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DISCUSSION PAPER SERIES

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ABSTRACT

Labour Costs and the Decision to Hire the First Employee*

Firms without paid employees account for up to 80% of all firms, but only a small minority ever hires. This paper investigates the relationship between labour costs and the decision to hire a first employee and become an employer. Leveraging a unique policy in Belgium that permanently reduced the labour cost of the first employee by 13%, we find that the number of new, first-time employers jumped by 31% immediately following the reform. The elasticity of the probability to hire the first employee with respect to the labour cost is –2.39 [95% CI: –3.45, –1.25].

JEL Classification: D22, H25, J08, J23, L26, M13

Keywords: nonemployers, hiring decisions, payroll taxes, small businesses

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1. Introduction

Since the seminal paper of Birch (1979), entrepreneurs are praised as the engine of job creation. Young businesses account for a disproportionate share of job creation (Criscuolo et al., 2014; Decker et al., 2014; Haltiwanger et al., 2013). At the same time, the vast majority of nonemployer firms, i.e., firms without paid employees, will never hire (Acs et al., 2009; Davis et al., 2007; Fairlie and Miranda, 2017).

Herein lies a paradox: new employers are the driving force of job creation, but only a tiny fraction of nonemployers will ever become employers. In 2014, nonemployers accounted for 82% of all firms in the US (Bento and Restuccia, 2019). However, while the share of nonemployers has steadily increased over the last three decades, the rate at which nonemployers become employers has slowed down. This observation could explain the declining business dynamism, as measured by the entry rate of new employers, observed in the US (see Akcigit and Ates (2021), Decker et al. (2016), among others) and elsewhere (see Bijnens and Konings (2020) for Belgium). Understanding the determinants of the decision to hire a first employee can help explain why fewer nonemployers hire their first employee and can help evaluate numerous policies that aim at reviving business dynamism by supporting entrepreneurs and small businesses (e.g., Acs et al. (2016)).

This paper focuses on a specific but important determinant of the hiring decision: the labour cost of the first employee. When labour costs decline, economic theory predicts that nonemployers will hire their first employee. Endogenous entry of employers is a major component of general equilibrium models of firm dynamics à la Lucas (1978) or Hopenhayn (1992), as well as search and matching models with random search à la Diamond-Mortensen-Pissarides, or with directed search à la Moen (1997). Karahan et al. (2019), for instance, build on Hopenhayn (1992) to explain the decline in the entry rate of new employers since the late 1970s as a result of the slowdown in labour supply growth. The key mechanism in their paper is that the declining labour supply exerts upward pressure on labour costs, reducing the entry rate of new employers. In equilibrium, the decline in labour supply is completely absorbed by a decline in the entry rate of new employers, with no effect on real wages.

Remarkably few empirical studies have explored the inverse relationship between labour costs and the hiring decision of the first employee, despite the fact that this relationship is undisputed in economic theory. Studies investigating the determinants of the decision to hire the first employee have focused on the socio-economic characteristics of firm owners (Burke et al., 2002; Coad et al., 2017; Dvouletý, 2018; Fairlie and Miranda, 2017), including personality traits (Caliendo et al., 2022) and entrepreneurial ability (Henley, 2005). A few studies have explored the role of institutional features such as access to financial assets (Burke et al., 2000), entrepreneurs' personal tax rate (Carroll et al., 2000), mandated health insurance for employees (Mathur, 2010) and employment protection legislation (Millán et al., 2013). However, to the best of our knowledge, not a single empirical study has investigated the relationship between labour costs and the decision to hire the first employee.

We see two reasons for the limited previous research on this topic. First, few firm-level datasets include nonemployer firms and contain precise information on the timing of the first hire. Second, one needs exogenous variation in the labour cost of the first employee. A simple regression of the decision to hire on the (average) labour cost of the first employee, thereby exploiting variation in labour costs over time, is unlikely to uncover the causal relationship between labour costs and hiring decisions. The variation in average labour costs over time is limited (Karahan et al., 2017). In addition, the business cycle influences both hiring decisions and labour costs, making it difficult to separate the effect of the business cycle from the effect of lower labour costs.

This paper overcomes both challenges. First, we take advantage of exogenous, policy-induced variation in the labour cost of the first employee in Belgium. From January 1, 2016, the first employee of a new employer is permanently exempt from Social Security Contributions (SSC). This exemption reduced the labour cost of the first employee by up to 16%, leading to a sudden windfall profit. Second, we have access to administrative micro-level panel data of the population of Belgian firms that includes the exact date at which a (non)employer was established and hired its first employee.

We observe that potential employers quickly responded to the reduction in labour costs. The prompt response permits us to estimate a Regression Discontinuity in Time (RDiT). We find that the number of new employers increased by 31% immediately following the reform. This effect can be entirely attributed to a jump in the probability of hiring among nonemployer firms. The policy did not affect the number of firms that were established and hired their first employee within the same month. The impact of the reform is more pronounced for private limited liability companies than for sole proprietors and in sectors dominated by nonemployers.

Strategic behaviour does not explain the surge in new employers. We find no evidence that existing employers closed down firms and established new ones to qualify for the SSC exemption. Furthermore, the self-employed did not re-register as employees in order to claim the subsidy.

This paper's main contribution is to quantify the relationship between labour costs and the decision to hire the first employee. By doing so, we contribute to the literature on labour demand. Most papers study how labour costs affect labour demand among existing employers (Hamermesh, 1993; Lichter et al., 2015). By contrast, we study how labour costs determine the decision to hire a first employee and become an employer. In other words, we focus on labour demand elasticity at the extensive margin, whereas the literature estimates labour demand elasticities at the intensive margin.[1] We estimate the elasticity of the decision to hire a first employee with respect to the labour cost at −2.39, albeit with a large 95% confidence interval of [−3.45, −1.25]. This finding suggests that labour demand elasticities at the extensive margin are larger than those at the intensive margin, which are typically in the range of −1 and 0 (Lichter et al., 2015).[2]

There are two possible explanations for this result. First, typical features of (potential) employers, such as liquidity and credit constraints[3] (Fairlie, 1999; Holtz-Eakin et al., 1994), high recruitment and training costs (Muehlemann and Leiser, 2018) and high compliance costs (Harju et al., 2019) may explain why new employers are more sensitive to labour costs than existing employers. Second, the majority of new employers employ a single employee (Bijnens and

[1] De Mel et al. (2019) is a notable exception. The authors set up a RCT in Sri Lanka offering a temporary hiring subsidy to micro-enterprises, most of which did not employ paid workers prior to the intervention. In line with our findings, they find that nonemployer firms responded strongly to the subsidy and hired their first employee. But, they also noted that the positive effects disappeared once the policy expired.

[2] A recent paper estimates labour demand elasticity in Belgium at −0.6 (Bijnens et., 2023).

[3] 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 and Duflo (2014), Kersten et al. (2017) for a review).

Konings, 2020) so that the SSC exemption for the first employee subsidises the marginal employee. Cahuc et al. (2019) demonstrate that employers respond more forcefully to subsidies that reduce the labour cost of the marginal employee as opposed to subsidies that reduce the labour costs of all employees.

The paper also contributes to the vast literature on wage and hiring subsidies. The SSC exemption is distinct from wage or hiring subsidies in at least two ways. The first distinguishing characteristic is that the SSC exemption is a mix of a temporary hiring subsidy and a permanent wage subsidy. The SSC exemption is only granted for new employees—a typical feature of hiring subsidies—but is permanent—a typical feature of wage subsidies. Consistent with recent studies on payroll tax cuts (Bíró et al., 2022; Saez et al., 2012, 2019), we find that employers respond strongly to the SSC exemption. This finding contradicts the standard prediction that payroll tax cuts increase wages with no effect on employment. The second factor that differentiates the SSC exemption from wage and hiring subsidies is that these subsidies often target vulnerable populations such as young (Albanese et al., 2022; Saez et al., 2019) or older workers (Albanese and Cockx, 2019; Bíró et al., 2022). By contrast, the SSC exemption is targeted at new employers, regardless of the characteristics of the first employee. This aspect of the SSC exemption relates our paper to the recent work of Cahuc et al. (2019) who show that a temporary hiring subsidy targeted at firms with fewer than ten employees during the Great Recession successfully created new jobs at a net cost per job of about zero.

Finally, the paper adds to the large literature on whether and which policies supporting start-ups have the potential to create jobs (Acs et al., 2016; Dvouletý et al., 2021). Payroll tax cuts may help nonemployers cover the one-time fixed costs of hiring their first employee or may convince them to take the risk of hiring their first employee, which could result in a virtuous circle of continued job creation. Several studies have demonstrated that investment subsidies that favour smaller employers over larger ones have a greater impact on employment and investment.[4] Our evaluation demonstrates that a generous SSC exemption targeted at nonemployers is effective in encouraging them to hire their first employee. While this is a necessary first step to a successful policy, a comprehensive evaluation of the policy goes beyond the scope of this paper.

The paper is structured as follows. In the next section, we discuss the 2016 policy reform. Section 3 presents the two datasets used in this paper. Sector 4 discusses the Regression Discontinuity in Time. Section 5 examines the relationship between labour costs and the decision to hire the first employee, tests for strategic behaviour by employers and the self-employed, and presents a heterogeneity analysis. Section 6 concludes.

2. The SSC exemption for the first employee

On October 10, 2015, the Belgian government unexpectedly[5] announced that new employers would be permanently exempt from Social Security Contributions (SSC) for the first employee.[6]

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

[5] 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.

[6] The law initially stipulated that the SSC exemption would only be granted for employees hired before December 31, 2020. In 2020, the new government extended the policy until the end of 2021. Since 2022, the SSC reduction is capped at €4,000 per quarter, but remains permanent.

The government stated that the permanent SSC exemption would encourage the solo self-employed to hire their first employee, thereby creating new jobs.

