# Payroll tax incidence for hiring the first employee

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**Abstract**

This paper studies payroll tax incidence at firm entry, a setting rarely examined in the literature. We exploit a 2016 Belgian reform that permanently exempted new employers from payroll taxes for their first employee, sharply reducing hiring costs and encouraging entry of lower-productivity firms. Using administrative data and a cohort-based difference-in-differences design, we find that the reform reduced payroll taxes while raising both wage rates and working time. To interpret these results, we develop a unified model linking firm entry, firm productivity, and tax incidence. We then propose a theory-guided trimming method to address compositional changes of firm entry. The yielded estimates remain close to the baseline results, suggesting productivity differences matter little for entry incidence. Employees capture about one-third of the payroll tax relief on a per-FTE basis, which suggests the importance of wage bargaining at firm entry.

*JEL* codes: D20, H22, H55, J31, J38, J23

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# 1 Introduction

Payroll taxes, primarily consisting of social security contributions (SSCs), are a major source of public revenue and a key determinant of labour costs. In OECD countries, SSCs account for roughly one quarter of total tax revenues, making them the largest component of income-based taxation (OECD, 2024). A vast literature in public and labour economics studies their *incidence*—how statutory payroll taxes are borne by employers and workers through wages and employment.

Most empirical evidence concerns established *incumbent* firms, where tax changes affect ongoing wage-setting relationships and where the composition of firms and jobs is largely fixed (e.g. Bozio et al., 2025; Lehmann et al., 2013; Saez et al., 2012, 2019, 2021). At that margin, incidence depends mainly on the relative elasticities of labour supply and demand, and modern evidence points to partial incidence on workers: both sides of the market bear part of the burden. These evidences usually contrast the canonical incidence view that employees absorb nearly all payroll taxes because their labour supply is much less elastic than firms' labour demand (Benzarti, 2025; Gruber and Krueger, 1991).

However, researchers rarely study payroll tax incidence *at entry*—the moment when a solo entrepreneur becomes an employer by hiring their first worker.[1] In standard theory of industry dynamics, entry plays a pivotal role in shaping market performance: fixed entry costs and heterogeneous productivity draws jointly determine not only how many firms operate but also the aggregate distribution of productivity, wages, and employment (e.g. Asplund and Nocke, 2006; Ericson and Pakes, 1995; Jovanovic, 1982; Melitz, 2003). Because every incumbent firm was once an entrant, the incidence pattern at entry provides crucial insights into the long-run distributional and efficiency consequences of labour taxation over the life cycle of firms. As only a small fraction of small firms ever make it to grow larger (Sterk et al., 2021), do small entrants respond differently

[1] Throughout this paper, we define 'entry' as into the employer status by hiring the first employee, but not entry into the business status of firm operation. We shall also use 'entrant' and 'new employer' interchangeably.

to payroll taxes as large incumbents?

This paper thus bridges the gap and studies how payroll taxes are borne by employers and employees when a firm first enters the market as an employer. Belgium offers a sharp and distinctive context for studying payroll tax incidence at entry. Employer SSCs[2] are high by international standards, while business dynamism[3] remains comparatively low among OECD economies (Bijnens and Konings, 2020; Dumont, 2021). To improve labour market competitiveness, the Belgian government introduced the *zéro coti* ('zero contribution') reform in 2016, granting prospective new employers a *permanent* exemption (hiring subsidy) from paying SSCs on their first employee, provided that the hire occurred on or after 1 January 2016. This measure represented the most generous payroll tax cut for small firms, and the nominal tax relief amounted to 20% of the gross wages of that first employee. It substantially increased the probability of employer entry in the labour market by 31% (Cockx and Desiere, 2024), but these *excess* entrants were of lower productivity than normal entrants by about half (Deng et al., 2026).

The primary role of this paper empirically studies the effects on wages and employment among the first hires. We employ a cohort-based difference-in-differences design with matched employer–employee administrative data that record employment, wages, and SSCs. Because the tax exemption applies from the moment of employer creation, no within-firm before–after variation is available. Instead, we exploit variation across entrant firms' cohort time, when which they hired their first employee, and contrast the wage rates (the price of labour) and employment (the quantity of labour) between post-reform entrants and pre-reform ones. To control for business-cycle effects, we add a comparison group of small incumbent firms of similar growing employers that were ineligible for the 2016 *zéro coti* and thus unaffected by the reform. This simple DiD method

[2]Throughout this paper, we use SSCs and payroll taxes interchangeably, referring specifically to the employer's share rather than employee contributions. We also assume that employees earn their gross wages including their shares of the SSCs.

[3]Dumont (2021) claims that both the entry and exit rates of Belgian small firms have stayed consecutively low and lower than most OCED countries.

assumes that the composition of new entrants do not matter for the treatment effects (we shall return to this point later).

Our DiD strategy yields robust findings for both the wage and employment margins of the initial hires. In the canonical incidence framework, tax changes shift labour supply and demand at the aggregate market level. In our setting that only affected the labour market for micro firms (where the average entrant employs only 1.5–1.6 workers), we capture the employment margin at the individual firm level through hours worked and retention rather than at the aggregate level. Working time rose: retention improved by 1.6–2.6 pps over one to three years, and the average full-time equivalent (FTE) rate increased by 2.1–2.4 pps (about 12 hours per quarter). The relative increases amounted to around 5%. Wage rates per FTE for entrants' initial hires increased modestly around the reform (about €80 per quarter, up to 1.2% compared to pre-reform wage rates), while employer payroll taxes per FTE fell sharply (about €140–186 per quarter, up to 18% compared to pre-reform). These opposite movements left per-FTE labour costs broadly stable, suggesting that the fiscal relief was largely channelled into higher wages and longer working hours.

Adjustments in working time play a central role in understanding the overall impact of the reform. Once longer hours are accounted for, workers' total quarterly earnings rose by more than €200, around 5% relative to pre-reform averages, while employers' payroll tax payments declined by about 20%. These effects indicate that even modest wage increases, when combined with higher working time and retention, translated into sizeable income gains for employees. This might seem modest, but the permanency feature of the reform implies substantial gains to both parties over time. While large-firm studies typically abstract from changes in the FTE rate since full-time employment dominates (e.g. Bozio et al., 2025; Saez et al., 2012), our results show that in small entrant firms, where part-time work is common, adjustments in hours constitute an essential channel through which payroll tax cuts affect earnings and labour input.

The estimated effects imply a strong multiplier from the nominal tax relief to employee earnings. Per FTE, the rise in gross wages amounts to about 0.6 times the tax relief, while on a per-employee basis; once higher working time is incorporated, the multiplier exceeds one, around 1.4. This reflects substantial employee gains without additional employer burden, since labour costs (per FTE or per employee) remained essentially unchanged. Because the subsidy is not exactly equal to the combined effect of higher wages and lower taxes, we redefine the incidence share as the ratio of the wage(-rate) effect to the sum of the wage(-rate) and tax-saving effects. Following the logic of Bozio et al. (2025) and Kline et al. (2019), we estimate that workers captured roughly one-third of the relief on a per-FTE (price) basis. This share doubles to two-thirds when accounting for longer working hours. The latter finding underscores that payroll tax cuts can raise effective earnings in small entrant firms even when total labour costs stay flat.[4]

Our policy setting involves three empirical complications. First, only one employee per entrant firm was eligible for the payroll tax exemption, creating a dilution effect since entrant firms hired on average 1.5–1.6 employees at entry. Second, due to informational frictions and behavioural biases (Boucq and López Novella, 2018a), only about 50–70% of eligible entrants actually claimed the subsidy. These two features[5] imply that the realised treatment intensity was lower than the statutory generosity, so our main estimates should be interpreted as intent-to-treat effects. When restricting the sample to the explicitly designated *first employee* in payroll filings (those directly eligible for the exemption), we find wage and working-time effects roughly 50–100% larger. The corresponding incidence shares are also higher in the first year after hiring but gradually converge toward the baseline average by the third year. This convergence suggests

[4] In this paper, the main outcomes of interest include both the *wage rate (per FTE)* and the *earnings per employee*, because in very small firms, adjustments occur through working hours and contract duration rather than through hourly wage renegotiation. These adjustments reflect how the fiscal relief is transmitted to employees in practice. Hence, our 'per-employee total' measure does not redefine tax incidence theoretically, but provides an empirical complement that captures the realised distribution of fiscal benefits between firms and workers when hours are flexible.

[5] They reflect the institutional design of the policy, which targeted only entrants. In contrast, most tax-incidence studies exploit economy-wide tax changes that apply automatically to a majority of firms or workers, such as in Benzarti and Carloni (2019); Fuest and Neumeier (2023); Fuest et al. (2018); Kennedy et al. (2024).

intra-firm wage alignment consistent with the fairness mechanism emphasised by Saez et al. (2019, 2021), whereby firms tend to equalise treatment across similar workers once wage norms are established.

The third complication concerns composition. The reform not only changed wage and employment outcomes but also affected which firms entered as employers, leading to excess entry by relatively low-productivity firms. This selection margin is crucial for interpreting incidence, because the measured sharing of tax relief may depend on who enters the market. Firms that would have hired regardless of the tax cut (*inframarginal entrants*) differ systematically from those induced to hire only after the reform (*marginal entrants*). If these new, less productive, marginal firms adjust wages or hours differently, simple comparisons of pre- and post-reform cohorts may conflate behavioural responses with compositional changes.

To rationalise our empirical findings on wage and employment adjustments alongside this excess entry of lower-quality firms, we develop a partial-equilibrium model of small firms' labour-market behaviour that jointly explains firm entry responses and payroll tax incidence. The framework builds on the classic view of heterogeneous managerial ability (Evans and Jovanovic, 1989; Jovanovic, 1982; Lucas, 1978), combined with a competitive labour market for workers following Bozio et al. (2025); Feldstein (1974); Gruber (1997). Entrepreneurs differ in their managerial ability (also the firm productivity), which determines the marginal product of labour and thus in equilibrium whether hiring a worker is profitable compared to solo self-employment.

At the entry stage, the hiring decision is itself endogenous to policy: a payroll tax cut may not only change wages but also who enters as an employer. From a *market-participant* aspect, this 'entry incidence' combines the intensive margin of wage bargaining with the extensive margin of new firm formation, making the standard assumption of fixed firm population inadequate. From a *market-clearing* perspective, payroll tax incidence at entry matters because firm entry and exit shape the long-run demand for labour. By endogenising the entry threshold, equilibrium wage, and labour demand elasticity, our model predicts that payroll tax reductions shift down the profitability threshold, enabling less productive firms to enter and hire their first employees.

The model delivers two main insights. First, the productivity threshold for becoming an employer is inversely related to the payroll tax rate, so lowering taxes induces entry by lower-productivity firms and generates the observed 'excess entries'. Second, wage elasticities with respect to payroll taxes depend on an *effective* labour demand elasticity that combines the intensive margin (within-market labour adjustment) and the extensive margin (new firm entry). Employment effects at entry amplify the aggregate elasticity of labour demand, implying that employers at the entry margin ultimately bear a significant share of the tax change through profits, while part of the relief is passed on to workers once employment relationships are established. In turn, in sub-markets where labour demand is more elastic, wage increases are correspondingly larger. When entry is held fixed, our framework collapses to the competitive-incidence for incumbents (Benzarti, 2025; Harberger, 1962), aligning with recent evidence of partial but incomplete pass-through of employer-side payroll tax cuts to wages (Bennmarker et al., 2009; Benzarti and Harju, 2021; Rubolino, 2022; Saez et al., 2012, 2019, 2021).

We next address the issue of excess entry using a sample-trimming selection correction guided by our theoretical framework. The approach draws on the intuition of Lee (2009) but adapts it to a firm entry context in which marginal entrants have no observable counterfactual outcomes. Because their employment and wages are undefined in the absence of participation, their incidence cannot be recovered, and we thus aim to exclude them from our analyses. We first implemented Lee-type agnostic bounds, but the resulting intervals always included zero and were therefore uninformative. To obtain sharper estimates, we use our model's predictions to guide a transparent selection correction. Specifically, we estimate each firm's hiring propensity using pre-reform sales, profits, and other characteristics that proxy productivity, and trim the lowest-propensity post-reform entrants by the fraction of excess entry estimated in Cockx and Desiere (2024). This approach recovers a common support comparable to that in the propensity score literature (Abadie and Imbens, 2006, 2011) and targets the incidence among inframarginal firms.

