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Subsidy for the first hires and firm performance

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Abstract

This paper studies how employment subsidies for start-ups shape their performance. We exploit an unexpected policy reform in Belgium that permanently exempted start-ups hiring their first employee from payroll taxes for that employee. Using firm-level administrative data and a regression-discontinuity-in-time design, we find that subsidized post-reform start-ups employed fewer workers and generated lower output, value added, and profits compared to pre-reform start-ups. However, post-reform start-ups were more likely to survive as employers. These effects emerged within the first year after hiring and remained stable over a medium horizon of three years. Our findings indicate a compositional shift: the subsidy primarily induced low-productivity firms to enter the market. As most firms nowadays are nonemployers, our results meaningfully generalize the theoretical implications of standard neoclassical entrepreneurship models (employee–employer margin) and fill the important gap of the nonemployer–employer margin.

Keywords: entrepreneurship; start-up; employment subsidy; tax reduction; labor demand; small firms
JEL codes: H25; J23; J24; J38; L25; L26; M51

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1 Introduction

Business creation and business dynamism are central to employment growth and economic activity (e.g., Acs and Audretsch, 1990, 2010; Acs et al., 2018, 2016; Decker et al., 2014). While the roles of high-growth start-ups in the economy are well established (Dejardin, 2011; Haltiwanger et al., 2016, 2013), and the importance of economic institutions and governmental regulation for entrepreneurship is widely documented (e.g., Dau and Cuervo-Cazurra, 2014; Iwasaki et al., 2022; North, 1990, 1991), much less is known about how entrepreneurs respond to policy-induced changes in *labor costs*.

Two entry margins exist for small firms. First, entrepreneurs start a business as solo self-employed individuals without salaried employees. Second, they may subsequently become employers by hiring their first employees. Fairlie and Miranda (2017) call this transition from nonemployers to new employers a major leap, as most firms operate as *nonemployers* and very few would ever hire their first employee (Bento and Restuccia, 2025; Cockx and Desiere, 2024). New employers are important because only firms that eventually employ workers contribute to job creation.

Two recent studies document that labor costs negatively affect the *quantity* of new employer firms in Belgium (Cockx and Desiere, 2024) and the US (Guo and Wallskog, 2025). However, these studies do not examine whether labor cost reductions also affect the subsequent performance of these new employers (their *quality*). Do these reductions lead to the entry of smaller (worse-performing) or larger (better-performing) new employers?

We fill this gap by evaluating a Belgian reform *zéro coti* ("zero contribution") that substantially reduced labor costs for prospective first-time employers[1]. From January 1, 2016 onward, new employers hiring their first employee were permanently exempt from paying social security contributions (SSC) for that worker. This permanent exemption acted as an employment subsidy and amounted, on average, to a 13% labor cost reduction for the first hire (Cockx and Desiere, 2024). It was intended to support small businesses and encourage nonemployers to become employers, thereby creating jobs.

On the *quantity* side, the policy generated an immediate 24% increase in the inflow of new employers during the year following its implementation (Figure 1), underscoring its effectiveness in stimulating employer creation. However, the reform was designed almost exclusively with entry in mind and remained silent on new firms' subsequent performance. Whether these post-reform newly created employers are systematically smaller, less productive, or less growth-oriented than earlier cohorts is a first-order question for evaluating the broader consequences of such policies.

By the logic of Lucas's (1978) entrepreneurial decision model, lowering labor costs has two implications. First, for firms already determined to employ workers (holding the composition of new employers fixed), lower labor costs directly increase the profitability of hiring, implying positive employment responses along the intensive margin, consistent with the standard law of labor demand (Cahuc et al., 2014; Hamermesh, 1993). Second, lower labor costs also relax the threshold for becoming an employer and induce additional firms with relatively lower productivity to hire their first employee (thereby changing the composition). This mechanism is closely related to the literature on entry cost reduction, which documents that policies lowering barriers increase business creation, often through the entry of firms operating at the margin.[2]

Under standard assumptions[3] and terminology (Branstetter et al., 2014; Hombert et al., 2020), such policies conceptually separate new employer firms at this margin into two groups: *marginal* firms, which are induced by the labor cost reduction to hire their first employee, and *infra-*

[1] We use "first-time employers", "new employers", and "start-ups" interchangeably. However, in a strict sense, the term "start-ups" also includes solo-entrepreneurs without employees.

[2] These barriers include regulatory burdens (Amici et al., 2016; Branstetter et al., 2014), financial constraints (Bertrand et al., 2007; Binks and Ennew, 1996; De Mel et al., 2008; Kerr and Nanda, 2009), or business risks (Camarero Garcia and Murmann, 2025; Gottlieb et al., 2022; Hombert et al., 2020).

[3] Assumptions include that firm output and firm profit increase in (1) labor productivity and (2) level of employment.

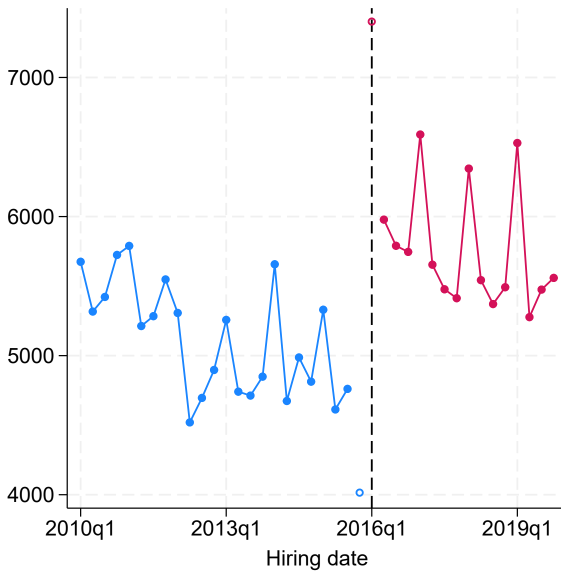


Figure 1. The quarterly flow of new employers

Notes: The figure shows the inflow of new employers eligible for the payroll tax exemption in each quarter from 2010 to 2019. The reform was announced in October 2015 and came into force on January 1, 2016, as indicated by the vertical dashed line. The two quarterly cohorts just before and after the implementation of the policy, plotted with hollow circles, are not used to calculate the increase to avoid bias due to policy anticipation; see Section 2 for details.

marginal firms, which always hire regardless of the policy. Inframarginal firms are expected to grow larger—measured by employment, sales, and profits—with the help of subsidies. Marginal firms, entering solely because of the subsidy, are less productive and can only achieve smaller firm sizes on average than inframarginal firms. The size of this marginal–inframarginal gap depends on the underlying productivity distribution and the importance of entry barriers that prevent them from hiring.

As a result, how this employment subsidy shapes the average firm performance among subsidized new employers (both marginals and inframarginals), compared to the unsubsidized ones (only inframarginals), depends on two counteracting forces. A more generous and permanent reduction in labor costs improves the performance of inframarginal firms (supportive effect), but it also substantially lowers entry barriers for marginal firms (compositional effect). The first effect is positive and raises the average firm performance; the second is negative as it adds low-performing marginal firms to the pool of new employers. If the compositional effect dominates the supportive effect, average firm performance may decline even though labor costs fall for all firms. However, *theory* alone falls short of predicting the net effect of these two forces.

Thus, we *empirically* examine subsidized new employers' performance. We employ quarterly firm-level administrative data and regression-discontinuity-in-time (RDiT) designs where start-ups' hiring dates determine their subsidy eligibility, and model the evolution of firm performance by entry cohort. We adopt the local randomization framework (Cattaneo and Titiunik, 2022), and compare firm performance (employment, production, and labor productivity) between treated and untreated cohorts of start-ups, within a short time window of two years around the reform date. The untreated cohort hired their first employees between 2014Q4 and 2015Q3 (henceforth "Cohort 2015"), i.e., the year before the reform's announcement; the treated cohort entered between 2016Q2 and 2017Q1 (henceforth "Cohort 2016"), i.e., the year after the reform's implementation.

Our identification rests on the assumption that the treatment effect on firm performance identified by RDiT is independent of the hiring date, which is plausible for two reasons. First, the economy evolved steadily around 2016 and no other major policies targeting new employer firms took place simultaneously. Second, several placebo and validation tests directly support the notion that, absent the reform, the performance of new employers hiring in 2016 would have resembled the performance of those in 2015.

Our empirical findings reveal that lower labor costs primarily induced entry by smaller and lower-performing start-ups rather than by high-performing scale-ups (in terms of both employment and sales), consistent with Belgium's declining business dynamism (Bijnens and Konings, 2020). On average, compared to Cohort 2015, Cohort 2016 firms hired 4.2% fewer employees, were 2.4 percentage points (pps) less likely to hire more than one employee, yet were 2.8 pps more likely to survive as employers one year after hiring. Lower employment levels corresponded with a decline in average firm production by approximately 4–6%.

Subsidized start-ups were small for two reasons. First, since the exemption was only for the first employee regardless of the employment type, it created an incentive for new employers to hire one full-time employee rather than several part-time ones. More importantly, the subsidy triggered a compositional shift leading to the entry of low-quality *marginal* employers. We find that the average labor productivity of post-reform new employers (both marginal and inframarginal ones) is 10% lower than that of pre-reform new employers (only inframarginal). We estimate that marginal start-ups are only half as productive as inframarginal ones and reached 80% of the average size of inframarginal firms. Medium-run analyses over three years reinforce this compositional interpretation: estimated gaps become slightly smaller in magnitudes but the patterns do not revert. Thus, differences in firm performance between Cohorts 2016 and 2015 materialized soon after hiring, suggesting that subsidized marginal firms could not catch up to inframarginal ones even with sustained cost reductions.

We primarily contribute to the literature on entry cost reduction and start-up performance by shifting the focus from business creation per se to the decision to become an employer. Mainstream entrepreneurial decision models following Lucas (1978) and Hopenhayn (1992) emphasize heterogeneity in managerial ability and fixed costs of operation, and are widely used to study firm entry and employment dynamics. In these frameworks, entrepreneurs may remain nonemployers either because entry costs make hiring unprofitable (Branstetter et al., 2014; Hombert et al., 2020) or because their productivity is insufficient to sustain salaried employment.[4]

However, this literature typically abstracts from two distinctions that are central in our setting. First, while some papers (Albanese and Bronzini, 2025; Branstetter et al., 2014) remain close to the notion that firm entry support leads to increased entry of low-quality firms—a quantity–quality tradeoff—they do not distinguish between entry into self-employment and the subsequent transition into employer status. This distinction matters because hiring the first employee is a significant milestone that signals business success (Bento and Restuccia, 2025; Fairlie and Miranda, 2017). Second, related policies are usually interpreted as reducing *one-off* entry barriers—through administrative deregulation (Amici et al., 2016; Branstetter et al., 2014), financial access (Bertrand et al., 2007; De Mel et al., 2008; Kerr and Nanda, 2009), or reductions in income risk and career costs (Gottlieb et al., 2022; Hombert et al., 2020). In contrast, the policy we study *permanently* reduces labor costs, making it substantially more generous and allowing us to study not only entry into employer status but also subsequent firm performance under sustained support.

Second, our paper contributes to the literature evaluating policies supporting small firms and entrepreneurship. A large body of work studies the effectiveness of subsidies in increasing employment, investment, and profits, with a primary focus on industrial policies supporting capital accumulation, investment, or R&D. This literature generally finds that such subsidies are more effective for smaller firms (Autor et al., 2022; Cerqua and Pellegrini, 2014; Criscuolo et al., 2019; Decramer and Vanormelingen, 2016; House and Shapiro, 2008). A relatively smaller body studies the effect of employment subsidies on firm performance, including wage subsidies (Groh et al.,

[4] An alternative perspective highlights non-pecuniary motives for remaining solo self-employed, such as preferences for independence (Hurst and Pugsley, 2011). Hurst and Pugsley (2015) show that subsidizing such entrepreneurs may reduce welfare.

2016), on-the-job training subsidies (Konings and Putseys, 2025), and most related to us, two field experiments helping small firms to hire labor in developing countries (Hardy and McCasland, 2023; de Mel et al., 2019). de Mel et al. (2019) show that temporary wage subsidies raised employment only during the subsidy period, with effects fading thereafter; Hardy and McCasland (2023) document that facilitating hiring (without subsidies) can improve firm performance by overcoming hiring frictions. However, whether these insights extend to permanent subsidies implemented through real-world policy in developed economies remains an open question. The Belgian *zéro coti* reform is particularly informative in this respect, as it represents a large and permanent reduction in labor costs for first-time employers, especially relative to typical labor cost changes studied in the US (Guo and Wallskog, 2025).

In this literature, few papers consider firm composition as a response to employment subsidies. Our findings shift the focus from causal effects for *existing* firms (thus holding firm types fixed) to effects on the composition of *nascent* firms. Understanding the composition of start-ups is crucial at early stages of business creation, where entrepreneurial orientations are shaped by owners' growth motivations—such as opportunity- versus necessity-driven entrepreneurship (Baptista et al., 2014; Block et al., 2015; Fairlie and Fossen, 2019; van der Zwan et al., 2016) or growth- versus survival-oriented types (Camarero Garcia and Murmann, 2025; Delmar and Wiklund, 2008). Many papers study how outside options (such as unemployment benefits or health insurance) or self-employment subsidies shape entry into entrepreneurship (Bilan and Apostoaie, 2023; Caliendo et al., 2015; Camarero Garcia and Murmann, 2025; Fossen et al., 2025; Hombert et al., 2020) but few discuss the role of labor costs, a central input for small firms. Our results suggest that firms induced to become employers in response to labor cost reductions tend to be survival-oriented, with limited growth potential.

The paper is structured as follows. Section discusses the policy. Section 3 discusses the datasets and descriptive statistics. Section 4 outlines the identification strategy. Section 5 examines the composition effects of the reform on shaping start-ups' average performance. Section 6 concludes.

2 The policy

This section provides an overview of the Belgian payroll tax system and the payroll tax reform for the first employee, comparing the effective payroll tax rate paid by start-ups before and after the reform.

In Belgium, payroll taxes are equivalent to social security contributions (SSCs)[5] and are levied on gross wages to fund social security programs, including health care, pensions, and unemployment benefits. The *nominal* payroll tax rate currently amounts to 25% of wages. However, after various SSC reductions targeted at specific workers, the *effective* payroll tax rate is substantially lowered.

Therein, *les premiers engagements* ("the first recruitment") is one SSC reduction (employment subsidy) targeting the first employee of new employers. The reduction applies to the first *physical* worker hired rather than the first full-time equivalent (FTE) worker, and restricts eligibility to firms that did not employ any workers in the preceding four quarters. Self-employed individuals and owner-managers are not employees by definition and not eligible for these reductions. Because SSC are roughly proportional to earnings, this feature incentivizes firms to hire a single full-time worker rather than multiple part-time workers.

Old measure. *les premiers engagements* was introduced in 2004 and became more generous with subsequent reforms in 2012, 2014, and 2015. In 2015, the SSC reduction was available for a maximum duration of 13 quarters: start-ups could claim a quarterly SSC reduction of up to €1,550 for a full-time worker during the first five quarters following recruitment. This amount

[5] Payroll taxes may include other taxes not defined within social security in other countries (e.g., Medicare taxes in the US, housing funds in China). We always refer to employers' contributions to SSC; employees' contributions were not affected by the reform.

decreased to €1,050 for the next four quarters, and to €450 for the final four quarters. Over the 13-quarter period, the total SSC reduction could thus amount to €13,750.

New measure. We focus on one special reform within this broader fiscal package, which was also the most significant reform of *les premiers engagements*. On October 10, 2015, the government unexpectedly announced that start-ups hiring their first employee from January 1, 2016 onward would be permanently exempt from payroll taxes for that employee, a zero-contribution (*zéro coti*) rule for the first hire. The exemption is tied to the firm rather than to the worker, allowing firms to retain it as long as they continue to employ someone. Workers' social security entitlements remain unchanged. Policy documents describe this reform as a targeted instrument to reduce structural labor costs for new employers and to encourage self-employed individuals (nonemployers) to take the step toward becoming employers and creating jobs.[6]

The 2016 reform significantly increased the generosity of the subsidy in two key ways: (1) the payroll tax exemption became permanent, rather than being limited to a 13-quarter period; (2) the subsidy was no longer capped. Figure 2 shows that Cohort 2016—which consists of firms hiring their first employee from 2016Q2 to 2017Q1—consistently paid a payroll tax rate of less than 5% across all quarters following the first hire. This tax rate was not exactly zero due to imperfect take-up of the subsidy.

As an immediate consequence of this reform, more new employer firms entered the labor market by about 24% (see Figure 1 in the introduction). We calculate this increase as the difference between the one-year window after the implementation (6,026 firms per quarter over 2016Q2–2017Q1) and the one-year window before the announcement (4,879 firms per quarter over 2014Q4–2015Q3). Throughout our analyses, we exclude two quarterly cohorts (Cohorts 2015Q4 and 2016Q1) because some firms that would have hired in 2015Q4 intentionally postponed until 2016Q1 to benefit from the reform (Cockx and Desiere, 2024). We then group every four adjacent quarterly cohorts into a redefined annual cohort such that Cohort 2015 comprises the quarterly cohorts 2014Q4–2015Q3 and Cohort 2016 comprises cohorts 2016Q2–2017Q1.[7] All references to annual cohorts correspond to this redefined structure and align with the empirical strategy in Section 4.