The new policy went into effect for private sector firms that hired their first employee after January 1, 2016. Private sector firms are eligible for the SSC exemption if they did not employ a worker subject to SSC in the previous four quarters. The law prohibits existing employers from splitting into smaller units or from establishing new firms in order to qualify for the subsidy. The National Social Security Office monitors and enforces these conditions (Court of Audit, 2021).

The SSC exemption has some remarkable features. First, the exemption is not time-limited, making it a very generous subsidy. Second, firms retain the exemption even as they continue to expand. Third, the exemption is not tied to a specific individual. Firms retain the exemption if the ‘first’ employee leaves and is replaced. Fourth, firms with several employees can designate the employee for whom the exemption is claimed. To maximise the subsidy, employers will claim the SSC exemption for the employee with the highest gross wage.

The 2016 reform replaced previously existing temporary hiring subsidies for the first employee that had been in effect with some modifications since 2004. When they hired their first employee in 2015, new employers could get a €1,550 quarterly SSC reduction for the first five quarters, €1,050 for the next four quarters, and €450 for the last four quarters. These reductions could not exceed the theoretical maximum SSC.

The median gross monthly wage of subsidised first employees hired in 2016 is €2,050, which is equal to a gross quarterly wage of €6,150 (Court of Audit, 2021, Table 8). Without any reductions, the employer would pay quarterly SSC of €1,530. With the temporary SCC reductions in place before the 2016 reform, this employer starts paying SSC after five quarters. With the permanent SSC exemption, in place after the reform, the median employer pays a SSC of 4%.[7] This example illustrates that the temporary hiring subsidy is equally generous as the SSC exemption during the first five quarters after hiring. The permanent exemption becomes, however, more generous after five quarters. Hence, for most employers, it is the permanent duration of the subsidy rather than the higher quarterly reductions that makes the subsidy far more generous after the reform.

The empirical analysis compares the number of new employers before and after the 2016 reform. To be able to interpret the findings in terms of elasticity, we compute the expected reduction in labour costs induced by reform (Table 1). To do so, we consider the temporary SSC reductions in place before the reform and the permanent SSC exemption after the reform.

In this computation, the chosen values of the annual discount rate and the survival rate of employers matter. These parameters determine the value of the temporary SSC reductions before the reform and, therefore, the extent to which the SSC exemption reduces labour costs. A lower annual discount rate and exit rate make the permanent SSC exemption more generous than the temporary SSC reductions. Nevertheless, we demonstrate that the elasticity is not very sensitive to reasonable alternative choices of these parameters.

We make the standard assumptions that entrepreneurs have an infinite time horizon and discount the future stream of benefits not only by the regular discount rate but also by a firm destruction rate. We follow the search and matching literature (e.g., Kaas and Kircher (2015),

[7] Even with the SSC exemption and the temporary SSC reductions, the SSC rate is not equal to 0% because employers are only exempt from paying the “base contribution”, but still have to pay “specific SSC” that cover specific costs (e.g., sectoral training, occupational injuries, short-time work).

Cahuc et al. (2019)) and set the annual discount rate at 5%. We use the exit rate of new employers reported by Novella (2021) for the first seven years after hiring their first employee and assume that employers no longer die after seven years. Appendix A documents the sensitivity of our results to those assumptions.

Table 1 shows the expected reduction in labour costs when hiring a first employee with a gross monthly wage of €1,580, €2,050, or €3,270. These wages correspond to the 10th, 50th, and 90th percentiles of the wage distribution of employees hired with a subsidy in 2016 (Court of Audit, 2021, Table 8). Without any SSC reduction, an employer hiring an employee at the median wage would face a SSC rate of 24.8% (*Table 1*, column 1). The temporary reductions for the first employee granted until the end of 2015 reduced the expected SSC rate to 19.5% (*Table 1*, column 2). After the reform, the SSC rate equals 4% (*Table 1*, column 3).

The reform decreased the expected labour costs for the median employee by 13.0% relative to the pre-reform period (*Table 1*, column 4). The labour cost reduction depends on the gross wage of the employee. The policy reduced labour costs by 9.5% for employees with a gross wage of €1,580 and by 16.3% for employees with a gross wage of €3,270.

Table 1: Expected SSC rate before and after the reform for the first employee and the reduction in expected labour costs

Gross monthly wage	**Average expected SSC rate over the firms' lifetime**			**Reduction labour costs**
	SSC rate without reductions	Pre-reform SSC rate (temporary SSC reduction)	Post-reform SSC rate (permanent SSC exemption)	
€ 1,580 (10th percentile)	19.8%	14.9%	4%	-9.5%
€ 2,050 (median wage)	24.8%	19.5%	4%	-13.0%
€ 3,270 (90th percentile)	27.7%	24.3%	4%	-16.3%

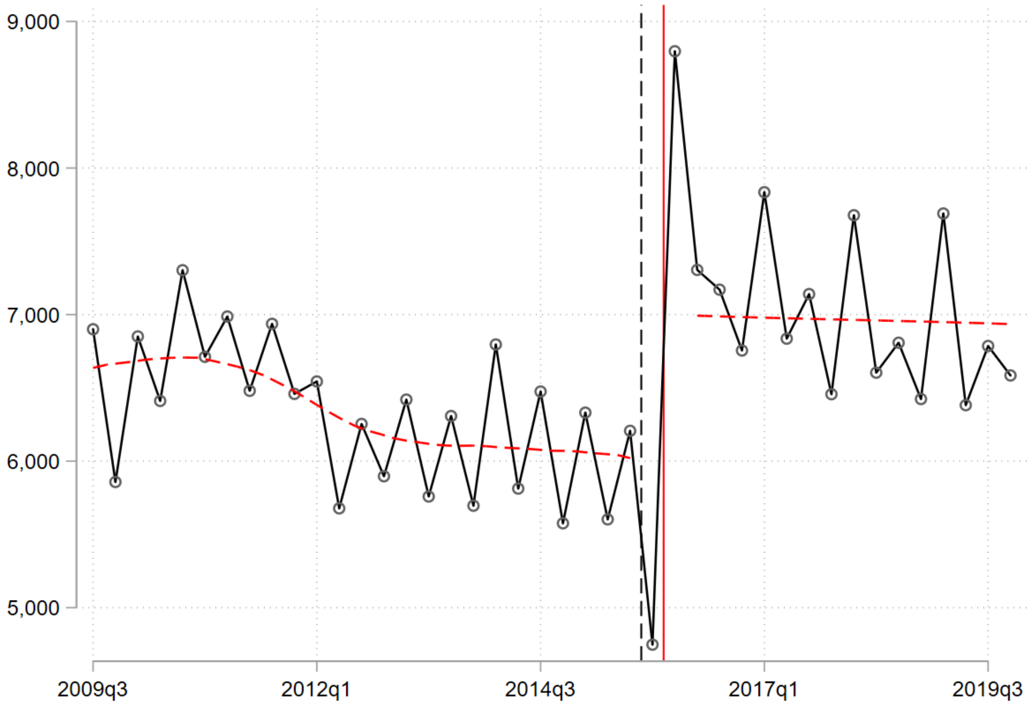
Note: The expected labour cost over the firm's lifetime is computed as $E(labour\ cost) = \sum_{t=0}^{k} \frac{w(1+\tau_t)S_t}{(1+r)^t}$, where w represents the gross wage; τ_t the SSC rate t quarters after having hired the first employee; S_t the probability of still employing workers at quarter t; r the discount rate; and k the time horizon of the entrepreneur. The temporary SSC reductions in the pre-reform period and permanent exemption in the post-reform period alter τ_t. After the reform, τ_t equals 4% 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. The probability of continuing to employ workers in the first seven years after hiring a first employee was computed by the Federal Planning Bureau for the cohort of firms that hired their first employee in 2012 (Novella (2021), Figure 1). We assume that all firms continue to employ workers after having done so for seven years. The quarterly interest rate is set at 1.25%. The time horizon of the entrepreneur, k, is set at infinity.

The SSC exemption proved popular. Until 2016, the number of subsidised first employees with a temporary SSC reduction fluctuated around 22,000 full-time equivalents. Since 2016, the number of subsidised employees has gradually increased, surpassing 30,000 full-time equivalents by 2019 (approximately 1% of the employees in Belgium). The cost of the subsidy amounted to 277 million euros in 2019 (approximately 0.06% of GDP) and continues to increase.

One of the policy's objectives was to encourage entrepreneurs to hire their first employee and become employers. Whether the policy achieved this objective, is the focus of this paper. The sharp jump in the number of firms hiring their first employee per quarter immediately following the 2016 reform already strongly hints at a positive effect of the SSC exemption on the hiring decision (*Figure 1*).

Note that the figure 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 rebound in 2016Q1. The latter effect consists of a 'real' effect, i.e., entrepreneurs hiring their first employee who would not have hired absent the reform, and a 'catch-up' effect, i.e., entrepreneurs hiring in 2016Q1 after postponing hiring in 2015Q4. The empirical strategy will take anticipation and catch-up effects into account.

Figure 1: The flow of new employers, by quarter



Source: CBE. Own compilation.
Note: The population consists of all Belgian firms. The number of new employers is grouped by quarter. The full red vertical line indicates the implementation of the policy on January 1, 2016. The black dashed line indicates the announcement of the policy on October 10, 2015. The dashed red line fits a local polynomial regression to the data.

3. Data and descriptives

We use two complementary administrative datasets: micro-level panel data from the Crossroads Bank for Enterprises (CBE) and aggregate data from the National Institute for the Social Security of the Self-Employed (NISSE). The main analyses are carried out on the CBE dataset, while the NISSE dataset is used to test for strategic behaviour by the self-employed.

The CBE dataset

The CBE administers a comprehensive database of all companies and businesses in Belgium. By law, all firms are required to register with the CBE before starting their activities. This obligation holds for sole proprietors as well as incorporated firms and for non-profit as well as for-profit companies. We obtained micro-level data on the population of firms registered at the CBE on June 30 for the years from 2009 to 2019.

