, *108*(8), 2277-2304. warn against this approach. We report conventional standard errors. The 95% CI intervals do not change much when clustering the standard errors by mass point.

month.[20] This is the only outcome that can be considered as we need more than one observation at each point in time. We again adjust the outcome for seasonality using the two-step procedure discussed earlier.

The policy was announced in early October 2015 and came into force on January 1st, 2016. Potential employers could anticipate the SSC exemption by postponing hiring in the period October-December 2015 until January 2016, leading to a dip in hiring just before the reform and a 'catch-up' effect just after the reform. This behaviour is clearly visible in Figure 2. To address this issue, we estimate a "donut" RDiT, excluding observation just before and after the reform. We follow Benzarti and Harju (2021) to determine the size of the donut. We first estimate the number of "missing hires" in the period October-December 2015, i.e. the number of firms that postponed hiring. We then determine the point in time moment in the post-reform period, T_e, so that the number of "excess hires" in the period January, 1st 2016 until T_e equals the number of "missing hires". The continuity-based approach requires extrapolation within the donut from both sides of the cutoff. This extrapolation is not required in the local randomization approach.

5. The impact on the hiring decision

In this section, we first examine the effect of the SSC exemption on (1) the number of firms hiring a first employee, (2) the number of new firms established each month and (3) the probability to hire a first employee using the continuity-based RDiT. We show that, at least shortly after the reform, the policy increased the number of firms hiring a first employee by increasing the probability to hire among (existing) firms without employees, without any effect on the number of new firms established each month.

We then re-examine the impact of the reform on the probability to hire among firms without employees using the local randomization framework. The results of the continuity-based and local randomization framework are similar which supports the validity of our empirical approach.

Next, we test whether strategic firm behaviour, in which existing employers established new firms to benefit from the subsidy, explains the findings. The key evidence we present to rule out strategic firm behaviour is that the positive effects remain nearly identical when focussing on the subset of firms without employees established before the policy was announced.

Finally, we present results for different subpopulations thereby providing evidence that the findings hold across different populations.

RDiT: continuity-based approach

Figure 3 shows the log of the number of firms hiring a first employee from June 2009 until December 2019, after adjusting for seasonal patterns. The running variable is calendar time in days and months in Panel A and B, respectively. Both figures clearly show that the 2016 reform increased the number of firms hiring a first employee. The outcome is nosier when using daily (Panel A) rather than monthly data (Panel B), suggesting that we do not fully capture the seasonal patterns in the daily data. This is unsurprising as roughly one out of three firms hiring a first employee in a given month do so on the first day of the month. This is the reason why we use monthly rather than daily data in the remaining analyses.

[20] Note that the choice of this outcome implies that we do not consider the impact of the reform on firms that are established and hire a first employee within the same month. These firms are included when defining the outcome as the number of firms hiring a first employee, as we do in the continuity-based approach.

The figures confirm the presence of anticipation effects and catch-up effects. The number of firms hiring their first employees sharply decreases after the policy was announced, indicating that firms postponed hiring to benefit from the subsidy. This effect is particularly pronounced in December 2015: the number of firms hiring in this month is less than half the number in September 2015. We also observe that the number of firms hiring a first employee is (slightly) higher in January than in February 2016, suggesting that firms postponed hiring from October-December 2015 to January 2016.

As discussed earlier, we estimate the number of "missing" hires in October-December 2015 to determine the time period after the reform which has to be excluded when estimating the donut RDiT to avoid catch-up effects biasing the estimates. The size of the donut differs when using daily or monthly data. When using daily data, excluding January 2016 is already sufficient to compensate for the number of missing hires just before the reform; when using monthly data, both January and February 2016 have to be excluded.[21] The dashed vertical lines in Figure 3 indicate the donut. Observations within this donut are excluded when estimating the RDiT, but we extrapolate within the donut to determine the causal effect of the policy at the cutoff.

Figure 3: Impact of the reform on the number of firms hiring a first employee

Note: The figures show the log of the number of firms hiring a first employee by day (Panel A) and by month (Panel B), after adjusting for seasonal patterns. When using daily data, we correct for seasonality by regressing the log of the number of firms hiring a first employee on dummies indicating the day of the year (365 dummies) and the day of the week (Monday-Sunday). When using monthly data, we regress the outcome on the month of the year (January-December). In both cases, observations within the donut are excluded. The figures show the residuals of these regressions. These residuals are binned by month in Panel A. The solid red line indicates the implementation of the policy (January 1st, 2016). The dashed black lines indicate the donut, which starts when the policy was announced (October 10, 2015) and ends when the number of missing hires in the period October-December 2015 equals the number of excess hires after the reform. Observations within the donut are excluded when estimating the RDiT.

The first two regressions presented in Table 2 quantify the graphical evidence presented in Figure 3. According to the first specification which uses daily data, the 2016 reform increased the number of firms hiring a first employee by 21 log points [bootstrapped 95% CI: -0.007 – 0.41], while the second specification which defines the outcome as the number of firms hiring a first employee in a given month estimates the positive effect at 29 log points [95% CI: 0.17 – 0.40]. In other words, the 2016 reform increased the number of firms hiring a first employee by 23% to 33%. Note that, in line with the graphical evidence, the estimate is much less precise when using daily rather than monthly data.

[21] Using monthly data, the number of missing hires in the period October-December 2016 is estimated at 776, while the number of excess hires in January and February 2016 is estimated at 743. Using daily data, the number of missing hires between October 10, 2015 and December 31, 2015 is estimated at 638 and the number of excess hires in January 2016 is estimated at 638.

The optimal bandwidth is relatively large in both specifications: the first regression uses daily observations from January 11, 2015 (272 observations left of the donut) to December 21, 2016 (325 observations right of the donut), while the second regression uses monthly observations from January 2015 (9 observations left of the donut) to December 2016 (10 observation right of the donut). As a result, observations far from the cutoff are used for extrapolation near the cutoff. This is an intrinsic limitation of the continuity-based RDiT because the running variable, calendar time, is discrete.

Table 2: RDiT for several outcomes

	(1)	(2)	(3)	(4)	(5)
Outcome	Number of firms hiring a first employee (log)		Number of firms established each month (log)	Probability to hire among nonemployers	
Daily/monthly data	Daily	Monthly	Monthly	Monthly (log)	Monthly (level)
RDiT treatment effect	0.214	0.287	0.0130	0.299	0.000858
Robust 95% CI	[0.036 – 0.38]	[0.20 – 0.37]	[-0.10 – 0.099]	[0.22 – 0.38]	[0.00061 – 0.0012]
Bootstrapped robust 95% CI	[-0.007 – 0.41]	[0.17 – 0.40]	[-0.10 – 0.12]		
Bandwith	355.8	11.74	15.00	12.01	10.23
Number of observations used:					
Left of the cutoff	272	9	12	9	7
Right of the cutoff	325	10	13	10	8

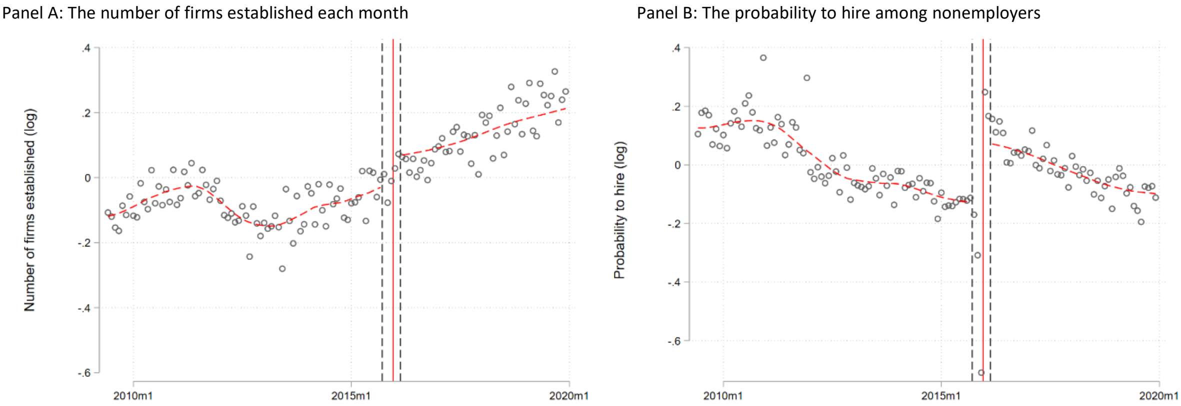
Note: The table shows the results of estimating a RDiT for different outcomes using daily (regression 1) or monthly data (regression 2-5). The discrete nature of the running variable implies that we have one observation per day/month when applying the continuity-based method. Observations within the donut are excluded from the regressions. This donut corresponds to the period October 10, 2015, until January 31, 201,6 when using daily data and to the period October 2015 – February 2016 when using monthly data. The outcomes are the residuals of a regression which corrects the seasonal patterns in the raw outcome, as explained in the text. The point estimates are constructed using a linear polynomial (p=1) with a triangular kernel. The bandwidth is estimated using the data-driven method developed by Calonico et al. (2014). We also report the number of observations effectively used on either side of the cutoff. Two types of 95% confidence intervals are reported: 95% CI based on bias-corrected robust standard errors as derived by Calonico et al. (2014), which do not account for the two-step procedure used to correct for seasonality, and 95% CI based on bootstrapped robust standard errors. The bootstrapped 95% CI intervals are obtained by drawing a random sample with replacement of the original dataset and estimating a RDiT after correcting for seasonality using this new dataset. For each replication, the upper and lower bound of the bias-corrected robust 95% CI intervals are recorded. The bootstrapped 95% CI is the average value of the lower and upper bound over 5,000 replications. As expected, the bootstrapped robust 95% CI are slightly larger than the robust 95% CI, which neglect the first step in the estimation procedure.