Transitional measure. However, this 2016 reform seemed unfair to start-ups that just hired their employees shortly before. To address fairness concerns, the government also announced *transitional measures* simultaneously as a second part of this reform for new employers who had hired their first employees in 2015 (Cohorts 2015Q1–Q4). This transitional measure elevated the subsidy generosity for these cohorts, by bridging the old and the new measures. Starting from 2016Q1, these firms were also granted the uncapped payroll tax exemption (same as the new measure), but only within the remaining duration of their old measure (of at most 12 quarters). We therefore plot two average payroll tax rates for Cohort 2015, their anticipated tax rate according to the old measure and their realized tax rate according to the transitional measure, in Figure 2.[8]

Since the transitional measure was only announced in October 2015, Cohort 2015 made their hiring decisions based on the anticipated payroll tax rate, and their composition was not affected by the reform. In contrast, Cohort 2016's composition was directly influenced by the reform. At the new employer margin, Cohort 2015 consists of only inframarginal start-ups while Cohort 2016 consists of both inframarginal and marginal ones.

Figure 2 shows that the realized payroll tax rate for Cohort 2015 aligned with the anticipated rate in the first quarters after hiring, but diverged when the 2015 Cohort became eligible for the transitional measures. However, during the 12 quarters of the temporary full exemption, the realized payroll tax rate for Cohort 2015 remained 2 to 5 pps higher than that of Cohort 2016. This discrepancy arises because our definition of Cohort 2015 includes firms from 2014Q4 to 2015Q3,

[6] See a federal press release (https://news.belgium.be/en/node/18790/pdf) for the policy intention.

[7] "Cohort" time refers to the time of hiring a start-up's first employee.

[8] The anticipated payroll tax rate for Cohort 2015 is inferred from the tax rate paid by Cohort 2014, which did not benefit from the transitional measure.

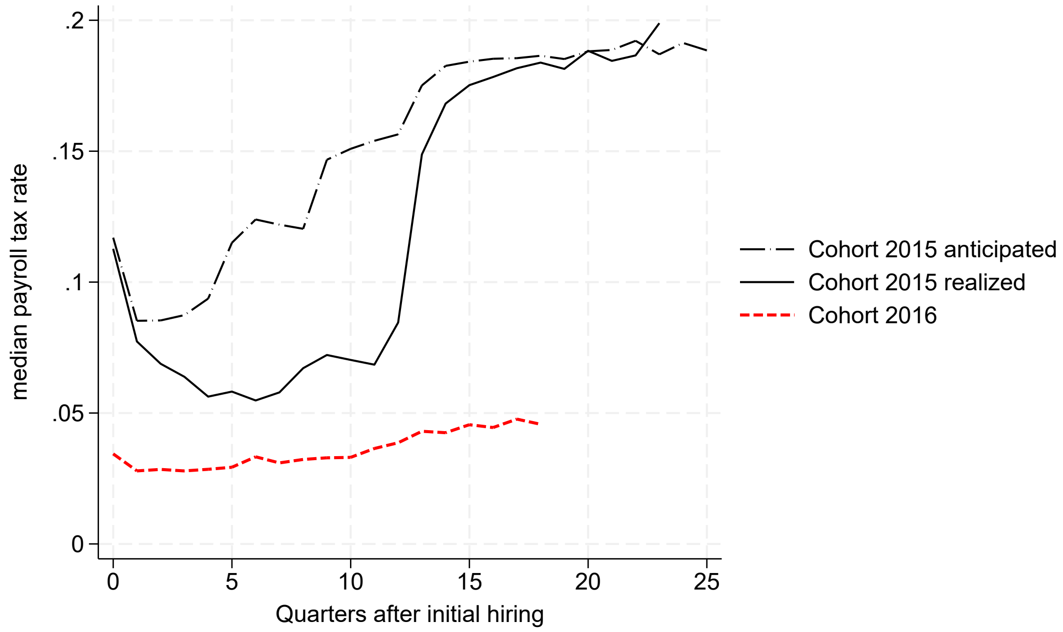


Figure 2. Payroll tax rate for the first employee by annual cohort of new employers

Notes: Data source: National Social Security Office. The effective payroll tax is computed as the payroll tax divided by gross wage for new employers that employ exactly one employee. The horizontal axis represents the number of quarters elapsed since the hiring of the first employees. Cohort 2015 consists of Cohorts 2014Q4-2015Q3; Cohort 2016 consists of Cohorts 2016Q2-2017Q1. The *anticipated* tax rate is the tax rate Cohort 2015 expected to pay when hiring their first employee; the *realized* tax rate is the rate they eventually paid, given the unexpected temporary exemption granted to these firms from 2016Q1 onwards.

and firms in Cohort 2014Q4 were not eligible for the transitional exemption.[9] Nevertheless, the gap between Cohort 2016 and other lines indicating the anticipated and realized tax rates for Cohort 2015 highlights a substantial, immediate, and long-lasting effect of the 2016 reform on the payroll tax rate.

3 Data and descriptives

Our analysis relies on firm-level data drawn from three primary sources: the Crossroads Bank for Enterprises (CBE), the National Social Security Office (NSSO), and the Value-Added Tax (VAT) database. The dataset has a quarterly frequency and spans the period from 2010Q1 to 2019Q4, a stable period between the financial crisis and the COVID-19 pandemic. These datasets contain information about basic firm characteristics such as the sector and the firm's age; the number of employees on the last day of the quarter; the wage bill and labor costs over the quarter; and VAT submissions containing turnover, investment, and intermediate inputs. These three VAT variables are only observed for firms paying VAT, and approximately 90% of the start-ups in our sample pay VAT. We set these outcomes for the remaining 10% of the firms to zero as firms that do not pay VAT are typically very small. All monetary variables are deflated to 2018 euros using the Belgian quarterly Consumer Price Index and winsorized at the 2.5$^{\text{th}}$ and the 97.5$^{\text{th}}$ percentiles to mitigate the impact of outliers.

We define our sample of start-ups as firms that hired their first employee in a given year and

[9] We provide alternative figures in Appendix A.1 showing the realized payroll tax rates for Cohorts 2015Q2 and 2016Q2. These figures indicate that the payroll tax rate for firms that hired their first employee in the first three quarters of 2015 almost coincides with the tax rate for firms that hired in 2016 during the period that both cohorts were fully exempt from payroll taxes.

had not employed any employees in the preceding four quarters. This latter condition ensures that they are eligible for the payroll tax exemption. Furthermore, we impose two restrictions to exclude *spurious entrants* from our dataset, which appear to be new start-ups but are actually reclassified due to corporate events like mergers or acquisitions. First, following Geurts and Van Biesebroeck (2016)[10] and Bijnens and Konings (2020), we exclude start-ups with more than 10 employees at entry. Second, we exclude firms exhibiting exceptionally high employment growth rates (exceeding the 99$^{\text{th}}$ percentile). These restrictions removed similar proportions of firms from each cohort, amounting to 3.9% and 3.3% of firms in Cohorts 2015 and 2016, respectively.

Our baseline analysis contrasts start-ups that hired their first employees between 2014Q4 and 2015Q3 (Cohort 2015, N = 19,517) with those that hired between 2016Q2 and 2017Q1 (Cohort 2016, N = 24,104). Descriptive statistics for these cohorts are presented in the first two columns of Table 1. Column 3 provides statistics for all available cohorts from 2010Q1 to 2019Q4, demonstrating that the two key cohorts used for the comparison are representative in terms of their composition.

Table 1. Characteristics of start-ups

	Cohort 2015	Cohort 2016	All cohorts
Age in quarters	22.9	24.2	21.8
	(23.1)	(24.8)	(22.9)
Age			
$<$ 1 year	31.6%	30.9%	32.9%
1–3 years	14.7%	15.4%	15.7%
3–5 years	10.7%	9.8%	10.0%
5–10 years	16.7%	16.6%	15.6%
$\geq$ 10 years	26.4%	27.2%	25.8%
Legal form			
Private limited liability company	55.8%	57.2%	55.7%
Sole proprietors	27.4%	25.3%	26.3%
Partnerships	9.6%	10.4%	9.9%
Others	7.2%	7.1%	8.1%
VAT registration	88.7%	88.9%	88.4%
Sector			
Food and beverage service activities	18.7%	17.3%	17.5%
Retail trade, except of motor vehicles and motorcycles	15.2%	13.9%	14.4%
Specialised construction activities	11.9%	11.7%	12.5%
Wholesale trade, except of motor vehicles and motorcycles	5.4%	5.4%	5.5%
Activities of head offices; management consultancy activities	4.7%	5.9%	4.9%
Other sectors	44.0%	45.8%	45.2%
Region			
Flanders	53.8%	52.0%	53.4%
Wallonia	31.6%	33.0%	32.1%
Brussels-Capital	14.6%	15.0%	14.5%
N	19,517	24,104	204,026

Notes: Firm ages, legal forms, sectors, and regions are recorded at the time of hiring, while VAT registration statuses are reported one year after hiring. Standard deviations are provided in parentheses for firm age (measured in quarters). Age intervals in years are left-inclusive and right-exclusive. Partnerships include partnerships limited by shares, public limited companies, general partnerships, and ordinary limited partnerships.

New employers have a wide age dispersion. On average, they are five years old, yet some firms are pretty old: one-fourth of the new employers had been in operation for at least ten years before hiring their first employee. While the majority of non-employer firms were sole proprietors, they were significantly less likely to hire a first employee compared to limited liability companies. Consequently, across all cohorts, more than half of the start-ups were private limited liability companies, while only a quarter were sole proprietors.

[10] Geurts and Van Biesebroeck (2016) use advanced corporate record–linking methods and estimate that 57% of the start-ups in Belgium with more than 10 employees at entry are spurious entrants.

Firm entry concentrates in a small number of low-technology service sectors. We list the five most common NACE 2-digit sectors among new employer firms. Food and beverage services (e.g., bars and restaurants), retail (e.g., small shops), and construction together account for about 45% of all new employers. Sectoral distributions are stable across cohorts, suggesting no meaningful sectoral reallocation around the reform.[11]

4 Empirical strategy

4.1 Baseline identification

We employ the local randomization approach (Cattaneo and Titiunik, 2022) within a regression-discontinuity-in-time (RDiT) framework (Anderson, 2014; Godard et al., 2024; Hausman and Rapson, 2018) to evaluate the effect of the tax exemption on firm performance. This approach leverages the temporal discontinuity created by the reform: only firms that hired their first employee on or after January 1, 2016, were eligible for the tax exemption.

Our specification contrasts annual cohorts of start-ups that hired their first employee shortly before the policy announcement (untreated cohort) with those that did so shortly after its implementation (treated cohort). The untreated cohort consists of firms that hired their first employee in the rolling year prior to October 2015 and represents the counterfactual performance of inframarginal firms without the tax exemption, while the treated cohort includes firms that hired in the rolling year after 2016Q1, thereby excluding firms that delayed hiring in anticipation of the reform.

Formally, we estimate

$$Y_i = \beta_{2016}\mathbb{1}[T_i = 2016] + \mu_{2015} + \sum_{s=2010}^{2014} \beta_s \mathbb{1}[T_i = s] + \epsilon_i, \tag{1}$$

where i indexes new employers, and T_i indicates the year in which firm i hired its first employee. Y_i is the outcome of interest for firm i, at four points in time relative to the hiring date T_i: at entry (hiring), and one, two, and three years post entry. When considering outcomes for the years following entry, we assign a value of zero to firms no longer observed in the data. Some firms remain operational but cease to employ workers after one year, resulting in zero employees after that period. Others exit the market entirely, in which case both turnover and profits are set to zero. By doing so, we present unconditional estimates and avoid issues of sample selection bias.

The term μ_{2015} represents the average outcome for Cohort 2015 of new employers. The parameter of interest, β_{2016}, captures the difference in the average outcomes between Cohorts 2015 and 2016. β_s's captures differences between Cohort 2015 and the other pre-reform cohorts. To control for seasonality, cohorts are grouped by year. Standard errors are clustered by NACE 2-digit sectors (85 clusters). Appendix B.1 provides a derivation showing how this specification relates to the regression discontinuity treatment parameter.

In all regressions, start-ups hiring their first employees in 2015Q4 and 2016Q1 are excluded, following the "donut hole" approach described by Barreca et al. (2011) and Cattaneo and Titiunik (2022).[12] The reform, unexpectedly announced in 2015Q4, applied only to start-ups hiring their first employee after January 1, 2016. Consequently, some prospective employers postponed hiring from 2015Q4 to 2016Q1 to qualify for the tax exemption. This behavior could bias the average treatment effect, as the decision to postpone may correlate with firm performance or quality. To address this issue, we implement the donut-hole strategy and redefine annual cohorts by grouping

[11] We have conducted heterogeneity analyses by sector, region, and legal form. Within each group, we document qualitatively similar patterns to those observed in the entire population. The results are available upon request. However, we refrain from discussing differences between subgroups, as the decision to hire a first employee in a particular sector or region or by firms with a specific legal form is endogenous to the reform.

[12] Cockx and Desiere (2024) show that the "excess" number of start-ups in 2016Q1 surpasses the number of "missing" start-ups in 2015Q4, suggesting that excluding these two cohorts—Cohorts 2015Q4 and 2016Q1—is sufficient.

every four adjacent quarterly cohorts. Cohort 2015 includes start-ups hiring their first employee in quarters 2014Q4–2015Q3, while Cohort 2016 consists of those hiring in 2016Q2–2017Q1. References to annual cohorts always pertain to these redefined groups.

Ideally, we would compare start-ups close to the cutoff. Given the quarterly frequency of our data, this would involve contrasting Cohort 2015Q3 (the quarter just before the donut) with Cohort 2016Q2 (the quarter just after the donut). While we perform this comparison as a robustness check, our main specification uses annual cohorts to deal with seasonality.

Seasonality arises for two reasons. First, outcomes fluctuate across quarters within a year: holiday pay increases wage bills in Q2 and Q4, and firm sales often peak at year ends. Second, the composition of start-ups is also seasonal. As shown in Figure 1, most start-ups are established in the first quarter of the year, and these firms are likely different from those established in later quarters. By using annual cohorts, we avoid potential biases stemming from these seasonal patterns.

4.2 Identifying assumption

The local randomization framework relies on the identifying assumption that, within a narrow window around the cutoff, potential outcomes are independent of the running variable (Cattaneo and Titiunik, 2022). In our setting, this assumption implies that, in the absence of the payroll tax exemption, the average firm performance in Cohort 2016 would have been identical to that of Cohort 2015, and thus the effect of the payroll tax exemption can be estimated as the difference between the average outcomes within this window. Our annual comparison implicitly defines this window as one year on either side of the cutoff. We argue below that this choice delivers reliable treatment effect estimates.

Macroeconomic and policy environment A necessary condition for this identifying assumption is either (1) that start-ups hiring their first employee during 2014Q4–2015Q3 and 2016Q2–2017Q1 operate under broadly comparable macroeconomic conditions at the time of entry and in subsequent years, or (2) that firm-level outcomes of start-ups are not substantially affected by macroeconomic fluctuations. Regarding the first condition, the economy did not experience a major economic shock from 2015 to 2019. As shown in Figure A.2, quarterly GDP growth during the analysis period remained relatively stable. After a sharp decline from 2010 to the end of 2013, economic growth gradually recovered and stabilized between 2014 and 2019, with annualized GDP growth consistently around 1.5%. Additionally, we are not aware of any policy reforms during this period that differentially affected new employers within these two cohorts. As for the second condition, we provide further support through placebo analyses *in time* and *in groups* described below showing that firm-level outcomes of these small businesses are less sensitive to macroeconomic fluctuations than to this reform specifically.

Placebos and validations We further assess the credibility of the identifying assumption using three complementary validation exercises. First, we conduct placebo tests in time by applying the same RDiT design to pre-reform cohorts, treating earlier years as if the reform had occurred then. This allows us to verify that differences in firm outcomes across adjacent cohorts prior to 2016 are small relative to the estimated reform effects. Second, we implement placebo tests in groups by applying our identification strategy to the population of growing employers whose number of employees increased from one (over the past one year) to above one (in the current quarter). They were ineligible for the subsidy but experienced similar employment expansions. This test examines whether macroeconomic conditions around 2016 generated spurious effects unrelated to the policy. Third, we exploit quarterly variations in hiring dates and use the quarterly time as the running variable (with seasonality adjusted) as a robustness check on our baseline estimation exploiting annual variations. The quarterly estimates remain close to the baseline annual ones, which supports our local randomization assumption. Formal formulations can be found in Appendix B.2; results are reported in Section 5.7.