The CBE collects a limited set of firm characteristics, such as the sector[8] and the legal form. Importantly, the CBE is automatically notified by the National Social Security Office (NSSO) when a firm registers at the NSSO. Registration signals that the firm hired an employee subject to SSC. This data allows firms to be classified as either nonemployers (those that are not registered with

[8] 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'.

the NSSO) or employers (those that are registered with the NSSO). A limitation of the CBE data is that it only contains a dichotomous indicator indicating whether a firm is registered at the NSSO but does not contain information on the total number of employees employed by the firm.

Nonemployers differ from employers in several respects. The first two columns of Table 2 describe some features of these populations on June 30, 2015. Nonemployers account for 81% of all firms. Compared to employers, nonemployers are more likely to be sole proprietors (54% vs. 16%), are younger, and are overrepresented in certain sectors, such as the sector of 'professional, scientific and technical activities'.

The third column of Table 2 displays the characteristics of the population of new employers defined as firms that hired their first employee between July 1, 2014, and June 30, 2015. New employers are distinct from employers and nonemployers. They are much younger than the stock of employers: half of the new employers are less than one year old at the time of hiring, whereas 64% of the employers are more than ten years old. Compared to incumbent employers, new employers are more active in 'Construction' (17% vs. 14%) and 'Accommodation and food service activities' (16% vs. 10%).

Table 2: Characteristics of nonemployers, employers, and new employers

	Nonemployers	**Employers**	**New employers**
Legal form			
Sole proprietors	54%	16%	26%
Private limited liability company	29%	50%	54%
Other	17%	34%	21%
Firm's Age			
<1 year old	18%	10%	51%
1-5 years old	11%	8%	10%
5-10 years old	22%	18%	15%
> 10 years old	49%	64%	24%
Sector			
G: Wholesale and retail trade	19%	25%	24%
M: Professional, scientific and technical activities	16%	8%	8%
F: Construction	14%	14%	17%
I: Accommodation and food service activities	6%	10%	16%
Other	44%	42%	36%
N	**897,038**	**213,719**	**23,985**

Note: Columns 2 and 3 describe the population of nonemployers and employers on June 30, 2015. The population of new employers consists of firms that hired their first employee between July 1, 2014, and June 30, 2015. The registration date at the CBE defines the firm's establishment date. The firm's age of (non)employers is determined on June 30, 2015, and that of new employers at first hiring. For conciseness, we only show the four sectors with the highest number of nonemployer firms.

The NISSE dataset

The self-employed must register with the National Institute for the Social Security of the Self-Employed (NISSE), which administers the compulsory SSC of the self-employed. We obtained aggregate data on (1) the monthly flow of new individuals registering with NISSE and (2) the quarterly stock of self-employed individuals. This dataset will be used to test whether some self-employed individuals re-registered as employees in response to the policy.

4. Regression Discontinuity in Time

The immediate adjustment of labour demand after the announcement of the policy, as evidenced by *Figure 1*, leads us to adopt a Regression Discontinuity in Time (RDiT) to quantify the relation between labour costs and the decision to hire a first employee (Anderson, 2014; Cui et al., 2021; Godard et al., 2022; Hausman and Rapson, 2018).

We apply the canonical continuity-based sharp RD design with calendar time as the running variable. This approach allows us to quantify the 'jump' in several outcomes at the moment the policy was implemented. Our main outcome is the number of firms hiring their first employee per month, but we also consider the probability of hiring among nonemployers and the number of new firms that are established and hire their first employee within the same month. Furthermore, we use a RDiT to test whether strategic behaviour by existing employers or the self-employed explains the main findings.

In its most basic set-up, a sharp continuity-based RDiT consists in estimating the following regression:

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

where y_t denotes the outcome at calendar time t; and I_t is an indicator equal to 1 after the reform is implemented, and zero otherwise.

The identifying assumption of the RDiT is that, in the absence of the 2016 reform, the relation between the potential outcome and calendar time would have been continuous in the neighbourhood of the cutoff and is entirely captured by the flexible function $f(t)$. Under these conditions, the parameter β identifies the impact of the reform on the outcome.

In the RDD literature, it is now common practise to approximate the function $f(t)$ by a linear function in a small window around the cutoff. We follow the data-driven procedure developed by Calonico et al. (2014) to determine the optimal bandwidth on either side of the cutoff. [9] The optimal bandwidth is determined after removing the observations within the donut, which is discussed in more detail below.

While a RDiT resembles a RDD with calendar time as the running variable, a RDiT does not share all the attractive features of a RDD (Hausman and Rapson, 2018). Three complications are addressed: seasonality, the discreteness of the running variable, and anticipation and catch-up effects.

Let us first consider seasonality. A RDD relies on the assumption that the outcome would have evolved continuously in the neighbourhood of the cutoff absent the policy reform. Consequently, including covariates in a RDD may improve 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 where the outcome is a time series subject to seasonality. These seasonal patterns induce discontinuities in the outcome, which bias the RDiT estimates. For this reason, one needs to deseasonalize the time series before applying standard RDD procedures.

In our setting, seasonality results from the fact that new employment contracts typically start on the first day of the month, even when the first day of the month falls on a weekend or a public holiday. This 'first day of the month' effect creates discontinuities in our main outcome, thereby violating the identifying assumption. In addition to 'day' effects, we also observe monthly seasonality in hiring patterns since hiring in the first month of a quarter is more common than in the last two.

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

We deal with the daily and monthly patterns in the time series as follows. Although we know the exact day at which an event occurred, we define outcomes by month to eliminate 'day effects'. For instance, our main outcome simply counts the number of firms hiring their first employee in a given month. To address seasonality in the monthly data, we follow the recommendation of Hausman and Rapson (2018) and implement a two-step procedure. We first correct flexibly for seasonal effects by regressing the outcome on twelve monthly dummies using the entire time series, with the exception of observations within the 'donut', i.e., the time period characterised by anticipation and catch-up effects, discussed in more detail below. The residuals of this regression are subsequently used as the outcome in the RDiT. For our main specifications, we will report robust 95% confidence intervals (CI) based on bias-corrected robust standard errors, as derived by Calonico et al. (2014), as well as bootstrapped 95% robust CI that take the two-step estimation procedure into account. Since the bootstrapped 95% CI are only slightly larger than the robust 95% CI, we only bootstrap the standard errors in the main specifications reported in *Table 3*.

The second complication, the discreteness of the running variable, is the direct consequence of using monthly instead of daily data: each month constitutes a distinct mass point. As Cattaneo et al. (2018) show, when the running variable is discrete, estimating a RDD using all observations is essentially similar to estimating a RDD on data aggregated by mass point. For this reason, we collapse the data by month. As a result, we only have one observation per month. Aggregating the data by month has the additional advantage of substantially reducing the computational burden as we reduce the dataset from over 800,000 observations per month in some specifications to only one observation per month.

Finally, we estimate a donut RDiT to account for anticipation and catch-up effects. The policy was announced on October 10, 2015, and came into force on January 1, 2016. Potential employers anticipated 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 1*. Following the RDD literature (Barreca et al., 2016), we estimate a 'donut' RDiT, excluding observations 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 in the post-reform period, T_e, at which the number of 'excess hires' in the period January 1, 2016, until T_e equals the number of 'missing hires'. It turns out that excluding the period January-February 2016 is sufficient (see Appendix F). To determine the impact of the policy at the cutoff, we extrapolate within the donut from both sides of the cutoff.

Extrapolation within the donut requires parametric assumptions (Dowd, 2021). As an alternative to the continuity-based RDiT, we also implement a local randomization method, which does not require extrapolation (Cattaneo et al., 2018). The local randomization method can only be implemented when have multiple observations per month which is the reason why we only use this method for the outcome 'probability of hiring in month m among nonemployers in month $m-1$'. In our setting, the local randomization method consists in comparing the seasonality-adjusted hiring probability in September 2015—just before the announcement of the reform—to the hiring probability in March 2016—the month just after the donut.

5. Results

In this section, we first examine the effect of the SSC exemption on the number of firms hiring their first employee. The main conclusion of these analyses is that, immediately after the reform, the policy increased the number of firms hiring their first employee by increasing the probability of hiring among existing nonemployers, without having any effect on the number of new firms that are established and hire their first employee within the same month.

We then focus exclusively on the outcome 'hiring probability among existing nonemployers', which is the main margin of adjustment. We estimate the impact of the reform on this outcome using both the continuity-based RDiT and the local randomization approach. Both methods yield similar findings, which provides evidence that the results from the continuity-based RDiT are credible.

Next, again using the continuity-based RDiT, we examine whether strategic behaviour—which is explicitly forbidden by law but may occur in practise—explains the findings. We first show that existing employers did not establish new firms to benefit from the subsidy. We then explore whether the self-employed hired themselves, thereby switching from being self-employed to being an employee. The evidence does not support this interpretation either.

Finally, we present heterogeneity analyses that test three hypotheses from the literature on small businesses and firm dynamics. More specifically, we examine whether the impact of the reform is more pronounced for (1) private limited liability companies vs. sole proprietors; (2) for young vs. older firms; and (3) in sectors dominated by nonemployers.

Throughout the result section, we take the log of each outcome to express the treatment effects as semi-elasticities. We show in Appendix E that the findings remain robust when estimating the RDiT in levels.

5.1. Impact on the number of firms hiring their first employee

Figure 2 illustrates the impact of the SSC exemption on our main outcome: the number of firms hiring their first employee per month adjusted for seasonality. The graphical evidence reveals a substantial and immediate impact of the 2016 reform on the number of firms hiring their first employee. The number of firms hiring their first employee is surprisingly stable from June 2009 until the announcement of the policy in October 2015, but surges immediately following the implementation of the policy in January 2016.