We present several placebo tests in Appendix B. These placebo tests consist in pretending that the reform took place on January 1st 2013, 2014, 2015, 2017 or 2018 rather than on January 1st 2016 and in estimating a RDiT using these points in time as the cutoff. We again exclude observations in the donut on both sides of the cutoff so that the placebo RDiT also extrapolate the local polynomial within the donut. None of the placebo tests is significant at conventional levels. This strengthens our claim that our baseline RDiT indeed captures the effect of the 2016 reform.

We also examine whether covariates (legal form, region, sector and age) are continuous around the cutoff. To do so, we build a dataset which contains all firms without employees in a given month and evaluate whether the characteristics of this population change near the cutoff. We present graphical evidence and regressions for each covariate in Appendix C. We do not observe any large jumps near the cutoff. All 11 point estimates are small and only two coefficients are (marginally) significant: the share of private limited liability companies increases by 0.07 percentage points immediately after the reform from a base level of 27.5% (significant at the 10% level); and the share of firms based in Flanders increases by 0.04 percentage points from a base level of 59.8% (significant at the 1% level). Overall, the findings suggest that firm characteristics evolved continuously around the cutoff. This observation is perhaps less obvious than in a standard RDD because the population also includes firms that were established after the policy was announced and may, therefore, be established as a response to this policy. This may affect the composition of the population after the reform. For instance, firms may choose a different legal form as a response to the policy. The observation that the covariates did not 'jump' at the cutoff signals that, at least in the short run, there is either no effect of the policy on firm characteristics or the effect is too small to be picked up by the RDiT.

Two distinct channels can contribute to the positive effect of the 2016 reform on the number of firms hiring a first employee: (1) the number of new firms established each period may have increased after the reform, which would mechanically increase the number of firms hiring a first employee if the probability to hire remains unaltered; and/or (2) the probability to hire among nonemployers may have increased.[22]

Figure 4: Decomposing the effect on the number of firms hiring a first employee in the effect on:



Note: Panel A shows the number of new firms established each month. Panel B shows the probability to hire in month m among all firms without employees on the last day of month m-1. Both outcomes are corrected for seasonality by regressing the outcome on the month (January-December) using the entire time series, excluding observations within the donut. The figures show the residuals of this regression. The solid red line indicates the implementation of the policy; the dashed black lines indicate the donut.

Using the continuity-based RDiT, we examine the relative importance of each channel. Figure 4 shows the evolution of the number of firms established each month (Panel A) and the probability to hire in month m among all firms without employees in month $m-1$ (Panel B). Although the number of firms established each month has gradually increased since 2016, there is no jump near the cutoff (Panel A). The policy may have an impact on the number of new firms established each month in the medium term, but did not lead to a surge in new firms immediately after the reform was announced.[23] This is in line with the previous observation that the characteristics of the population of firms did not change after the reform.

By contrast, the probability to hire among nonemployers clearly jumped immediately after the reform (Figure 4, Panel B). The regressions confirm the graphical evidence (Table 2, regression 3 and 4): only the probability to hire among nonemployers increased significantly immediately after the reform (+29.9 log points).

The only outcome we can consider in the local randomization framework, presented in the next section, is the probability to hire among nonemployers. As we showed, this outcome is the main margin of adjustment in the short run, which justifies our focus on this outcome. Since firms either hire an

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[22] More formally: log(number of firms hiring a first employee)=log(number of nonemployers x probability to hire among nonemployers)=log(number of nonemployers)+log(probability to hire among nonemployers). Taking the derivative with respect to time shows that the increase in the number of firms hiring a first employee equals the sum of the increase in the number of nonemployers and the increase in the probability to hire among all firms without employees.

[23] To make the results presented in Table 2 consistent across specifications, we also excluded the observations within the donut when considering the outcome 'number of firms established each month'. Potential entrepreneurs might, however, respond to the announcement of the policy by establishing new firms immediately after the announcement of the policy or immediately after its implementation. To examine this issue, we also estimated a RDiT (without a donut) using as the cutoff the date at which the policy was (1) announced and (2) implemented. We do not observe a response immediately after the announcement of the policy. A RDiT using January 1st, 2016 as the cutoff (and including all observations within the donut) indicates that the number of firms established right after the reform increased by 5%, significant at the 10% level.

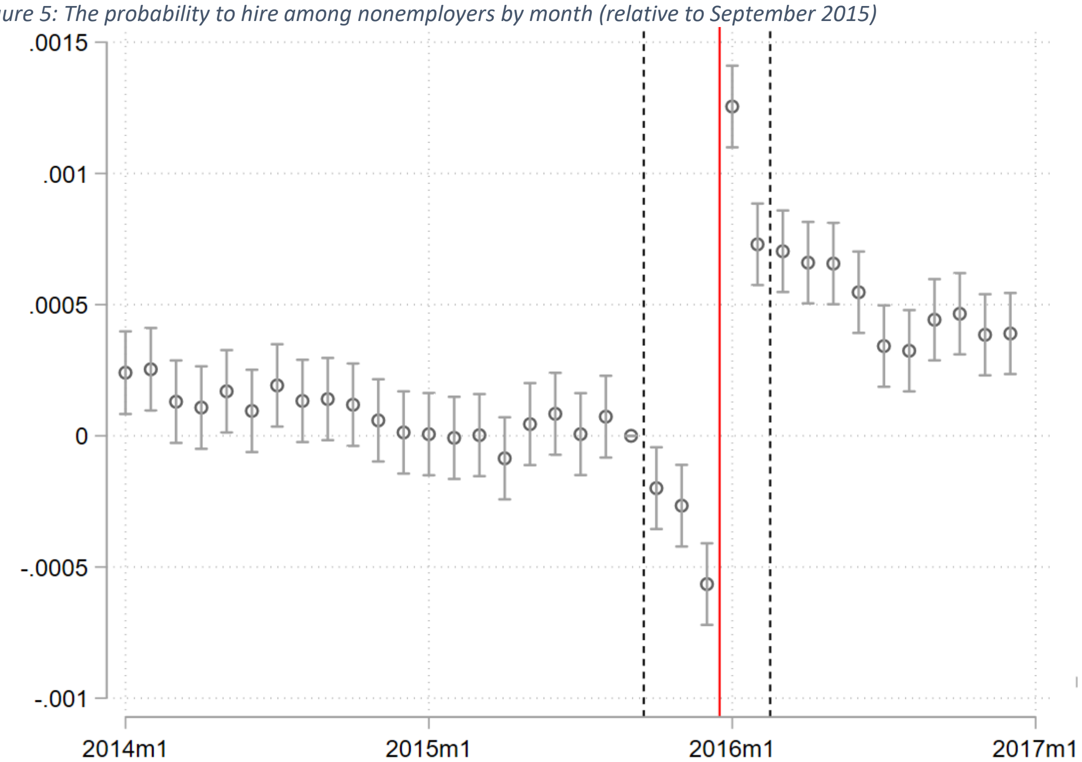
employee or do not hire, we cannot take the log of the outcome in the local randomization framework, as we do when applying the continuity-based method. To facilitate comparing the findings between both methods, we therefore also apply the continuity-based method using as the outcome the probability to hire among firms without employees measured in levels. According to this specification, the probability to hire increased by 0.086 percentages points (Table 2, Regression 5). The probability to hire among nonemployers in the window after the reform used in the RDiT is 0.28%, implying that this specification indicates that the reform increased the probability to hire by 44%.[24]

RDiT: local randomization method

To test the robustness of our findings, we also examine the effect of the SSC exemption on the probability to hire in month m among all firms without employees on the last day of month $m-1$ using the local randomization approach. As discussed in section 4, this approach does not rely on the assumption of continuity of the outcome in the neighbourhood of the cutoff, an assumption which is less likely to hold in settings with a discrete running variable such as calendar time. As in the continuity-based RDiT, the period October 2015 until February 2016 has to be excluded to rule out bias due to anticipation and catch-up effects. We again correct for seasonality following the two-step procedure discussed in section 4.

Figure 5 presents the results of an event study showing the probability to hire a first worker among nonemployers by month relative to September 2015. This figure confirms the sharp drop in the probability to hire just after the policy was announced but not yet implemented, and the sharp increase just after the policy came into force. Importantly, the outcome remained remarkably stable in the fifteen months preceding the announcement of the reform, thereby providing evidence that the increase observed from January 2016 onwards can be attributed to the reform.

Figure 5: The probability to hire among nonemployers by month (relative to September 2015)



Note: The figure shows the probability to hire in month m among firms without employees on the last day of month $m-1$ relative to September 2015, adjusted for seasonality. The dashed vertical lines indicate the donut. The full line separates the pre and post-reform period.

[24] = 0.00086/(0.002802- 0.00086)

We determine the impact of the 2016 reform by contrasting the outcome using the smallest possible window around the cutoff. More specifically, we compare the probability to hire in September 2015 – just before the policy was announced – and March 2016 – the first month right after the donut. A basic OLS regression indicates that the reform increased the probability to hire by 0.070 percentage points (Table 3, regressions 1), which is slightly smaller than the estimate from the continuity-based RDiT (0.086). The point estimate and its precision do not change when including covariates in the OLS regression (Table 3, regressions 2).