5 Effects on average firm performance

This section shows that the labor cost reduction induced by the reform changed the *composition* of new employer firms by encouraging less productive entrepreneurs to enter. As a result, the average *performance* of post-reform new employers was lower than that of pre-reform ones. Sections 5.1 and 5.2 document short-run effects on firm size, while Section 5.3 examines effects on firm production. Section 5.4 investigates changes in their productivity.

For each outcome, we examine both average and distributional effects. Average effects are estimated using outcomes measured in levels, while distributional effects rely on binary indicators capturing whether firms exceed certain thresholds. This approach mitigates sensitivity to outliers in these highly skewed firm outcomes (Cabral and Mata, 2003) and accounts for heterogeneous growth potential among start-ups (Sterk et al., 2021).

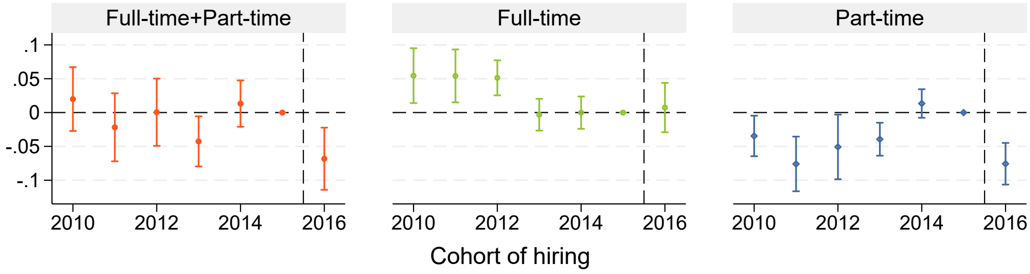
5.1 Employment

Employment at entry Figure 3a presents the evolution of employment at entry across annual cohorts, showing the number of employees and its breakdown into full-time and part-time workers. The left panel of Figure 3a shows that Cohort 2016 employed 0.068 fewer employees (4.2% lower in relative terms) on average at entry than Cohort 2015, which hired 1.61 employees on average. The second and third panels highlight that this effect was entirely driven by a reduction in hiring part-time employees (with an estimate of -0.076), while no significant effect was observed on hiring full-time employees (estimate close to 0). These findings show that the payroll tax exemption incentivized start-ups to hire a single full-time employee rather than several part-time employees, as this strategy maximizes the benefits of the exemption.

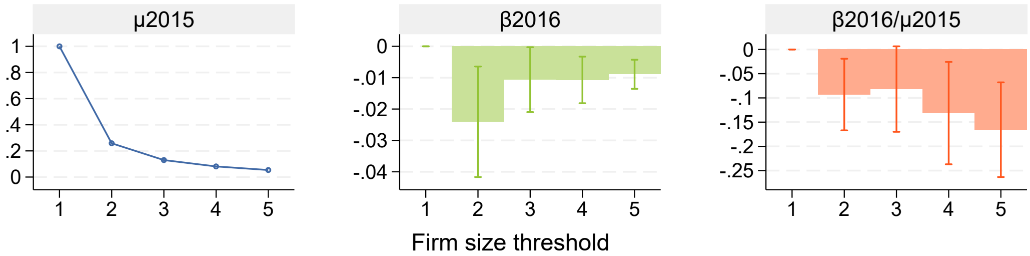
Moving beyond the average compositional effect, we investigate how Cohort 2016's probability of reaching specific firm sizes at entry deviated from Cohort 2015. Figure 3b, left panel, displays the firm size distribution at entry for Cohort 2015, where each dot represents $\hat{\mu}_{2015}$ from regression (1) for a different outcome, i.e., the probabilities of hiring at least one to five employees. Three-quarters of start-ups hired only a single employee at entry, and just 5.4% hired at least five employees. The middle panel shows $\hat{\beta}_{2016}$ and its 95% confidence interval for each outcome, where the point estimate reflects the compositional change due to the reform; the right panel divides the middle panel estimates by the left panel values, reporting the effect in relative terms. All point estimates are negative, and most are statistically significant. For example, the probability of employing at least two (five) employees was 2.4 (0.89) percentage points (pp) lower among post-reform start-ups; the relative drops are 9.3% and 16.6%.

Employment one year post entry Figure 4a presents the same outcomes measured one year after entry. One year after hiring their first employee, Cohort 2016 employed 0.025 fewer employees than Cohort 2015, a difference that is smaller than at entry and not statistically significant. In contrast, Cohort 2016's number of part-time employees remained significantly lower by -0.041.

A natural question is whether the overall difference in employment one year after entry reflects changes in employer survival or differences in firm size conditional on survival. Figure 4b examines how the probability of reaching specific employment thresholds one year after entry differed between Cohort 2016 and Cohort 2015. We observe a significantly higher probability of employer survival (still employing at least one employee; +2.8 pps in absolute terms, or 4.1% in relative terms), along with a lower probability of having two employees or more. The probability of employing at least five employees declined from 7.0% for Cohort 2015 to 6.1% for Cohort 2016 (12.5% lower in relative terms). Conditional on survival, Cohort 2016 employed 0.12 fewer employees (significant at the 1% level), which was primarily driven by a 0.09 decline in the number of part-time employees (Figure C.1).



(a) Average number of employees, full-time employees, and part-time employees



(b) Firm size distribution

Figure 3. Employment at entry

Notes: In Figure (a), the horizontal axis represents annual cohorts of start-ups, and the vertical axis displays the difference compared to Cohort 2015. Cohort 2015 employed an average of 1.61 employees, of which 0.86 worked full-time and 0.75 part-time ($\hat{\mu}_{2015}$). The dots represent point estimates ($\hat{\beta}_{2016}$), and the spikes indicate 95% confidence intervals, with standard errors clustered at the NACE 2-digit sector level. In Figure (b), the horizontal axis shows a series of firm size thresholds ranging from 1 to 5 employees. The first panel presents the mean probabilities for Cohort 2015 ($\hat{\mu}_{2015}$). For example, 26% of the start-ups in Cohort 2015 employed at least two employees. The second panel illustrates the estimated effect of the policy ($\hat{\beta}_{2016}$) across the five thresholds. For instance, the payroll tax exemption reduced the probability of employing at least two employees by 2.4 percentage points. The third panel presents the effect in relative terms ($\hat{\beta}_{2016}/\hat{\mu}_{2015}$). For instance, the exemption reduced the probability of employing at least two employees by 9.3%.

5.2 Wage bill

In Sections 5.2–5.4, we focus on short-run outcomes measured one year post entry and defer the discussion of outcomes at entry, as well as those measured two and three years after entry, to Section 5.6.[13]

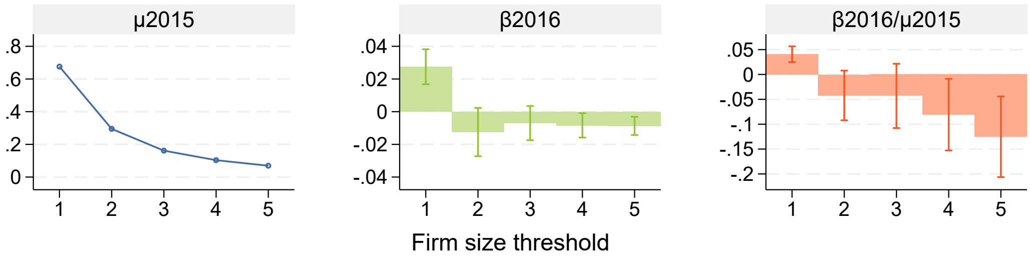
Wage bill Section 5.1 measures employment by the number of employees. However, since full-time employees and part-time employees differ in their working hours, and we lack data on actual working time, this metric cannot accurately measure labor input. To address this limitation, we use the wage bill (the total gross wages paid to all employees in the firm) as a complementary measure of employment. The RDiT estimates are plotted in Figure 5, left panel. This analysis, detailed in Appendix C.2, echoes the claims made in Section 5.1.

Wage bill per employee With a smaller firm size and a similar wage bill, post-reform new employers paid higher wage bills per employee. Since our data are at the firm level, we do not directly observe employee-level gross wages, and thus we approximate them in two ways: wage per employee and wage per full-time equivalent (FTE),[14] as plotted in the middle and the right

[13]One-year-post-entry outcomes are particularly informative because flow variables such as the wage bill and turnover reflect an average over a full quarter. This is not the case in the quarter of entry, as start-ups do not necessarily hire their first employee at the beginning of the quarter.

[14]Wage per employee = (Wage bill of the firm)/(Number of full-time employees+Number of part-time employees), and Wage per FTE = (Wage bill of the firm)/(FTE number). Since our data does not include working time, we

(a) Average number of employees, full-time employees, and part-time employees



(b) Firm size distribution

Figure 4. Employment one year post entry

Notes: In Figure (a), the horizontal axis represents the annual cohorts of start-ups, while the vertical axis displays the difference compared to Cohort 2015. One year post-entry, Cohort 2015 employed an average of 1.45 employees, with 0.85 employed full-time and 0.62 part-time. Dots represent point estimates, and spikes indicate the 95% confidence intervals, with standard errors clustered at the NACE 2-digit sector level. In Figure (b), the horizontal axis corresponds to a series of firm size thresholds ranging from 1 to 5. The first panel shows the mean probabilities for Cohort 2015 ($\hat{\mu}2015$). For example, 65% of start-ups still employed at least one person one year after entry. The second panel displays the estimated policy effect ($\hat{\beta}2016$) for each of the five thresholds. The third panel shows the relative effect ($\hat{\beta}2016/\hat{\mu}2015$).

Figure 5. Average wage bill, wage bill per employee and per FTE-employee, one year post entry

Notes: Each figure presents firm-level outcomes for the 2010–2019 cohorts of start-ups, with all results relative to Cohort 2015. The coefficient of interest, $\hat{\beta}_{2016}$, captures the effect of the payroll tax exemption. Standard errors are clustered at the NACE 2-digit sector level. The outcomes for Cohort 2015 ($\hat{\mu}_{2015}$) are €7,998, €5,411, and €6,942, respectively.

panels of Figure 5. Cohort 2015 paid a wage bill per employee (per FTE employee) of €5,411 (€6,942); Cohort 2016 paid €381 (€325) higher, or in relative terms, 7.0% (4.8%).

This wage increase suggests a different hiring strategy, but given our data, we cannot fully characterize the reason. The higher wages may stem from employees working longer hours, from employers attracting better qualified employees, or from employers sharing the subsidy with their

approximate this FTE estimate as (Number of full-time employees) + 0.5 × (Number of part-time employees).

employees by paying above-market wages.

5.3 Firm production

This section studies firm production outcomes derived from quarterly VAT reports.

Evolution of means Figure 6 presents the RDiT estimates on: turnover (interpreted as sales or output), inputs (purchases of intermediate goods and services), value added (turnover minus inputs), labor costs (wage bill plus payroll taxes net of subsidies), profits (value added minus labor costs), and investment. Value added measures the economic value generated by the firm, while profit presents the portion retained by the firm (owner).[15]

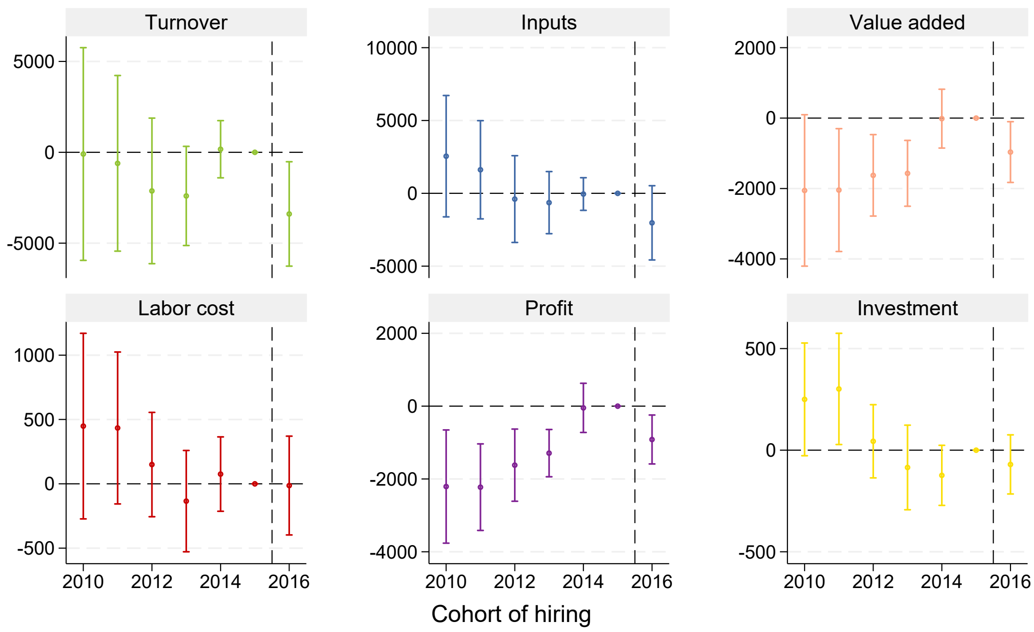


Figure 6. Effects on average firm production one year post entry

Notes: Each figure plots the firm-level average outcomes for Cohorts 2010 to 2016 of new employers, relative to Cohort 2015's average values. The coefficient of interest, $\hat{\beta}_{2016}$, identifies the impact of the payroll tax exemption. The outcomes for Cohort 2015 ($\hat{\mu}_{2015}$) are €82,794 (turnover), €58,873 (inputs), €23,407 (value added), €9,252 (labor cost), €14,193 (profit), and €3,201 (investment), respectively.

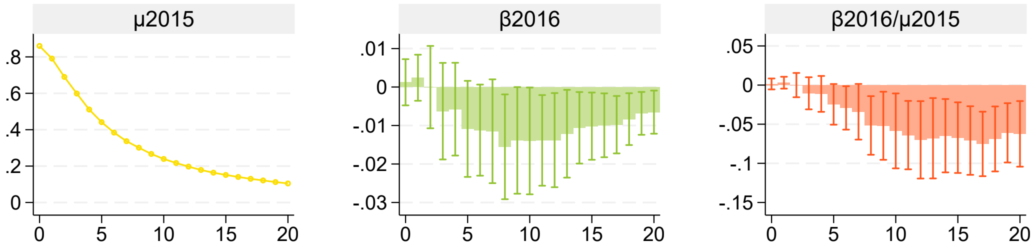
Cohort 2016 produced less output compared to Cohort 2015, as indicated by the coefficient $\hat{\beta}_{2016}$ in each panel of Figure 6. Specifically, their average quarterly turnover was €3,393 lower (4.1% in relative terms). Similarly, average quarterly value added was €965 (4.1%) lower, and profit dropped by €917 (6.5%). These estimates are summarized in Table 2 (Rows 3–5, Columns 1–2).

Distribution of turnover, value added, and profit To study the distributional changes, we select a range of thresholds from 0 to €200,000 for quarterly turnover, from 0 to €100,000 for value added, and from 0 to €50,000 for profits. One year post entry, a median start-up from Cohort 2015 achieved a quarterly turnover of €41,592, a value added of €12,532, and a profit of €7,463. Figures 7a, 7b, and 7c display the distributions of these three variables for Cohort 2015 in the left panels, the compositional changes in the middle panels, and the relative changes in the right panels. These figures document an overall decline in firm production across the entire distribution.

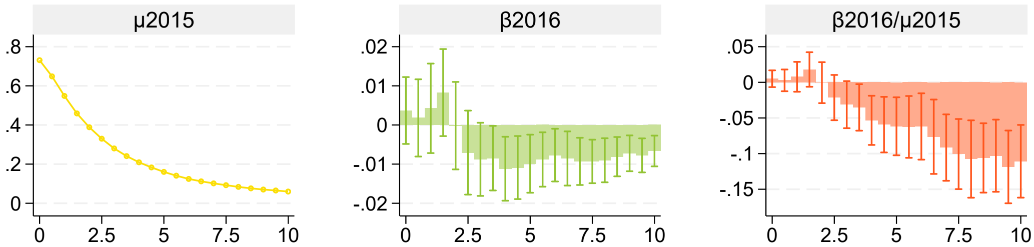
[15] Alternatively, profits can be defined by subtracting the subsidy from it, which assumes that the subsidy is entirely captured by the employer. Using this alternative definition, post-reform cohorts' profits were 7.48% lower, compared to a 6.48% drop using the standard definition.

Most point estimates in the middle and right panels are significantly negative, particularly at the higher end of the distribution, while the effects are less pronounced at the lower end.