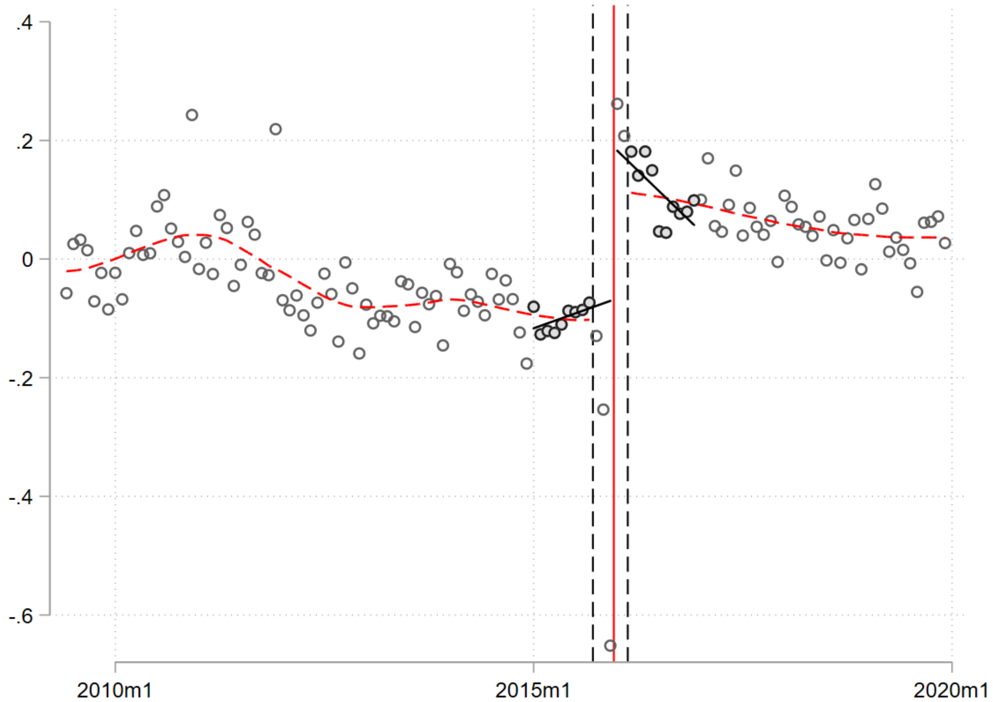
In addition, *Figure 2* reveals anticipation and catch-up effects. After the policy was announced, the number of firms hiring their first employee sharply decreased, indicating that firms postponed hiring to benefit from the subsidy. This effect is most obvious in December 2015: the number of firms hiring in December 2015 is less than half the number in September 2015. The number of firms hiring their first employee is slightly higher in January than in February 2016 as firms postponed hiring from October-December 2015 to January 2016.

As previously discussed, we borrow an approach from the bunching literature to determine the size of the donut (Benzarti and Harju, 2021; Kleven and Waseem, 2013). We first estimated the number of 'missing' hires in October-December 2015. We then use this estimate to determine the period after the reform that has to be excluded so that the number of missing hires in the pre-reform period equals the number of excess hires in the post-reform period. We estimate the number of missing hires at 784. Excluding January and February 2016 ensures that the number of excess hires exceeds the number of missing hires. The number of excess hires in

January and February 2016 is estimated to be 825. Appendix F provides more details on the estimation strategy.

The dashed lines in *Figure 2* indicate the donut. Observations within the donut are excluded when estimating the RDiT, but we extrapolate within the donut to the cutoff to determine the causal effect of the policy at the cutoff.

Figure 2: Impact of the reform on the number of firms hiring their first employee



Note: The figure shows the log of the number of firms hiring their first employee by month after adjusting for seasonal patterns for the period June 2009 through December 2019. The dashed red line fits a local polynomial regression to the data. The solid red line indicates the implementation date of the policy (January 1, 2016). The dashed black lines indicate the donut, which starts when the policy was announced (October 10, 2015) and ends at the end of February 2016, i.e., 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. Only the solid grey dots fall within the 'optimal window' and are used to estimate the RDiT.

The first regression in *Table 3* quantifies the graphical evidence presented in Figure 2. The RDiT estimate indicates that the 2016 reform increased the number of firms hiring their first employee by 27 log points, with a bootstrapped robust 95% CI of [0.15, 0.37].[10] This corresponds to a 31% increase in the number of firms hiring their first employee. The SSC exemption reduced the labour costs of the first employee with a median wage by 13%. Hence, the elasticity of the probability to hire a first worker with respect to the labour cost is estimated at −2.39 with a 95% CI of [-3.45, −1.25].

This finding demonstrates that the elasticity at the extensive margin, i.e., the elasticity of the probability to hire a first employee with respect to the labour cost, is larger than the elasticity at the intensive margin, i.e., the probability to hire an additional employee among existing

[10] Placebo tests that pretend that the reform took place on January 1, 2013, 2014, 2015, or 2017 instead of January 1, 2016 are reported in Appendix C. All placebo estimates are small and not significant at conventional levels. This finding strengthens our claim that the 2016 effect captures the effect of the reform.

employers. The elasticity at the intensive margin reported in the literature is typically in the range of −1 and 0 (Lichter et al., 2015).

By contrast, an elasticity of −2.39 is in line with elasticities reported in the empirical literature on hiring subsidies. Cahuc et al. (2019) prove that employers are more responsive to subsidies that reduce the labour cost of the marginal employee than to subsidies that reduce the labour cost of all employees, thereby also reducing labour costs of infra-marginal workers who would always have been hired. Empirical studies indicate that the elasticity of hiring subsidies—which often target marginal employees—is in absolute value larger than 1.[11]

In Section 2, we computed the impact of the SSC exemption on the (expected) labour cost of the first employee, assuming that employers discount future streams of benefits when making hiring decisions. As we showed, the permanent nature of the SSC exemption is the reason why this exemption is more generous than the previously existing temporary SSC reductions. Interestingly, the fact that employers responded to the SSC exemption proves that potential employers indeed consider the long-term benefits of the subsidy when making hiring decisions. As we document in Appendix A, the impact of the SSC exemption on labour costs and, consequently, the elasticity is sensitive to assumptions about the discount rate and the exit rate of employers. Under reasonable assumptions, the elasticity ranges from −2.72 to −2.06. Our preferred estimate of −2.39 lies in the middle of this range.

Table 3: RDiT for several outcomes

	(1)	(2)	(3)	(4)
Outcome (log)	Number of firms hiring their first employee	Number of firms that are established and hire within the same month	Existing nonemployers hiring their first employee	Probability of hiring among nonemployers
RDiT treatment effect	0.27	0.049	0.28	0.29
Robust 95% CI	[0.18, 0.34]	[−0.19, 0.21]	[0.20, 0.35]	[0.21, 0.37]
Bootstrapped robust 95% CI	[0.15, 0.37]	[−0.34, 0.39]	[0.17, 0.39]	[0.17, 0.40]
Bandwidth	11.65	10.23	11.99	12.17
Number of observations used:				
Left of the cutoff	9	7	9	9
Right of the cutoff	10	8	10	10
Donut RDiT	Yes	Yes	Yes	Yes

Note: The table shows the results of estimating a donut RDiT for four outcomes: (1) the number of firms hiring their first employee each month; (2) the number of new firms that are established and hire their first employee within the same month; (3) the number of firms hiring their first employee in month m among those firms that already existed at the end of month $m-1$; (4) the probability of hiring in month m among the population of nonemployers in month $m-1$. Observations within the donut are excluded from the regressions. This donut corresponds to the period October 2015 – February 2016. As explained in the text, the outcomes are the residuals of a regression that removes the seasonality in the raw outcome. The point estimates are constructed using a linear polynomial (p=1) with a triangular kernel. The optimal bandwidth is determined 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: robust 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% bootstrapped robust CI that, as suggested by Hausman and Rapson (2018), account for the two-step procedure. The bootstrapped 95% CI intervals are obtained by drawing a random sample with replacement of the original dataset and estimating a RDiT using this new dataset after correcting for seasonality. The upper and lower bounds of the bias-corrected robust 95% CI intervals are recorded for each replication. The bootstrapped robust 95% CI is the average value of the lower and upper bounds over 500 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.

Two mechanisms can contribute to the total increase in the number of new employers: (1) entrepreneurs might have launched new firms and these new firms might immediately have hired employees; or (2) existing nonemployer firms might have hired their first employee. To understand the relative importance of each channel, we estimate the effect of the reform on: (1) the number of firms that are established and hire their first employee in the same month; and (2) the number of firms hiring their first employee in month m among those firms that already existed at the end of month $m-1$. In the pre-reform period, only 8% of the firms hiring

[11] Desiere and Cockx (2022) report an elasticity of −1.0; Albanese et al. (2022) of −2.0; and Cahuc et al. (2019) of −4.0.

their first employee in a given month were established in the same month. In other words, most firms already exist a few months before hiring their first employee.

The overall effect is entirely driven by a positive effect among existing nonemployers firms: the reform had no significant effect on the number of new firms that are established and hire their first employee within the same month (*Table 3*, regression 2), whereas the number of existing nonemployers hiring their first employees increased by 28 log points immediately following the reform (regression 3).