The trimming results confirm that composition effects from subsidy-induced excess entry do not drive our conclusions. Removing the lowest-propensity entrants leaves the main coefficients virtually unchanged, and if anything, the estimated worker-side incidence share rises slightly. This modest shift reflects that new, low-productivity entrants typically pay lower wages, but such differences are largely muted by Belgium's sectoral wage-setting institutions[6] which compress wage dispersion and restrict downward flexibility for first hires. Hence, while firm productivity shapes entry decisions, it plays a limited role in explaining the observed incidence pattern, which appears to be driven jointly by institutional wage rigidity and the limited monopsony power of small firms in setting wages (Card et al., 2018; Manning, 2021).

**Our contribution is multifold.** Primarily, we extend the tax incidence literature by documenting how payroll tax incidence operates at the *firm entry margin*. While most existing evidence concerns ongoing employment relationships in established firms, we examine the first-hire stage where wage norms and bargaining relationships are still forming. We find that incidence at entry is substantial yet incomplete: roughly one-third of the payroll tax relief is passed on to employees through higher wage rates, consistent with the canonical view of partial pass-through (Bennmarker et al., 2009; Benzarti and Harju, 2021; Rubolino, 2022; Saez et al., 2012, 2019, 2021). Once working-time adjustments are accounted for, the employee share rises to about two-thirds, highlighting an amplification channel often neglected in standard incidence analyses. These findings are in line with recent reviews emphasising that labour-demand elasticities are endogenous to

[6]Belgium has high level of collective bargaining coverage at the level of joint committee, where minimum wages and fixed pay scales negotiated for a specific type of profession or industry. See Momferatou et al. (2008); OECD (2020). However, we do acknowledge that micro firms might not stick perfectly to these collective bargaining agreements and have higher degrees of flexibility in paying their own wages than large firms.

payroll taxation (Benzarti, 2025): if firms can adjust their production e.g. by demanding more labour, the tax incidence patterns can vary. Alternatively, one can argue that exact levels of incidence parameters can be definition-dependent (Melguizo and González-Páramo, 2013).

Second, we directly answer the discussion of the *insurance-premium* interpretation of SSCs, which views them not merely as a regular tax but as a deferred wage payment linked to future employee benefits. In systems where the contribution–benefit linkage is strong, employees tend to internalise these SSC changes, leading to near-full wage pass-through (Bozio et al., 2025; Gruber, 1994, 1997; Lehmann et al., 2013). In contrast, when this linkage is weak, such as in our case of employer-side exemptions, the pass-through should be lower. The 2016 Belgian reform illustrates this clearly: the exemption had to be claimed by employers, required administrative awareness and correct filing; additionally, it was tied to the firm rather than to individual employees while employee separations were high. Under such conditions, wage pass-through is far from complete, and part of the fiscal relief remains with employers. This result helps reconcile that the statutory side of taxes still matters for incidence. Quantitatively, our incidence share estimates are close to continental European averages reported in meta-analyses by Melguizo and González-Páramo (2013), who find that employees bear around two-thirds.

Third, we also contribute to the literature on entrepreneurship and policies supporting small firms. A large body of evidence shows that firm entry and entrepreneurship are highly sensitive to regulatory and labour cost conditions (Branstetter et al., 2014; Hombert et al., 2020; Klapper et al., 2006; Suárez Serrato and Zidar, 2016). In particular, changes in statutory employer labour costs directly affect how many firms begin hiring employees (Cockx and Desiere, 2024; Guo and Wallskog, 2025; Patel, 2019). The fact that new-employer entry rises when hiring costs fall, or vice versa, implies that part of the labour cost burden must be borne by employers themselves rather than being fully shifted to workers. Although the canonical view (full pass-through to employees) is usually contradicted by modern evidence, little is known about *how much of* the tax burden is exactly borne by the firm at the moment of entry, when wage norms and bargaining relationships are still emerging, and search frictions are strong (Fairlie and Miranda, 2017; Hardy and McCasland, 2023). This in turn determines the degree of which reducing tax charges can alleviate firms' financial constraints.

Our setting helps to better understand why hiring-subsidy programmes may show limited wage effects such as Cahuc et al. (2019), which document employment gains without wage responses, our results reveal that the institutional timing of the policy matters: because the exemption we study applies precisely at the moment of hiring, wages can adjust immediately at entry rather than through later bargaining. This mechanism allows both employers and employees to benefit from the reform. Moreover, we extend the notion of *rent sharing* beyond wage adjustments: employers share part of the tax relief by offering longer working hours and more stable employment, not merely higher pay, reflecting a broader form of sharing than typically emphasised in the literature (Card et al., 2018, 2014, 2013).

The remainder of the paper proceeds as follows. Section 2 outlines the institutional background and describes the evolution of the tax cut schemes. Section 3 presents the sample construction, variable definitions, and the empirical strategy. Section 4 discusses the baseline results and robustness checks. Section 5 develops the theoretical model linking incidence and firm entry, upon which, Section 6 proposes new strategies to cope with excess entries and present the results. Section 7 concludes.

## 2 Policy and backgrounds

This section introduces the payroll taxation system in Belgium, the hiring subsidy for small firms' first employee, and the 2016 reform of payroll tax exemption for this first employee.

## 2.1 Payroll taxation

In Belgium, payroll taxes, often used interchangeably with social security contributions (SSCs), are levied on gross wages and borne by both employers and employees. These contributions finance a wide range of social protection schemes, including pensions, healthcare, unemployment insurance, occupational accident coverage, and paid leaves.[7]

Payroll taxes are declared on a quarterly basis to the National Social Security Office (ONSS) of Belgium. The employees' share of the payroll tax rate has been stable (not changed at least since 2010): they pay 13.07% of their gross wages to the social security system. The employers' share of the payroll tax rate has gone through significant evolutions: before 2016, the statuary rate was roughly 30–35%; since 2017 this rate gradually decreased, and currently is 27%. These tax rates have remained higher than most countries in the world, making employment costly especially for small firms.

To ease small firms' cost burden of hiring employees, Belgium provides a long-standing set of payroll tax reductions known as *les premiers engagements* ('the first recruitments') (Cockx et al., 2025). These subsidies have existed since 2004 and apply to firms currently with five or fewer employees and reduce employer-share payroll taxes for each successive new hire, i.e., from the first up to the sixth employee. The most generous benefits are reserved for the first hire, and the benefits for subsequent employees gradually decrease.

For these six hires, firms have historically (before the reform year 2016) received capped and time-limited reductions in their employer contributions, lasting up to 13 quarters. Over our sample periods, *les premiers engagements* have gone through three minor reforms (2012Q4, 2014Q1, 2015Q1) that increased the quarterly caps and durations, meaning employers hiring in later times (i.e., later cohorts[8]) were granted higher tax benefits.

[7]The term 'social security' thus carries a broader meaning in Belgium than in countries such as the United States, where it typically only refers to pension, military survivor, and disability insurance.

[8]In this paper, we shall frequently use the term 'cohort' in the policy description and in our

Figure 1 illustrates how the generosity of the *les premiers engagements* subsidy evolved across hiring cohorts and employee ranks. Generosity depends on two elements: the duration of eligibility (number of subsidised quarters) and the quarterly subsidy cap. To summarise both dimensions, we compute for each hire the cumulative maximum subsidy—i.e. the sum of quarterly caps over all eligible quarters. The detailed parameters underlying these calculations are reported in Table A1.

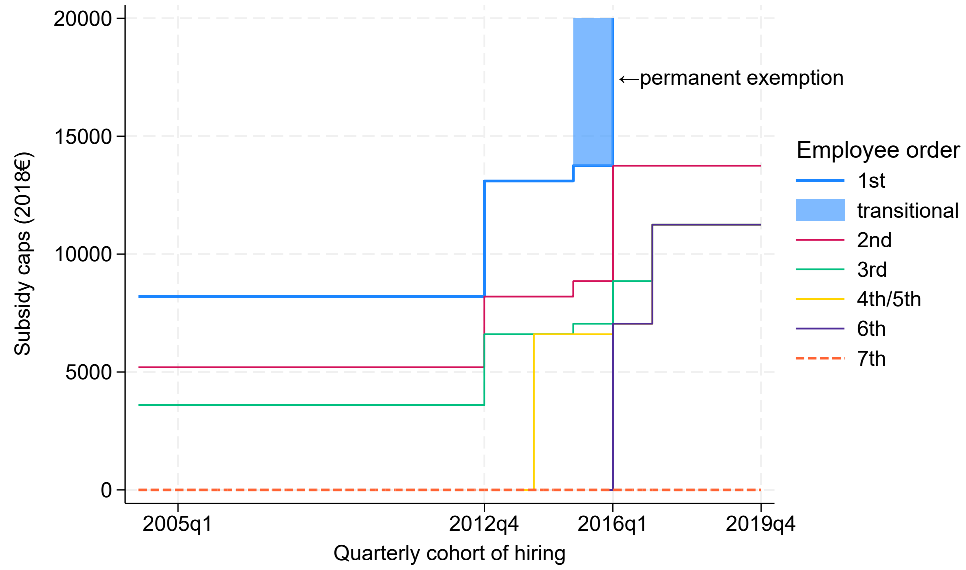



**Figure 1.** Maximum tax cuts, by employee order and hiring cohort

The horizontal axis indicates the hiring quarter, and the vertical axis shows the corresponding cumulative maximum subsidy. In each quarter, the realisable tax reduction is roughly proportional to (with some complex calculation rules) the gross earnings associated with the position: firms paying higher wages, or offering full-time rather than part-time employment, receive larger subsidies, up to the maximum amount specified in Table A1.

Each of the coloured polylines represents one employee rank, from the first to the sixth hire. Initial hires within a firm (lower employee ranks) always appear above subsequent ones, reflecting at least equal, and often greater, generosity;

analyses. A cohort of time $t$ means a group of firms that hired new employees at quarter $t$; it also means those new employees hired by the firms.

the polyline for a given employee order goes up vertically at the reform time when that hire is affected by the reform: either through longer duration or higher quarterly caps. Hence, overall generosity rises across cohorts but declines with the order of hiring within firms.

Note that the *les premiers engagements* subsidies for the second to sixth hires also evolved across cohorts, while hires beyond the sixth have never been subsidised. Hence, when referring to a given cohort time, this can equally denote the timing of these subsequent hires, not only that of the first hire.

## 2.2 The 2016 reform: *zéro coti*

In 2016Q1, the government introduced a permanent and uncapped exemption from payroll taxes (employers' share) for the first employee, applicable to new employer cohorts from 2016Q1 onward. This *zéro coti* ('zero contribution') measure replaced the previous regime of quarterly caps and finite durations. As a result, for the same employee (same contract type, same salary), a firm hiring its first employee in early 2016 faced a substantially lower cost burden than one hiring in late 2015. The was the most substantial reform in *les premiers engagements*, significantly increasing the generosity of this series of hiring subsidy.

Without prior circulation, it was announced one quarter earlier in 2015Q4, as part of a broader nationwide major fiscal reform named 'Tax Shift', and one of its main goals was to *shift* the taxation of labour (wages) to that of wealth and capital, thus improving Belgium's labour market competitiveness.[9]

Figure 1 marks this reform with a vertical line section for the first employee (1st) at time 2016Q1, indicating the upper limit of subsidies were entirely removed. This means that all the future cohorts of new employers, i.e., entrant firms into the labour market on the demand side, can benefit from this first-employee payroll tax exemption.

Simultaneously, this raised fairness concerns among firms that had just hired

[9]In this paper, we only study this reform *zéro coti*, which resided within the hiring subsidy series *les premiers engagements* but not the other contents of the 'Tax Shift'.

before its announcement. In response, the government introduced a *transitional arrangement* for Cohort 2015 entrants, granting them a partial top-up benefit towards that received by Cohort 2016. The duration of their subsidy remained unchanged (as 13 quarters), but its generosity increased—from capped amounts to temporary exemption—for the remaining eligible period starting from calendar time 2016Q1. This transitional measure is marked with a shaded block for Cohort 2015 firms hiring their first employee.

## 2.3 Empirical challenge: excess entry

Before turning to *individual*-level outcomes, it is important to address two *aggregate* challenges introduced by the 2016 reform: the surge in employer entry and the imperfect take-up of the subsidy. Both phenomena affect how we interpret observed firm behaviour and outcomes in the post-reform period.