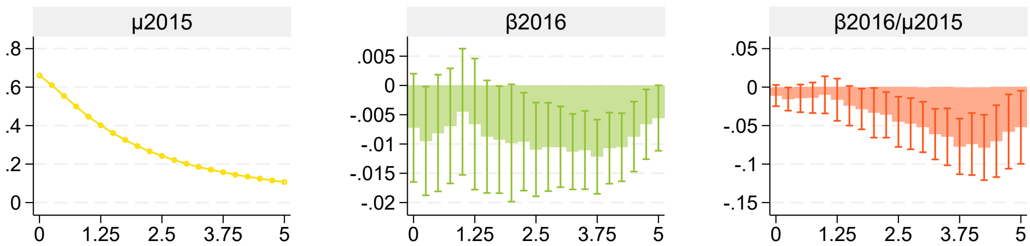
We define firms reaching the top quintiles of the distributions of these variables as *high-growth* start-ups. These quintiles, derived from the distributions of Cohort 2015, are approximately €120,000 for turnover, €40,000 for value added, and €30,000 for profits. Following the reform, start-ups benefiting from the tax exemption were less likely to reach the top quintiles of these distributions. Specifically, they were 1.4 pps (5.9% in relative terms) less likely to achieve high turnover, 1.1 pps (5.3%) less likely to achieve high value added, and 1.0 pp (5.2%) less likely to achieve high profits.



(a) Prob turnover > thresholds×€10 000



(b) Prob value added > thresholds×€10 000



(c) Prob profit > thresholds×€10 000

Figure 7. Effects on the distribution of firm production

Notes: The left panels display the probability of exceeding specific thresholds for (a) quarterly turnover, (b) quarterly value added, and (c) quarterly profits. The middle panels show the probability of exceeding these thresholds for Cohort 2016 relative to Cohort 2015, illustrating the impact of the payroll tax exemption. The right panels present the relative effects. Standard errors are clustered at the NACE 2-digit sector level.

The distributional changes in inputs, investment, and labor cost are documented in Appendix C.3. We do not observe meaningful effects.

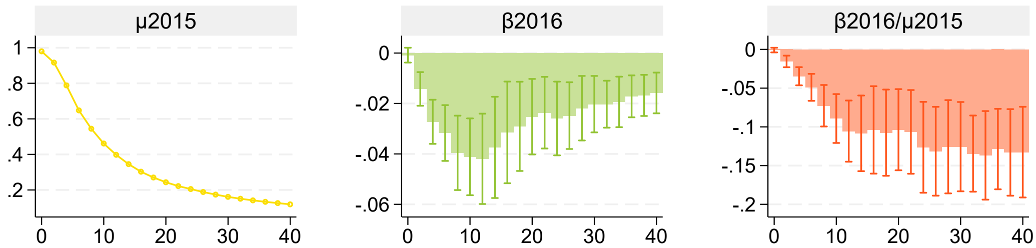
5.4 Employer quality

The observed reduction in average firm production documented above suggests that the quality of post-reform new employers must have been lower than pre-reform ones. To proxy firm quality, we use two measures: (1) labor-to-output productivity, defined as quarterly turnover divided by the quarterly wage bill, and (2) labor-to-value added productivity, defined as quarterly value added divided by the quarterly wage bill. These measures are calculated one year post entry and represent the amount a firm can generate per euro spent on wages. Figure C.5 presents RDiT estimates. On average, Cohort 2015 generated €19.8 of output and €5.6 of value added per €1 spent on wages. Cohort 2016 exhibited a 9.7% lower labor-to-output productivity and 8.7% lower labor-to-value added productivity.

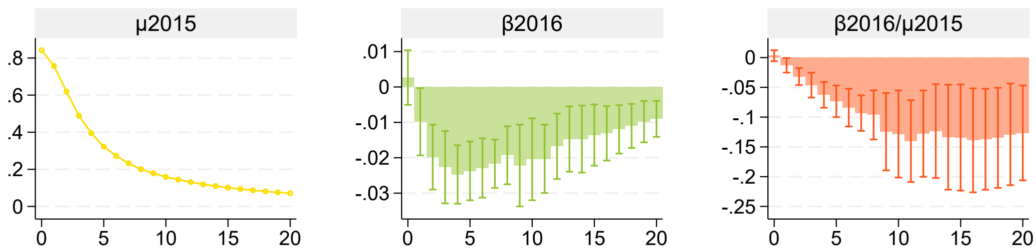
Figures 8a and 8b decompose the average productivity across the productivity distributions. The left panels show that the median firm of Cohort 2015 generated €9.0 in turnover and €2.9 in value added for each euro paid to the employees. The middle and the right panels show that productivity was consistently lower for Cohort 2016 compared to Cohort 2015 throughout the entire distribution. The absolute decline ranged from approximately 2 to 3 pps, while the relative decline varied between 10% and 15% at the upper tail of the distribution.

We consider employers achieving a labor-to-output productivity of €24 or a labor-to-value added productivity of €8 (the top quintiles of the productivity distributions) as high-productivity start-ups. Post-reform new employers were 2.6 pps (12.6%) less likely to achieve high labor-to-output productivity and by 1.9 pps (9.6%) less likely to reach high labor-to-value added productivity compared to their pre-reform counterparts.

This evidence indicates that the subsidy induced a compositional shift among new employers by incentivizing low-quality *marginal* start-ups to hire their first employees. Prior to the reform, only inframarginal start-ups (those with relatively high labor productivity) chose to become employers. After the reform, not only inframarginal firms but also marginal ones (those with lower labor productivity) decided to become employers. These marginal firms entered the labor market solely because of the labor cost reduction, and they would have remained non-employers had the payroll exemption been unavailable. The entry of these marginal firms explains why the average firm size (firm production) among post-reform cohorts was smaller (lower) than than of pre-reform ones.



(a) Output per wage bill



(b) Value added per wage bill

Figure 8. Distributional changes in labor productivity

Notes: We measure firm quality using two proxies (a) labor-to-output productivity, defined as the ratio of quarterly turnover to the wage bill, and (b) labor-to-value added productivity, defined as the ratio of quarterly value added to the wage bill, both calculated one year after hiring the first employees. The left panels illustrate the probability of firms exceeding specific labor productivity levels. The panels in the middle depict the probability of surpassing these thresholds for Cohort 2016 relative to Cohort 2015, providing an estimate of the impact of the payroll tax exemption. The right panels present the relative effects. Standard errors are clustered at the NACE 2-digit sector level.

5.5 The effects for marginal firms

While we conceptually distinguish marginal from inframarginal firms, it is infeasible to identify which firm belongs to each group. However, we can estimate the upper bound of the performance of the marginal firms relative to the performance of the inframarginal ones.

We make two simplifying assumptions. First, the 24% increase in entry of first-time employers (see Figure 1) is entirely driven by marginal firms. Second, following Hombert et al. (2020), we assume that the performance of inframarginal firms is not affected by the reform. While this second assumption is unlikely to hold exactly—inframarginal firms are expected to benefit from the tax exemption—it provides a lower bound for the effects on firm performance for marginal firms.

The average effect on post-reform new employers represents a weighted average of the effects on inframarginal firms and marginal firms. Consequently, the upper bound of the effect on marginal firms can be estimated. Denote the performance or the quality of marginal and inframarginal firms as q_m and q_i, respectively. $\delta = 24\%$ indicates the increase in the inflow of start-ups. The pre-reform cohorts exhibit an average performance of q_i, while the post-reform cohorts have an average performance of $\frac{q_i+\delta q_m}{1+\delta}$.

Our RDiT coefficient β captures the difference between these averages: $\frac{q_i+\delta q_m}{1+\delta} - q_i = \beta$. This implies that the difference between marginal firms and inframarginal firms is given by $q_m - q_i = \frac{1+\delta}{\delta}\beta$. Alternatively, performance of marginal firms relative to that of inframarginal ones is given by $\frac{q_m}{q_i} \approx 1 + \frac{1+\delta}{\delta} \times \frac{\beta}{\mu}$.

Table 2. Performance of marginal vs. inframarginal firms

	Cohort 2015 vs. 2016		Marginal vs.	Marginal as %
	Absolute	Relative	inframarginal firms	inframarginal
Performance of the firm				
At entry				
Number of employees	-0.068***	-4.2%***	-0.351	78.3%
	(0.023)	(0.015)		
One year post entry				
Survival	0.028***	4.1%***	0.145	121.2%
	(0.005)	(0.008)		
Quarterly turnover (2018€)	-3,393***	-4.1%***	-17,531	78.8%
	(1,444)	(0.015)		
Quarterly value added (2018€)	-965**	-4.1%**	-4,986	78.8%
	(434)	(0.017)		
Quarterly profits (2018€)	-917***	-6.5%***	-4,738	66.4%
	(338)	(0.022)		
Wages paid to employees				
One year post entry				
Average gross wage (2018€)	381***	7%***	1,969	136.2%
	(50.8)	(0.010)		
Quality of employers				
Labor-output productivity	-1.92***	-9.7%***	-9.92	49.9%
	(0.28)	(0.014)		
Labor–value added productivity	-0.489***	-8.7%***	-2.53	55.1%
	(0.087)	(0.017)		

Notes: Columns 1 and 2 summarize the absolute and the relative effects of the 2016 reform on average performance. Column 3 estimates the absolute differences between marginal and inframarginal firms. Column 4 estimates the performance of marginal firms as a fraction of inframarginal ones. Standard errors reported in parentheses and clustered at the NACE 2-digit sector level. Statistical significance is denoted by ***,**,* for the 1%, 5%, and 10% levels, respectively.

The first two columns of Table 2 summarize the absolute (β_{2016}) and relative effects $\left(\beta_{2016}/\mu_{2015}\right)$ for several key outcomes discussed earlier. Using the formulas presented above, the following two columns compute the difference between the performance of marginal and inframarginal firms, and the performance of marginal firms relative to that of inframarginal ones. We focus on post-reform start-ups' short-run outcomes only, as their medium-run performance remained similar.

These computations show that marginal firms were only half as productive as inframarginal firms, while their average size—whether measured by employee count or firm production—reached only about 80% of that of inframarginal firms. Despite this, marginal firms were 121% as likely as inframarginal firms to survive as employers, and wages paid to employees were 136% higher.

5.6 Short-term versus medium-term effects

Table C.1 presents the effects of the reform, in both absolute and relative terms, for a selection of 19 outcomes discussed in the previous sections. These outcomes are measured at the time of hiring, and one, two, and three years post hiring.[16] The table reports effects on average continuous outcomes as well as on the probability that these outcomes fall within the top quintile of their respective distributions (which we define as high-performing).

Across the 75 RDiT estimates reported, roughly half are statistically significant at the 5% level. The estimates consistently indicate that Cohort 2016, on average, employed fewer workers and operated at smaller firm sizes, while exhibiting a higher likelihood of survival as employer firms. Across outcomes, only the estimates for full-time employment (within one year), the probability of still employing at least one employee, and the wage bill are positive; all other effects are negative.

Across time horizons, the relative stability of the coefficients suggests that the short-run differences observed between cohorts persist into the medium run. Although some estimates attenuate over time—most notably those related to firm production—the overall pattern remains remarkably stable over the three-year window. This indicates that the differences between post-reform and pre-reform cohorts emerged early and did not meaningfully narrow in subsequent years.

This stability is informative for interpretation. Because the subsidy represents a permanent reduction in recurring labor costs rather than a one-off entry incentive, marginal firms induced to hire by the reform had ample time to adjust, learn, and potentially converge toward the performance of inframarginal employers. If such catch-up dynamics were important on average, one would expect the performance gap between Cohort 2016 and Cohort 2015 to shrink over time as marginal firms' performance improved. Instead, the persistence of the differences suggests limited convergence within the first three years after entry.

Consistent with this interpretation, labor productivity of post-reform new employers remains approximately 10% lower than that of pre-reform cohorts throughout the three-year period. This pattern supports the view that the reform primarily altered the composition of new employer firms by inducing entry of lower-productivity start-ups, while having limited effects on post-entry growth trajectories. Overall, the evidence indicates that the subsidy operated mainly through the entry margin rather than by enabling marginal firms to catch up to inframarginal ones over time.

5.7 Validating the identifying assumption

In Section 4.2, we argued that no major macroeconomic or policy shock occurred in 2015 and 2016 and that the economy remained stable from 2014 until the outbreak of the COVID-19 pandemic in 2020. We conduct three additional analyses to validate the central identification assumption that, in the absence of the policy reform, the average firm performance of Cohort 2016 would have been similar to the performance of Cohort 2015.

[16] Our data allows us to analyze firm performance up to 11 quarters (nearly three years) after firms hired their first employee. Extending the time horizon further is not possible due to the onset of the COVID-19 pandemic in 2020Q1, which affects outcomes 12 quarters post hiring for Cohort 2017Q1.

As a baseline, Panel A of Table 3 reports the fraction of the 19 key outcomes from Table C.1 for which a statistically significant difference is observed between Cohort 2015 and Cohort 2016 at four points in time. In the first year, over half of the outcomes were significantly different from zero, but this fraction decreases in the longer run.

Placebos in time. We conduct our first placebo test in Panel B by pretending that the reform had taken place in another year. More specifically, the placebo tests estimate the difference between adjacent annual cohorts in the five pre-reform years for these 19 outcomes.

Row 1 performs a *strict test* and reports the fraction of point estimates for which differences between pre-reform adjacent cohorts are statistically different from zero for all five pre-reform cohorts. If, for most outcomes, these difference are small and insignificant, it would be plausible to assume that, in the absence of the policy reform, outcomes for Cohort 2016 and 2015 would have been similar. However, between 17.9% and 36.7% of the comparisons between pre-reform cohorts are statistically significant. A similar pattern emerges when restricting the comparisons to Cohorts 2015 vs 2014 (Row 2), the most recent pair before the reform. The detailed estimates for all pre-reform cohorts are presented in Tables C.2–C.4. The small standard errors mean that even economically small differences between adjacent cohorts can be statistically significant, making these tests too conservative.

Table 3. Validation of the identifying assumption: Rejection rates

Years post hiring	0	1	2	3
A. Treated cohort	44.4%	68.4%	36.8%	31.6%
B. Pre-reform cohorts				
$\Delta\hat{\beta}_s \neq 0$	36.7%	17.9%	18.9%	28.4%
$\hat{\beta}_{2014} \neq 0$	44.4%	5.3%	10.5%	15.8%
$\lvert\Delta\hat{\beta}_s\rvert > \lvert\hat{\beta}_{2016}\rvert$	0.0%	0.0%	1.1%	2.1%
C. Ineligible firms				
$\hat{\beta}^g_{2016} \neq 0$	5.9%	21.1%	10.5%	21.1%
$\lvert\hat{\beta}^g_{2016}\rvert > \lvert\hat{\beta}_{2016}\rvert$	0.0%	0.0%	0.0%	0.0%
D. Annual vs. quarterly estimates	0.0%	5.3%	0.0%	5.3%

Notes: Panel A calculates the fractions of 19 key outcomes listed in Table C.1 that are significant at the 5% levels, at four points in time. Panel B (C) conducts placebo-in-time (placebo-in-group) tests for these 19 outcomes, with 5% as the critical values, and present the rejection rates. Panel D compares whether the annual point estimates lie within the 95% confidence intervals of the quarterly estimates, and report the rejection rates.

To address this issue, Row 3 in panel B adopts a *weaker test* and examines whether the point estimates in 2016 consistently exceed, in absolute values, the point estimates of the five adjacent pre-reform cohorts comparisons. This is indeed the case for nearly all outcomes and time periods. This finding indicates that the effects on firm performance observed in 2016 clearly stand out compared to variation in firm performance across pre-reform cohorts, suggesting that this change should be mainly attributed to the policy reform.

Placebos in group. We conduct our second placebo test in Panel C by analyzing the evolution of growing employer firms. These firms were not eligible for the payroll tax exemption for the first employee, but are similarly affected by the business cycle as the population of interest. The empirical results for this group are reported in Table C.5. Similarly to Panel B, we provide the strict tests for these estimates against zero and the weak tests against our policy estimates in the two rows in Panel C. The strict test yields rejection rates between 5.9% and 21.1%. However, our weak tests yield zero rejection rates, meaning that all the effects due to business cycles are economically smaller than (outweighed by) the policy effects we identified. Therefore, our empirical estimates are again valid.

Quarterly estimation. In Panel D, we verify whether the annual estimates fall within the 95% CIs of the quarterly estimates. As noted earlier, the quarterly estimates compare the performance of Cohort 2016Q2 against Cohort 2015Q3 after adjusting for seasonality. Because these cohorts experienced a similar business cycle, the assumption that the business cycle affected both cohorts similarly is more likely to hold. Reassuringly, these rates range from 0 to 5.3%, indicating that the annual and quarterly estimates align closely.

As an example, we use Figure 9 to show the average number of employees hired at entry by quarterly cohorts of start-ups after adjusting for seasonality. This figure reveals that average employment decreases immediately following the 2016 reform. The quarterly RDiT estimate is the first solid dot, $\hat{\beta}_{2016Q2} = -0.082$, indicating that Cohort 2016Q2 employed 0.082 fewer employees at entry than Cohort 2015Q3. Its 95% confidence interval covers the annual RDiT estimate of $\hat{\beta}_{2016} = -0.068$, and thus we cannot reject the null hypothesis that the quarterly estimate and the annual estimate yield consistent results. Results for all other key outcomes can be found in Table C.6.