5.2. Impact on the hiring probability

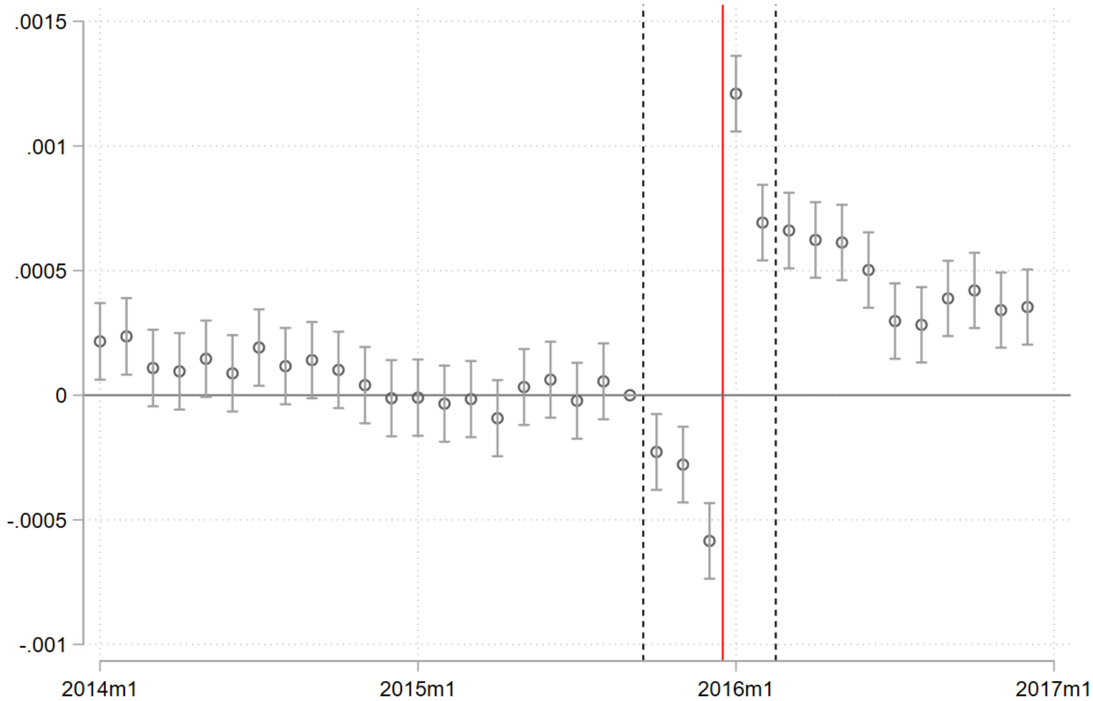
The observation that the reform increases hiring among existing nonemployers, without any effect on the number of newly established employer firms, justifies our focus on the outcome 'hiring probability' in this section. This outcome equals one if a nonemployer firm active at the end of month $m-1$ hires a first employee in month m, and zero otherwise. Using the continuity-based RDiT, regression 4 in Table 3 shows that the hiring probability among nonemployers jumped by 29 log points following the 2016 reform.

A key advantage of focusing on this outcome is that the effect of the reform can also be estimated using a local randomization approach, which provides an alternative to the continuity-based RDiT used previously (Cattaneo et al., 2018). This approach is only possible for this outcome because it requires many (instead of a single) observations per month. The local randomization approach eliminates the need to select a bandwidth and to extrapolate within the donut to the cutoff. In our setting, the local randomization approach consists in comparing the monthly hiring probability among nonemployers just before and after the donut.

Figure 3 shows the seasonality-adjusted probability that nonemployers will hire their first employee in the next month relative to this probability in September 2015. The figure confirms the sharp drop in the probability of hiring just after the policy was announced but not yet implemented, and the sharp increase following its implementation. Importantly, the outcome remained remarkably stable in the fifteen months preceding the reform's announcement, thereby providing evidence that the increase observed from January 2016 onward can be attributed to the reform.

Comparing the probability of hiring in September 2015 to March 2016 indicates that the reform increased the probability of hiring by 0.066 percentage points [95% CI: 0.00050, 0.00082]. The probability that nonemployers hire their first employee in September 2015 is 0.26%. Hence, in relative terms, the reform increased the probability of hiring by 25%, which is slightly lower than the continuity-based RDiT estimate (see regression 4 in *Table 3*), but still falls inside its 95% CI.

Figure 3: The probability of hiring among nonemployers by month (relative to September 2015)



Note: The figure shows the probability of hiring in month m among nonemployers on the last day of month $m-1$ relative to September 2015, adjusted for seasonality. The dashed vertical lines indicate the donut. The solid red line separates the pre-reform and post-reform periods. The local randomization approach consists in comparing the outcome in September 2015 (just before the announcement of the policy) to March 2016 (just right of the donut). The probability of hiring in September 2015 is 0.26%.

5.3. Real effects or strategic behaviour?

A major concern with the interpretation of the results is that the estimates may reflect strategic behaviour by either existing employers or the self-employed, rather than revealing a true response by nonemployers. In order to qualify for the subsidy, existing employers may decide to close down existing firms and start new ones or may set up new firms when expanding rather than recruiting an additional employee in existing firms. Similarly, the self-employed have an incentive to hire themselves and re-register as employees to benefit from the subsidy.

Strategic behaviour by employers or the self-employed is not allowed by law. The NSSO actively monitors whether existing employers have started new firms to qualify for the subsidy (Court of Audit, 2021). The self-employed are not allowed to register as employees or, with a few exceptions, to employ close family members in their own company (Hendrickx and Engels, 2017). Belgian labour law requires a subordinate relationship between the employee and the employer and this condition is not met if the employee is also the firm owner or a close relative of the firm owner.

The rules governing small employers and the self-employed are, however, complex and enforcement might be patchy in practice, which might create room to engage in strategic behaviour. This is the reason why we present additional evidence showing that strategic behaviour does not explain the results.

Strategic behaviour by existing employers

We first examine strategic behaviour by existing employers. We start by investigating how three outcomes, which should jump at the cutoff if strategic behaviour by existing employers is important, evolve at the cutoff. These three outcomes, (1) the number of new firms established and hiring their first employee in the same month (discussed earlier); (2) the number of firm closures;[12] and (3) the number of firms that employed employees at the end of month $m-1$, but no longer employed employees at the end of month m, evolve continuously at the cutoff. The RDiT estimates for the latter two outcomes are reported in columns (1) and (2) of *Table 4*. Graphical evidence is presented in Appendix B. These findings do not support the hypothesis that existing employers promptly closed down existing firms and set up new ones after the reform.

Table 4: RDiT estimates: testing for strategic behaviour by employers

	(1)	(2)	(3)
Outcome (log)	Firm closures	Exit employers	Firms hiring their first employee: population fixed to nonemployers on January 1, 2012
RDiT treatment effect	0.087	0.0073	0.31
(Robust) 95% CI	[−0.40, 0.42]	[−0.33, 0.32]	[0.26, 0.36]
Bandwidth	11.91	18.86	Global polynomial approach
Number of observations used:			
Left of the cutoff	6	16	56
Right of the cutoff	7	17	46
Donut RDiT	Yes	Yes	Yes

Note: The three regressions test for strategic behaviour by employers. Regression (1) shows that firm closures evolved continuously at the cutoff. Regression (2) shows that the number of employers in month $m-1$ that no longer employed employees at the end of month m evolved continuously at the cutoff. Regression (3) selects the population of nonemployers on January 1, 2012, and examines whether the number of firms hiring their first employee in this population jumped at the cutoff. For this regression, the data-driven method to select the bandwidth did not converge. For this reason, we estimate the RDiT by fitting a linear polynomial at both sides of the cutoff using the entire time series. All three outcomes are adjusted for seasonality.

We then examine how the SSC exemption affected hiring decisions among nonemployers established many years before the reform. These firms cannot engage in strategic behaviour when hiring their first employee because they were not established as a response to the policy. The observation that the impact of the SSC exemption on the stock of nonemployers selected on January 1, 2012,[13] is comparable to the impact in the baseline specification provides additional support for rejecting strategic firm behaviour. Recall that the population in the baseline specification includes firms that were established and hired their first employee after the reform and may have been established to circumvent the law. The population considered here excludes, by design, firms established after January 1, 2012, thereby ruling out strategic firm behaviour.

Figure 4 shows how the number of firms hiring their first employee among this population evolved over the period 2012-2019. The data-driven approach to selecting the optimal bandwidth does not converge. We, therefore, follow Lee and Lemieux (2010) and fit a linear spline to the entire time series (excluding observations from October 2015 to February 2016) on both sides of the cutoff, including monthly dummies to account for seasonal patterns.

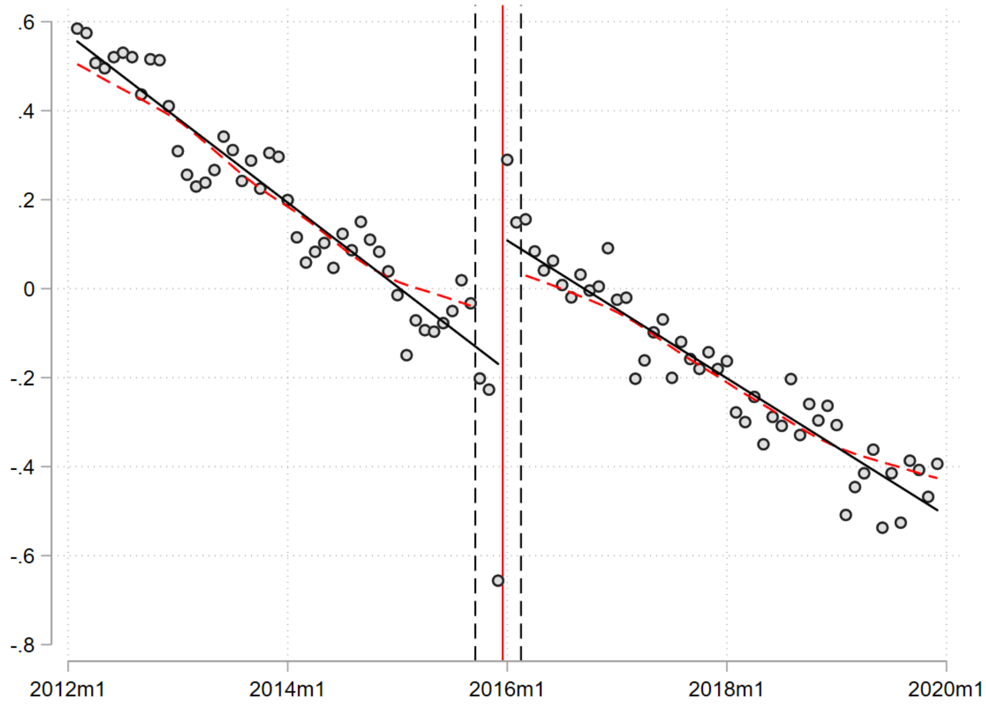
The graphical evidence and the regression (Table 4, regression 3) indicate that the number of firms hiring their first employee increased by 31 log points at the cutoff for this population. This estimate is within the confidence interval of the baseline estimate. It is obvious from Figure 4 that the point estimate is sensitive to the choice of the bandwidth. But the main takeaway message from this figure is not the exact point estimate, but the observation that the effect of

[12] While the CBE accurately records the date at which firms start 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.