**Excess entry and lower firm quality.** Although the reform was clearly announced and thus anticipated, it not only increased generosity but also altered incentives and composition. The expected cost of becoming an employer fell sharply, likely inducing marginal or less productive firms to hire or influencing whom they chose to hire. These changes mean that post-reform cohorts differ from earlier ones not only in treatment status but also in firm characteristics.

A direct and observable effect of the reform was a sharp increase in employer entry, as shown by the solid pink line in Figure 2a. Before 2016Q1, about 5,000 new employers hired their first employee each quarter; in the year directly following the reform, this number rose to between 6,000 and 7,000. Existing studies show that both the quantity and the quality of entrant firms significantly differed as a response to the reform.

On the quantity side, using monthly (a finer level than here) ONSS data and a regression-discontinuity-in-time design, Cockx and Desiere (2024) estimate that the reform decreased the employers' labour costs by 13%[10] and increased

[10]Their paper is only about the hiring margin response and assumes that employers bore full

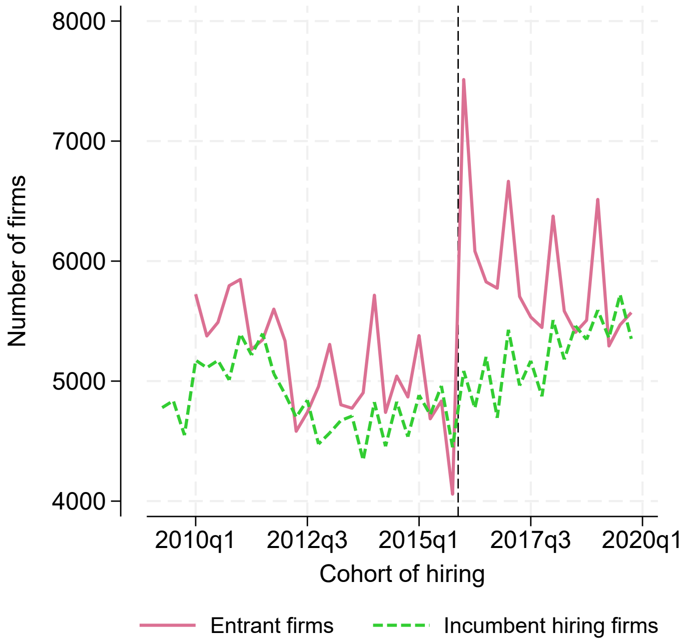
new-employer entry by 31%, implying a high entry elasticity with respect to cost reduction. As a sanity check, the dashed green line in Figure 2a plots the number of incumbent firms hiring additional employees, which had a pre-hiring firm size of 6 to 10 employees and thus were not eligible for any additional hiring subsidies according to Figure 1. Around the reform date, this sample size evolved smoothly, reassuring that the broader economic environment in 2016 was relatively steady.

On the quality side, Deng et al. (2026) find that the reform encouraged relatively low-productivity firms to enter and remain with higher survival rates in the market. Compared to entrants from Cohorts 2013–2015, Cohorts 2016 of entrants on average had lower labour productivity (10%; their innate abilities) and achieved worse firm performance (4–8%; smaller firm size) in sales, profit, and employment. This dynamic is consistent with a broader body of evidence showing that when entry barriers or fixed costs are relaxed, firm creation rises but the average quality of entrants tends to fall, or vice versa (e.g. Branstetter et al., 2014; Guo and Wallskog, 2025; Klapper et al., 2006; Suárez Serrato and Zidar, 2016).

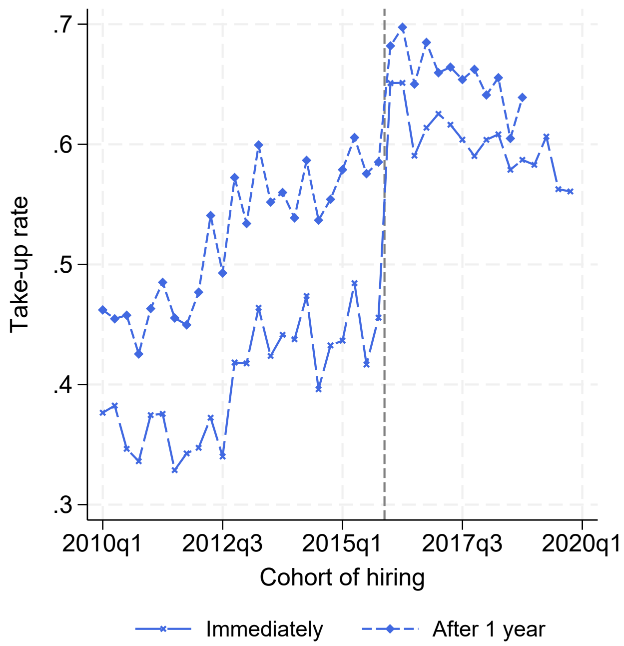
This simple observation carries two key implications. First, the concern is not merely that there are *more* post-reform entrants (*quantity*), but that they are systematically *different* in *quality* and composition. This heterogeneity complicates the empirical design, as part of the observed treatment effect may reflect selection rather than genuine economic behavioural change. Second, the strong entry response itself provides indirect evidence that firms capture at least part of the reform's benefit. If the entire subsidy were passed on to workers through higher wages, there would be little reason for additional firms to enter. The observed increase in employer formation therefore implies that the exemption also raised firms' expected surplus or latent profits, confirming that some of the incidence lies on the employer side.

Such compositional shifts complicate the interpretation of treatment effects if not explicitly accounted for. In what follows, we focus on this entry margin as a

incidence of this payroll tax cut.




**(a)** Number of firms




**(b)** Take-up rate

**Figure 2.** New employer entry and first-employee subsidy take-up rate

*Notes:* Vertical line indicates the reform time (2016Q1). In Panel a, the solid pink line plots the number of employer entries per quarter; the dashed green line the number of incumbent firms hiring additional employees but ineligible for additional hiring subsidies. In Panel b, the blue dots show the take-up rate of the first-employee subsidy among these entrant firms: crosses indicate immediate take-up, and diamonds the share at the end of the first year after hiring.

central identification concern. Section 5 develops a theoretical framework linking firms' entry and hiring decisions to their productivity levels, and then Section 6 proposes an empirical method that ranks firms by productivity to correct for this selection. In this way, the potential imbalance created by excess entry is explicitly modelled and adjusted for.

**Take-up and imperfect exemption.** The second issue concerns the take-up of the subsidy. Although employers did not need a formal application, they had to correctly declare the reduction in their quarterly payroll submissions to the ONSS. They had five years after hiring to activate the reduction, after which eligibility expired (a very generous reflection period). In practice, however, many new employers delayed or never claimed the subsidy, often due to limited administrative knowledge, reliance on inattentive accountants, fear of bureaucracy, or the mistaken belief that the exemption was automatic (Boucq and López Novella, 2018a). As a result, as shown by the blue lines (with diamonds or crosses) in Figure 2, the take-up rate was only about 40–50% of entrants between Cohorts 2013 and 2015.[11] Similar to the increased number of firm entry, this take-up rate also increased significantly after the reform date, leading to around 65% among Cohort 2016.

Unlike the entry problem, this take-up behaviour is not systematically related to firm productivity or other monotone characteristics, but reflects heterogeneous informational and mental biases and administrative frictions that are infeasible to model given typical administrative data. Therefore, rather than attempting to identify or correct this channel directly, we reinterpret the reform not as a literally *zero-tax* exemption but as a highly generous and persistent payroll tax reduction. This interpretation preserves the economic essence of the policy change while ensuring that measurement error in take-up does not undermine the analysis that follows.

[11] These rates are broadly consistent with ONSS-based estimates by Boucq and López Novella (2018b), who report immediate take-up around 45% for the 2013–2015 cohorts.

# 3 Data and empirical strategy

## 3.1 Data source and linkage

We use employer–employee matched administrative data from the National Social Security Office (ONSS), complemented by the Crossroads Bank for Enterprises (CBE) and Value-Added Tax (VAT) declarations. The ONSS records contain quarterly information on all employment relationships in the private sector, including gross wages, working time, and employer social security contributions, which form the core of our analysis. Because the ONSS also administers contribution reductions, these data allow us to observe both statutory and reduced payroll tax amounts for each pair of employer and employee.

The CBE provides firm identifiers, sector and location codes, legal form, and establishment date. VAT records add quarterly turnover, intermediate inputs, and investment. These variables are not outcomes of interest here but later serve to predict firm entry as described by Section 6.2. All monetary values are deflated to 2018 euros, and the observation period spans 2010Q1–2019Q4, before the COVID-19 shock.

## 3.2 Sample definition

**Cohorts of interest.** In the policy section, we defined several policy regimes of the *les premiers engagements* subsidy over time. However, due to the frequent adjustments to the subsidy before 2013 and the transitional design of Cohort 2015, only *Cohorts 2013, 2014, and 2016* are suitable for our main analysis. The earlier cohorts faced different subsidy durations and caps, while the 2015 cohort experienced a retroactive top-up that makes its treatment difficult to interpret consistently with the later regime. Appendix B.1 provides a justification for this decision.

Among the remaining cohorts, we focus primarily on a comparison between 2014 (pre-reform) and 2016 (post-reform). First, these two are close in time and thus less affected by business-cycle differences if any.[12] Second, due to COVID-19, 2016 is the post-reform cohort that can be followed for the longest horizon of three years. After 2017, as discussed in the policy section, the legal payroll tax rates (before applying the hiring subsidy reductions) began to change, making later cohorts less comparable. Hence, Cohorts 2014 (pre-reform) and 2016 (post-reform) serve as the main comparison groups for our incidence analyses.

A timing concern in the reform design is the one-quarter gap between its announcement and implementation. Because the measure specifically targeted new employers, this gap directly affected their hiring timing. To deal with their strategic manipulation, following Cockx and Desiere (2024); Deng et al. (2026), we exclude firms hiring in 2015Q4 and 2016Q1. As the reform was announced in 2015Q4, many potential entrants that would otherwise fall into this cohort postponed their hiring until the first eligible quarter in 2016Q1. This postponement reflects anticipation of the subsidy rather than a behavioural response to the tax cut itself. Including these firms would blur the distinction between marginal and inframarginal entrants and complicate the interpretation of the treatment effect.[13]

A further feature of our cohort construction is that we define cohorts annually rather than quarterly to mitigate seasonality in hiring. We regroup every four adjacent quarters into one annual cohort, following Deng et al. (2026). Hence, Cohort 2014 corresponds to firms hiring between 2013Q4 and 2014Q3, while Cohort 2016 covers hires between 2016Q2 and 2017Q1. We show in the results section that this grouping smooths quarter-specific fluctuations and yields clearer contrasts between pre- and post-reform outcomes.

[12]Although we try to eliminate the business cycle effects by adding the incumbent group in the next paragraph, choosing two close cohorts for comparisons is still the safest option if the business cycles for entrants would differ from those for incumbents.

[13]In Cockx and Desiere (2024); Deng et al. (2026), they use regression discontinuity (RD) in time as the empirical strategy with the time of hiring as the running variable. The strategy to exclude the interval where agents can strategically manipulate their treatment time (time of hiring) is called the 'donut hole' in the RD literature (e.g. Barreca et al., 2011; Benzarti and Harju, 2021; Cattaneo and Titiunik, 2022). Following Benzarti and Harju (2021), Cockx and Desiere (2024) estimated that excluding these two quarters (and not additional later ones) would suffice to ease the bias from this postponing manipulation.

**Entrants and incumbents.** To evaluate the reform, we distinguish between two employer groups. *Entrants* are firms that hire their first-ever employee and become eligible for the first-employee subsidy (corresponding to the blue polyline in Figure 1). *Incumbents* are existing employers with a pre-hiring firm size between 6 and 10 employees that hire additional employees in the same calendar periods. They are also small growing firms, similar to the entrants; yet their additional hiring decisions do not grant them any more hiring subsidies (see the dotted polyline at the bottom in Figure 1). Thus they serve as the control group likely in the same segment of the labour market, capturing aggregate conditions over time unrelated to the reform. Appendix B.2 provides additional sample selection criteria.

**Sample size** Reported by Table 1, the number of hiring firms increased in both the entrant and incumbent groups between 2014 and 2016, but the rise was much larger and clearly discontinuous among entrants, as illustrated by Figure 2a. The number of newly hired employees followed a similar pattern. By contrast, the average number of hires per firm remained relatively stable across cohorts.[14] that the reform encouraged firms to become employers but reduced the incentive to hire multiple employees at once. Overall, however, the average hires per firm did not differ substantially across cohorts, confirming that the expansion in employment largely reflected the sharp increase in the number of new employers rather than a change in firm-level hiring intensity.