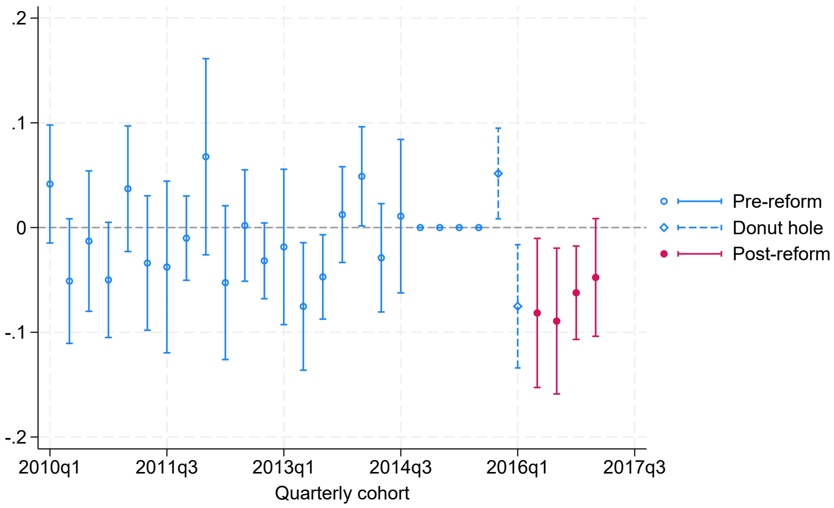


Figure 9. Number of employees at entry by quarterly cohorts

Notes: Cohorts 2014Q4 to 2015Q3 are taken as the base period cohorts, whose outcomes are normalized to 0.

6 Conclusion

Having more successful high-growth start-ups ranks high on many economic agendas. This paper studies whether fostering start-ups by subsidizing their first employee has the potential to contribute to this objective. We use administrative data on the population of start-ups to examine how a Belgian policy that exempts new employers from payroll taxes for their first employee shapes the performance of newly created employer firms.

Earlier work by Cockx and Desiere (2024) showed—also clearly illustrated in Figure 1—that the policy strongly increases the *quantity* of start-ups hiring their first employee. This paper goes one step further by documenting a decline in the *average quality* of new employers following the reform. This quantity–quality *trade-off* confirms that labor costs constitute an important barrier at the nonemployer–employer margin, and that reducing labor cost allows firms at this margin—firms that would have remained solo self-employed in the absence of the subsidy—to emerge as new employers. Despite the generosity of the subsidy, post-reform cohorts of new employers hire fewer workers and achieve lower turnover and profits both at entry and over the subsequent three years, compared to earlier cohorts that were eligible for a temporary subsidy. Importantly, these outcomes reflect the average performance of all new employers, combining *inframarginal* firms that would have hired regardless of the policy and *marginal* firms induced to hire by the subsidy.

Our analysis documents a compositional shift in the population of new employers created with the employment subsidy. Ideally, one would like to estimate the subsidy effects on inframarginal firms alone, holding the composition fixed, so that the effect on average firm performance can be decomposed into the subsidy's (positive) effect on inframarginal firms and a (negative) compositional effect driven by the entry of marginal firms. However, marginal firms only emerge as employers in the post-reform period, and therefore have no direct counterpart in a no-subsidy environment. This limitation is central to the interpretation of our results: we can estimate only the average performance of new employer cohorts, not isolate causal effects for inframarginal firms specifically. Another limitation concerns the time horizon. Due to COVID-19, we only observe firm outcomes for up to three years after entry. Although longer-run effects cannot be ruled out, the absence of catch-up within three years suggests that large positive effects emerging only later are unlikely.

Despite these limitations, the paper makes two distinctive contributions. First, by focusing on the transition from nonemployer to employer, we empirically extend the Lucas (1978) framework to a margin that is central to entrepreneurship but largely overlooked in the literature. Rather than studying entry into firm creation in general, we show that firm productivity plays a key role in crossing the discrete threshold of hiring the first employee. Second, unlike much of the entry-cost literature that emphasizes *one-off* fixed costs at firm creation (e.g., Branstetter et al., 2014), we study a permanent reduction in *recurring* labor costs. Our findings indicate that even a generous and lasting reduction in labor costs is not sufficient to enable marginal firms to converge to the performance of inframarginal employers.

We deliberately refrain from welfare judgments or policy prescriptions, although finding a balance between the positive effects of governmental regulation (Bennett and Nikolaev, 2021; Boudreaux and Nikolaev, 2019; Urbano et al., 2019) and the negative effects of over-regulation (Dutta and Sobel, 2016; Estrin et al., 2013; Lerner, 2009, 2013; Nyström, 2008) is essential. A full-fledged cost-benefit analysis goes beyond the scope of this paper as both the benefits and costs are hard to measure. On the benefit side, the policy simultaneously increased the number of firms yet discouraged them from growing larger; how to aggregate the two dimensions is subjective. On the cost side, the reform entailed a direct fiscal burden (forgone tax revenue) and opportunity costs (higher taxes elsewhere in the economy, potential crowding-out effects if subsidized firms compete with unsubsidized firms), the latter of which were unobserved by researchers.

This policy is an interesting instrument that reshapes the entrepreneurial landscape by expanding the number of employers and supporting their survival while altering the composition and average quality of new firms. Whether such a quantity–quality trade-off is desirable depends on societal objectives.

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Appendices

A Background details

A.1 Policy details

We provide more details about the reduction measures *les premiers engagements* for the first employees in this appendix.

The effective payroll tax rate is defined as the payroll tax amount due (net of all types of SSC reductions) divided by the gross wage. The 2016 reform significantly lowered the labor cost for the first employee. In Figure 2 in the policy section, we plot this effective payroll tax rate over quarters post hiring the first employee, by the (redefined) annual cohorts. This plot aligns with the empirical strategy of excluding a donut hole in the RDiT design. However, a drawback arises because the Cohort 2015 actually includes one ineligible quarterly cohort (Cohort 2014Q4), and thus the tax rate among Cohort 2015 is higher than that among Cohort 2016 even during the temporary exemption periods.

To address this, Figure A.1 focuses exclusively on the 2015Q2 and 2016Q2 cohorts, which share the same seasonality. Cohort 2015Q2 received a temporary exemption for the first employee, while Cohort 2016Q2 benefited from a permanent exemption. As in the main text, for Cohort 2015Q2, we distinguish between the anticipated tax rate, defined as the rate they expected to pay at the time of hiring, and the realized rate, defined as the rate they eventually paid. Starting in the third quarter post-hiring (2016Q1), Cohort 2015Q2 began receiving the temporary exemption, causing their realized tax rate to drop toward the rate paid by Cohort 2016Q2. After 13 quarters, the subsidy expired, and the realized tax rate of Cohort 2015Q2 reverted to anticipated tax rate.

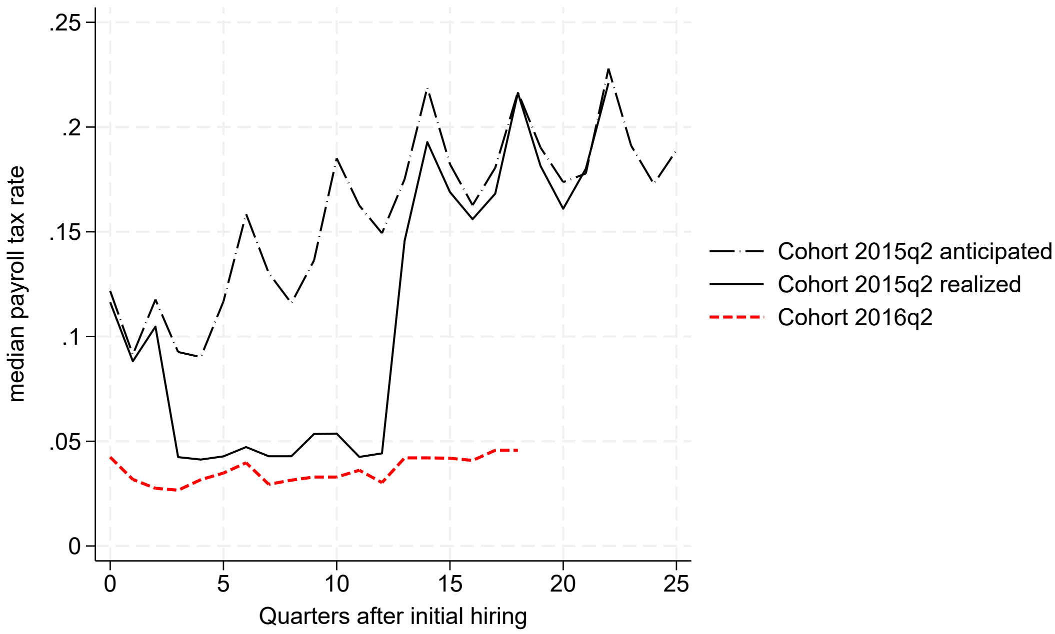


Figure A.1. Median effective payroll tax rate for the first employee of Cohort 2015Q2 and Cohort 2016Q2

Notes: The effective payroll tax is computed as the payroll tax divided by gross wage for new employers that employ exactly one employee. The horizontal axis is the number of elapsed quarters after hiring the first employees. We assume that Cohort 2015Q2 anticipated the same rate as Cohort 2014Q2 as no reform happened between 2014 and 2015.

A.2 GDP growth

From 2014 until COVID-19, the Belgian economy grew steadily at a rate of around 2%, close to the OECD average. Figure A.2 plots the annual real Belgian GDP growth rates in the left panel, and the quarterly real year-on-year growth rates in the right panel. The stable macroeconomic conditions during this period suggest that economic fluctuations were unlikely to affect comparisons across Cohorts 2014 to 2016, or in the subsequent three years up to 2019.

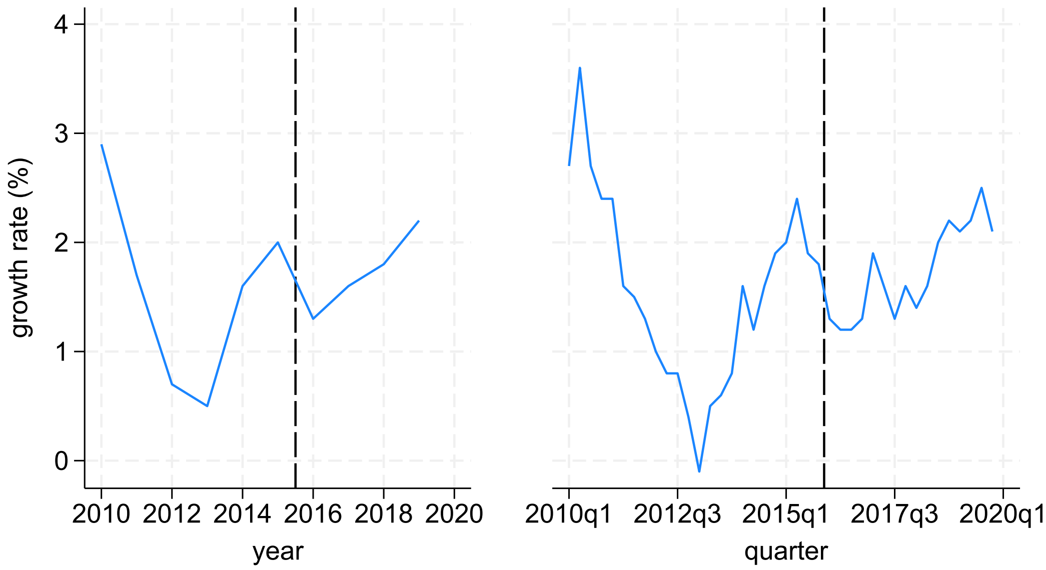


Figure A.2. Belgian real GDP growth rate, annual and quarterly

Notes: Belgian annual real GDP growth rates plotted in the left panel, and quarterly growth rates in the right panel, from 2010q1 to 2019q4. The rates are calculated in a chain-linked year-to-year change basis. The series has been working-day and seasonally adjusted. Source: National Bank of Belgium online statistics - Gross value added by industry - Total economy, at stat.nbb.be.

B Method details

B.1 Regression discontinuity design parameter derivation

This appendix argues how we yield our empirical strategy specification from the concept of local randomization framework, and discusses some details of the method choices.

The payroll tax exemption took effect on the first day of 2016, denoted as t_c. This allows us to design a regression discontinuity (RD) framework by modeling eligible new employer firm i's outcome variable $Y_i = Y(t_i^*)$ as a function of their hiring date t_i^*. With quarterly frequency data, the true continuous hiring date t_i^* is unobserved; instead, only the discrete quarter of hiring t_i is observed. When a new employer hires without the subsidy ($t_i^* < t_c$), they draw the unsubsidized outcome $Y^0(t_i^*)$; when hiring occurs with the subsidy ($t_i^* \geq t_c$), they draw the subsidized outcome $Y^1(t_i^*)$. The observed outcome is then

$$Y_i = \mathbb{1}[t_i^* < t_c] \cdot Y^0(t_i^*) + \mathbb{1}[t_i^* \geq t_c] \cdot Y^1(t_i^*).$$

A standard RD design seeks to identify the conditional treatment effect at the cutoff value, $\tau^{\texttt{SRD}} = \mathrm{E}[Y_i^1 - Y_i^0 | t_i^* = t_c]$. This involves modeling the potential outcome variable within a small bandwidth around the cutoff, separately on the left and on the right, and then extrapolating the two potential outcome values at the cutoff value. However, due to the discrete nature of our hiring date, we modify this strategy to the specification in Eq (1), which is more suitable in our context.

By specifying Eq (1), we adopt a *local randomization* framework (Cattaneo et al., 2015; Cattaneo and Titiunik, 2022; Cattaneo et al., 2016) of the RD design, without a parametric specification. In this framework, the outcome variables are represented as nonparametric means of different hiring years. We opt for annual comparisons instead of quarterly comparisons for two reasons. First, quarterly comparisons are reserved for testing the local randomization assumption, ensuring the validity of the framework. Second, annual aggregation mitigates potential seasonality embedded within the treatment effect parameter β_{2016}. Note that addressing the seasonality of the outcome variable differs from addressing seasonality in the treatment effect parameter, with the latter a severer issue.

Local randomization rests on the local randomization assumption (Cattaneo and Titiunik, 2022) that individuals are as well as randomly distributed to either side of the cutoff, enabling a comparison within a narrow window around it to estimate the local treatment effect. This is akin to observing outcomes from a localized randomized experiment. However, in our policy setting, this assumption does not fully hold, as firms' hiring decisions and the timing of those decisions are clearly endogenous. Since our research is intended to discover the composition changes of new employers, the crucial point is to accurately identify the difference between subsidized and unsubsidized new employers. Here we choose am ad-hoc window of two years, acknowledging that while a different window could provide more observations, this is not feasible due to the lack of predetermined covariates for window selection.

As our running variable is time, we label our method as regression discontinuity in time (RDiT). However, in most RDiT applications, only one observation per unit of time is available, and the continuity-based approach of separate spline fitting at both sides of the cutoff is used (Hausman and Rapson, 2018). In contrast, we have a cross section (multiple observations) of new employers per quarter (unit of time). The quarterly, discrete nature of the hiring quarter in our data motivates our choice for a local randomization framework instead. This case is referred to as "mass points" by Cattaneo and Titiunik (2022), where multiple individuals share the same running variable value. Identification and inference are typically more challenging, and they advise against using the continuity-based approach. To be exact, the estimand we aim is not entirely at the cutoff value ($\tau^{\texttt{SRD}}$), but slightly different: $\tau^{\texttt{discrete}} = \mathbb{E}[Y^1(t_i) | 2016\text{Q}2 \leq t_i \leq 2017\text{Q}1] - \mathbb{E}[Y^0(t_i) | 2014\text{Q}4 \leq t_i \leq 2015\text{Q}3]$.

B.2 Validation and placebos details

Placebos in time. By including pre-reform cohorts into the annual regression specified in Eq. (1), we implicitly conduct a placebo-in-time test by pretending that the reform occurred in earlier years. Had the reform taken place in year s, the placebo-in-time parameter would be $\Delta\beta_s = \beta_s - \beta_{s-1}$ ($s \leq 2015$). We select 19 key outcomes, and compare their placebo coefficients against two criteria, (1) zeros, and (2) our empirical estimates for the cohort of interest, $\hat{\beta}_{2016}$. The first criterion tests whether firms' performance over adjacent cohorts remains stable in the years leading to the reform. If so, it suggests that the counterfactual firm performances of subsequent cohorts absent the reform would also have remained the same. In this case, our empirical estimates would fully reflect the effect of the 2016 reform. However, this test is conservative, as economically small differences between annual cohorts are often significant, making a statistical rejection not informative.