Table 3: OLS regression contrasting the probability to hire between the pre-and post-reform period

	(1)	(2)	(3)
Period	**Smallest window** (2015m9 vs 2016m3)		**Largest window** (2014m11-2015m9 vs 2016m3-2016m6)
Treatment effect	0.00070	0.00070	0.00062
95% CI	[0.00054 - 0.00086]	[0.00054 - 0.00086]	[0.00056 - 0.00069]
Covariates		X	
Number of observations	1,600,317	1,600,317	11,870,161
Probability to hire in the post-reform period	.002746	.00274	.00273

Note: Results of an OLS regression regressing the probability to hire, corrected for seasonality, in month m among firms without employees in month $m-1$ on a simple dummy indicating the post-reform period. Conventional standard errors are used to construct 95% CI. The first two regressions compare the outcome between the period September 2015 and March 2016. The second regression includes the following covariates: age (5 groups), legal form (3 groups) and sector (16 groups). The last regression uses a larger window and contrasts the outcome between the period November 2014 to September 2015 versus the period March to June 2016.

We then gradually expand the window, i.e. the number of months before and after the reform, used in the regression. We extend the window to increase the precision of the estimate and to show that it remains robust to using a larger window. The window is typically determined as the largest window in which the differences in (predetermined) covariates between the pre-and post-reform period are not statistically significant (Cattaneo et al., 2019). However, in contrast to most applications of the local randomization framework, the number of observations is already very large when simply comparing the outcome between two points in time (n=1,600,317 in regression 1 and 2 in Table 3). This leads us to follow a slightly different approach.

First, we compare firm characteristics of the population of nonemployers (legal form, age, sector and region)[25] in a given month relative to the characteristics of the population of nonemployers in September 2015. Put differently, using simple T-tests, we test whether the composition of the population remained stable before and after September 2015. Second, to summarize the p-values across the 11 T-tests, we follow standard practice and determine the minimum p-value across all comparisons, but we also compute the average p-value across all comparisons. Given the large sample size and the relatively large number of firm characteristics we consider, the average p-value provides a better summary statistic of the evolution of the p-values than the minimum p-value.

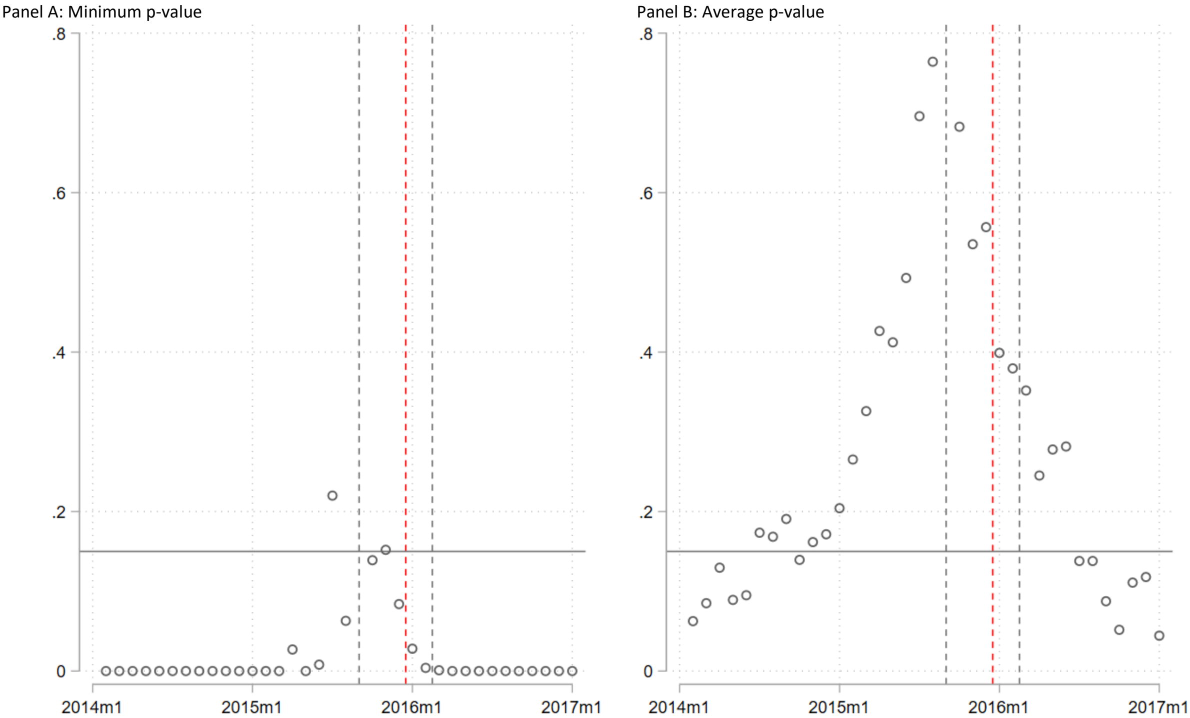
Figure 6 shows the minimum p-values (Panel A) and average p-value (Panel B) of the 11 T-tests. Even very small differences are highly statistically significant. For instance, the average age of nonemployers in May 2015 is 13.36 compared to 13.45 in September 2015. This small difference is already highly significant (p-value<0.1%). This is the reason why it is more informative to examine the average than the minimum p-value, as we do in Panel B of Figure 6. The average p-value gradually decreases as we

[25] The legal form is grouped into three categories (sole proprietors, private limited liability companies and other legal forms); the sector is grouped into four groups (sector F, G, M and other sectors).

move further away from September 2015, but suggests that the composition of the population remains stable for a relatively long period in the pre-reform and post-reform periods.

We determine the size of the window by including those months in the pre-reform and post-reform period for which the average p-value is above 0.15. Using this threshold, the pre-reform period includes the months November 2014 to September 2015 and the post-reform period includes the months March to June 2016. Contrasting the probability to hire among firms without employees in the pre-reform to the post-reform period shows that the SSC exemption increased the probability to hire by 0.062 percentage points [95% CI: 0.056-0.06853] (Table 3, regressions 3). Note that the 95% CI is much smaller than the 95% CI using the smallest possible window or than the 95% CI based on the continuity-based RDiT. In relative terms, the policy increased the probability to hire among nonemployers by 29.6%[26], which is indistinguishable from the previously reported estimates.

Figure 6: Determining the size of the window: the minimum and average p-value of 11 T-tests



Note: The figures show the minimum and average p-value of the 11 T-tests (legal form, age, sector and region) we conducted, by month. The dashed line indicates the donut. The red line distinguishes the pre-reform and post-reform periods.

Real effects or strategic firm behaviour?

A key concern with the interpretation of the results is that the estimates may capture strategic firm behaviour in which existing employers stop their activities and start a new firm immediately after the reform is in place to benefit from the subsidy. This type of behaviour is not allowed by law and is monitored by the NSSO, but we cannot rule out that it nevertheless happens.

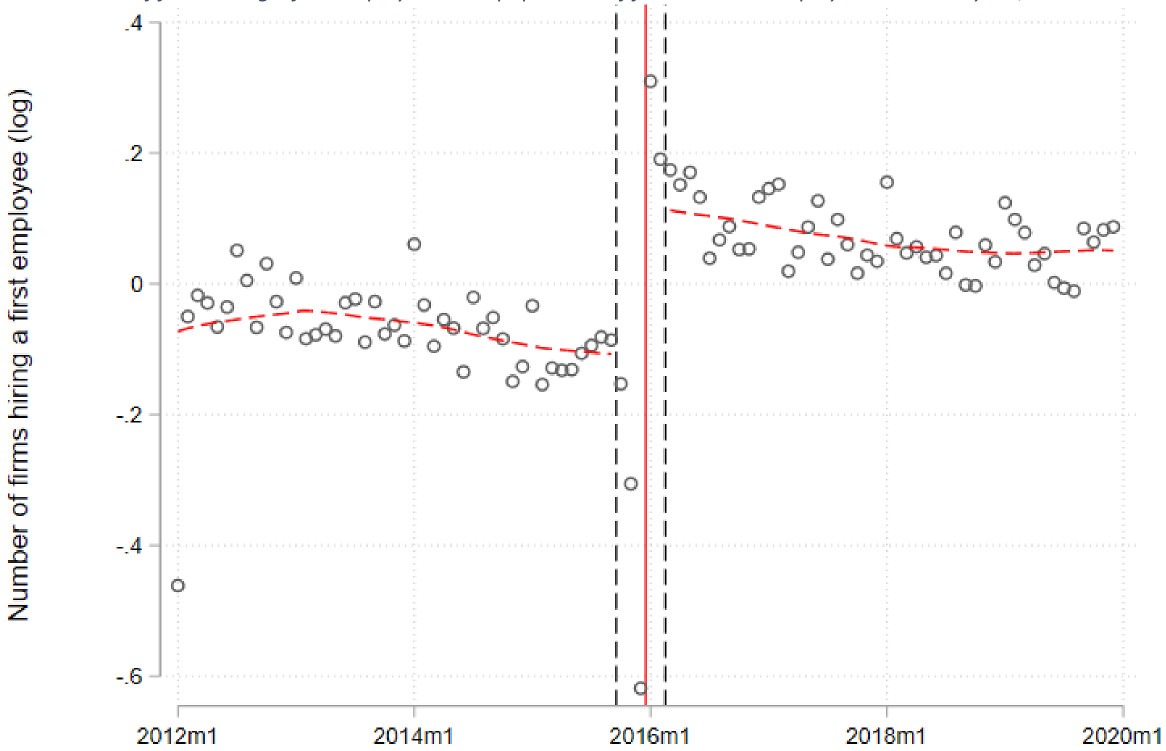
We conduct several complementary analyses which show that strategic firm behaviour cannot explain the findings. Our most important piece of evidence consists in evaluating how firms established before the policy was announced responded to the policy. To do so, we restrict the population to firms

[26] 0.0006238 /(0.0027306-0.0006238) where .0027306 is the average probability to hire in the period May-June 2016.

without employees on January 1st, 2012[27] and examine the number of firms in this population which hired a first employee in a given month over the period 2014-2019. In contrast to our baseline analyses, we fix the population of firms at a given point in time. Hence, these estimates cannot be affected by firms established after January 1st, 2012, which rules out bias due to strategic firm behaviour.