### 3.3 Employee characteristics and outcome variables

**Employee characteristics.** Panel A of Table 1 summarises employees' population characteristics. Differences in education, age, and nationality are small across cohorts and groups. Entrant firms consistently hired slightly more female, foreign, and white-collar workers than incumbent firms. The two-by-two com-

[14] Actually, Deng et al. (2026) show that, among entrants, Cohort 2016 hired slightly fewer workers on average than Cohort 2014. This is because the permanent exemption scheme incentivised firms to hire only their first employee to benefit fully from the subsidy, rather than hiring several workers simultaneously.

**Table 1.** Descriptive statistics of new hires by annual cohort and by group

| | Entrant cohorts | | Incumbent cohorts | |
|---|---|---|---|---|
| | 2014 | 2016 | 2014 | 2016 |
| *N* employees | 31,469 | 36,084 | 33,059 | 38,268 |
| *N* firms | 20,400 | 24,349 | 18,451 | 20,098 |
| Hires per firm | 1.62 | 1.54 | 1.97 | 2.06 |
| **A. Population characteristics** | | | | |
| Age | 34.8 | 35.0 | 33.0 | 33.4 |
| | (12.0) | (11.8) | (11.5) | (11.7) |
| Education | | | | |
| High | 18.8% | 18.4% | 19.3% | 16.7% |
| Medium | 43.9% | 42.8% | 43.9% | 43.7% |
| Low | 37.3% | 38.7% | 36.8% | 39.6% |
| Nationality | | | | |
| Belgium | 81.9% | 80.2% | 83.6% | 82.5% |
| EU | 94.4% | 93.0% | 96.0% | 94.9% |
| Male | 57.0% | 58.3% | 60.2% | 61.0% |
| Blue-collar | 59.4% | 58.6% | 62.6% | 62.0% |
| **B. Labour market positions: 1 quarter before hiring** | | | | |
| Working | 51.7% | 52.9% | 43.3% | 47.2% |
| Employed | 47.2% | 47.4% | 39.3% | 43.7% |
| Self-employed | 7.7% | 9.0% | 6.3% | 6.2% |
| **C. Employee separation** | | | | |
| 1 quarter | 24.0% | 23.6% | 28.3% | 29.9% |
| 1 year | 48.2% | 48.1% | 51.8% | 54.3% |
| 3 years | 72.3% | 73.7% | 73.3% | 75.8% |
| **D. Employment and wage records: within 1 year** | | | | |
| Full-time | 21.3% | 23.2% | 21.7% | 22.1% |
| FTE rate | 0.56 | 0.59 | 0.56 | 0.57 |
| | (0.31) | (0.31) | (0.32) | (0.33) |
| Earnings | 4,006 | 4,125 | 4,266 | 4,180 |
| | (2,762) | (2,778) | (2,845) | (2,862) |
| FTE gross wage | 6,716 | 6,681 | 6,857 | 6,738 |
| | (2,379) | (2,332) | (2,380) | (2,303) |
| FTE payroll tax | 952 | 545 | 1,612 | 1,347 |
| | (1,080) | (910) | (1,070) | (1,007) |
| FTE labour cost | 7,668 | 7,232 | 8,473 | 8,088 |
| | (3,235) | (2,879) | (3,340) | (3,187) |
| FTE subsidy | 578 | 788 | 4 | 23 |
| | (662) | (819) | (22) | (105) |
| Eff payroll tax rate | 0.117 | 0.068 | 0.211 | 0.175 |
| | (0.132) | (0.123) | (0.116) | (0.116) |

*Notes:* See Appendix B.3 for detailed definitions on education and labour market positions. Panel D reports the quarterly average amounts within the first year after hiring. Means above, standard deviations below in parentheses. The FTE rate is the average occupancy rate expressed as a full-time job during the first four quarters of employment. If this rate is exactly 100%, then this employee is classified as a full-time one. FTE gross wage equals to the sum of wages divided by the sum of the occupancy rates, during this first employment year; similarly for the payroll tax, the labour cost, and the tax deduction. Payroll tax is net of deductions, employers' share only. Labour cost equals to the sum of the gross wage and the payroll tax. Effective payroll tax rate equals to the payroll tax divided by the gross wage. We winsorize these outcomes at the 1$^{st}$ percentiles on the left side, and the 2.5$^{th}$ on the right side, respectively among the entrant and the incumbent groups, by their annual cohort of hiring.

parison suggests that 2016 entrants hired somewhat better-educated employees than 2014 entrants, as the share of high-education workers rose marginally among entrants while it fell among incumbents. We examine this shift in Section 4.4.2, but the overall composition differences remain limited.

Panel B shows that most newly hired employees were not previously working, where 'working' includes both paid employment and self-employment. Only about half of them held a job in the preceding quarter. This confirms that new employers often recruit people who were inactive or recently unemployed. Panel C documents very high separation rates: roughly one-quarter of new hires left within one quarter, and almost half within one year. After three years, only one quarter of employees still remained with their entry-time employer, similar across groups. These figures indicate that employment relationships in very small firms are short-lived, making persistence of employment a meaningful outcome in our analysis.

**Outcome definitions.** We focus on two sets of outcomes corresponding to the quantity and price margins of labour. On the quantity side, we measure (i) employment continuity (extensive margin)—whether the employee remains with the hiring firm at one-year and three-year horizons (Panel C)—and (ii) the intensive margin, proxied by the full-time-equivalent (FTE) rate in Panel D. The FTE rate represents the fraction of a full-time job the employee effectively provides during the first employment year; its average ranges from 0.56 to 0.59 for entrants and 0.56 to 0.57 for incumbents, showing a relative increase for the entrants.

On the price side, we analyse gross earnings, payroll taxes, subsidies, and total labour costs. Total earnings are modest because many employees work part-time or start mid-quarter. To obtain a consistent measure of pay, we normalise earnings by the FTE rate, yielding the FTE wage, as done by most incidence studies (Bozio et al., 2025; Fuest and Neumeier, 2023; Saez et al., 2019). These monetary variables are computed as cumulative sums over the first year (four quarters) and then divided by cumulative FTE. Analogously, we define the FTE payroll tax (employer share only), the FTE subsidy (first-employee reduction divided by FTE), and the FTE labour cost (gross wage plus payroll tax). All are winsorised at the 1st and 97.5th percentiles within cohort and group.

Panel D shows that wage levels were broadly similar across cohorts, whereas payroll taxes dropped sharply for entrants after the reform. The average FTE payroll tax fell from about €950 in 2014 to €545 in 2016, confirming a substantial reduction in employers' tax burden. This decrease was not accompanied by any marked change in wages, indicating that the 2016 exemption effectively operated as a payroll tax cut rather than a wage subsidy. However, the tax amount was not literally zero: employers still paid small training contributions, some did not immediately claim the reduction, and many hired more than one employee at entry. Hence, the measure should be interpreted as a large and permanent reduction in payroll taxes, not a complete exemption, with long-lasting effects on labour costs for new employers.

Appendix B.3, Table A2 reports the same outcome variables measured as three-year averages, corresponding to the longest observable horizon for the treated Cohort 2016 entrants. The results remain very similar to the one-year measures discussed above. The appendix also provides sector and joint-committee distributions by cohort (Table A3). Both entrant and small incumbent firms are mainly concentrated in food and beverages, hotel, retail and wholesale trade, and construction industries. These distributions are stable across cohorts, supporting the validity of our cohort comparisons.

### 3.4 Empirical strategy: Difference-in-differences

To estimate the incidence of the payroll tax exemption, we begin with a simple cohort-based difference-in-differences (DiD) design. This framework exploits variation across hiring cohorts and firm groups. The first dimension is to compare outcomes for entrant firms that hire their first employee with those similar, but ineligible, growing incumbents that also hire at the same time. The second dimension is to compare the outcomes of the post-reform cohorts against the pre-reform cohorts. Since the exemption applies only to the first employee, and entrants are by definition new to the labour market as employers, no within-firm before–after comparison can be defined. The treatment thus occurs at the moment of firm formation, necessitating that we use past cohorts of the same groups of firms as pre-reform counterparts, instead of the firm's own profile.[15]

Formally, we estimate the following event-study–style DiD model:

$$y_{ijt} = \alpha_t + \gamma\, Entrant_i + \sum_{\substack{c=2010 \\ c\neq 2014}}^{2018} \beta_c\, Entrant_i \times \mathbf{1}_{t=c} + \epsilon_{ijt}, \tag{1}$$

where $y_{ijc}$ is the outcome of interest for employee $j$ hired by firm $i$ in cohort time $t$. The dummy $Entrant_i$ indicates whether firm $i$ is an entrant employer (0 if it's a growing incumbent firm), and $\alpha_t$ are fixed effects for the hiring cohort. Standard errors $\epsilon_{ijt}$ are clustered at the firm level.

The outcomes are evaluated at three time horizons: one, two, and three years since the initial hiring time $t$. As mentioned, we have the quantity (employment) and the price (wage rate) of labour as our outcomes. The extensive margin of employment relationship measures whether the initial hires still work in the firm evaluated at the end of the horizon; the intensive margin of employment is the FTE rate evaluated as the cumulative average within the horizon. On the price side, we use per-FTE gross wage, payroll tax, labour cost, and subsidy as defined by Section 3.3, all of which are defined as the cumulative averages within the horizon.

The coefficient $\beta_c$ traces the evolution of outcome differences between entrant and incumbent firms across cohorts, normalised to zero in the base year 2014 ($\beta_{2014} = 0$). The entrant–incumbent difference in this base cohort 2014 is captured by $\gamma$. Our data is available from Cohort 2010 to 2018, but our analysis shall focus on three cohorts, 2013, 2014, and 2016; we use other cohorts as a robustness checks even when the macroeconomic and policy conditions were

[15]This setting is conceptually close to studies where the treatment is determined at birth or entry, such as Dechezleprêtre et al. (2023); Faggio and Silva (2014); Saez et al. (2012); Sedláček and Sterk (2017).

significantly different.

The main analyses rely on annual cohort comparisons, effectively contrasting wages in Cohort 2016 with those in Cohort 2014. Compared with quarterly cohort comparisons, aggregating to the annual level bypasses the necessity to control for seasonality in outcome variables, thereby yielding cleaner point estimates and easier interpretation of the coefficients. The trade-off is, however, that only one pre-reform cohort (2013) is available to test the parallel trends assumption.

The central placebo comparison is therefore between Cohort 2014 and Cohort 2013: if the corresponding coefficient, $\beta_{2013}$, is close to zero, the identifying assumption is credible. To reinforce this test, we additionally decompose the cohorts to the quarterly level, re-estimating Eq (1) with the cohort index $c$ defined at the quarter rather than year level. This provides up to eight pre-reform cohorts (2012Q4–2014Q4) and allows a more granular assessment of pre-trends. Importantly, these quarterly specifications are used solely for validating the identifying assumption: the treatment effects of interest remain estimated from the annual comparison, where precision is higher and interpretation is more straightforward.

We shall additionally optionally control for fixed effects of sectors, arrondissements, and joint committees; as later results will show, they do not affect the effects much (both levels and statistical significance). Note that since we do not rely on the panel structure of firms but rather the cohort structure of firms, and most entrant firms only hire one single employee, we cannot control for firm- or employee-level fixed effects.

# 4 Simple DiD results

This section presents the empirical results on wage incidence using the simple difference-in-differences strategy, offering comparisons between post-reform entrants and pre-reform ones. We will tackle with two channels of the incidence following the 2016 reform, the *quantity* and the *price* of labour, namely, working hours and wage rate, respectively in Sections 4.1 and 4.2. Based on these DiD estimates, we calculate payroll tax incidence parameters in Section 4.3.