Thus, our second criterion tests these placebo coefficients against our RDiT estimates (null hypothesis: $|\Delta\beta_s| < |\hat{\beta}_{2016}|$). Even if differences between pre-reform cohorts are not exactly zero, as long as they do not exceed the magnitude of $\hat{\beta}_{2016}$, our empirical estimates can still largely be attributed to the reform.[17]

Placebos in groups. We conduct this placebo test by applying the identification strategy outlined in Eq. (1) to growing employers, i.e. firms that expanded employment from one employee to at most ten employees at T_i. These firms were not treated, but experienced a similar simultaneous employment expansion as the treated firms.[18] The placebo-in-group parameters are the coefficients on Cohort 2016 for this population, denoted as β^g_{2016}. This coefficient enables us to assess whether the timing of hiring reflecting prevailing macroeconomic conditions impacted firms that were ineligible for the payroll tax exemption but otherwise similar to eligible firms during the 2016 reform period. We will again report two tests for 19 outcomes. First, we test whether $\beta^g_{2016} = 0$, which requires that these these ineligible firms exhibited no sensitivity to macroeconomic shocks in 2016. If this holds, it is reasonable to infer that, absent the policy introduction, the population of interest would likewise have remained unaffected by such shocks during the analysis period. Second, we test whether $|\beta^g_{2016}| < |\hat{\beta}_{2016}|$, which requires only that placebo effects do not exceed the real policy effects. If this holds, our estimates for the population of interest still stands out from the placebo-in-group parameters, and should be mainly driven by the 2016 reform.

Quarterly identification. To further support our identifying assumptions, we exploit quarterly variation by comparing quarterly cohorts of new employers. Specifically, we extend Eq. (1) to the following regression using quarterly data:

$$Y_i = \mu_{2015\text{Q}3} + \sum_{q=1,2,4} \alpha_q + \sum_{\substack{s=2010\text{Q}1 \\ s\notin 2014\text{Q}4\text{–}2015\text{Q}3}}^{2017\text{Q}1} \beta_s \mathbb{1}[t_i = s] + \epsilon_i, \tag{2}$$

where t_i denotes the quarter of hiring. We select Cohort 2015Q3 as the base quarterly cohort, and α_q captures seasonal effects within the previous base annual cohort (i.e., deviations in Cohorts 2014Q4, 2015Q1, and 2015Q2 relative to Cohort 2015Q3). If, as implicitly assumed in (2), seasonality remains constant over the observation period (2010Q1-2019Q4), these dummies α_Q appropriately adjust for the seasonality inside the two-year local randomization window. Under this assumption, the parameter $\beta_{2016\text{Q}2}$ captures the difference between the two quarterly cohorts closest to the donut hole—Cohorts 2016Q2 and 2015Q3—and identifies the treatment effect of interest. If the assumption of constant seasonality holds, the treatment effects obtained from annual and quarterly cohorts—based on models (1) and (2), respectively—should be equal. To verify

[17] Rambachan and Roth (2023) formalize a similar argument in the context of difference-in-differences and event-study designs.

[18] To be specific, we select the population of growing employers with the following requirements similar to those imposed for the treated population: (1) During the past year before hiring, they consistently employed exactly a single employee; (2) At time of hiring at T_i, they hired no more than 10 employees.

this, we will check whether the annual estimate (β_{2016}) lies within the 95% confidence interval of the quarterly estimate ($\beta_{2016\mathrm{Q}2}$).

C Additional empirical results

C.1 Employment

Figure C.1 examines the impact on employment conditional on only the surviving employers one year post entry. It is a replication of the unconditional employment in Figure 4a, by removing nonemployer firms one year post entry (those with no employees). These three point estimates $\hat{\beta}_{2016}$ are -0.121 (s.e.=0.0331), -0.0272 (s.e.=0.0324), and -0.0938 (s.e.=0.0216), respectively, which are roughly two to three times the magnitudes of the point estimates of unconditional employment. This means that the reform affected both the employer survival rate and average employment conditional on survival.

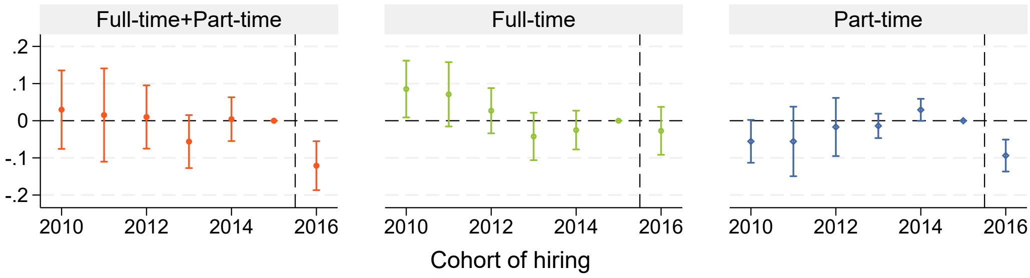


Figure C.1. Average number of employees, full-time employees, and part-time employees, one year post entry, conditional on survival

Notes: The horizontal axis indicates annual cohorts of start-ups that still employed anyone one year post entry. The vertical axis shows the difference compared to Cohort 2015. One year post entry, Cohort 2015 employed 2.17 employees, of which 1.26 worked full-time and 0.91 part-time. Dots are point estimates and spikes are 95% confidence intervals using standard errors clustered at the NACE 2-digit sector level.

C.2 Wage bill

Suppose the reform did not trigger a change in the market wage rate, an assumption supported by the targeted nature of the policy, which primarily affected start-ups rather than the broader labor market. Under this assumption, a higher wage bill per firm corresponds to higher labor input. Importantly, the wage bill excludes payroll taxes and is therefore not mechanically affected by the payroll tax exemption. The wage bill captures multiple dimensions of labor input, as it is the product of the number of employees, the average hours worked per employee, and the average wage rate. This allows firms to respond to the tax exemption in various ways, such as hiring fewer but more productive workers or increasing hours worked per employee. To assess how the payroll tax exemption influenced hiring strategies, we examine its impact on the distribution of the wage bill.

Benefiting from the exemption, Cohort 2016 opted a different hiring strategy, concentrating the work schedule into fewer number of employees. Cohort 2015 paid an average quarterly wage bill of €7,998, and Cohort 2016 paid €171 higher, a difference that is small and statistically insignificant (Figure 5, left panel). However, decomposing the wage bill into probabilities of reaching different thresholds reveals significant effects. The first panel of Figure C.2 plots the probability that the quarterly wage bill exceeds thresholds ranging from 0 to €20,000. About one-third of the firms no longer employed workers after one year, resulting in zero wage bills (consistent with the left panel of Figure 4b), while the median employer paid about €4,500 per quarter. The second and the third panels plot the absolute and relative effects of the exemption on post-reform start-ups' wage bills, showing hump-shaped probability increases centered around €4,000, primarily at the lower sections of the wage bill distribution. The exemption raised the probability of new employers paying wage bills higher than €4,000 by 5.18 pps (10.3% in relative terms). This threshold is close to the average gross wage of a single employee, suggesting that post-reform start-ups were more likely to hire one employee but not subsequent ones. This finding aligns with our earlier results on employment and highlights how the reform incentivized firms to optimize their labor input.

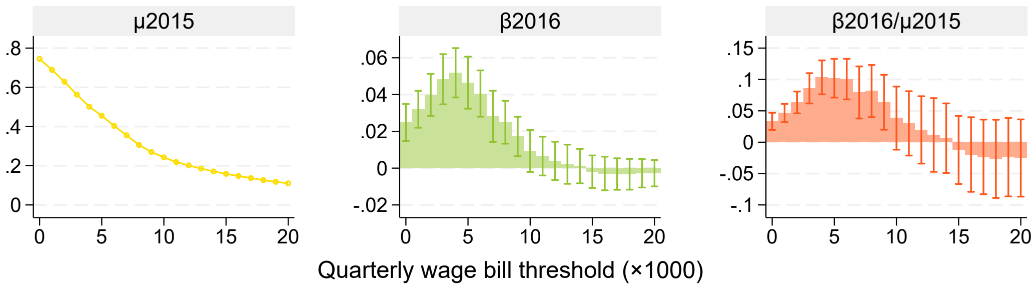


Figure C.2. Effects on the wage bill distribution

Notes: The horizontal axis is a series of firm size thresholds for the quarterly wage bill. The first panel shows the mean probabilities among Cohort 2015 ($\hat{\mu}_{2015}$). For instance, one year post entry. the wage bill exceeds €40,000 for 50% of the start-ups. The second panel shows the estimated effect of the policy ($\hat{\beta}_{2016}$) for the 21 thresholds. The third panel shows the effect in relative terms ($\hat{\beta}_{2016}/\hat{\mu}_{2015}$).

C.3 Firm production

Firm production outcomes include firm turnover, inputs, value added, labor cost, profits, and investment. Except labor costs, these variables are only observed when firms pay VAT. Cohorts 2015 and 2016 are equally likely to pay VAT: One year post entry, 88.7% of Cohort 2015 firms paid VAT, and the coefficient $\hat{\beta}_{2016}$ in Figure C.3 is close to zero and insignificant. This suggests that the 2016 reform did not significantly alter the VAT reporting rate for Cohort 2016, and thus missing data does not introduce a sample selection issue. For the remaining 11.3% of firms that either do not pay VAT or have exited the market, we set the outcomes to zero. Consequently, additional analyses of firm production metrics conditional on VAT reporting are unnecessary.

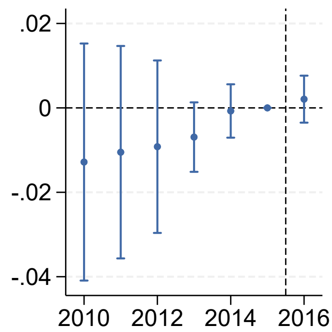


Figure C.3. VAT report rate by annual cohort, one year post entry

Notes: The mean report rate among Cohort 2015 is 88.7%.

We briefly discussed the effects of the reform on new employer firms' quarterly turnover, inputs, labor costs, profits, value added, and investment, summarized in Figure 6. While the main text focuses on the distributions of turnover, value added, and profits, here we investigate the distributions of the other three variables—inputs, labor costs, and investment—as well as changes in mean levels of all six outcomes. Notably, the outcome variables are heavily right-skewed, with means approximately twice as large as the medians, and as a result, Figure 6 shows relatively large standard errors for the estimates.

Cohort 2015 generated an average quarterly turnover of €82,794, purchased inputs for €58,873, paid an average labor cost of €9,252. This resulted in an average value added of €23,921 and profits of €14,193, implying a profit margin (profit divided by turnover) of 17.1%. Labor costs, excluding the compensation of the self-employed owner-manager, accounted for only one-seventh of operating costs (inputs plus labor costs). Capital appeared to play a small role for start-ups, as only one-third reported positive quarterly investments. On average, start-ups invested €3,201 into capital goods, equivalent to 22.6% of their profits.

We observe a modest decrease in inputs, which suggests that firms may have substituted materials for labor in production, or alternatively that they shifted the purchase of labor services into formal employment due to the tax exemption. In contrast, we find no noticeable effects on investment, indicating that start-ups did not substitute capital for labor.

The treatment effects on the mean inputs, investment, and labor cost one year post entry are $-2,025$ (s.e.=1,281), -€70.14 (s.e.=73.17), and -13.51 (s.e.=193.1), corresponding to the estimates $\hat{\beta}_{2016}$ of Figure 6. These effects are small and insignificant at the 10% level. We further decompose these variables to examine distributional changes.

Figure C.4 plots the distributions of quarterly inputs, investment, and labor costs for Cohort 2015 in the left panels, the treatment effects across different levels of the distributions in the middle panels, and the relative effects in the right panels. These figures follow the same approach as in Figure 7. We select a range of thresholds from 0 to €200,000 for quarterly inputs, and 0 to €20,000 for both quarterly investment and quarterly labor cost. One year post entry, a median start-up from Cohort 2015 purchased a quarterly inputs of €23,631, made no investment, and incurred a labor cost of €4,524.

Across the distributions, the treatment effects on (a) inputs and (b) investment are negative but statistically insignificant. The point estimates for inputs and investment probabilities generally decreased from lower thresholds to higher thresholds in absolute terms. The largest decrease of inputs probabilities occurred at the threshold of €20,000, with a drop of 1.61 pps (3.0% in relative terms), significant at the 5% level. Several surrounding thresholds also showed significant decreases, suggesting that Cohort 2016 new employers used slightly fewer intermediary materials than Cohort 2015. The largest decrease of investment probabilities occurred at the threshold of 0, with a drop of 1.92 pps (6.3% in relative terms), significant at the 1% level. Thus, Cohort 2016 were less likely to invest at all compared to Cohort 2015, though the overall effect on investment was limited.

The effects on the distribution of (c) labor costs closely resembled those of the wage bill in Figure C.2. The absolute treatment effects across the distribution were hump-shaped, peaking at a quarterly labor cost of €4,000, with an incremental probability of 4.8 pps (9.1% in relative terms). From the thresholds of €4,000 to €9,000, where the cost of one employee typically lied, the probability increments were positive and significant at the 5% level. This reinforces the findings in Section 5.2 that the reform created more post-reform start-ups hiring a single employee.

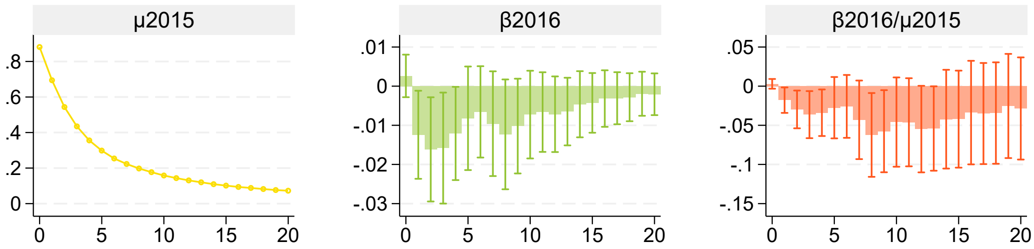
C.4 Employer quality

The main text of Section 5.4 examines the distributional changes of employers' labor productivity, and here we present estimates of mean productivity. Figure C.5 presents the average labor-to-output productivity and the average labor-to-value added productivity over employer cohorts, compared to Cohort 2015. The point estimates $\hat{\beta}_{2016}$ are -1.92 (s.e.=0.28) for labor-to-output productivity and -0.498 (s.e.=0.087) for labor-to-value added productivity, corresponding to a relative decrease of approximately 9% compared to Cohort 2015.

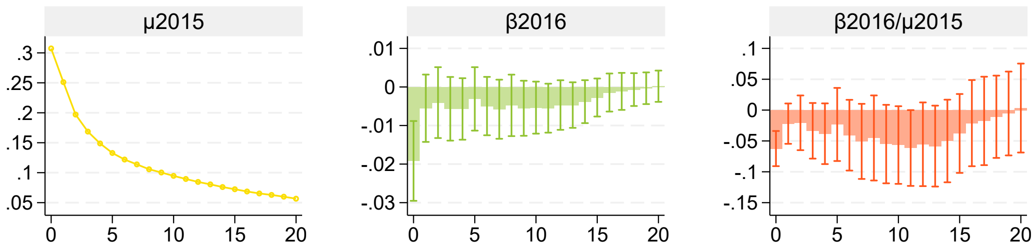
C.5 Medium-run effects

Table C.1 summarizes the absolute ($\hat{\beta}_{2016}$, Columns "Abs") and the relative effects ($(\frac{\hat{\beta}_{2016}}{\hat{\mu}_{2015}})$, Columns "Rel") of the reform across 19 key outcomes, tracking firms from the hiring quarter up to three years (11 quarters) post-hiring. This table complements graphical results in Section 5 and Appendix figures. We categorize outcomes into employment, wage bill, firm production, and employer quality, in the four panels. Rows that begin with "Pr>" refer to the probability of the variables above exceeding the specified threshold.

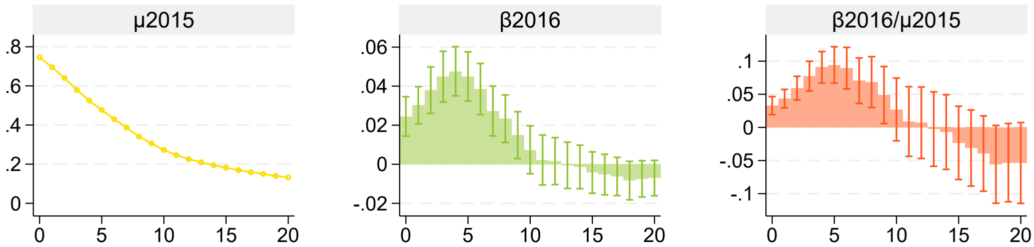
Effects at hiring are only discussed for the outcomes of employment in the main text Section



(a) Prob inputs > thresholds×€10 000



(b) Prob investment > thresholds×€1 000



(c) Prob labor cost > thresholds×€1 000

Figure C.4. Probability of quarterly inputs, investment, and labor cost exceeding thresholds, one year post entry

Notes: Panels in the left column show the probability of exceeding certain levels of quarterly (a) inputs, (b) investment, and (c) labor cost, for Cohort 2015 of new employers one year after hiring the first employees. Panels in the middle column show the probability of exceeding certain thresholds among Cohort 2016 relative to Cohort 2015 and are interpreted as the absolute effects of the payroll tax exemption. Panels in the right column show the relative effects. Standard errors are clustered at the NACE 2-digit sector levels.