The graphical evidence (Figure 7) and the regression-based analysis (Table 4, reg 1) show that firms in this population responded to the SSC exemption: the number of firms hiring a first employee increased by 29.1 log points immediately after the reform. This estimate is almost identical to the baseline estimates and rules out that strategic firm behaviour explains the results. Interestingly, it also indicates that the SSC exemption not only increased hiring among recently established firms but also among older firms.

Figure 7: Number of firms hiring a first employee in the population of firms without employees on January 1st, 2012



Note: The population consists of all firms without employees on January 1, 2012. Given this population, the figure shows (the log of) the number of firms hiring a first employee per month, adjusted for seasonality. The vertical, dashed lines indicate the donut. The vertical, full line indicates the implementation of the policy.

Three additional analyses provide further evidence that strategic firm behaviour is unlikely to be a major concern. First, we find no evidence that the number of firm closures increased after the reform (Figure D.1 in Appendix D; Table 4, reg 2). This provides some evidence that entrepreneurs did not shut down existing firms in order to open new ones.[28] Second, the number of employers who stop

[27] We experimented with the choice of the point in time used to determine the population. Ideally, we would fix the population just before the policy was announced so that the population includes all firms without employees established before the policy was announced. This is obviously not possible because we need a sufficient number of observations left of the cutoff to estimate the RDiT. While the graphical evidence remains very similar when choosing January 1st, 2013 or 2014, the RDiT only converges if the population is defined at January 1st, 2012.

[28] While the CBE accurately records the date at which firms starts and stop employing workers subject to SSC, the point in time at which the firm closes down is less accurately recorded because (1) it might take several months before a firm is declared bankrupt and (2) firms might stop their activities without formally shutting down.

employing employees appears to remain unaltered after the reform, which provides evidence that employers did not fire employees in order to start a new firm and benefit from the SSC (Figure D.2 in Appendix D; Table 4, reg 3). We do observe that the number of firms which stop employing workers is lower than expected in December 2015 (just before the reform) and higher in January 2016 (just after the reform). This is a counterintuitive finding because one would expect the inverse pattern if firms stop employing workers just before the reform. Fortunately, this behaviour cannot bias the RDiT estimates because we only observe this pattern close to the cutoff, and these observations are within the donut. Third, we estimate the 'net effect' of the reform defined as the (log of the) the number of firms hiring a first worker divided by the number of employers which stop employing workers in a given month. This ratio increased by 34.4 log points after the reform (Figure D.3 in Appendix D; Table 4, reg 3), which is larger than the previous estimates, but only significant at the 10% level. This estimate is most likely less precise because the adjustment for seasonality is less accurate as hiring a first employee and no longer employing workers shows different seasonal patterns.

Table 4: RDiT: testing for strategic firm behaviour

	(1)	(2)	(3)	(4)
Outcome	Number of firms hiring a first employee (Population of firms without employees on January 1, 2012)	Number of firms exiting the market	Number of employers which stop employing workers	Net effect
RDiT treatment effect	0.291	0.0421	0.00427	0.354
Robust 95% CI	[0.197 – 0.407]	[-0.290 – 0.289]	[-0.349 – 0.327]	[-0.0272 – 0.756]
Bandwith	7.80	11.91	17.22	13.56
Number of observations used:				
Left of the cutoff	5	8	14	11
Right of the cutoff	6	9	15	12

Note: Using the continuity-based RDiT, the table presents evidence ruling out strategic firm behaviour in which existing employers stop their activities and start a new firm to benefit from the benefit. The first regression restricts the population to nonemployers active on January 1st, 2012. This regression quantifies the graphical evidence presented in Figure 7. Regressions 2, 3 and 4 consider different outcomes, namely the number of firms exiting the market, the number of employers which stop employing workers and the "net effect". The net effect is defined as the log of the number of firms hiring a first worker divided by the number of employers which stop employing workers. Graphical evidence for specifications 2-4 is presented in Appendix D. All outcomes are adjusted for seasonality.

Sample selection and heterogeneity

We also examine the effect of the reform on the hiring decision of firms without employees along several dimensions, namely the region (Brussels, Flanders, Wallonia), the legal form of the firm (sole proprietors vs private limited liability companies), the prevalence of hiring before the reform (contrasting sectors with high vs low probability to hire among nonemployers before the reform), and the sector (16 sectors[29]).

We conduct these analyses primarily as a robustness check to show that the positive effect holds along many different dimensions. In addition, we may also discover heterogeneity, which may help to shed light on the underlying mechanisms explaining the results. The take-away message of the estimates presented in Table 5 is that the SSC exemption positively affected the probability to hire a first employee in most of the subpopulations considered.

While the point estimates differ along the different dimensions, the average effect for the entire population (30 log points) is included in the robust 95% CI intervals of each estimate. The point estimates suggest that the SSC exemption is more effective for private limited liability companies than sole proprietors; for sectors characterized by a higher likelihood to hire among nonemployers before the reform; and indicates some heterogeneity across sectors, with larger effects in, for instance, the

[29] We only present results for sectors with, on average, at least 50 firms hiring a first employee each month in the pre-reform period.

sector of real estate activities (+64 log points) than in the sector of accommodation and food service activities (+X log points).

Table 5: The effect of the SSC exemption on the probability to hire among nonemployers along different dimensions (continuity-based approach on monthly data)

	Point estimate	95% robust CI
Total population	0.30***	[0.22 – 0.38]
Region		
Flanders	0.27***	[0.18-0.38]
Brussels	0.38***	[0.21-0.54]
Wallonia	0.31***	[0.19-0.43]
Legal form		
Sole proprietors	0.23*	[0.04-0.39]
Private limited liability company	0.36***	[0.23-0.52]
Prob to hire among nonemployers before the reform		
Low	0.26***	[0.17-0.35]
High	0.35***	[0.24-0.44]
Sector		
1	-0.13	[-1.44-1.44]
2	0.01	[-1.19-1.00]
3	0.15	[-0.28-0.48]
4	0.16	[-0.09-0.38]
5	0.18***	[0.10-0.25]
6	0.18	[-0.39-0.65]
7	0.23**	[0.01-0.43]
8	0.25**	[0.01-0.53]
9	0.26**	[0.01-0.5]
10	0.38***	[0.14-0.52]
11	0.46***	[0.24-0.66]
12	0.55***	[0.24-1.07]
13	0.56***	[0.15-1.06]
14	0.58	[-0.02-1.37]
15	0.64***	[0.26-1.17]
16	1.23	[-0.38-3.24]

Note: The table shows the results of estimating the continuity-based RDiT along different dimensions. The outcome is the log of the probability to hire in month m among firms without employees on the last day of month $m-1$. Robust 95% CI are reported.

6. Impact on employment

The reform increased the number of firms hiring a first employee by roughly 30% in the short run. Assuming nothing else changes, a persistent increase in the number of new employers mechanically translates into a commensurate increase in the total number of employers and employees in the long run (Sedlacek & Sterk, 2020). Put differently, in the long run, the total number of employers and the total number of employees should also increase by 30%.

Reality is more complex and such a large impact of the SSC exemption is not credible. The reason is that the 2016 reform is unlikely to only affect the hiring decision. Firms that would not have hired in the absence of the policy may have a lower productivity level than firms that would have hired anyway. Low-productivity firms are more likely to fail and/or grow slower than higher-productivity firms. The entry of low-productivity firms into the market will therefore reduce the positive impact of the reform on employment. Moreover, general equilibrium effects may cause job losses in (small) firms not eligible for the subsidy due to increased competition in the labour and product market. Similarly, in the longer run, the SSC exemption could increase labour market tightness, thereby increasing market wages and reducing employment in all firms.

In this section, we use aggregate quarterly data from the NSSO to go beyond the impact of the SSC exemption on the decision to hire a first employee. Using a RDiT[30], we show in Appendix E that the quarterly NSSO data also indicates a jump of 25% to 30% in the number of first-time employers immediately after the introduction of the SSC exemption. This is already a relevant finding in itself in that it shows that both datasets point towards similar trends.

Having established the positive impact of the SSC on the entry of new employers, we use the NSSO dataset to identify the impact of the policy on the total number of employers and employees. To this end, we estimate difference-in-differences regressions contrasting growth in the number of employers and employees between small versus slightly larger firms within province-sector cells before and after the reform.