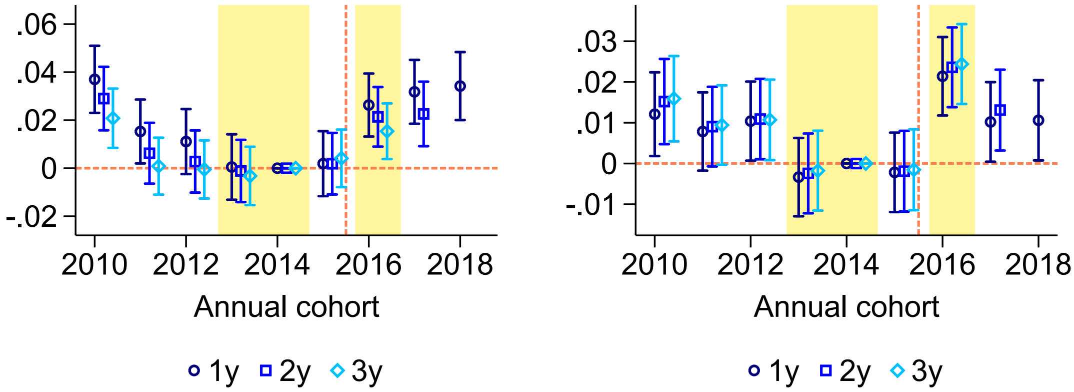
## 4.1 Quantity effects (working time)

While employment patterns are usually considered to be full-time and stable in large, incumbent, and mature firms, this is not the case for small entrants. Although the quantity effects have been predicted by theory with different assumptions on the elasticities (as argued by Bozio et al., 2025), this has seldom been the empirical focus. We thus start with the effect of the reform on labour quantities (working time) for newly hired employees, since incidence ultimately depends on how much labour firms demand as well as the wage they pay. The quantity is the equilibrium of both labour demand and supply and includes two margins, the extensive margin—whether this employee still works in the firm (employee retention)—and the intensive margin—the full-time equivalence (FTE) rate conditional on still working.

Figure 3 plots effects on these two margins, the differences between entrant firm employees and incumbent ones, over hiring cohorts against the base cohort 2014 (coefficients normalised to 0). This baseline specification does not include any covariates or fixed effects. For each margin (panel), we evaluate the outcome at three horizons—1 year, 2 years, and 3 years post hiring—next to each other in one cohort. Just as descriptive evidence, we plot the whole available annual cohorts from 2010 till 2018, but highlight only the most relevant three years 2013, 2014, and 2016.

**Before the reform** During cohorts 2013–2014, no other reform regarding the first hires happened and thus 2013 is a valid year for testing the common trend assumption of the DiD method. We see in both panels the coefficients $\hat{\beta}_{2013}$ on outcomes evaluated at three horizons are all close to zero, suggesting that we have chosen ideal small incumbent firms as the control group cohorts. Before

2013, even with double-dip economy fluctuations and a different first-employee subsidy reform, the coefficients remained somewhat stable. We reinforce the pre-reform parallel trend testing with quarterly cohort regressions in Figures A3a–A3f of Appendix C.3.1.




**(a)** Employee retention **(b)** FTE rate

**Figure 3.** Effects on working time: Simple DiD estimates

*Notes:* Simple DiD estimates using Eq (1) on employer–employee matched level outcomes. Three annual cohorts (2013, 2014, and 2016) are within a stable period for the DiD design as indicated by the highlighted shade. No covariates or fixed effects. Coefficients show entrants versus incumbents; base cohort is 2014. Horizontal axis is the redefined annual cohort of hiring. Vertical dashed line between 2015 and 2016; horizontal dashed line at 0. For each annual cohort, the outcome is evaluated in three different horizons: 1 year (circle), 2 years (square), and 3 years (diamond). Spikes show the 95% confidence intervals with standard errors clustered at the firm level. Employee separation in Panel (a) is a state variable and thus defined at the end of the respective evaluation period; FTE rate in Panel (b) is defined as a cumulative average within the respective evaluation period.

**Treatment effects** We focus on the coefficient on Cohort 2016 for treatment effect interpretation. At the extensive margin (Figure 3a), employees in treated firms were more likely to continue to work for the same firm. Retention rates[16] were about 2.6 percentage points higher after one year, 2.1 pps after two years, and 1.6 pps after three years, suggesting that the exemption modestly increased employment stability among initial hires. Though not large in absolute terms, retention rates were higher by 5–6% in relative terms, compared to pre-reform entrants.

At the intensive margin, firms' first workers expanded their working hours

[16] Retention rate$=1-$separation rate.

(conditional on staying employed). We measure the working time mainly by their FTE rate expressed as a percentage between 0 and 100%, averaged within one, two, or three years post hiring with Figure 3b. For Cohort 2016, this rate increased by 2.1–2.4 pps (4% relatively, compared to the average of around 57% FTE rate in Cohort 2014) consistently through three years post hiring. This implies that they worked 12 hours[17] more per quarter.

We also have a few complementary measures. The share of full-time employees[18] rose by 2 pps (10% relatively) on average during the first three years (Figure A1a). Put differently, part-time jobs were slightly more often converted into full-time positions: we see an increase of a little more than two full-time days (18 hours) worked (Figure A1b) per quarter and a decline of about half a discounted part-time day (4.4 hours) worked (Figure A1c). Adding up the two yields a resultant working hour similar to above (12 hours).[19]

Taken together, these results indicate that firms used part of the tax relief to deepen labour input with their initial hires. The main adjustment was not only in creating or destroying jobs (as the case for large firms), but in increasing working time for those already employed, because employees in small firms tend to work below full-time. For the analysis of incidence, this matters because workers' total earnings can rise through longer hours even if wage rates remain constant.

**Other cohorts** So far we only analysed Cohort 2016 for treatment effects. For Cohort 2015, they received an unexpected subsidy top-up in the first year, after they had already decided to hire their first worker. This subsidy top-up thus could not have altered the composition of firms' entry but might have affected their post-entry behaviour from year 2 onwards. However, for these two quantity outcomes, no effects were observed, consistent with Deng et al. (2025), a dedicated study on the subsidy top-up for Cohort 2015.

For later cohorts, we can only track for shorter periods of time: 2 years for

[17] $2.4\% \times 7.6$ hours/FTE day $\times$ 65 working days/quarter $= 11.856$ hours/quarter.

[18] Full-time employees are those who work with a FTE rate of 100%. Others are part-time ones.

[19] The number of hours is not exact. We do not observe this variable directly but only provide a approximate conversion for easy interpretation.

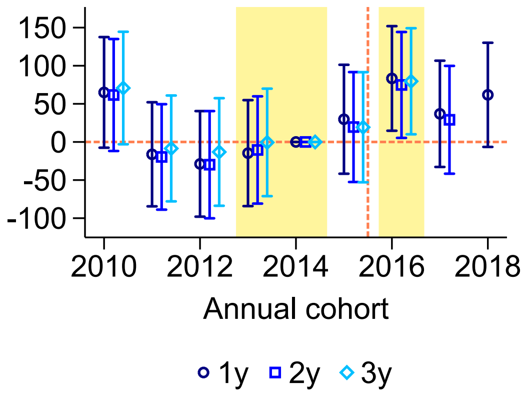
2017 and 1 year for 2018. These coefficients are still in the same direction and statistically significant at the 5% level. However, with a subsequent broader payroll tax reform, it might be difficult to claim they are treatment effects of this 2016 reform. In following sections, we shall still focus on Cohort 2016.

## 4.2 Price effects (wage rate and unit labour cost)

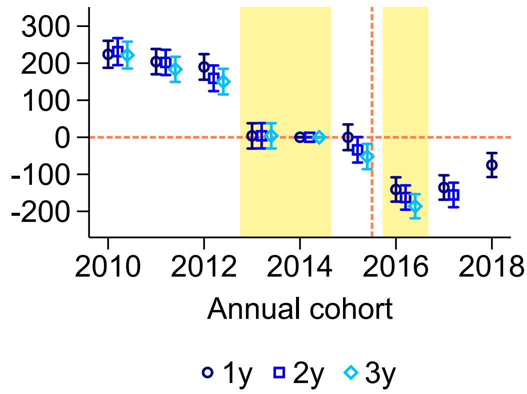
We next turn to the price side of labour, with four outcomes jointly displayed in Figure 4: (a) per-FTE wage (wage rate), (b) per-FTE payroll tax (after deducting d), (c) per-FTE labour cost (the sum of the first two), and (d) per-FTE subsidy from the exemption scheme. These variables are defined as cumulative average ratios within 1, 2, or 3 years. Before the reform, placebo estimates for Cohort 2013 are again close to zero across all horizons, for all four outcome variables, confirming that the 2014 cohort and our choice of the small incumbent control group provide a valid counterfactual. Focusing on Cohort 2016, three findings emerge.

**Effects on wage rate for employees** First, entrant firms' wage rates (Figure 4a) rose modestly by around €80 per quarter in a medium horizon of three years relative to incumbents, significant at 5% levels. Compared to the three-year average quarterly wage rate of Cohort 2014 (€6,765), this translated into a relative increase of 1.2%. Both the absolute and relative effects on wage rates are fairly stable along the three evaluation horizons.

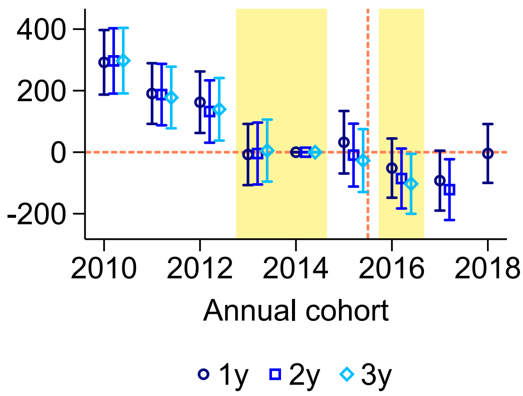
Coefficients for the placebo Cohort 2013 as well as earlier cohorts are all statistically insignificant from zero at 5% levels. Despite cyclical fluctuations and subsidy adjustments before 2013, this pattern further supports the parallel-trends assumption for our DiD method. Only Cohort 2010 deviated a bit more than the others, at the beginning of this double-dip recession period. Coefficients of shorter evaluation horizons for cohorts later than 2016 are still positive but not significant at 5% levels.



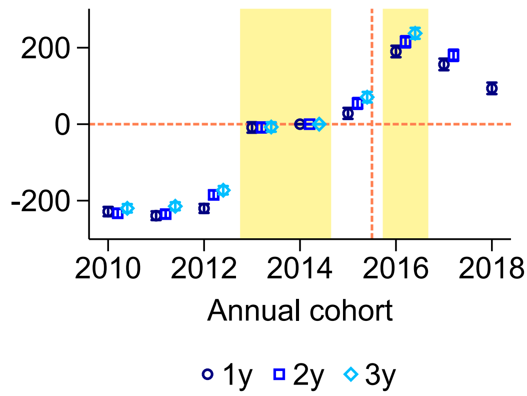

**(a)** FTE wage




**(b)** FTE payroll tax




**(c)** FTE labour cost (a+b)




**(d)** FTE subsidy

**Figure 4.** Effects on per-FTE wages and costs: Simple DiD estimates

*Notes:* Simple DiD estimates using Eq (1) on employer–employee matched level outcomes. Three annual cohorts (2013, 2014, and 2016) are within a stable period for the DiD design as indicated by the highlighted shade. No covariates or fixed effects. Coefficients show entrants versus incumbents; base cohort is 2014. Horizontal axis is the redefined annual cohort of hiring. Vertical dashed line between 2015 and 2016; horizontal dashed line at 0. For each annual cohort, the outcome is evaluated in three different horizons: 1 year (circle), 2 years (square), and 3 years (diamond). Spikes show the 95% confidence intervals with standard errors clustered at the firm level. All outcomes defined as a cumulative average within the respective evaluation period.

**Effects on tax saving for employers** Second, the exemption had direct effects on tax saving on employers' side. Figure 4b plots the coefficients on payroll taxes per FTE compared to Cohort 2014. Payroll tax is defined as the due amounts and thus already subtracted by hiring subsidies. Over three years, this effect grew stronger (shown by the lowering coefficients three in a row at 2016); averaged within three years, payroll taxes per FTE dropped sharply by €186 per quarter (18.4% relative to the quarterly payroll taxes due by Cohort 2014). As a result, labour costs per FTE (Figure 4c), defined as the sum of the two, did not statistically significantly (or only marginally) change over the three-year horizon.

We briefly discuss the tax cuts for other cohorts. Cohort 2013 paid a similar per-FTE tax to Cohort 2014, confirming the parallel trend assumption. Earlier cohorts paid higher payroll taxes because of a less generous subsidy scheme. Cohort 2015 paid lower taxes for their initial hires starting from year 2 (consistent with Deng et al., 2025[20]). For later cohorts, both the sign and the significance level this effect were maintained with magnitude attenuated.

**Claimed subsidy amounts** Finally, we examine the claimed subsidy amounts per FTE, as reported in the data (Figure 4d). The subsidy variable captures the initial deduction granted under the specific first-hire scheme, while payroll taxes measure the due amounts after subtracting all applicable reductions. They therefore do not coincide by construction, but both reflect the fiscal relief induced by the exemption.