Figure C.5. Average productivity by annual cohort

Notes: The left panel plots the labor productivity in generating turnover, and the right in generating value added. The average productivity values ($\hat{\mu}_{2015}$) among Cohort 2015 are 19.8 and 5.6.

5.1. Here we make the correspondence between the main analyses and this table for reference. The absolute effects of Rows 1 to 3 correspond to the estimates $\hat{\beta}_{2016}$ in the three panels of Figure 3a; the absolute (absolute) effects of Rows 5 to 7 corresponds to the estimates at the thresholds of 2, 3, and 5 employees in the middle (right) panel of Figure 3b. Flow variables (e.g., wage bills, turnover) are excluded from the main text due to imprecise timing of hiring dates in the first quarter, which complicates measurement of partial-quarter work. For completeness, these estimates are included here despite potential inaccuracies.

Effects post hiring are discussed for the one-year horizon for outcomes employment, the wage bill, and firm production in Figures 4, 6, and 7, respectively. This table focuses on the mean changes of the three variables, and the probability of reaching a relatively high threshold for these variables, the top quintiles according to Cohort 2015's distribution one year post entry. These thresholds identify "high-growth" start-ups. Although thresholds in later years' distributions rise, retain the original values for consistency across analyses, including placebo tests in Appendix C.6.

The robustness of labor productivity proxies In the main analyses, we calculate the labor productivity proxies with firm production variables measured one year post entry. In the last four rows of Table C.1, we proxy this unobserved productivity with variables observed at different times. Since we assume that employers' productivity are inherent and thus time-invariant, these different columns only provide alternative proxies instead of showing the evolution of labor productivity. The stability of these values across columns thus confirms the robustness of our two labor productivity proxy measures.

Table C.1. Impact of the tax exemption on key outcomes, up to three years post entry

Outcomes	At hiring		After one year		After two years		After three years	
	Abs	Rel	Abs	Rel	Abs	Rel	Abs	Rel
Employment								
Full-time+Part-time	-0.068***	-0.042***	-0.025	-0.017	-0.032	-0.022	-0.054	-0.038*
	(0.023)	(0.015)	(0.025)	(0.017)	(0.028)	(0.019)	(0.033)	(0.022)
Full-time	0.007	0.009	0.016	0.018	-0.009	-0.010	-0.035	-0.040
	(0.018)	(0.022)	(0.021)	(0.026)	(0.022)	(0.025)	(0.027)	(0.028)
Part-time	-0.076***	-0.100***	-0.041***	-0.066***	-0.023**	-0.040**	-0.018	-0.034
	(0.015)	(0.023)	(0.014)	(0.023)	(0.011)	(0.019)	(0.012)	(0.020)
Pr Emp≥1	-	-	0.028***	0.041***	0.015***	0.026***	0.012**	0.022**
			(0.005)	(0.008)	(0.005)	(0.008)	(0.005)	(0.010)
Pr Emp≥2	-0.024***	-0.093**	-0.012	-0.042*	-0.010	-0.032	-0.013**	-0.046**
	(0.009)	(0.038)	(0.008)	(0.025)	(0.006)	(0.021)	(0.006)	(0.019)
Pr Emp≥3	-0.011**	-0.082*	-0.007	-0.043	-0.011**	-0.061***	-0.009*	-0.052*
	(0.005)	(0.045)	(0.005)	(0.033)	(0.004)	(0.023)	(0.005)	(0.027)
Pr Emp≥5	-0.009***	-0.166***	-0.009***	-0.125***	-0.008**	-0.096***	-0.007**	-0.083**
	(0.002)	(0.050)	(0.003)	(0.041)	(0.003)	(0.036)	(0.003)	(0.037)
Wage bill								
Wage bill (€)	-70.2	-0.014	171	0.021	150	0.018	-42.2	-0.005
	(81.8)	(0.016)	(147)	(0.019)	(147)	(0.019)	(174.0)	(0.021)
Pr>€12 000	-0.009**	-0.097***	0.004	0.020	0.005	0.021	0.004	0.020
	(0.004)	(0.033)	(0.005)	(0.027)	(0.004)	(0.022)	(0.005)	(0.022)
Firm production								
Turnover (€)	-1,905	-0.028	-3,393**	-0.041***	-2,180	-0.026*	-1,445	-0.018
	(1,369)	(0.019)	(1,444)	(0.015)	(1,424)	(0.016)	(1,498)	(0.017)
Pr>€120 000	-0.010*	-0.064**	-0.014**	-0.070***	-0.006	-0.030	-0.008*	-0.038**
	(0.006)	(0.032)	(0.006)	(0.025)	(0.006)	(0.026)	(0.004)	(0.018)
Value added (€)	90.6	0.006	-965**	-0.041**	-382	-0.016	-34.2	-0.001
	(356.0)	(0.023)	(434)	(0.017)	(517)	(0.021)	(384.7)	(0.016)
Pr>€40 000	-0.004	-0.025	-0.011***	-0.053***	-0.004	-0.018	0.001	0.005
	(0.004)	(0.023)	(0.004)	(0.018)	(0.004)	(0.020)	(0.003)	(0.015)
Profit (€)	362	0.036	-917***	-0.065***	-282	-0.019	9.71	0.001
	(389)	(0.040)	(338)	(0.022)	(490)	(0.033)	(269.64)	(0.018)
Pr>€30 000	-0.005	-0.026	-0.010***	-0.052***	-0.004	-0.019	0.004	0.018
	(0.005)	(0.026)	(0.004)	(0.017)	(0.005)	(0.024)	(0.003)	(0.014)

Employer quality								
Labor-output productivity	-2.96***	-0.080***	-1.92***	-0.097***	-1.77***	-0.093***	-1.29***	-0.070***
	(0.79)	(0.020)	(0.28)	(0.014)	(0.41)	(0.018)	(0.33)	(0.018)
Pr>24	-0.024***	-0.070***	-0.026***	-0.126***	-0.019***	-0.102***	-0.015**	-0.081**
	(0.007)	(0.016)	(0.007)	(0.030)	(0.009)	(0.039)	(0.006)	(0.032)
Labor–value added productivity	-0.432	-0.047	-0.489***	-0.087***	-0.316**	-0.059**	-0.234*	-0.044*
	(0.280)	(0.029)	(0.087)	(0.017)	(0.139)	(0.027)	(0.139)	(0.026)
Pr>8	-0.012*	-0.038*	-0.019***	-0.096***	-0.016*	-0.085*	-0.015***	-0.081***
	(0.007)	(0.021)	(0.004)	(0.021)	(0.009)	(0.049)	(0.005)	(0.028)

Notes: For each point in time, the first column shows the absolute effect, the coefficient on Cohort 2016, and the second column shows the relative effect, by dividing the coefficient on Cohort 2016 by the mean outcome of Cohort 2015. Emp is short for the number of employees, full-time plus part-time numbers. The thresholds of €12,000 (wage bill), €120,000 (turnover), €40,000 (value added), €30,000 (profit), 24 (turnover/wage bill), and 8 (value added/wage bill) are approximately the top quintile (precisely, the top 20.2%, 19.7%, 20.2%, 20.5%, and 20.1%) among Cohort 2015, one year post hiring. The standard errors, clustered at the NACE-2 digit level, are reported in parentheses. ***,**,* denote statistical significance at the 1%, 5%, and 10% level, respectively.

C.6 Validation and placebo test results

This section provides backup materials documenting all the empirical results from the validation and placebo analyses argued in Section 5.7. We provide the full estimates ($\hat{\beta}_s,\ s = 2010, ..., 2016$ and $\hat{\mu}_{2015}$) of Eq (1) on the 19 key outcomes, in Tables C.2 (for employment outcomes), C.3 (for firm production outcomes), and C.4 (for labor productivity proxy outcomes). Table C.5 documents the placebo effects on these 19 outcomes for growing employers that hired their second employees in 2016. Table C.6 documents the quarterly estimates ($\hat{\beta}_{2016\mathrm{Q2}}$) on these 19 outcomes using the quarterly regression (2).

Table C.2. RDiT estimates on all annual cohorts: Employment outcomes

Panel A. Employee count

	Full-time + Part-time				Full-time				Part-time			
Years post hiring	At hiring	1	2	3	At hiring	1	2	3	At hiring	1	2	3
$\hat{\beta}_{2010}$	0.020	0.071	0.002	-0.085	0.055***	0.089**	0.035	-0.040	-0.035**	-0.018	-0.033	-0.045
	(0.024)	(0.048)	(0.064)	(0.063)	(0.020)	(0.037)	(0.043)	(0.038)	(0.015)	(0.021)	(0.029)	(0.037)
$\hat{\beta}_{2011}$	-0.022	0.014	-0.082	-0.139***	0.054***	0.051	-0.042	-0.090***	-0.076***	-0.036	-0.040	-0.049
	(0.025)	(0.054)	(0.063)	(0.050)	(0.020)	(0.035)	(0.034)	(0.025)	(0.020)	(0.034)	(0.042)	(0.039)
$\hat{\beta}_{2012}$	0.000	-0.007	-0.038	-0.074*	0.051***	0.010	-0.026	-0.059**	-0.051**	-0.017	-0.012	-0.015
	(0.025)	(0.034)	(0.041)	(0.039)	(0.013)	(0.022)	(0.025)	(0.026)	(0.024)	(0.028)	(0.031)	(0.034)
$\hat{\beta}_{2013}$	-0.043**	-0.040	-0.077**	-0.113**	-0.003	-0.030	-0.077***	-0.089***	-0.039***	-0.010	-0.000	-0.025
	(0.019)	(0.031)	(0.038)	(0.043)	(0.012)	(0.024)	(0.026)	(0.025)	(0.012)	(0.014)	(0.017)	(0.029)
$\hat{\beta}_{2014}$	0.013	0.019	-0.034	-0.048	-0.000	-0.008	-0.034	-0.045**	0.013	0.027**	-0.000	-0.002
	(0.017)	(0.020)	(0.033)	(0.030)	(0.012)	(0.018)	(0.021)	(0.021)	(0.011)	(0.011)	(0.017)	(0.020)
$\hat{\beta}_{2016}$	-0.068***	-0.025	-0.032	-0.054	0.007	0.016	-0.009	-0.035	-0.076***	-0.041***	-0.023**	-0.018
	(0.023)	(0.025)	(0.028)	(0.033)	(0.018)	(0.021)	(0.022)	(0.027)	(0.015)	(0.014)	(0.011)	(0.012)
$\hat{\mu}_{2015}$	1.611***	1.468***	1.445***	1.432***	0.856***	0.852***	0.871***	0.883***	0.754***	0.617***	0.574***	0.549***
	(0.068)	(0.053)	(0.056)	(0.058)	(0.077)	(0.083)	(0.088)	(0.098)	(0.138)	(0.106)	(0.100)	(0.096)

Panel B. Probability of employee count reaching thresholds

	Pr employees≥1			Pr employees≥2				Pr employees≥3				Pr employees≥5			
Years post hiring	1	2	3	At hiring	1	2	3	At hiring	1	2	3	At hiring	1	2	3
$\hat{\beta}_{2010}$	0.023**	-0.004	-0.021**	0.009	0.015	0.000	-0.017	0.008	0.011	-0.003	-0.014	0.001	0.002	-0.004	-0.010*
	(0.011)	(0.010)	(0.010)	(0.007)	(0.011)	(0.012)	(0.011)	(0.005)	(0.008)	(0.010)	(0.009)	(0.003)	(0.005)	(0.006)	(0.006)
$\hat{\beta}_{2011}$	0.002	-0.023***	-0.030***	-0.007	0.000	-0.020*	-0.028***	-0.003	0.002	-0.015	-0.024***	-0.004	-0.002	-0.012**	-0.014***
	(0.010)	(0.008)	(0.008)	(0.008)	(0.012)	(0.010)	(0.009)	(0.006)	(0.008)	(0.009)	(0.008)	(0.003)	(0.005)	(0.006)	(0.005)
$\hat{\beta}_{2012}$	-0.006	-0.020***	-0.022***	0.003	-0.006	-0.013*	-0.018***	0.003	-0.002	-0.009	-0.015***	-0.002	-0.002	-0.006	-0.007
	(0.006)	(0.004)	(0.006)	(0.007)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)
$\hat{\beta}_{2013}$	-0.001	-0.016***	-0.022***	-0.010*	-0.008	-0.017***	-0.023***	-0.006	-0.004	-0.014**	-0.016***	-0.006**	-0.006**	-0.008*	-0.009**
	(0.007)	(0.004)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.002)	(0.003)	(0.004)	(0.004)
$\hat{\beta}_{2014}$	0.007	-0.005	-0.005	0.009*	-0.000	-0.007	-0.009	0.007*	0.006*	-0.007	-0.008	0.000	-0.002	-0.007*	-0.008***
	(0.006)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.002)	(0.002)	(0.003)	(0.003)
$\hat{\beta}_{2016}$	0.028***	0.015***	0.012**	-0.024***	-0.012	-0.010	-0.013**	-0.011**	-0.007	-0.011**	-0.009*	-0.009***	-0.009***	-0.008**	-0.007**
	(0.005)	(0.005)	(0.005)	(0.009)	(0.008)	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)	(0.005)	(0.002)	(0.003)	(0.003)	(0.003)
$\hat{\mu}_{2015}$	0.676***	0.577***	0.523***	0.259***	0.295***	0.296***	0.290***	0.130***	0.162***	0.177***	0.179***	0.054***	0.069***	0.079***	0.083***
	(0.018)	(0.018)	(0.018)	(0.025)	(0.012)	(0.010)	(0.009)	(0.016)	(0.011)	(0.010)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)

Notes: Standard errors are clustered at the NACE-2 digit level and reported in parentheses. ***,**,* denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table C.3. RDiT estimates on all annual cohorts: Firm production outcomes

Panel A. Means

	Wage bill (€)				Turnover (€)				Value added (€)				Profit (€)			
Years post hiring	At hiring	1	2	3	At hiring	1	2	3	At hiring	1	2	3	At hiring	1	2	3
$\hat{\beta}_{2010}$	-14	71	-173	-529	-1,899	-95	-1,907	-2,085	-1,389*	-2,209***	-2,789***	-2,285***	-1,514*	-2,055*	-2,692***	-2,224**
	(123)	(280)	(320)	(337)	(1,911)	(2,943)	(2,547)	(2,254)	(737)	(781)	(424)	(685)	(818)	(1,081)	(622)	(921)
$\hat{\beta}_{2011}$	-86	-8	-563**	-932***	-147	-608	-4,107*	-4,501**	-861	-2,226***	-1,963***	-1,987***	-1,062	-2,044**	-2,298***	-2,591***
	(93)	(226)	(216)	(219)	(2,106)	(2,430)	(2,214)	(2,111)	(721)	(598)	(472)	(579)	(884)	(877)	(613)	(741)
$\hat{\beta}_{2012}$	-52	-172	-350*	-684***	-354	-2,124	-2,104	-2,383	-267	-1,621***	-1,175***	-973***	-333	-1,626***	-1,267***	-1,280***
	(95)	(164)	(180)	(190)	(2,101)	(2,014)	(2,029)	(1,779)	(655)	(498)	(375)	(292)	(801)	(582)	(451)	(409)
$\hat{\beta}_{2013}$	-205***	-310*	-517***	-750***	-2,752**	-2,401*	-2,331*	-3,473**	-876**	-1,291***	-1,079***	-1,350***	-1,171**	-1,569***	-1,415***	-1,848***
	(59)	(170)	(195)	(209)	(1,223)	(1,372)	(1,396)	(1,501)	(394)	(326)	(263)	(382)	(474)	(470)	(403)	(554)
$\hat{\beta}_{2014}$	3	-91	-341**	-493***	-1,034	169	-571	-546	-766**	-48	-455	-572	-764**	-15	-678	-693
	(72)	(117)	(145)	(171)	(821)	(791)	(971)	(937)	(291)	(340)	(381)	(399)	(318)	(420)	(452)	(526)
$\hat{\beta}_{2016}$	-70	171	150	-42	-1,905	-3,393**	-2,180	-1,445	362	-917***	-282	10	91	-965**	-382	-34
	(82)	(147)	(147)	(174)	(1,369)	(1,444)	(1,424)	(1,498)	(389)	(338)	(490)	(270)	(356)	(434)	(517)	(385)
$\hat{\mu}_{2015}$	5,075***	7,998***	8,102***	8,351***	66,935***	82,794***	82,649***	82,303***	9,984***	14,193***	14,587***	14,894***	15,727***	23,407***	23,967***	24,538***
	(248)	(551)	(619)	(695)	(7,116)	(9,039)	(9,117)	(9,497)	(1,522)	(1,003)	(1,028)	(1,190)	(1,727)	(1,505)	(1,632)	(1,923)