The objective of the DiD regressions is threefold. First, it lends additional credence to the previous findings and can, as such, be considered a robustness check. Most importantly, the analysis shows that new employers did not simply replace existing employers. This confirms that strategic firm behaviour does not explain the findings, but also suggests that negative displacement effects – i.e. existing ineligible employers might be driven out of the marker by new, subsidized employers – do not offset the positive effects. Second, the RDiT only allowed assessing the impact of the policy in the short run, i.e. right after its implementation. By contrast, the DiD regressions provide some insights into the effect of the policy over a longer period. Third, although the identification strategy does not capture all general equilibrium effects, it provides the first attempt to assess whether the policy created additional employment, which is arguably the most policy-relevant outcome.

Figure 8 provides a preview of the main findings. It shows the evolution of the number of employers in the pre and post-reform period by firm size. Panel A shows the evolution in absolute numbers since 2015Q3; Panel B shows the evolution relative to 2015Q3. These figures reveal four key points.

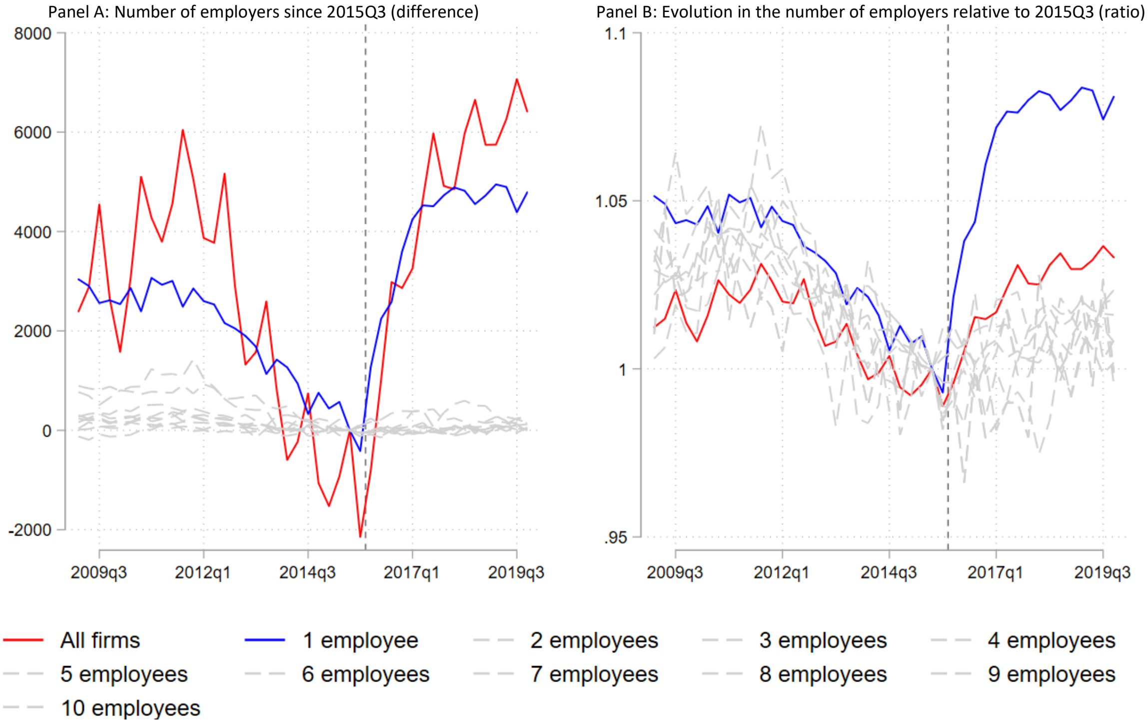
First, the number of firms with one employee (blue curve) sharply increased after the reform, while the number of firms with 2 to 10 employees remained stable (grey curves). By 2017Q1, there were 5,235 more firms with one employee than in 2015Q3 (Panel A). This corresponds to an increase of 8.9% (Panel B). The increase in the number of firms with one employee, as measured by the NSSO, has a similar order of magnitude as the estimates from the RDiT presented earlier based on the CBE data. The CBE dataset indicated that the number of firms hiring a first employee increased by 30% immediately after the reform. Extrapolating this estimate implies that about 6,700 additional first-time employers[31] entered the market in the year after the reform.

Second, at least until 2017, the increase in the total number of employers (red curve) can be entirely attributed to the increase in the number of firms with exactly one employee (Panel A). This further alleviates concerns that the positive effect on the number of firms hiring a first employee occurs at the expense of existing employers leaving the market without any positive net effect on the total number of employers.

[30] Because the NSSO data is quarterly data, we have too few observations to restrict the sample to the observations in the neighbourhood of the cutoff. We follow Lee and Lemieux (2010) and fit global linear or quadratic polynomials to the NSSO data using 42 quarterly observations in the period 2009Q1-2019Q4, excluding the quarters 2015Q4 and 2016Q1 to rule out bias due to anticipation and catch-up effects.

[31] According to the CBE, 22,526 firms without employees hired a first employee over the period 2014Q4-2015Q3.

Figure 8: Number of employers, by firm size



Note: Panel A shows the difference between the number of employers at a given point in time and the number of employers in 2015Q3, by firm size. Panel B shows the ratio of the number of employers at a given point in time to the number of employers in 2015Q3, by firm size. In 2015Q3 Belgium counted 184,512 employers, of which 59,134 employed exactly one employee. Following NSSO conventions, firm size is defined as the number of employees subject to SSC employed by the firm on the last day of the quarter.

Third, the baseline DiD specification contrasts the growth in the number of employers in firms with 1 employee (treatment group) to growth in firms with 7 to 10 employees (control group). Panel B of Figure 8 suggests that firms with 7 to 10 employees are a valid control group: the outcome in the treatment and control group evolves in parallel in the pre-reform period.

Fourth and finally, the number of firms with one employee quickly jumped after the reform, but stabilises from 2017 onwards and appears to have reached a new equilibrium since then. This suggests that the policy has a one-time level effect and did not permanently increase the growth rate.

To quantify the compelling graphical evidence, we estimate a DiD contrasting the evolution in the number of employers and employees (relative to 2015Q3) in firms with 1 employee (treatment group) versus firms with 7 to 10 employees (control group). The data is aggregated by province-sector -firm size cells. Hence, we examine how the outcome for firms with 1 versus firms with 7 to 10 employees within province-sector cells evolves before and after the reform.

The control group is composed of firms with 7 to 10 employees because firms with 7 to 10 employees are the smallest firms of which the majority were not directly affected by the 2016 reform. As discussed earlier, the 2016 reform introduced the SSC exemption for the first employee but also increased temporary subsidies for the 2nd to the 6th employee. Over the period 2017-19, employers with 7 to 10 employees claimed the SSC exemption for 0.6% of their employees and claimed SSC reductions for the 2nd to 6th employee for 4.9% of the employees (Table A.2. in Appendix A). This provides evidence that only a limited number of firms in the control group benefitted from the 2016 reform. We group firms with 7 to 10 employees to have a sufficient number of employers in each of the 165 province-sector cells. Firms with more than 10 employees are not included in the control group because the parallel trend does not hold in the pre-reform period for these larger firms (Appendix F).

Figure 9 shows the results of an event study for the two outcomes we consider. We only use one observation per year, namely the outcome in the 3rd quarter of each year. This choice avoids the need to correct for seasonality in the outcome. Appendix G presents the results of similar event studies using four observations per year. We plot the evolution of the growth in the number of employers (Panel A) and employment (Panel B) in the treatment and control group relative to 2015Q3. The common trend holds in the pre-reform period. Both the number of employers and employees immediately increases in the treatment group after the reform and reaches a new equilibrium after one year.

We summarize the results of the event study by estimating a DiD comparing the outcome between four periods: the pre-reform period (2009Q1-2015Q3); the anticipation period (2015Q4) during which the policy was announced but not yet implemented; an adjustment period (2016Q1-2016Q4) during which a new equilibrium was reached; and the equilibrium period (2017Q1-2019Q4). Table 6 presents the results of several DiD specifications. We only report the estimates for the equilibrium period.

Figure 9: Event study contrasting firms with 1 to firms with 7 to 10 employees

Note: The figure shows the results of an event study contrasting growth in the number of firms (Panel A) and employees in full-time equivalents (Panel B) between firms with 1 employee (treatment group) and firms with 7 to 10 employees (control group) using repeated cross-sectional data disaggregated by sector-province (15 x 11) cells. We only use one observation per year, namely the outcome in the 3rd quarter of each year. The base period is 2015Q3. The event study consists in regressing the outcome in a province-sector cell on province, sector, time and group (control vs treatment group) dummies and interactions between the group and the time dummies. The figures show the interaction terms together with their 95% CI. Standard errors are clustered by province-sector cells. The regression is weighted by the average outcome in the pre-reform period. The dashed vertical line distinguishes the pre and post-reform periods.

Regressions 1 and 4 in Table 6 quantify the graphical evidence presented in Figure 9. The policy increased the number of firms with one employee by 6.2% (regression 1). Put differently, 3,678 [95% CI: 1,777-5,579] firms with one employee would not have been established in the absence of the policy. These new firms increased employment in firms with one employee by 8.5%, thereby creating jobs for 3,571 [95% CI: 2,296-4,847] full-time equivalent employees (regression 4).