Over the first three years, firms claimed on average about €238 per FTE per quarter in subsidies, compared to a €186 decline in payroll taxes. The close magnitudes provide a useful robustness check, confirming that the observed drop in payroll taxes is indeed driven by the scheme. Beyond this validation, the subsidy amounts are directly useful for our incidence analysis: they provide the denominator in the pass-through calculations, allowing us to quantify what share of the subsidy accrues to workers as higher wages and what share remains with employers as lower costs.

## 4.3 Payroll tax incidence

To summarise the baseline DiD results, we compute two ratios that are standard in the incidence literature. The first is the *employee incidence parameter* $\varsigma$, defined as the ratio of the wage effect to the sum of the wage effect and the payroll tax

[20] The effects documented here (below €100) are smaller than in Deng et al. (2025) (€200–400), because of two reasons. First, the outcome is defined as a cumulative average, even including non-treated periods when the payroll taxes are not exempted; Deng et al. (2025) define it in each directly treated period. Second, here we include Cohort 2014Q4, which was never exempted, into the redefine annual structure. These two forces diluted the documented coefficients.

saving effect, which indicates how a euro of tax relief is split between workers and employers. The second is the *subsidy–wage pass-through* $\rho$, defined as the ratio of the wage effect to the subsidy amount effect, which indicates how much of the granted subsidy is reflected in wages.

Ideally, one would identify incidence from a structural welfare framework that maps tax wedges into stakeholder surpluses (e.g. Benzarti and Carloni, 2019; Duan and Moon, 2025; Fuest et al., 2018; Suárez Serrato and Zidar, 2016). In our setting, however, we lack the external information these models require (markups, elasticities, demand/supply slopes, etc.), and the short-lived employment relationships of small entrants make steady-state assumptions questionable.

We therefore follow the alternative reduced-form standard, the Wald (2SLS) approach[21], used in empirical incidence and rent-sharing work (Bozio et al., 2025; Kline et al., 2019). This delivers transparent, money-metric ratios that answer 'who got what' per euro of policy.

Both ratios are reported on two bases: per-FTE (prices) and totals (price $\times$ hours). The per-FTE outcome estimates ($\hat{\beta}^{\text{FTE}}$) are presented in Section 4.2; we report the DiD estimates ($\hat{\beta}^{\text{tot}}$) of these four outcomes on the per-worker total basis (earnings, taxes, costs, and subsidy amounts) in Figure A2. For each horizon $K \in \{1, 2, 3\}$ years, and each of four outcome variables ($y$) in each of the two bases ($basis \in \{\text{FTE}, \text{tot}\}$), we denote the 2016 DiD treatment effect estimate as $\hat{\beta}^{basis}_{y,K}$. Then we can define the employee incidence and subsidy–wage pass-through parameters formally as

$$\textit{Incidence share} \quad \varsigma_K^{\text{FTE}} \equiv \frac{\hat{\beta}^{\text{FTE}}_{\text{wage},K}}{\hat{\beta}^{\text{FTE}}_{\text{wage},K} + |\hat{\beta}^{\text{FTE}}_{\text{tax},K}|} \qquad \varsigma_K^{\text{tot}} \equiv \frac{\hat{\beta}^{\text{tot}}_{\text{earnings},K}}{\hat{\beta}^{\text{tot}}_{\text{earnings},K} + |\hat{\beta}^{\text{tot}}_{\text{tax},K}|},$$

which is the *workers' share of the total gain* (workers over workers + employer),

[21] The only difference is, we do not estimate a two-step procedure but simply divide the estimated coefficients. Thus we have point estimates but not confidence intervals.

and

$$\textit{Pass-through} \quad \hat{\rho}^{\text{FTE}}_{K} \equiv \frac{\hat{\beta}^{\text{FTE}}_{\text{wage},K}}{\hat{\beta}^{\text{FTE}}_{\text{subsidy},K}}, \qquad \hat{\rho}^{\text{tot}}_{K} \equiv \frac{\hat{\beta}^{\text{tot}}_{\text{earnings},K}}{\hat{\beta}^{\text{tot}}_{\text{subsidy},K}}.$$

**Table 2.** Incidence ($\varsigma$) and pass-through ($\rho$) estimates, with building blocks

| | | 1 year | 2 years | 3 years |
|---|---|---|---|---|
| | **Panel A:** Per-FTE basis: price margin | | | |
| (1) | Wage effect | € 83.33 | € 74.78 | € 79.63 |
| (2) | Tax saving effect | € 140.90 | € 162.70 | € 186.10 |
| (3) | Subsidy effect | € 190.10 | € 215.40 | € 237.60 |
| (4) | Employee incidence $\hat{\varsigma}^{\text{FTE}}_{K} = (1)/((1)+(2))$ | **37.2%** | **31.5%** | **30.0%** |
| (5) | Pass-through $\hat{\rho}^{\text{FTE}}_{K} = (1)/(3)$ | **43.8%** | **34.7%** | **33.5%** |
| | **Panel B:** Per-worker total basis: price$\times$hours | | | |
| (1) | Earnings effect | € 205.00 | € 219.20 | € 224.10 |
| (2) | Tax saving effect | € 93.94 | € 112.00 | € 129.70 |
| (3) | Subsidy effect | € 153.10 | € 179.10 | € 197.90 |
| (4) | Employee incidence $\hat{\varsigma}^{\text{tot}}_{K} = (1)/((1)+(2))$ | **68.6%** | **66.2%** | **63.3%** |
| (5) | Pass-through $\hat{\rho}^{\text{tot}}_{K} = (1)/(3)$ | **133.9%** | **122.4%** | **113.2%** |

*Notes:* In rows (1)–(3) of both panels, per-FTE effects come from Figure 4 and per-worker total effects from Figure A2; they are the per-quarter amounts. Incidence $\varsigma$ is the workers' share of the combined gain (workers over workers+employer). Pass-through $\rho$ maps euros of subsidy into euros of wages.

On the *per-FTE* (price) margin, within three years, wages account for roughly $\hat{\varsigma}^{\text{FTE}}_{3} = 30.0\%$ of the combined gain with the remainder accruing to employers as lower payroll taxes, and about one-third ($\hat{\rho}^{\text{FTE}}_{3} = 33.5\%$) of the subsidy shows up in wage rates. These ratios across the horizon of three years are fairly stable. This is consistent with partial pass-through where employer social contribution cuts are not tightly linked to individual benefits (Bozio et al., 2025).

When we switch to the per-worker *total* basis that incorporates the longer hours documented earlier, both parameters rise. The incidence share nearly doubles to 63.3% in a three-year horizon. The subsidy–wage pass-through above one (113.2%) on the total basis reflects the same hours channel: each euro of subsidy is associated with more than a euro of extra earnings. This wedge between price-margin and total-margin incidence echoes broader findings that statutory relief and economic incidence can diverge once behavioural adjustments are accounted for (e.g. Benzarti and Carloni, 2019).

## 4.4 Parallel trends and robustness checks

### 4.4.1 Parallel trends with quarterly regressions

This subsection provides additional evidence on the identifying assumption of our DiD design. The key concern is whether treated and control cohorts followed parallel trends prior to the reform. In the annual-cohort specification, which we use for the main results, pre-trends can only be assessed with a single placebo year, 2013. The coefficients for Cohort 2013 are consistently close to zero, but to reinforce this evidence we also return to the quarterly-cohort specification.

Figure A3 reports estimates from the same regression equation as (1), except that annual cohorts are disaggregated into quarterly cohorts. We re-estimate the six core outcomes: two measures of working time (Rows 1–2, corresponding to Figure 3) and four per-FTE price outcomes (Rows 3–6, corresponding to Figure 4). Each row is an outcome, and each column is an evaluation period: 1 year, 2 years, or 3 years. For comparability we highlight the quarterly counterparts of the three most relevant annual cohorts: 2013, 2014, and 2016. The left-hand side of each panel illustrates the pre-reform dynamics; the right-hand side displays the post-reform treatment effects.

Three observations emerge. First, the quarterly estimates exhibit pronounced seasonality. Because we cannot fully address this seasonality, statistical significance is difficult to interpret. Second, despite the volatility, the 95% confidence intervals for the pre-reform coefficients almost always include zero, which is consistent with parallel pre-trends. Third, normalisation to a single base quarter generates mechanical shifts: if the base quarter is high, all other pre-reform coefficients appear negative (as in the retention outcomes in Panels A3a and A3b); if the base quarter is low, the opposite pattern emerges (as for the FTE wage in Row 3 and FTE labour cost in Row 5). These artefacts do not indicate genuine pre-trend differences. The only outcomes with relatively limited seasonality are per-FTE payroll taxes and the subsidy. However, here even small fluctuations appear statistically significant, because the confidence intervals are very narrow.

For these reasons, we treat the quarterly specification primarily as a diagnostic tool. Our preferred estimates are those from the annual cohorts, which average out seasonal noise and provide a clearer identification of the treatment effects. The quarterly regressions are useful to unpack dynamics and to corroborate the parallel-trends assumption, but not for precise effect magnitudes.

#### 4.4.2 Adding controls

Our baseline DiD estimates are reported without controls in order to present the cleanest possible specification, free of discretionary choices. This approach is strongly documented in the figures above. A natural question is whether the inclusion of controls—either fixed effects at the firm level or pre-determined worker characteristics—would materially alter the results. Analyses in this section show the answer is no.

We first consider firm-level heterogeneity. We sequentially add fixed effects for three dimensions: (i) the joint committee, which governs the relevant wage bargaining unit (note that not all employees in the same firm necessarily share the same joint committee) (101 units); (ii) arrondissement of the head office (43 units); and (iii) the 2-digit NACE sector of economic activity (98 units). Sector and joint committee are related but distinct: the sector classification reflects the firm's activity, whereas the joint committee determines the institutional setting for wage negotiations. When these fixed effects are included jointly, the six core outcomes are re-estimated (Figure A4). The results remain very similar to the baseline.

We then consider worker-level characteristics. Before adding them as controls, we first treat worker attributes themselves as outcomes to assess whether the composition of new hires shifted after the reform (Figure A5). Although we do not pursue this question in depth, some patterns are visible: post-reform entrants' initial hires tended to be more often male, foreign-born, better educated, younger, and previously unemployed; among those with past jobs, they also had higher prior wages and longer tenure. This pattern was not restricted to Cohort

2016 only but consistently for subsequent Cohorts as well. These suggestive findings indicate that the reform may have facilitated hiring of relatively vulnerable but more employable (abler) workers, but we do not explore this further.

We therefore include a standard set of worker-level controls: age, education (indicator for higher education), nationality (EU vs. non-EU), gender, past employment status (working vs. not), and occupational type (blue- vs. white-collar). The resulting estimates for our six outcomes are reported in Figure A6; Figure A7 adds these worker controls alongside the three fixed effects above. In both cases, the estimated effects are almost unchanged relative to the baseline.

Overall, the robustness checks confirm that the main findings are not driven by firm or worker composition. Working time rises on both the extensive and intensive margins, per-FTE wages increase modestly, and per-FTE payroll taxes fall sharply. The magnitudes remain very close to the baseline and would imply essentially the same incidence parameters if recalculated.

#### 4.4.3 Tax incidence of designated first employees only

So far, the analysis has included all employees hired at entry. On average, entrant firms hired 1.6 employees in the entry quarter, although only one of them could be designated as the 'first employee' eligible for the payroll tax exemption. It is therefore natural to ask whether focusing on this first employee changes the results.

There are several reasons why effects for first employees might differ. First, the exemption applies to one *physical* employee rather than one FTE. Firms therefore have an incentive to assign the exemption to their ablest worker, often with higher wages, making treatment effects potentially stronger. Second, not all firms immediately claimed the subsidy (take-up was about 50%) so information frictions or behavioural biases may affect which workers are labelled as the 'first' (Boucq and López Novella, 2018a). Third, the fact that most firms hire more than one worker means that our baseline estimates mechanically dilute the subsidy effect: the tax cut does not scale with the number of initial hires.

The six core outcomes are re-estimated for the subsample of first employees only, keeping the control group unchanged (Figure A8). Unsurprisingly, the levels of the effects differ. Retention rates are nearly twice as high for first employees compared to all initial hires, FTE rate effects are about one percentage point stronger, and wage rates and earnings rise by roughly 50% more. Because first employees are typically higher earners, however, the relative increases in wages are similar. Payroll taxes and subsidies, by construction, also display different relative magnitudes: the first employees capture most of the deductions and thus show larger absolute reductions.