[13] We selected the population four years before the reform because this allows us to show and compute the trend in the pre-reform period. This trend is extrapolated within the donut (Figure 4). We could also have selected a population closer to the reform, but this choice would have made it more difficult to determine the pre-reform trend.

the SSC exemption is also sizeable among a subset of nonemployer firms that, by definition, did not engage in strategic behaviour.

Figure 4: Number of firms hiring their first employee among the population of nonemployers fixed at January 1, 2012



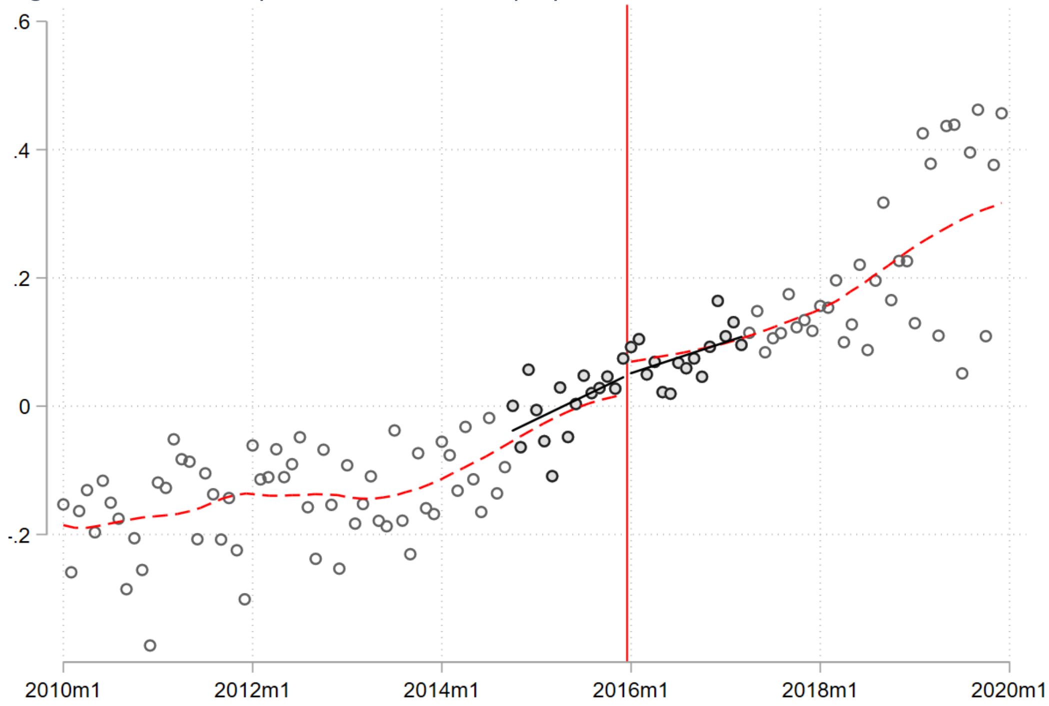
Note: The population consists of all nonemployer firms as of January 1, 2012. Keeping this population fixed, the figure shows the number of firms hiring their first employee per month, adjusted for seasonality. The dashed black lines indicate the donut. The full red line indicates the implementation of the policy. The dashed red line fits a local polynomial regression to the data. All dots are used in the RDiT (Table 4, regression 3).

Strategic behaviour by the self-employed

We now turn to strategic behaviour by the self-employed. To this end, we use aggregate data from the National Institute for the Social Security of the Self-employed (NISSE) on (1) the monthly flow of new self-employed individuals and (2) the quarterly stock of the self-employed. The flow variable allows examining whether becoming self-employed became less attractive after the reform; the stock variable allows testing whether the total number of self-employed individuals *decreased* after the reform, which would be the case if the self-employed re-registered as employees after the reform.

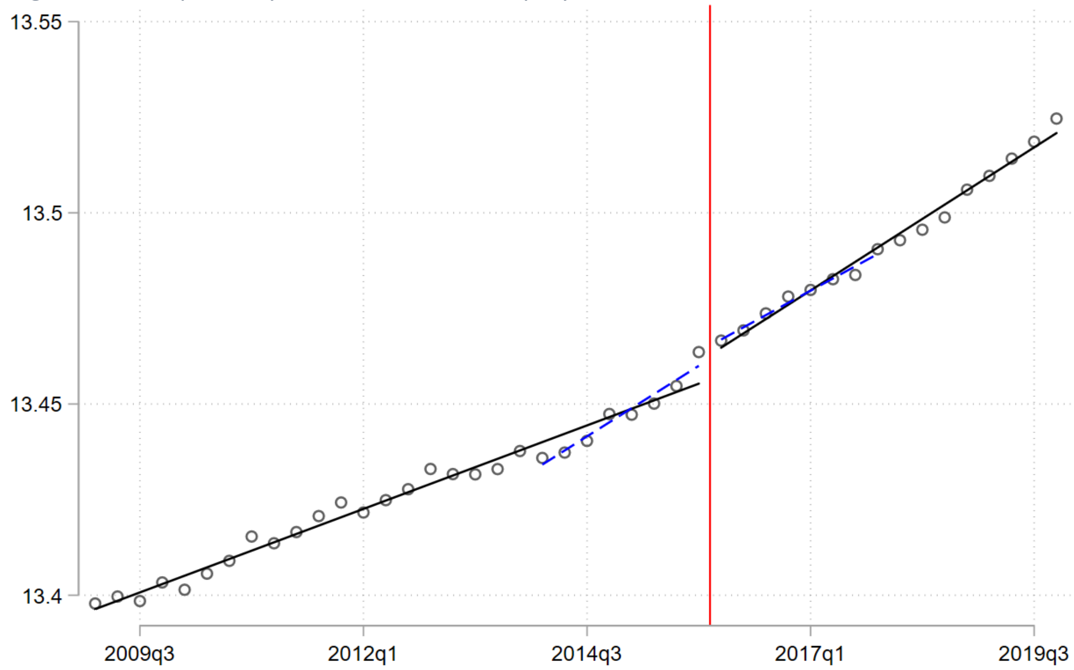
The flow of new self-employed individuals has been increasing continuously since 2010, and this trend continued after the 2016 reform (*Figure 5*). The graphical evidence and the RDiT estimate (*Table 5*, regression 1) show that the flow of new self-employed individuals evolved continuously at the cutoff. This finding does not support the idea that self-employment became less attractive after the reform or that entrepreneurs who started their own businesses after the reform did so (illegally) as employees instead of as self-employed workers.

Figure 5: The monthly flow of new self-employed workers



Note: The figure shows the log of the number of new self-employed individuals registering at the NISSE by month, adjusted for seasonality. On average, 8,600 individuals became self-employed per month in 2015. The red line indicates the 2016 reform. The dashed red line fits a local polynomial regression to the data. Only the solid grey dots are used in the RDiT (Table 5, regression 1).

Figure 6: The quarterly stock of the self-employed



Note: The black, full line shows the log of the number of self-employed individuals by quarter. In 2015Q4, self-employment was the main activity for 703,338 individuals. The full black and blue dashed lines fit a linear spline to the pre-reform and post-reform data using the entire time series and the years 2014 to 2017, respectively.

Table 5: RDiT: testing for strategic behaviour by the self-employed

	(1)	(2)	(3)
Outcome (log)	Flow of new self-employed	Stock of self-employed	
RDiT treatment effect	0.0041	0.0072	0.0035
(Robust) 95% CI	[−0.041, 0.058]	[0.0042, 0.010]	[0.0019, 0.0082]
Bandwidth	14.73	Global polynomial: entire time series	Global polynomial: 2014-2017
Number of observations used:			
Left of the cutoff	15	28	8
Right of the cutoff	15	16	8
Donut RDiT	No	No	No

Note: The three regressions test for strategic behaviour by the self-employed. The flow is defined as the log of the number of new self-employed individuals registering per month at the NISSE, adjusted for seasonality. The stock is defined as the log of the number of self-employed individuals. Regression (2) and (3) estimate the RDiT by fitting a linear trend at both sides of the cutoff using the entire time series and the years 2014 to 2017, respectively, including quarterly dummies to account for seasonality.

The stock of self-employed individuals has also been increasing rapidly since 2019 by about 1% in the pre-reform period (*Figure 6*). The figure suggests that the 2016 reform increased self-employment. The RDiT estimates, which consist in fitting a linear trend to the entire time series at both sides of the cutoff including quarterly dummies to account for seasonal patterns, indicate that self-employment increased significantly at the cutoff (*Table 5*, regressions 2).

We caution against attributing these positive RDiT estimates to the 2016 reform because the point estimates are small and to some extent sensitive to the choice of the bandwidth. For instance, as regression 3 in *Table 5* shows, the point estimate halves but remains significant when restricting the window to the years 2014 to 2017 (16 observations). But, taken together, these findings refute the hypothesis that the 2016 reform *reduced* self-employment, providing compelling evidence that strategic behaviour by the self-employed does not explain our main findings.

5.4. Treatment heterogeneity

In this section, we test three hypotheses formulated in the literature on small businesses and firm dynamics. To this end, we conduct a heterogeneity analysis investigating the impact of the SCC exemption on the probability of hiring among nonemployers by (1) the firms' legal form; (2) the firm's age; and (3) the sector.

We focus on the probability of hiring among existing nonemployers, which is, as we showed earlier, the principal margin of adjustment. The findings remain unaltered when considering the outcome 'number of firms hiring their first employee', which also captures the impact of the policy on the establishment of new (nonemployer) firms (Appendix D). In addition, the findings remain qualitatively similar when applying the local randomization approach rather than the continuity-based RDiT (Appendix D).