Table 6: DiD estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	**Growth in the number of firms**			**Employment growth**		
Treatment vs control group	1 vs 7-10	1-2 vs 7-10	1-6 vs 7-10	1 vs 7-10	1-2 vs 7-10	1-6 vs 7-10
Effect (average 2017-19)	0.0622***	0.0441***	0.0313**	0.0845***	0.0491***	0.0232**
	(0.0164)	(0.0147)	(0.0127)	(0.0154)	(0.0133)	(0.0112)
N	14,426	14,427	14,427	14,426	14,427	14,427
R-squared	0.106	0.093	0.106	0.098	0.084	0.080
Number of firms/employment in the treatment group in 2015Q3	59,134	89,509	136,288	42,266	86,451	232,113
Additional firms/employment (fte) created	3,678	3,947	4,266	3,571	4,245	5,385
95% CI	[1,777-5,579]	[1,368-6,526]	[873-7,658]	[2,296-4,847]	[1,991-6,498]	[290-10,480]

Note: The table shows the results of a DiD regression regressing the outcome on time, group, province and sector dummies and interaction terms between the group indicator (control vs treatment) and four time periods (pre-reform period; anticipation period; adjustment period and equilibrium period). Only the coefficient of the interaction term between the group indicator and the equilibrium period (2017Q1-2019Q4) is reported. Two outcomes and three treatment groups are considered. The control group always consists of firms with 7 to 10 employees. Standard errors are clustered by province-sector (11 x 15) cells. ***,**,* indicate statistical significance at the 1%, 5% and 10% level, respectively. At the bottom of the table, the number of employers or full-time equivalent employees in firms in the treatment group in 2015Q3 is reported, which allows us to compute the number of firms or fte jobs created by the policy.

We use different treatment groups in the remaining regressions in Table 6. Appendix G includes graphical evidence showing that the parallel trend holds in the pre-reform period in all specifications. We first test whether the results remain unaltered when the treatment group consists of firms with one to two employees (regressions 2 and 5). This is a relevant treatment group because, over the period 2017-19, almost 20% of the employees employed by firms with 1 or 2 employees were exempt from SSC (Table A.2 in Appendix A). As a result, the policy may also have a positive effect on firms with two employees. The 2016 reform also increased the temporary SSC reduction for firms hiring a second employee, but, as discussed in section 2, the generosity of the SSC reduction for the 2nd employee only slightly increased after the reform and is unlikely to have a substantial effect on the likelihood to hire a second employee. The results indicate that the policy created 3,947 new firms with 1 to 2 employees and created 4,245 new full-time equivalent jobs in those firms.

In regressions 3 and 6, the treatment group is defined as firms with one to six employees. We choose this treatment group because the 2016 reform not only introduced the SSC exemption for the first employee but also increased the generosity of temporary SSC reductions for firms hiring a second to fifth worker and introduced temporary SSC reduction for firms hiring a sixth employee. By defining the treatment group as firms with 1 to 6 employees, we capture the entire impact of the 2016 reform. As explained in section 2, the SSC exemption for the first employee is by far the most important policy reform so that the effect of the temporary SSC reduction for the 2nd to 6th worker is likely to be much smaller than the effect of the SSC exemption for the first employee. The entire reform created 4,266 firms with 1 to 6 employees which created 5,385 additional full-time equivalent jobs.

Although the difference in the number of new employers and new jobs created by the reform are not statistically significant across the three specifications considered, it is reassuring to observe that the impact of the 2016 reform appears t increase when gradually expanding the treatment group from firms with one employee to firms with 1 to 2 employees and, finally, firms with 1 to 6 employees. This gradual increase is what we expected given that (1) firms hiring a first employee after 2016 might have more than one employee by 2017Q1-2019Q4 and (2) the temporary SSC reductions targeted existing firms with 1 to 5 employees.

The DiD framework has several limitations. Most importantly, the aggregate data only allows us to examine the net effect of the policy on employment in small firms. We cannot test whether ineligible employers which already employed workers prior experienced job losses due to increased competition in the product and labour market. The aggregate nature of the data also prevents us from examining the impact of the reform on the wages of workers. Entrepreneurs make hiring decisions based on the labour costs over the entire lifetime of the job, taking the temporary and permanent SSC reductions into account. This net present value cannot be computed without panel data.

Nevertheless, the analyses confirm our key finding: the SSC exemption encouraged entrepreneurs to become employers. In addition, it provides evidence that the policy created new jobs, at least in the short run. Put differently, the positive impact of the subsidy on new employers outweighs the negative displacement effects whereby new employers force existing employers out of the market. Finally, in line with the literature on dynamic labour demand adjustment (Hamermesh, 1989), the RDiT demonstrate that entrepreneurs quickly adjust labour demand to a decrease in wage costs with a delay that is infra-annual. The DiD suggests that the market quickly reaches a new equilibrium, characterized by a shift in the firm size distribution towards smaller firms benefitting from the subsidy. While we cannot pinpoint the exact mechanism, this suggests that the reform either increased gross wages in all firms, leading to lower employment growth in existing firms or increased the firm exit rate.

7. Conclusion

This paper exploited a unique Belgian policy which permanently reduced the labour cost of the first employee for new employers to establish the relation between the labour cost and the decision to hire a first employee.

Two findings stand out. First, in line with the literature on dynamic labour demand (Hamermesh, 1989), new employers responded instantaneously to the drop in labour costs by adjusting labour demand: the number of firms hiring a first worker jumped by 30% immediately after the reform. This finding confirms the “free entry condition”, commonly made in standard firm dynamics models (e.g. Hopenhayn (1992)) as well as in search and matching models (e.g. Kaas and Kircher (2015)), which assumes that new employers immediately enter the market so that the expected value of profits over the firm’s lifetime equals the fixed entry cost.

Second, the elasticity of the decision to hire a first worker with respect to the labour cost is estimated at -1.67. This estimate is higher than the estimates of labour demand among existing firms which are typically in the range of -1 and zero (Lichter et al., 2015). This has two implications. First, in line with recent papers (e.g. Saez et al. (2019)), this finding rejects the standard tax incidence model which predicts that SSC reductions are fully passed on to the employee and have no impact on employment. Second, the finding suggests that SSC reductions targeted at first-time employers might be a more cost-effective strategy than across-the-board SSC reductions for all (existing and new) employers to create jobs.

While we do find positive effects of the SSC exemption on employment, our paper should only be considered a partial evaluation of the policy at hand. The most important limitation is that we only consider the short-term effects, which might be very different from the long-term effects. There are several reasons to expect a less positive evaluation in the longer term. First, in the long term, all firms will eventually benefit from the permanent SSC exemption for the first worker. As a result, the SSC exemption may increase wages in the longer term among all workers, as the standard tax incidence model predicts, thereby reducing the positive effects on employment. Second, the policy creates a disincentive to grow and is likely to distort the firm size distribution towards smaller, less productive firms. Disincentives to grow can have a substantial impact on average productivity (Garicano et al., 2016). Given the generosity of the subsidy, the Belgian economy may eventually count too many small firms at the expense of employment in larger, more productive firms.

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Appendix A: The policy

Table A.1: SSC reductions for the 1st to the 6the employee: 2004-2021

Employee	January 1, 2004	October 1, 2012	January 1, 2014	January 1, 2015	January 1, 2016	January 1, 2017
1	Q1-5: G1 Q6-13: G2 **Total reduction: €8,200**	Q1-5: G8 Q6-9: G1 Q10-13: G2 **Total reduction: €13,100**	Q1-5: G8 Q6-9: G1 Q10-13: G2 **Total reduction: €13,100**	Q1-5: G14 Q6-9: G15 Q10-13: G16 **Total reduction: €13,750**	Permanent exemption of SSC	Permanent exemption of SSC
2	Q1-13: G2 **Total reduction: €5,200**	Q1-5: G1 Q6-13: G2 **Total reduction: €8,200**	Q1-5: G1 Q6-13: G2 **Total reduction: €8,200**	Q1-5: G15 Q6-13: G16 **Total reduction: €8,850**	Q1-5: € 1,550 (G14) Q6-9: € 1,050 (G15) Q10-13: € 450 (G16) **Total reduction: €13,750**	Q1-5: € 1,550 (G14) Q6-9: € 1,050 (G15) Q10-13: € 450 (G16) **Total reduction: €13,750**
3	Q1-9: G2 **Total reduction: €3,600**	Q1-5: G1 Q6-9: G2 **Total reduction: €6,600**	Q1-5: G1 Q6-9: G2 **Total reduction: €6,600**	Q1-5: G15 Q6-9: G16 **Total reduction: €7,050**	Q1-5: € 1,050 (G15) Q6-13: € 450 (G16) **Total reduction: €8,850**	Q1-9: € 1,050 (G15) Q10-13: €450 (G16) **Total reduction: €11,250**
4			Q1-5: G1 Q6-9: G2 **Total reduction: €6,600**	Q1-5: G1 Q6-9: G2 **Total reduction: €6,600**	Q1-5: € 1,050 (G15) Q6-9: € 450 (G16) **Total reduction: €7,050**	Q1-9: € 1,050 (G15) Q10-13: €450 (G16) **Total reduction: €11,250**
5			Q1-5: G1 Q6-9: G2 **Total reduction: €6,600**	Q1-5: G1 Q6-9: G2 **Total reduction: €6,600**	Q1-5: € 1,000 (G1) Q6-9: € 400 (G2) **Total reduction: €6,600**	Q1-9: € 1,050 (G15) Q10-13: €450 (G16) **Total reduction: €11,250**
6					Q1-5: € 1,000 (G1) Q6-9: € 400 (G2) **Total reduction: €6,600**	Q1-9: € 1,050 (G15) Q10-13: €450 (G16) **Total reduction: €11,250**

Note: Abbreviation: Q: quarter. The letters 'G' are used by the NSSO and refer to the amount of the SSC reduction. G1=€1,000; G2=€400; G8=€1,500; G14=€1,550; G15=€1,050; and G16=€450. Firms who hired an eligible employee in 2015 received slightly higher SSC reductions for the remaining quarters in 2016. The table gives the maximum SSC reduction for a full time worker. The reduction can never be higher than the maximum theoretically payable SSC. Reductions are reduced (almost proportionally) when employees do not work full-time.