To connect back to our main question, we recalculate the incidence parameters using these estimates (Cols. 4–6 of Table A4). Table 3 reports the results. The overall pattern is remarkably consistent with the baseline. Employees continue to bear only a partial share of incidence on the per-FTE (price) margin, and the pass-through ratios are larger when total hours are included. Over the medium horizon of three years, the parameters converge to those for all initial hires: employees captured roughly two-thirds of the tax cut benefits, and one euro of subsidy translated into about 1.4 euros of total earnings.

We stress that these estimates are more fragile than the baseline, since the designation of 'first employee' is itself subject to firm choices and potential biases we cannot fully characterise.[22] Nevertheless, the key takeaway is that the incidence parameters are essentially the same as when all initial hires are used. Even under the narrow focus on first employees, the reform benefits were shared in very similar proportions.

[22]Unlike the bounding strategy used later to address selection into hiring, there is no straightforward correction for information- or behaviour-driven selection into subsidy take-up.

**Table 3.** Incidence ($\varsigma$) and pass-through ($\rho$) estimates, first employees only

| | 1 year | 2 years | 3 years |
|---|---|---|---|
| **Panel A: Per-FTE basis:** price margin | | | |
| Employee incidence share $\hat{\varsigma}_K^{\text{FTE}}$ | 57.8% | 43.6% | 39.1% |
| Pass-through $\hat{\rho}_K^{\text{FTE}}$ | 152.7% | 75.3% | 60.8% |
| **Panel B: Per-worker total basis:** price$\times$hours | | | |
| Employee incidence share $\hat{\varsigma}_K^{\text{tot}}$ | 73.1% | 68.1% | **64.2%** |
| Pass-through $\hat{\rho}_K^{\text{tot}}$ | 249.6% | 173.1% | **143.3%** |

*Notes:* In rows (1)–(3) of both panels, per-FTE effects come from Panel B, Cols 4–6 of Table A4 and per-worker total effects from Panel C, Cols 4–6 of Table A4; they are the per-quarter amounts. Incidence $\varsigma$ is the workers' share of the combined gain (workers over workers+employer). Pass-through $\rho$ maps euros of subsidy into euros of wages.

# 5 A unified theory of firm entry and payroll tax incidence

This section develops a competitive partial-equilibrium model for small firms that merges the Lucas–Jovanovic managerial-ability framework (Evans and Jovanovic, 1989; Jovanovic, 1982; Lucas, 1978) with a competitive payroll tax incidence model. On the entry side, managerial ability pins down profitability and hence the decision to hire the first worker, following the standard selection logic. On the incidence side, we adopt a competitive labour-demand approach in the spirit of Bozio et al. (2025); Feldstein (1974); Gruber (1997) to derive the elasticities of wages with respect to payroll tax changes for those firms that do enter. The two pieces are embedded in a single, coherent setup so that composition (who enters), employment (how much they hire), and wages (how incidence is shared) are determined jointly.

Our focus on partial equilibrium is deliberate: the reform targets a very small segment of the firm population and a modest share of total employment, so general-equilibrium feedbacks are expected to be negligible. Firms' relevant decision margin is local—existing solo self-employed producers choose whether to become employers (enter or not). It delivers (i) a sharp, ability-indexed entry threshold that clarifies the non-random selection problem and motivates the trimming/bounding logic in Section 6.2; and (ii) an incidence formula in which the extensive margin (entry) and intensive margin (wage and employment per entrant) interact, providing a transparent mapping from the payroll tax schedule to equilibrium wages and employment among small entrants.

## 5.1 Setup

**Firm's problem** A potential employer (a firm owner who has not yet entered the employer status) decides whether to enter the labour market on the demand side by hiring the first worker. If the owner hires, the firm becomes an *entrant employer*; otherwise, it remains *solo self-employed*. The outside option of staying self-employed is normalised to zero. The decision to hire therefore depends on the net return from becoming an employer, and the owner hires if and only if this return is positive. We assume the labour market is competitive so firms are price-takers (Bozio et al., 2025). This is a plausible assumption in the market of small firms as they exhibit limited monopsony power (Manning, 2021).

In contrast to the modern general-equilibrium models adopting the maganerial-ability framework (e.g. Ando, 2021; Gourio and Roys, 2014), which typically study the division between becoming an employer and a waged worker, we focus on the partial equilibrium of the portion of the labour market occupied by small firms. Thus, we assume these firms have already decided to enter the product market as solo producers and now consider whether to enter the labour market by hiring their first worker.

We model the profit from hiring an employment of $L > 0$ as[23]

$$\Pi = y(L;\theta) - C(L;\tau,\kappa,w) = \theta\, g(L) - w(1+\tau)\, L - \kappa, \tag{2}$$

where $\theta$ denotes managerial ability (or firm productivity) with distribution density $f(\theta)$, $w$ is the gross wage received by workers, $\tau$ is the (average) employer payroll tax rate, and $\kappa > 0$ is the fixed cost of becoming an employer.

[23] $L$ should be interpreted as full-time equivalence employment numbers instead of the physical numbers of employees.

**Production** Following the canonical managerial-ability framework (Evans and Jovanovic, 1989; Jovanovic, 1982; Lucas, 1978), ability raises both output and marginal product, i.e. $\partial_\theta y = g(L) > 0$ and $\partial_{\theta L} y = g'(L) > 0$, which induces a monotone mapping from ability to revenue and marginal revenue. Additionally, we assume that marginal product decreases so that $g''(L) < 0$.

**Payroll tax cut** We separate a *variable* and a *fixed* component in modelling the labour cost structure. Two reasons motivate this approach: first, the tax exemption is only for the first physical employee but we are interested in the full-time employment; second, the exemption changes the *average* payroll tax rate in an earnings-dependent way (rather than via a uniform proportional cut), The variable piece is the payroll tax rate $\tau$, which loads per unit of labour and therefore shifts *the intensive margin*. The fixed piece is the overhead $\kappa$, a sunk cost of becoming an employer within the period, which shifts the *extensive margin* only. The incidence is governed by both. This split is robust at the entry margin under non-linear exemptions: changes that act like lump-sum relief map into $\kappa$, whereas changes to per-unit cost map into $\tau$.

The two parameters can also be justified in an alternative way. The fixed entry cost $\kappa$ captures the extensive margin (the fixed cost of becoming an employer) and is standard in models of size-dependent policies (e.g. Garicano et al., 2016). The variable payroll tax rate $\tau$ instead governs the intensive margin, affecting the cost of employing each unit of labour once the firm has entered. In practice, the 2016 reform altered the latter but only for new employers, effectively lowering both the marginal cost of hiring and the expected fixed cost of becoming an employer. Our framework therefore abstracts from these institutional details by modelling the reform as a joint reduction in $\tau$ and $\kappa$, providing both a simplification of the Belgian policy and a general formulation of entry incidence applicable to broader settings.

**Labour supply** Workers supply labour competitively according to an exogenous Marshallian schedule $S(w) = A\, w^{\varepsilon_s}$ with $A > 0$ and elasticity $\varepsilon_s > 0$.

This reduced-form specification allows wages to adjust in the local labour market while treating aggregate supply conditions as exogenous, consistent with our small-segment partial-equilibrium focus. Appendix D.1 provides a microfoundation delivering this supply curve.

### 5.2 Entry with exogenous wage (pre-equilibrium)

Throughout this subsection we fix the gross wage $w$ and analyse only the firm's profit maximisation problem. This provides an illustrative benchmark by alternatively assuming that employers bear full payroll tax incidence (as in Cockx and Desiere 2024; Guo and Wallskog 2025 when they calculate the entry response elasticities) and that wages cannot be changed by this specific tax reform. The subsequent subsection will endogenise wages and study incidence.

Let $L^*(\theta; w, \tau, \kappa)$ denote the unique optimiser solving the first-order conditions of (2), $\theta\, g'(L^*) = w(1+\tau)$, under $g'(L) > 0, g''(L) < 0$. Define the optimised profit as $\Pi^*(\theta; w, \tau, \kappa)$. By the envelope theorem,

$$\Pi^*_\theta(\theta; w, \tau, \kappa) = g(L^*) > 0, \qquad \Pi^*_\tau(\theta; w, \tau, \kappa) = -\, w\, L^* \leq 0, \qquad \Pi^*_\kappa(\theta; w, \tau, \kappa) = -1 < 0,$$

so $\Pi^*$ is strictly increasing in $\theta$ and (weakly) decreasing in each cost parameter. Single-crossing holds under $g'' < 0$ (Milgrom and Shannon, 1994).

Then firm owners will have to compare the optimised value from employment ($\Pi^*$) against the outside option as non-employers of zero. This implies that only firm that can make a positive net profit will choose to hire their first employee. Under each given tax policy, their managerial ability is the sole determinant of this decision: only owners with high enough ability can afford hiring because their revenue from hiring can compensate the labour costs. This leads to

**Lemma 1** (Entry threshold and monotonicity). *For any fixed $w$, there exists a unique threshold $\theta^*(w, \tau, \kappa)$ such that entry occurs iff $\theta \geq \theta^*(w, \tau, \kappa)$. Moreover, $\theta^*$ is (weakly) increasing in $\tau$ and in $\kappa$.*

As an illustrative check, a Cobb–Douglas example of the production function in Appendix D.2.1 delivers closed-form expressions that verify the threshold's monotonicity properties.

With Lemma 1 linking firm productivity (managerial ability) levels directly to their entry decisions, we can get the tax reform implications on aggregate firm entry and the composition (average productivity) of new entrants.

**Proposition 1** (Tax cuts at a fixed wage: entry and composition)**.** *Fix $w$. Consider (i) a fixed-cost cut $\kappa_1 < \kappa_0$ holding $\tau$ constant, or (ii) a per-unit payroll tax cut $\tau_1 < \tau_0$ holding $\kappa$ constant. Then:* (a) *Aggregate entry (weakly) increases: the threshold falls from $\theta^*(w, \tau, \kappa_0)$ to $\theta^*(w, \tau, \kappa_1)$ in case (i), and from $\theta^*(w, \tau_0, \kappa)$ to $\theta^*(w, \tau_1, \kappa)$ in case (ii).* (b) *The average ability among entrants weakly decreases as the entrant set expands toward lower $\theta$. A sufficient condition is log-concavity of the ability density on $\mathbb{R}_0^+$.*

*Proofs.* See Appendix D.2. Part (a) follows from monotone comparative statics; part (b) follows from properties of truncated log-concave distributions (Bagnoli and Bergstrom, 2005). A fixed $w$ is assumed here to isolate the entry margin.

For any pair of policies leading to a tax cut, where the older high-tax policy is $(\tau_0, \kappa_0)$, and the newer low-tax policy $(\tau_1, \kappa_1)$ with $\tau_1 \leq \tau_0, \kappa_1 \leq \kappa_0$, we define marginal firms ($\mathcal{M}_w$) and inframarginal ones ($\mathcal{F}_w$) with fixed wage $w$ as

$$\mathcal{M}_w(\tau_0, \kappa_0; \tau_1, \kappa_1) = \left[\theta^*(w, \tau_1, \kappa_1),\, \theta^*(w, \tau_0, \kappa_0)\right), \quad \mathcal{F}_w(\tau_0, \kappa_0; \tau_1, \kappa_1) = \left[\theta^*(w, \tau_0, \kappa_0), \infty\right). \tag{3}$$

Inframarginals are the firms with sufficiently high productivity so they hire their first employee irrespective of the tax policy change (decision margin unchanged). Marginals are those with relatively lower productivity so they are selected into hiring because of the tax cut: they do not hire (belong to the solo self-employed) in the high-tax policy, and they hire (belong to the entrants) in the low-tax policy (decision margin changed).[24]

[24]Note that this margin is only about whether they enter the market or not, but not about how much labour they hire. The exact value of solution $L^*$ still varies with tax reforms even for inframarginals.

Under the simple assumption of a fixed wage, we see that firm composition is not balanced because of the tax cut. This poses empirical challenges if we are going to analyse the effect of this reform on firm-specific outcomes and thus is a problem we need to solve.