Panel B. Probabilities

	Pr Wage bill>€12k				Pr Turnover>€120k				Pr Value added>€40k				Pr Profit>€30k			
Years post hiring	At hiring	1	2	3	At hiring	1	2	3	At hiring	1	2	3	At hiring	1	2	3
$\hat{\beta}_{2010}$	0.002	0.002	-0.005	-0.013	-0.009	-0.003	-0.005	-0.011*	-0.018**	-0.023***	-0.026***	-0.022***	-0.017**	-0.020**	-0.025***	-0.020**
	(0.005)	(0.009)	(0.010)	(0.009)	(0.006)	(0.009)	(0.007)	(0.007)	(0.008)	(0.008)	(0.006)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)
$\hat{\beta}_{2011}$	-0.003	0.003	-0.017**	-0.024***	-0.003	-0.006	-0.010*	-0.013**	-0.011	-0.019**	-0.022***	-0.021***	-0.009	-0.017**	-0.019***	-0.023***
	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.009)	(0.008)	(0.006)	(0.007)	(0.008)	(0.009)	(0.007)	(0.007)
$\hat{\beta}_{2012}$	-0.000	-0.005	-0.012**	-0.017***	-0.001	-0.008	-0.006	-0.010**	-0.008	-0.024***	-0.015***	-0.013***	-0.006	-0.020***	-0.013***	-0.012***
	(0.005)	(0.006)	(0.006)	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)	(0.008)	(0.005)	(0.004)	(0.003)	(0.008)	(0.005)	(0.005)	(0.004)
$\hat{\beta}_{2013}$	-0.008**	-0.008	-0.013**	-0.018***	-0.012**	-0.009**	-0.008**	-0.011***	-0.015***	-0.018***	-0.015***	-0.012***	-0.013**	-0.020***	-0.015***	-0.017***
	(0.003)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
$\hat{\beta}_{2014}$	0.002	-0.001	-0.009*	-0.008	-0.004	-0.001	0.000	-0.004	-0.012***	-0.003	-0.008**	-0.003	-0.008**	-0.001	-0.007*	-0.006
	(0.004)	(0.004)	(0.005)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)
$\hat{\beta}_{2016}$	-0.009**	0.004	0.005	0.004	-0.010*	-0.014**	-0.006	-0.008*	-0.005	-0.010***	-0.004	0.004	-0.004	-0.011***	-0.004	0.001
	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.004)	(0.005)	(0.004)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)
$\hat{\mu}_{2015}$	0.098***	0.202***	0.212***	0.215***	0.153***	0.197***	0.197***	0.199***	0.176***	0.202***	0.200***	0.193***	0.157***	0.210***	0.210***	0.210***
	(0.009)	(0.016)	(0.017)	(0.018)	(0.026)	(0.029)	(0.027)	(0.026)	(0.022)	(0.020)	(0.019)	(0.019)	(0.020)	(0.019)	(0.018)	(0.018)

Notes: Standard errors are clustered at the NACE-2 digit level and reported in parentheses. ***,**,* denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table C.4. RDiT estimates on all annual cohorts: Labor productivity measures

Panel A. Means

	Turnover/Wage bill				Value added/Wage bill			
Years post hiring	At hiring	1	2	3	At hiring	1	2	3
$\hat{\beta}_{2010}$	1.008	0.039	-0.114	0.167	-0.215	-0.410**	-0.380**	-0.251
	(0.998)	(0.457)	(0.504)	(0.587)	(0.306)	(0.164)	(0.181)	(0.197)
$\hat{\beta}_{2011}$	1.385	-0.401	0.140	0.595	-0.088	-0.477***	-0.091	-0.131
	(1.074)	(0.464)	(0.325)	(0.569)	(0.232)	(0.119)	(0.088)	(0.195)
$\hat{\beta}_{2012}$	1.534	-0.297	0.053	0.222	0.253	-0.275***	0.093	-0.008
	(1.056)	(0.482)	(0.511)	(0.624)	(0.345)	(0.094)	(0.129)	(0.175)
$\hat{\beta}_{2013}$	-0.722	-0.397	0.133	0.073	-0.488*	-0.317***	0.085	-0.159
	(0.829)	(0.321)	(0.316)	(0.396)	(0.289)	(0.078)	(0.099)	(0.141)
$\hat{\beta}_{2014}$	-1.213**	-0.317	-0.245	-0.118	-0.441**	-0.045	0.022	-0.054
	(0.518)	(0.260)	(0.405)	(0.514)	(0.172)	(0.131)	(0.143)	(0.189)
$\hat{\beta}_{2016}$	-2.964***	-1.925***	-1.774***	-1.288***	-0.432	-0.489***	-0.316**	-0.234*
	(0.785)	(0.279)	(0.409)	(0.332)	(0.280)	(0.087)	(0.139)	(0.139)
$\hat{\mu}_{2015}$	37.07***	19.80***	19.09***	18.42***	9.123***	5.614***	5.324***	5.327***
	(2.968)	(1.302)	(1.404)	(1.356)	(0.707)	(0.266)	(0.231)	(0.229)

Panel B. Probabilities

	Pr>24				Pr>8			
Years post hiring	At hiring	1	2	3	At hiring	1	2	3
$\hat{\beta}_{2010}$	-0.003	0.002	-0.004	-0.002	-0.009	-0.017***	-0.011	-0.014**
	(0.008)	(0.006)	(0.008)	(0.006)	(0.007)	(0.005)	(0.007)	(0.005)
$\hat{\beta}_{2011}$	0.005	-0.003	0.010	0.011	-0.003	-0.016***	-0.000	-0.004
	(0.007)	(0.005)	(0.007)	(0.007)	(0.005)	(0.004)	(0.004)	(0.006)
$\hat{\beta}_{2012}$	0.003	-0.003	0.005	0.006	0.002	-0.012***	-0.003	-0.001
	(0.004)	(0.006)	(0.008)	(0.007)	(0.005)	(0.003)	(0.007)	(0.005)
$\hat{\beta}_{2013}$	-0.001	-0.003	0.007	0.004	-0.007	-0.012***	0.003	-0.007
	(0.004)	(0.003)	(0.004)	(0.006)	(0.006)	(0.004)	(0.005)	(0.005)
$\hat{\beta}_{2014}$	-0.013***	0.000	0.001	0.001	-0.011**	0.001	0.001	-0.000
	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.007)	(0.005)
$\hat{\beta}_{2016}$	-0.024***	-0.026***	-0.019**	-0.015**	-0.012*	-0.019***	-0.016*	-0.015***
	(0.007)	(0.007)	(0.009)	(0.006)	(0.007)	(0.004)	(0.009)	(0.005)
$\hat{\mu}_{2015}$	0.344***	0.205***	0.190***	0.185***	0.316***	0.201***	0.186***	0.184***
	(0.028)	(0.020)	(0.020)	(0.019)	(0.014)	(0.007)	(0.007)	(0.007)

Notes: Standard errors are clustered at the NACE-2 digit level and reported in parentheses. ***,**,* denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table C.5. The placebo effects on growing employers

Outcomes	At hiring		After one year		After two years		After three years	
	Abs	Rel	Abs	Rel	Abs	Rel	Abs	Rel
Employment								
Full-time+Part-time	0.016*	0.008*	0.035	0.020	0.021	0.013	0.068**	0.042**
	(0.009)	(0.004)	(0.029)	(0.017)	(0.025)	(0.015)	(0.029)	(0.018)
Full-time	0.024	0.020	0.017	0.016	0.016	0.016	0.034	0.034
	(0.023)	(0.019)	(0.029)	(0.028)	(0.025)	(0.025)	(0.025)	(0.026)
Part-time	-0.008	-0.008	0.018	0.026	0.005	0.007	0.034**	0.054**
	(0.026)	(0.028)	(0.032)	(0.046)	(0.014)	(0.021)	(0.016)	(0.023)
Pr Emp≥1	-	-	0.005	0.005	-0.007	-0.008	-0.005	-0.007
			(0.008)	(0.009)	(0.006)	(0.007)	(0.006)	(0.008)
Pr Emp≥2	-	-	-0.002	-0.003	0.011	0.021	0.016	0.034
			(0.011)	(0.019)	(0.012)	(0.022)	(0.012)	(0.026)
Pr Emp≥3	0.007*	0.083*	0.008	0.050	0.009	0.048	0.020**	0.100**
	(0.004)	(0.046)	(0.010)	(0.065)	(0.007)	(0.038)	(0.010)	(0.048)
Pr Emp≥5	0.001	0.149	0.007***	0.501***	0.000	0.013	0.011***	0.259***
	(0.001)	(0.218)	(0.002)	(0.170)	(0.003)	(0.093)	(0.003)	(0.080)
Wage bill								
Wage bill (€)	-161	-0.018	195	0.020	246	0.025	336	0.034
	(114)	(0.012)	(143)	(0.015)	(156)	(0.016)	(212)	(0.022)
Pr>12 000	-0.008	-0.031	0.018*	0.056	0.018*	0.056*	0.013	0.041
	(0.009)	(0.033)	(0.011)	(0.036)	(0.010)	(0.032)	(0.009)	(0.030)
Firm production								
Turnover (€)	-2,584	-0.021	-1,204	-0.010	-1,516	-0.013	255	0.002
	(2,506)	(0.019)	(2,245)	(0.018)	(2,686)	(0.022)	(1,916)	(0.016)
Pr>€120 000	-0.000	-0.000	0.003	0.010	-0.002	-0.006	0.011*	0.040
	(0.009)	(0.032)	(0.008)	(0.027)	(0.007)	(0.024)	(0.007)	(0.025)
Value added (€)	983	0.033	2,467**	0.080**	2,039**	0.066**	1,481	0.048
	(929)	(0.032)	(995)	(0.032)	(871)	(0.029)	(1,134)	(0.036)
Pr>€40 000	0.009	0.032	0.016*	0.056*	0.015*	0.052*	0.007	0.026
	(0.011)	(0.042)	(0.009)	(0.031)	(0.009)	(0.029)	(0.009)	(0.031)
Profit (€)	1,432	0.074	2,405***	0.125***	1,789**	0.093**	1,100	0.058
	(901)	(0.049)	(900)	(0.048)	(789)	(0.043)	(1,063)	(0.056)
Pr>€30 000	0.006	0.022	0.017*	0.062*	0.012	0.047	0.011	0.043
	(0.010)	(0.039)	(0.009)	(0.033)	(0.010)	(0.037)	(0.010)	(0.040)

Employer quality								
Labor-output productivity	-0.081	-0.005	0.017	0.001	-0.442	-0.026	-0.399	-0.024
	(0.423)	(0.025)	(0.257)	(0.016)	(0.315)	(0.018)	(0.376)	(0.021)
Pr>24	0.004	0.020	0.004	0.026	-0.005	-0.029	-0.003	-0.017
	(0.007)	(0.040)	(0.005)	(0.031)	(0.006)	(0.032)	(0.005)	(0.030)
Labor–value added productivity	0.194	0.046	0.247*	0.058*	0.084	0.019	-0.071	-0.016
	(0.133)	(0.032)	(0.131)	(0.032)	(0.110)	(0.025)	(0.173)	(0.038)
Pr>8	0.016**	0.104**	0.022***	0.153***	0.000	0.000	0.002	0.012
	(0.006)	(0.045)	(0.008)	(0.055)	(0.005)	(0.031)	(0.008)	(0.054)

Notes: For each point in time, the first column shows the absolute effect, the coefficient on Cohort 2016, and the second column shows the relative effect, by dividing the coefficient on Cohort 2016 by the mean outcome of Cohort 2015. Emp is short for the number of employees, full-time plus part-time numbers. The thresholds of €12,000 (wage bill), €120,000 (turnover), €40,000 (value added), €30,000 (profit), 24 (turnover/wage bill), and 8 (value added/wage bill) are approximately the top quintile (precisely, the top 20.2%, 19.7%, 20.2%, 20.5%, and 20.1%) among Cohort 2015, one year post hiring. The standard errors, clustered at the NACE-2 digit level, are reported in parentheses. ***,**,* denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table C.6. RDiT quarterly estimates

Outcomes	At hiring		After one year		After two years		After three years	
	Abs	Rel	Abs	Rel	Abs	Rel	Abs	Rel
Employment								
Full-time+Part-time	-0.082**	-0.051**	-0.005	-0.003	-0.069	-0.048	-0.076	-0.052
	(0.036)	(0.024)	(0.043)	(0.029)	(0.042)	(0.030)	(0.048)	(0.033)
Full-time	0.015	0.018	0.027	0.032	-0.030	-0.035	-0.053	-0.059
	(0.025)	(0.030)	(0.034)	(0.041)	(0.037)	(0.042)	(0.044)	(0.047)
Part-time	-0.097***	-0.129**	-0.032	-0.052	-0.039*	-0.069	-0.022	-0.040
	(0.028)	(0.051)	(0.028)	(0.048)	(0.023)	(0.044)	(0.024)	(0.042)
Pr Emp≥1	-	-	0.039***	0.058***	0.011	0.019	0.010	0.019
			(0.006)	(0.008)	(0.010)	(0.018)	(0.009)	(0.017)
Pr Emp≥2	-0.030**	-0.120**	-0.011	-0.036	-0.017*	-0.059*	-0.018*	-0.064*
	(0.012)	(0.051)	(0.012)	(0.040)	(0.010)	(0.035)	(0.010)	(0.035)
Pr Emp≥3	-0.013*	-0.106*	-0.011	-0.067	-0.023***	-0.129***	-0.012	-0.069
	(0.007)	(0.063)	(0.009)	(0.056)	(0.007)	(0.043)	(0.008)	(0.047)
Pr Emp≥5	-0.012***	-0.230**	-0.008	-0.112	-0.012**	-0.155**	-0.011**	-0.132**
	(0.004)	(0.094)	(0.006)	(0.088)	(0.005)	(0.068)	(0.005)	(0.064)
Wage bill								
Wage bill (€)	-61.3	-0.014	289	0.039	186	0.025	-193	-0.022
	(105.4)	(0.023)	(242)	(0.034)	(268)	(0.036)	(293)	(0.033)
Pr>12 000	-0.008	-0.106*	0.005	0.029	0.009	0.047	0.001	0.007
	(0.005)	(0.064)	(0.009)	(0.050)	(0.008)	(0.040)	(0.009)	(0.041)
Firm production								
Turnover (€)	-4,134	-0.067	-3,217	-0.042	-3,494	-0.045	-4,530	-0.054
	(3,205)	(0.051)	(2,740)	(0.033)	(3,424)	(0.042)	(3,364)	(0.038)
Pr>€120 000	-0.014	-0.101	-0.010	-0.055	-0.010	-0.053	-0.015	-0.072*
	(0.011)	(0.079)	(0.010)	(0.049)	(0.011)	(0.057)	(0.009)	(0.043)
Value added (€)	5.61	0.000	-1,118*	-0.051*	-96.4	-0.004	-119	-0.004
	(846.35)	(0.058)	(653)	(0.029)	(1,002.3)	(0.043)	(754)	(0.028)
Pr>€40 000	-0.007	-0.050	-0.005	-0.027	-0.005	-0.025	-0.009	-0.038
	(0.009)	(0.068)	(0.008)	(0.039)	(0.009)	(0.046)	(0.006)	(0.028)
Profit (€)	302	0.032	-1,638***	-0.120***	-308	-0.021	-263	-0.016
	(938)	(0.099)	(548)	(0.040)	(939)	(0.064)	(669)	(0.040)
Pr>€30 000	-0.011	-0.066	-0.011	-0.059	-0.007	-0.037	-0.004	-0.021
	(0.011)	(0.066)	(0.007)	(0.037)	(0.012)	(0.062)	(0.007)	(0.035)

Employer quality								
Labor-output productivity	-2.46	-0.055	-3.23***	-0.159***	-3.26***	-0.172***	-2.87***	-0.160***
	(2.06)	(0.045)	(0.72)	(0.030)	(0.88)	(0.043)	(0.83)	(0.044)
Pr>24	-0.011	-0.030	-0.038**	-0.176***	-0.031*	-0.165**	-0.023*	-0.128*
	(0.015)	(0.038)	(0.016)	(0.063)	(0.017)	(0.080)	(0.013)	(0.068)
Labor–value added productivity	0.296	0.027	-0.724***	-0.124***	-0.601**	-0.111**	-0.404*	-0.074*
	(0.688)	(0.063)	(0.207)	(0.037)	(0.243)	(0.046)	(0.241)	(0.044)
Pr>8	-0.004	-0.013	-0.031***	-0.147***	-0.033**	-0.169**	-0.029***	-0.150***
	(0.018)	(0.052)	(0.008)	(0.035)	(0.014)	(0.066)	(0.007)	(0.035)

Notes: For each point in time, the first column shows the absolute effect, the coefficient on Cohort 2016, and the second column shows the relative effect, by dividing the coefficient on Cohort 2016 by the mean outcome of Cohort 2015. Emp is short for the number of employees, full-time plus part-time numbers. The thresholds of €12,000 (wage bill), €120,000 (turnover), €40,000 (value added), €30,000 (profit), 24 (turnover/wage bill), and 8 (value added/wage bill) are approximately the top quintile (precisely, the top 20.2%, 19.7%, 20.2%, 20.5%, and 20.1%) among Cohort 2015, one year post hiring. The standard errors, clustered at the NACE-2 digit level, are reported in parentheses. ***,**,* denote statistical significance at the 1%, 5%, and 10% level, respectively.