Table A.2: Share of subsidised workers over the period 2017Q1-2019Q4, by firm size

Firm size	**Subsidised first employee** (% of fte-employees)	**Subsidised 2-6 employee** (% of fte-employees)
1	29.8%	1.8%
1-2	19.6%	7.6%
1-6	3.9%	14.1%
7-10	0.6%	4.9%
All firms	1.02%	1.61%

Note: The table shows the share of the full-time equivalent (fte) employees for whom the employer receives the permenant SSC exemption (first employee) or a SSC reduction for the 2^{nd} to 6^{th} worker averaged over the period 2017Q1-2019Q4, by firm size. Source: NSSO.

Appendix B: Placebo tests

Table B.1: Using different cutoffs in the continuity-based RDiT

	Jan 2013	Jan 2014	Jan 2015	Jan 2017	Jan 2018
RDiT treatment effect	0.0033	-0.0172	-0.0151	0.0519	0.0747
Robust 95% CI	[-0.23 - 0.21]	[-0.14 - 0.06]	[-0.23 - 0.21]	[-0.08 - 0.28]	[-0.19 - 0.40]
Bandwith	10.36	12.58	13.8	9.7	7.58
Number of observation used:					
Left of the cutoff	7	10	11	7	5
Right of the cutoff	8	11	12	8	6

Note: This table shows the results of five placebo tests which pretend that the reform took place on January 1st, 2013, 2014, 2015, 2016 or 2017 rather than on January 1st, 2016. With the exception of the choice of the cutoff, the RDiT estimated here are exactly similar to the baseline specification (Table 2, reg 1). The outcome is the number of firms hiring a first employee. Importantly, as in the baseline specification, observations within the donut (i.e the period October-February) are also excluded here.

Appendix C: Testing continuity of the covariates

Figure C.1: Evolution of the legal form of nonemployers

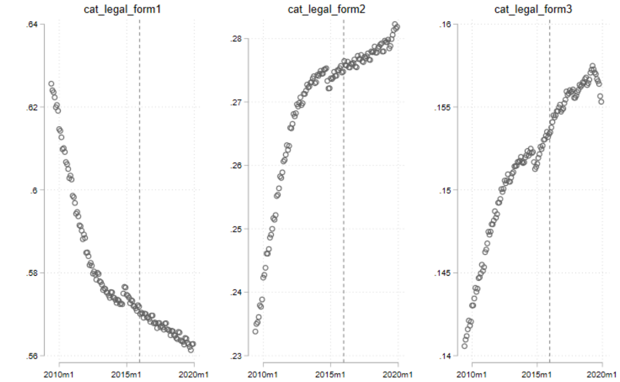


Table C.1: Testing continuity in the legal form among nonemployers around the cutoff

	Sole propietors	Private limited liability company	Other
RDiT treatment effect	-0.00085	0.00075	0.00000
Robust 95% CI	[-0.00193 - 0.00009]	[-0.00011 - 0.00167]	[-0.00078 - 0.00067]
Bandwith	12.08	13.01	9.95
Number of observation used:			
Left of the cutoff	12	13	10
Right of the cutoff	12	13	10

Note: The table shows the results of estimating a RDiT with as outcome the share of firms without employees registered as (1) sole proprietors, (2) private limited liability companies and (3) an other legal form. We test whether there is a discontinuity at January 1st, 2016. In contrast to the baseline specification, observations within the donut are not excluded.

Figure C.2: Evolution of the region of nonemployers

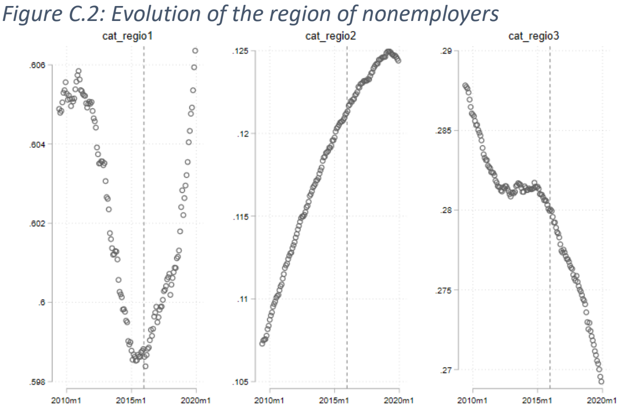
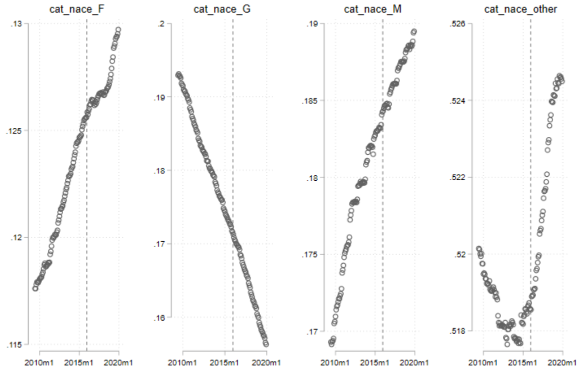


Table C.2: Testing continuity in the region among nonemployers around the cutoff

	Flanders	Brussels	Wallonia
RDiT treatment effect	-0.00042	0.00017	0.00025
Robust 95% CI	[-0.00075 - -0.00021]	[-0.00033 - 0.00074]	[-0.00034 - 0.001]
Bandwith	8	10.44	9.7
Number of observation used:			
Left of the cutoff	8	10	10
Right of the cutoff	8	10	10

Note: The table shows the results of estimating a RDiT with as outcome the share of firms based I (1) Flanders, (2) Brussels and (3) Wallonia. We test whether there is a discontinuity at January 1st, 2016. In contrast to the baseline specification, observations within the donut are not excluded.

Figure C.3: Evolution of the sector of nonemployers



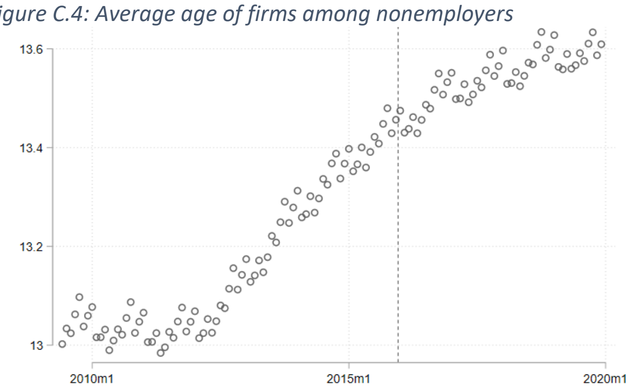
Note: We classified firms without employees in four sectors. We kept the three most important sectors (F: construction; G: Wholesale and retail trade; repair of motor vehicles and motorcycles; M: Professional, scientific and technical activities) and classified all other firms without employees in the group 'other sector'.

Table C.3: Testing continuity in the sector among nonemployers around the cutoff

	F	G	M	Other
RDiT treatment effect	-0.00001	-0.00027	0.00033	0
Robust 95% CI	[-0.00018 - 0.0001]	[-0.00112 - 0.00042]	[-0.00034 - 0.00099]	[-0.00041 - 0.00051]
Bandwith	9.3	14.02	14.44	7.94
Number of observation used:				
Left of the cutoff	9	14	14	8
Right of the cutoff	9	14	14	8

Note: The table shows the results of estimating a RDiT with as outcome the share of firms without employees by sector. We keep the three most important sectors, and classify all other firms in the group 'other sector'. We test whether there is a discontinuity at January 1st, 2016. In contrast to the baseline specification, observations within the donut are not excluded.

Figure C.4: Average age of firms among nonemployers



Note: The age of each firm within the population of firms without employees is defined at the 15th of each month.

Table C.5: Age (RDiT)

RDiT treatment effect	-0.02796
Robust 95% CI	[-0.07368 - 0.00894]
Bandwith	16.29
Number of observation used:	
Left of the cutoff	16
Right of the cutoff	16

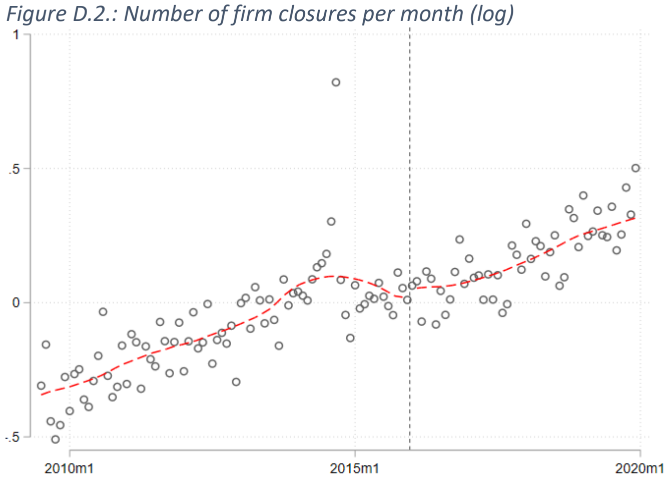
Note: The table shows the results of estimating a RDiT with as outcome the average of the firms without employees. We test whether there is a discontinuity at January 1st, 2016. In contrast to the baseline specification, observations within the donut are not excluded.