The first hypothesis we test is whether the SSC increased hiring more among private limited liability companies compared to sole proprietors. It is well documented that many sole proprietors do not intend to hire (Fairlie and Miranda, 2017; Hurst and Pugsley, 2011) and might therefore be unresponsive to the SSC exemption. By contrast, private limited liability companies are more often established with the intention of expanding the business beyond the owner-manager, and these firms might take the opportunity offered by the SSC exemption to hire their first employee. In addition, in the event of bankruptcy, the owners of private limited liability companies are better protected than sole proprietors, which makes hiring employees less risky for private limited liability companies than for sole proprietors. In the pre-reform period, private limited liability companies are five times more likely to hire than sole proprietors (*Table 6*). Private limited liability companies account for 26% of nonemployer firms but for 56% of the firms hiring their first employee. Furthermore, the effect of the SSC exemption on the probability of hiring is more pronounced for private limited liability companies (+35 log points) than for sole proprietors (+23 log points). These estimates imply that the elasticity of the probability of hiring the first employee with respect to the labour cost is −3.23 [95% CI: −5.39, −1.80] for private limited liability companies and −1.99 [95% CI: −3.34, −0.56] for sole proprietors.

The second hypothesis relates to the firm's age. The influential paper of Haltiwanger et al. (2013) shows that young employers account for a disproportionate share of job creation. This literature typically defines the 'birth' of a firm as the moment it hires its first employee. By contrast, we define the birth of a (non)employer firm as the moment it is established. As reported in *Table 6*, young nonemployers are much more likely to hire their first employee than older firms. Nonemployers established less than a year ago account for 7% of the population of nonemployers but for 33% of the firms hiring their first employee. Nonemployer firms more than ten years old account for 48% of the population of nonemployers and for 25% of the firms hiring their first employee. However, the impact of the SSC exemption on the probability of hiring is not associated with the firm's age.

Table 6: The effect of the SSC exemption on the probability of hiring among nonemployers along different dimensions

	% of nonemployers	% of firms hiring	Prob of hiring	Effect reform (log)	Robust 95% CI
Legal form					
Sole proprietors	58%	25%	0.11%	0.23	[0.07, 0.36]
Private limited liability company	26%	56%	0.55%	0.35	[0.21, 0.53]
Other	16%	18%	0.29%	0.26	[0.07, 0.41]
Age					
<1 year old	7%	33%	1.17%	0.18	[0.02, 0.30]
1-5 years old	25%	27%	0.27%	0.40	[0.30, 0.56]
5-10 years old	20%	15%	0.19%	0.27	[0.06, 0.43]
>10 years old	48%	25%	0.14%	0.31	[0.11, 0.51]
Sectors grouped by share of employer firms					
Low: <28% of firms in the sector are employers	82%	68%	0.21%	0.31	[0.23, 0.41]
High: >28% are employers	18%	32%	0.45%	0.23	[0.11, 0.31]

Note: The first three columns show descriptive statistics for each subpopulation in the pre-reform period: column 1 shows the share of the subpopulation in the population of nonemployers; column 2 gives the share of the subpopulation in the population of firms hiring their first employee in a given month; and column 3 gives the probability of hiring among nonemployers. For instance, sole proprietors account for 58% of nonemployer firms; account for 25% of the firms that hired their first employee in a given month; and have a probability of 0.11% of hiring their first employee in the next month. The last two columns show the point estimate and robust 95% CI of the impact of the SSC exemption on the probability of hiring the first employee using the continuity-based RDiT. The outcome is the log of the probability of hiring in month m among firms without employees on the last day of month $m-1$, adjusted for seasonality.

The third hypothesis is that the effect of the policy will be more pronounced in sectors dominated by nonemployers. Two papers show that some sectors are more appealing to the nonemployers than others, and their arguments apply equally to the decision to become an employer. Hurst and Pugsley (2015) predict that the self-employed have a comparative advantage in sectors with strongly decreasing returns to scale and should select into these sectors. Hiring a single employee in sectors with strongly decreasing returns to scale is also more viable than in sectors with large economies of scale. In a similar vein, Hombert et al. (2020) argue that sector-specific fixed production costs determine the decision to start a firm and show that the effect of a policy that reduced the risk of becoming self-employed is more pronounced in sectors dominated by small firms. Sector-specific costs may also affect the likelihood of hiring the first employee.

To examine to what extent sector characteristics explain the impact of the reform, we computed the median share of employers among all firms in the 78 sectors in the pre-reform period and used this information to classify sectors as having a low or high share of employers. In line with the predictions of the aforementioned models, we observe that the policy increased the probability of hiring more in sectors with a low share of employers (+31 log points) compared to sectors with a high share of employers (+23 log points).

6. Conclusion

This paper leveraged a unique Belgian policy that permanently reduced the labour cost of the first employee to understand to which extent labour costs affect the decision to hire a first employee and become an employer.

New employers responded instantly to the drop in labour costs: the number of firms hiring their first employee jumped by 31% immediately after the reform. This jump was primarily driven by existing nonemployers that hired their first employee as opposed to hiring by new firms established in response to the policy. The effect is more pronounced among private limited liability companies and in sectors dominated by nonemployer firms. Importantly, our findings are not explained by strategic behaviour. At least in the short run, we do not find evidence that employers closed down existing firms and started new ones or that the self-employed registered as employees in order to benefit from the subsidy.

The main contribution of the paper is to quantify labour demand elasticity at the extensive margin. The elasticity of the decision to hire a first worker with respect to the labour cost is estimated at −2.39 with a 95% CI of [−3.45, −1.25]. Comparing elasticities requires caution, but our finding suggests that potential employers are more sensitive to labour costs than existing employers. Put differently, the extensive-margin elasticity of labour demand appears higher than the intensive-margin elasticity.

We can draw an intriguing parallel between labour *demand* elasticities at the intensive and extensive margins, studied in this paper, and labour *supply* elasticities, reported in the literature. Many studies report larger extensive-margin than intensive-margin labour supply elasticities (Cahuc et al., 2014). We find exactly the same pattern for labour demand elasticities. The two reasons offered in the labour supply literature to explain this pattern—indivisible labour supply (Chetty et al., 2013) and optimization frictions due to fixed costs (Chetty, 2012)—may also explain larger extensive than intensive-margin labour demand elasticities. Labour demand is also indivisible in the sense that one either employs or does not employ a worker. If many nonemployers are indifferent between employing or not employing a worker at the prevailing labour cost, a small labour cost reduction will convince many nonemployers to hire their first employee. Furthermore, hiring a first employee comes with large fixed costs, and a slight labour cost reduction may suddenly make employing a first employee profitable, without having large effects on labour demand among existing employers.

While we do find positive effects of the SSC exemption on hiring decisions, 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 could be very different from the long-term effects. There are several reasons to expect a less positive evaluation in the long term. First, in the long term, general equilibrium effects are likely to be important (Cahuc et al., 2019). Spillover effects from increased competition in labour and product markets may reduce job creation among existing small employers who are not eligible for the subsidy. In addition, 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 might distort the firm size distribution towards smaller, less productive firms. This distortion could lead to a misallocation of resources and have a substantial impact on average productivity (Garicano et al., 2016; Guner et al., 2008). 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. We leave a comprehensive evaluation of the policy for future work.

7. References

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Appendix A: Sensitivity of the labour cost reduction and the elasticity of the hiring probability to the discount rate and the exit rate

This appendix documents the sensitivity of the labour cost reduction and the elasticity of the hiring probability to the annual discount rate and the exit rate of employers. These parameters determine the expected value of the temporary hiring subsidies in place before the reform, as well as the labour cost reduction after the reform and the elasticity of the hiring probability with respect to the labour cost. We computed the expected SSC rate in the pre-reform period at the median monthly wage of 2,050 euros. The SSC rate in the post-reform period is always equal to 4%.

Our preferred assumptions (row 1, Table A.1.), reported in the main text, are to set the discount rate at 5% and to use the exit rate of new employers reported by Novella (2021) for the first seven years after hiring while assuming that the exit rate equals zero thereafter. Under these assumptions, the expected SSC rate prior to the reform is 19.5% (column 4), the SSC exemption reduces labour costs by 13% (column 6) and the elasticity of the probability of hiring the first employee with respect to the labour costs is −2.39 (column 7).

Table A.1. assesses the sensitivity of these results to the underlying assumptions. We first evaluate the sensitivity to the choice of the discount rate. Following Huttunen et al. (2013), we set the annual discount rate at 2% (rather than 5%). This raises the expected SSC rate before the reform to 22.4% and lowers the elasticity to −2.06.

We then assess the sensitivity of the results to the assumption that the exit rate of employers over the age of seven is equal to zero. We now set the annual exit rate after seven years at 3%, which corresponds to the average exit rate in Belgium (Bijnens and Konings, 2020). The expected SSC rate in the pre-reform period decreases to 17.4%, and the elasticity increases to −2.72.

Taken together, these sensitivity analyses show that the elasticity of the hiring probability with respect to the labour cost ranges from −2.06 to −2.72. Our preferred estimate of −2.39 is in-between the upper and lower bound of the estimates.

Table A.1.: Sensitivity of the labour cost reduction and the elasticity of the hiring probability to the discount and exit rate

Discount rate	Exit rate	Pre–reform SSC rate	Post–reform SSC rate	Labour cost reduction	Elasticity
Baseline specification					
5%	Zero exit rate after 7 years	19.50%	4.00%	−13.0%	−2.39
Alternative specifications					
2%	Zero exit rate after 7 years	22.40%	4.00%	−15.0%	−2.06
5%	Annual exit rate of 3% after 7 years	17.40%	4.00%	−11.4%	−2.72

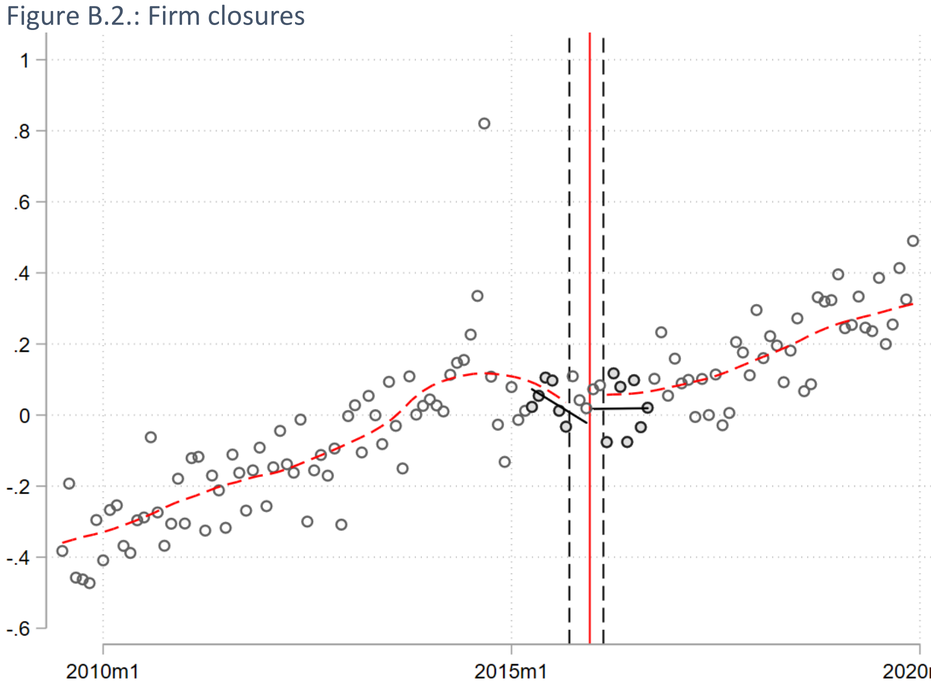
Note: The elasticity is computed by dividing the increase in the number of firms hiring their first employee (+31%) by the labour cost reduction reported in column (6).

Appendix B: Additional results

Figure B.1.: Number of firms that are established and hire their first employee within the same month

Note: The figure shows the log of the number of firms established and hiring their first employee within the same month after adjusting for seasonal patterns. The solid red line indicates the implementation date of the policy (January 1, 2016). The dashed black lines indicate the donut. The dashed red line fits a local polynomial regression to the data. Observations within the donut are excluded when estimating the RDiT. Only the solid grey dots are used in the RDiT (Table 3, regression 2).

Figure B.2.: Firm closures



Note: The figure shows the log of the number of firm closures in a given month, after adjusting for seasonality. The solid red line indicates the implementation date of the policy (January 1, 2016). The dashed black lines indicate the donut. Observations within the donut are excluded when estimating the RDiT. The dashed red line fits a local polynomial regression to the data. Only the solid grey dots are used in the RDiT (Table 4, regression 1).

Figure B.3.: Exit of employers

Note: The figure shows the log of the number of firms that employ employees at the end of month $m-1$, but no longer employ employees at the end of month m, after adjusting for seasonality. The solid red line indicates the implementation date of the policy (January 1, 2016). The dashed black lines indicate the donut. The dashed red line fits a local polynomial regression to the data. Observations within the donut are excluded when estimating the RDiT. Only the solid grey dots are used in the RDiT (Table 4, regression 2).

Appendix C: Placebo tests

Table C.1: The impact of placebo reforms on the number of firms hiring their first employee

Placebo reform on	Jan 2013	Jan 2014	Jan 2015	Jan 2017
RDiT treatment effect	0.0057	−0.019	−0.035	0.060
Robust 95% CI	[0.23, 0.24]	[−0.14, 0.061]	[−0.25, 0.18]	[−0.052, 0.29]
Bandwith	12.40	12.58	13.75	9.46
Number of observation used:				
Left of the cutoff	9	9	11	6
Right of the cutoff	10	10	12	7

Note: This table shows the results of four placebo tests that pretend that the reform took place on January 1, 2013, 2014, 2015, or 2017 rather than on January 1, 2016. With the exception of the choice of the cutoff, the RDiT specifications are exactly the same as in the baseline specification.

Appendix D: Robustness of treatment heterogeneity

Table D.1: Robustness of treatment heterogeneity to (1) a different method and (2) a different outcome

	Local Randomization (outcome: probability of hiring among nonemployers)				**Continuity-based RDiT (outcome: number of firms hiring their first employee)**	
	Prob of hiring in September, 2015	**Effect (level)**	**95% CI**	**Effect (relative terms)**	**Effect (log)**	**95% CI**
Legal form						
Sole proprietors	0.13%	0.03%	[0.0001, 0.0005]	21%	0.21	[0.070, 0.31]
Private limited liability company	0.53%	0.14%	[0.0011, 0.0017]	27%	0.34	[0.21, 0.50]
Other	0.30%	0.07%	[0.0003, 0.0011]	24%	0.23	[0.09, 0.29]
Age						
<1 year old	1.02%	0.12%	[0.0006, 0.0018]	11%	0.16	[−0.04, 0.30]
1-5 years old	0.30%	0.08%	[0.0005, 0.0011]	27%	0.40	[−0.31, 0.54]
5-10 years old	0.21%	0.06%	[0.0003, 0.0009]	27%	0.23	[0.00, 0.39]
> 10 years old	0.21%	0.05%	[0.0003, 0.0007]	24%	0.31	[0.11, 0.49]
Sectors grouped by share of employer firms						
Sectors where less than 21% of firms are employers	0.17%	0.06%	[0.0004, 0.0008]	34%	0.44	[0.28, 0.61]
Sectors where 21% to 35% of the firms are employers	0.38%	0.07%	[0.0005, 0.001]	20%	0.19	[0.08, 0.26]
Sectors where more than 35% of the firms are employers	0.46%	0.12%	[0.0004, 0.002]	26%	0.26	[−0.02, 0.53]

Note: The first four columns examine the impact of the reform on the probability of hiring their first employee among nonemployers using the local randomization method, which consists in comparing the outcome (adjusted for seasonality) in September 2015 to the outcome in March 2016. The last two columns examine the impact of the reform on the number of firms hiring their first employee using the continuity-based RDiT.

Appendix E: Estimating RDiT in levels

Table E.1. replicates *Table 3*, but estimates the RDiT in levels rather than in logs. This choice does not affect the results.

Table E.1: RDiT in levels

	(1)	(2)	(3)	(4)
Outcome	Number of firms hiring their first employee	Number of firms that are established and hire within the same month	Existing nonemployers hiring their first employee	Probability of hiring among nonemployers
RDiT treatment effect	637	2.86	609	0.0008
Robust 95% CI	[446, 824]	[−46.21, 36.68]	[443, 775]	[0.00059, 0.001]
Bandwith	10.51	9.45	11.16	10.81
Number of observations used:				
Left of the cutoff	8	6	8	8
Right of the cutoff	9	7	9	9
Donut RDiT	Yes	Yes	Yes	Yes
Mean outcome pre-reform period	1,976	159	1,817	0.22%
Relative effect	32%	1.79%	33%	36%

Note: The table shows the results of estimating a RDiT in levels for different outcomes using monthly data. To correct for seasonality, we first regress the outcome on monthly dummies using the entire time series, excluding observations within the donut. We then use the residuals of this regression as the outcome in the RDiT. When estimating the RDiT, we exclude observations within the donut, i.e. the period October 2015 – February 2016. 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 and report 95% CI based on bias-corrected robust standard errors as derived by Calonico et al. (2014). At the bottom of the table, we report the mean outcome in the 12 months preceding the announcement of the reform (October 2014 until September 2015), which allows us to compute the relative effects. These relative effects can be compared to the results reported in in the main text. This confirms that estimating the RDiT in levels or in logs does not alter the conclusions.

Appendix F: Estimating the size of the donut

Following Benzarti and Harju (2021), we estimated the size of the donut in two steps.

Step 1: Determine the number of missing hires from October-December 2015

The policy was announced on October 10, 2015, and came into force on January 1st, 2016. After the announcement of the policy, some firms postponed hiring their first employee. As a result, the number of firms hiring in October-December is lower than it would have been if the policy had not been announced.

We estimate the number of missing hires in October-December 2015 by predicting the number of firms that would have hired in October, November and December 2015 if the policy had not been announced. To this end, we implemented the following procedure:

1. To adjust for seasonality, we regressed the number of firms hiring their first employee by month on 12 monthly dummies using the entire time series, excluding observations in the donut. We then obtained the residuals of this regression.
2. To predict the number of firms that would have hired their first employee if the policy had not been announced, we regressed the residuals on the running variable (the month) centred at the cutoff (January 2016) using all observations before the policy was announced (June 2009 to September 2015).
3. We estimated the number of missing hires in October-December 2015 by computing the difference between the predicted number of hires and the observed number of hires in this period.
4. The total number of missing hires in October-December 2015 was estimated at 748 (122 missing hires in October 2015, 187 in November 2015 and 475 in December 2015).

Step 2: Determine the period in the post-reform period that has to be excluded so that the number of missing hires (estimated in step 1) is lower than the number of excess hires

Prospective employers postponed hiring from October-December 2015 to the post-reform period. As a result, there is an excess of hiring immediately after the reform. We determine the number of months that have to be excluded so that the number of missing hires in the pre-reform period (748) is equal to or less than the number of excess hires in the post-reform period by implementing an iterative process, gradually excluding subsequent months in the post-reform period.

1. We start by excluding January 2016.
2. We then estimate the number of excess hires in January 2016. To do so, we run a regression of the seasonality-adjusted number of firms hiring a first employee on the running variable centred at the cutoff using all observations in the post-reform period, except January 2016. We then estimate the number of excess hires in January 2016 by subtracting the number of predicted hires from the number of observed hires in January 2016.
3. We verify whether the number of excess hires is greater than or equal to the number of missing hires in October-December 2015, as estimated in step 1.
4. We gradually exclude subsequent months and repeat steps (1) and (2) until the aforementioned condition is met.

Only excluding January 2016 was insufficient because the number of excess hires in January 2016 was estimated at 739. The number of excess hires was estimated at 825 in January and February 2016 (635 and 190 excess hires in January 2016 and February 2016, respectively).