Appendix D: Additional outcomes

Figure D.1: Number of firms which stop employing workers per month (log)

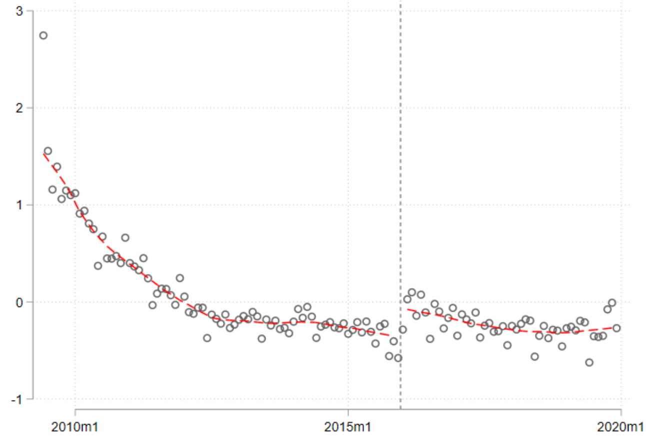
Note: The figure shows the (log of) number of employers which stop employing workers subject to SSC in a in a given month, after adjusting for seasonality.

Figure D.2.: Number of firm closures per month (log)



Note: The figure shows the (log of) number of firms which leave the market (i.e. firm closure) in a in a given month, after adjusting for seasonality.

Figure D.3.: The net effect (=number of firms hiring a first employee/number of employers which stop employing workers)



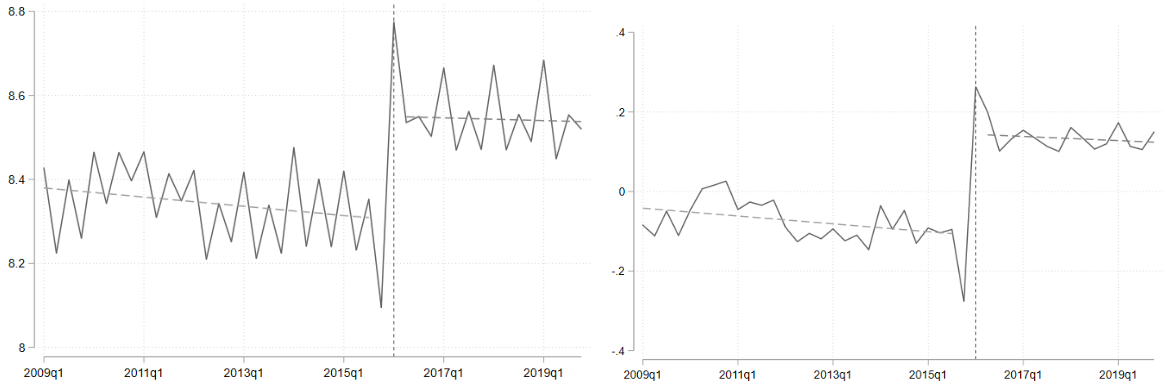
Note: The net effect is defined as the (log of) ratio of the number of firms hiring a first worker to the number of employers which stop employing workers.

Appendix E: RDiT – quarterly NSSO data (global polynomial)

Firms have to register at the NSSO when hiring a first worker. Figure X shows the number of firms registering for the first time at the NSSO by quarter. The sharp jump in the number of new employers in 2016Q1 confirms the positive impact of the SSC exemption on the decision to hire a first worker.

Figure E.1: Number of new employers registering at the NSSO (quarterly data)

Panel A: Raw data (logs) Panel B: corrected for seasonality (log)



Note: The Figures show the number of firms registering for the first time at the NSSO, which implies that these firms started employing a first worker subject to SSC.

We quantify the jump observed in Figure E.1 by estimating a RDiT. The quarterly nature of the data prevents us from restring the time series to the period just before and after the introduction of the policy, as we did when analysing the monthly CBE data. Instead, we follow (Lee and Lemieux, 2010) and fit a global polynomial to the time series separately for the pre- and post-reform period. We consider linear or quadratic polynomial of the quarterly time variable, i.e. cantered at January 1st, 2016. We exclude quarter 2015Q4 and 2016Q1 to avoid bias due to anticipation and catch-up effects.

Table E.2 shows that, according to the NSSO data, the SSC exemption increased the number of employers by 25 (linear polynomial) to 30 log points (quadratic polynomial). This is in line with the findings from the more detailed CBE data reported in the main text.

Table E.2: RDiT (global polynomials, quartely NSSO data)

	Linear polynomial	**Quadratic polynomial**
Treatment effect	0.254***	0.300***
	(0.0282)	(0.0476)
N	42	42
R^2	0.913	0.917

Note: Results of estimating a RDiT using a linear or quadratic global polynomials of the quarterly time variable, centered at January 1st 2016. The outcome is the log of the number of new employers as registered by the NSSO.

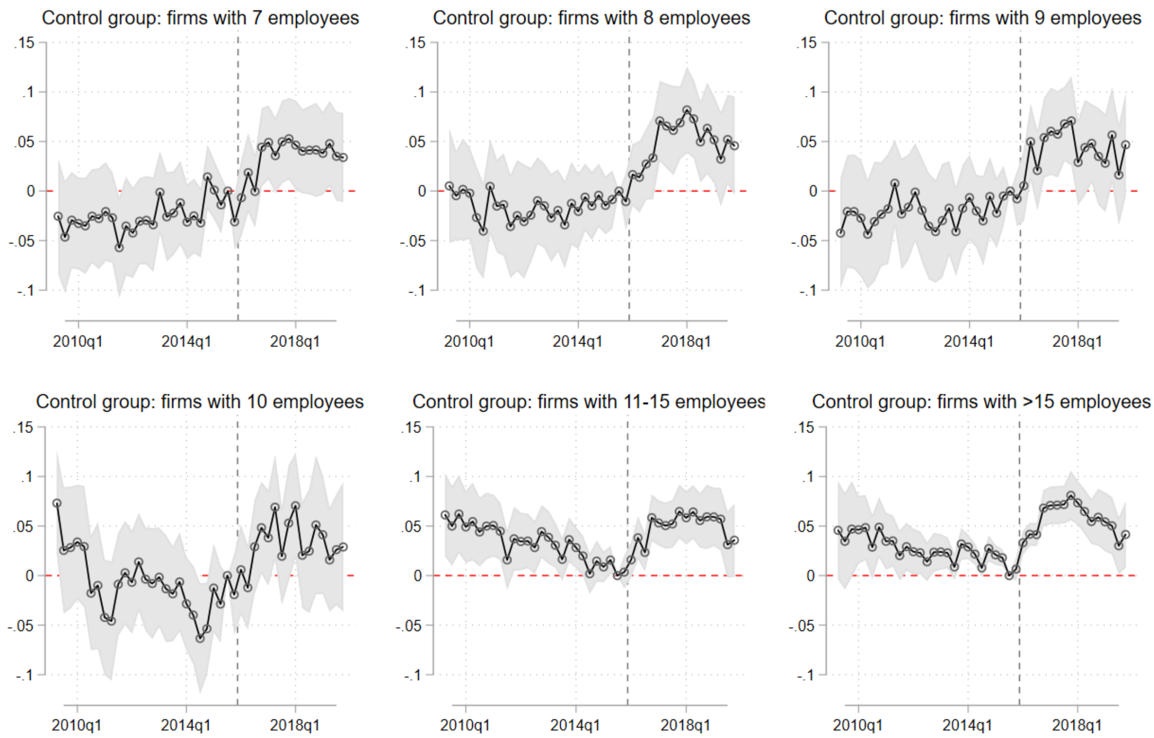
Appendix F: DiD – different control groups

This appendix shows event studies contrasting growth in the number of employers in firms with one employee (treatment group) to firms in control groups. Six different control groups are considered: firms with 7 employees; firms with 8 employees; firms with 9 employees; firms with 10 employees; firms with 11 to 15 employees; and firms with at least 16 employees.

These event studies show that the outcome in treatment and control group evolves in parallel for all control groups in the pre-reform, except when using firms with 11 to 15 or firms with at least 16 employees as control group.

This is the reason why we use firms with 7 to 10 employees as the control group in the all specifications in the main text. Instead of selecting one specific group, we group firms with 7 to 10 employees so that we have sufficient employers in each province-sector cell. This makes the estimates more precise.

Figure G.1: Growth in the number of employers (treatment group: firms with one employee; different control groups)



The figure shows the results of an event study contrasting growth in the number of firms between firms with 1 employee (treatment group) and firms in the control group using repeated cross-sectional quarterly data disaggregated by sector-province (15 x 11) cells. We consider six different control groups. The event study consist in regressing the outcome in a province-sector cell on province, sector, time and group (control vs treatment group) dummies and interactions between the group and the time dummies. The figures shows the interaction terms together with their 95% CI. Standard errors are clustered by province-sector cells. The regression is weighted by the average outcome in the pre-reform period. The dashed vertical line distinguishes pre and post-reform period.

Appendix G: Event studies – all specifications in Table 6

Figure G.1 shows event studies for all the specifications reported in Table 6 for different treatment groups and two outcomes (growth in the number of employers and employment growth). The control group always consists of firm with 7 to 10 employees. The figure shows that the parallel trend holds in the pre-reform period in all specifications. Also not that employment growth shows a seasonal pattern, with lower levels of employment in the third quarter. This is the reason why we only use one observation per year in Figure X in the main text.

Figure G.1: Event studies using different treatment groups for two outcomes