## 5.3 Entry and incidence with endogenous wages (partial equilibrium)

We now clear the local labour market:

$$A\, w^{\varepsilon_s} = L^D(w, \tau, \kappa),$$

where the left-hand side is the labour supply and the right-hand side the aggregate labour demand by integrating $L^*(\theta; w, \tau, \kappa)$ over the entrant region of managerial ability. The competitive partial-equilibrium wage $w^{\mathrm{eq}}(\tau, \kappa)$ solves this equation. With this wage endogenised, we redefine the entry threshold of managerial ability as

$$\theta^\dagger(\tau, \kappa) \;\equiv\; \theta^*\big(w^{\mathrm{eq}}(\tau, \kappa),\, \tau,\, \kappa\big),$$

i.e., the same threshold function (of exogenous wage in Section 5.2) evaluated at the equilibrium wage. We then carry the notations from the previous subsection directly to here by replacing the superscript $*$ with $\dagger$.

**Lemma 2** (Threshold with endogenous wages)**.** *Under the maintained assumptions on technology and supply, a unique $w^{\mathrm{eq}}(\tau, \kappa)$ exists and $\theta^\dagger(\tau, \kappa)$ is well-defined. Moreover, the threshold is* weakly increasing *in each cost component:*

$$\frac{\partial \theta^\dagger(\tau, \kappa)}{\partial \tau} \;\geq\; 0, \qquad \frac{\partial \theta^\dagger(\tau, \kappa)}{\partial \kappa} \;\geq\; 0.$$

Intuition. *A higher per-unit tax $\tau$ raises the effective unit cost $w^{\mathrm{eq}}(1+\tau)$, and a higher fixed fee $\kappa$ raises the entry bar directly; both shift the cutoff upward even after wages adjust. (Proof in Appendix D.3)*

**Wage elasticities (incidence primitives).** Define the two wage elasticities at equilibrium:

$$\Phi(\tau,\kappa) \;\equiv\; -\frac{d\ln w^{\text{eq}}}{d\ln(1+\tau)} \geq 0, \qquad \Psi(\tau,\kappa) \;\equiv\; -\frac{d\ln w^{\text{eq}}}{d\ln\kappa} \;\geq\; 0.$$

Using market clearing, one obtains the competitive-incidence forms

$$\Phi(\tau,\kappa) = \frac{\mathcal{E}_D(\tau,\kappa)}{\varepsilon_s + \mathcal{E}_D(\tau,\kappa)}, \qquad \Psi(\tau,\kappa) = \frac{\Gamma_\kappa(\tau,\kappa)}{\varepsilon_s + \mathcal{E}_D(\tau,\kappa)}, \tag{4}$$

where $\mathcal{E}_D(\tau,\kappa)$ is the *effective labour-demand elasticity* with respect to total unit cost $w(1+\tau)$, and $\Gamma_\kappa(\tau,\kappa)$ is the *entry elasticity* of aggregate labour demand with respect to $\kappa$. (Formal definitions and the decomposition in Appendix D.3) These expressions mirror modern competitive-incidence derivations and would collapse to the standard incidence elasticities if entry were held fixed (c.f. Benzarti, 2025; Bozio et al., 2025).[25]

**Proposition 2** (Reform decomposition: entry expansion and wage increase)**.** *Consider a move from $(\tau_0,\kappa_0)$ to $(\tau_1,\kappa_1)$ with $\tau_1 \leq \tau_0$ and $\kappa_1 \leq \kappa_0$. In partial equilibrium:*

*(i)* ***Entry expansion.*** *The threshold (weakly) falls: $\theta^\dagger(\tau_1,\kappa_1) \leq \theta^\dagger(\tau_0,\kappa_0)$. Hence the entrant set expands. If the ability density is (locally) log-concave, the average ability among entrants weakly decreases (same truncation logic as in the fixed-wage case).*

*(ii)* ***Wage increase (incidence).*** *The equilibrium wage responds as (taking a full differentiation)*

$$d\ln w^{\text{eq}} = -\Phi(\tau,\kappa)\, d\ln(1+\tau) \;-\; \Psi(\tau,\kappa)\, d\ln\kappa,$$

*with $\Phi(\tau,\kappa) \in (0,1)$ and $\Psi(\tau,\kappa) \geq 0$. Thus: a per-unit cut ($\tau \downarrow$) raises wages via* both *per-unit incidence and entry; a fixed-fee cut ($\kappa \downarrow$) raises wages* only *through entry.*

[25] Intuition: When $\Gamma_\kappa \to 0$, the entry-fee elasticity vanishes $\Psi \to 0$ and the tax-rate elasticity collapses to a constant $\Phi \to \frac{\varepsilon_d}{\varepsilon_s+\varepsilon_d}$ with $\varepsilon_d$ an exogenous constant labour-demand elasticity in the competitive-market literature.

*Interpretation of the per-unit rate $\tau$ (the wage elasticity $\Phi$).* Two forces enter $\Phi$: an *intensive* demand response (how incumbent employers adjust $L^*$ at a given entry set) and an *entry* response (how the cutoff $\theta^\dagger$ moves and new firms enter). The entry piece raises the *effective* labour-demand elasticity that sits inside $\Phi$; holding labour-supply elasticity fixed, stronger entry makes the wage more responsive to a per-unit shock. In the *no-entry benchmark* (fix $\theta^\dagger$), this entry term vanishes and $\Phi$ collapses to the conventional competitive incidence elasticity used in standard models and applications (Harberger 1962; Salanié 2011; see also empirical implementations such as Saez et al. 2019).

*Interpretation of the fixed fee $\kappa$ (the wage elasticity $\Psi$).* $\kappa$ has no per-unit price effect; it operates *only* through entry. A cut in $\kappa$ lowers $\theta^\dagger$, increases the *number of entering employers* and the *employment mass* attached to these marginals, and thereby shifts total labour demand. Wages adjust through market clearing according to the increase in the aggregate labour demand. Accordingly, $\Psi$ determines the aggregate induced employment among all the marginal entrants at a policy change from $(\tau, \kappa)$.

**Marginals vs. inframarginals (identification).** For a tax cut from $(\tau_0, \kappa_0)$ to $(\tau_1, \kappa_1)$, define marginal firms ($\mathcal{M}$) and inframarginals ($\mathcal{F}$) as

$$\mathcal{M}(\tau_0, \kappa_0; \tau_1, \kappa_1) = \left[\theta^\dagger(\tau_1, \kappa_1),\, \theta^\dagger(\tau_0, \kappa_0)\right), \qquad \mathcal{F}(\tau_0, \kappa_0; \tau_1, \kappa_1) = \left[\theta^\dagger(\tau_0, \kappa_0), \infty\right). \tag{5}$$

Our empirical target is *incidence among inframarginals*. Marginals lack pre-reform wages by construction and are treated here as composition forces that reshape equilibrium.

**Object of identification** In the competitive baseline with one homogeneous labour market facing micro firms, the equilibrium wage $w^{\text{eq}}(\tau, \kappa)$ is common to all entrants, so the theoretical incidence object is simply the change in the market wage rate and cannot be firm-specific by construction. In our theory, we thus see that the two elasticity parameters, $\Phi, \Psi$, are independent of individual firm types

$\theta$. Even if we predict the composition of firms change as a result of the reform in Equations (3) and (5), which is usually a identification threat, this composition shift does not lead to biases in the wage effect study.

However, in reality this ideal argument might not hold. First, even if the wages might remain common across firms, other outcomes (like the contract types, employment) are still likely to be firm-specific because they are not prices and need not be equalised across firms in the same market. Second, if there exist multiple sub-markets for entrant firms, the wage rates in each market may differ. Additionally, there might be external factors influencing wages that are not captured by our model. If small firms' labour market is not perfectly competitive, and the monopsony power is related to the firm productivity, then it will cause the wage rates to differ in firms with different productivity.

Thus, we need to carefully construct our comparison of outcome variables among the correct set of firms. Comparing *all* entrants across a high-tax regime and a low-tax regime is not well-defined: the low-tax regime includes *marginal* firms that would not hire under high tax, so their high-tax wages do not exist. Doing so would suffer from a compositional bias in identifying the reform effects. Marginals firms' existence shifts equilibrium but lack a valid firm-level counterfactual wage in the high-tax case. We therefore target payroll tax incidence for *inframarginals* (firms that hire under both regimes) in the subsequent analyses.

# 6 Sample-trimming treatment effects

Our simple DiD is informative but does not explicitly address that the reform changed both outcomes and the composition of entrants. Prior work (discussed in Section 2), along with our theory in Section 5, shows substantial induced entry of lower-productivity firms in 2016 (Cockx and Desiere, 2024; Deng et al., 2026). If these *marginal* entrants differ systematically from always-hiring *inframarginals*, cohort comparisons may not recover the incidence relevant for inframarginals (the policy counterfactual of interest). Thus we might suffer from a non-random sample selection problem, where the treatment affects both the outcomes of interest (wages and employment) as well as the probability of being observed in the sample (show up as entrant firms).

## 6.1 Positioning in the literature on non-random sample selection

The methodological foundation of our approach builds on the literature addressing identification under non-random sample selection. Manski and Horrowitz (Horowitz and Manski, 1995, 2000; Manski, 1989, 1990) first established partial-identification bounds that require almost no functional-form or exclusion assumptions but typically yield very wide intervals. Lee (2009) advanced this framework by introducing sharp bounds for treatment effects under monotone selection. His method, designed primarily for experimental or quasi-experimental settings under random assignment, became the standard tool for bounding causal effects when selection into the observed sample depends on treatment assignment but the direction of selection is known.

A natural idea in our context is therefore to restore composition balance by trimming, in the spirit of Lee (2009). Yet our challenge is conceptually more severe. We study two distinct cohorts—2014 and 2016—observed in different macroeconomic and policy environments with multiple potential confounders, and the selection process occurs at *formation*, when firms decide whether to hire and thus exist as employers at all, rather than at observation among an existing population. In this setting, the assumptions underpinning the Lee bounds are too weak to yield informative estimates (as noted similarly by Barrow and Rouse, 2018; Honoré and Hu, 2020). As highlighted by recent methodological extensions, informative inference can be improved or generalised by additional selection structure, such as conditioning selection on observables (Semenova, 2025) or exploiting multilayered selection (Kroft et al., 2024).

Still, as a trial, we combine the sample-trimming procedure of Lee (2009) within a difference-in-differences estimator. Specifically, we obtain Lee-style lower and upper bounds on the post-treatment mean outcome for the entrant (treated) cohort and difference these against the evolution of the outcome in incumbent (control) cohorts. This yields an identified interval for the treatment effect that is robust to selection and business cycle common shocks. Appendix E.1 provides the technical details of this approach and presents results. Unfortunately, the identified bounded effects often include zero and thus prove uninformative.

Another important limitation of the Lee bounds for settings such as ours is that they rely on trimming with respect to a *realised outcome*, typically the observed wage or employment status. The trimming rates are thus endogenously chosen by the algorithm within the bounding framework and cannot accommodate to external knowledge. As shown in Table A5, the Lee-bound trimming rates are often unrealistically small. The Lee assumption is thus only appropriate when the outcome itself governs participation, as in individual programme take-up, but it is problematic when multiple outcomes exist and when the participation decision is driven by a separate latent incentive. In our context, wage rates or working time are not monotone in firms' hiring incentives. Consequently, trimming on realised outcomes, as in Lee-type implementations, fails to isolate the relevant selection margin for firm entry and distorts inference on incidence.

## 6.2 Sample-trimming framework

Guided by insights from the selection-bounds literature and by our economic model of firm entry, we implement a *sample-trimming* rule that imposes stronger but economically grounded assumptions to recover comparability between post- and pre-reform inframarginal entrants. Our theoretical framework shows that payroll tax cuts lower the productivity threshold for hiring, thereby inducing entry of additional, lower-productivity firms at the extensive margin.

The proportion of these *marginal* entrants can be inferred from the observed excess-entry rate documented in prior work (Cockx and Desiere, 2024; Deng et al., 2026). We denote this induced-entry share by $\lambda = \frac{|\mathcal{M}|}{|\mathcal{F}|+|\mathcal{M}|}$, the number of marginal ($\mathcal{M}$) entrants divided by marginals plus inframarginals ($\mathcal{F}$), and treat it as an *a priori* range within $[\underline{\lambda}, \overline{\lambda}] = [0.20, 0.30]$,[26] an empirically credible interval. Note that since the reform did not alter the population of incumbent firms, they need not be trimmed and their role is solely to control for business cycle effects in the outcome variables.

Formally, we model the hiring decision in cohort $c$ as a latent-index process governed by the firm's optimal profit from entry $\Pi^{\dagger}(\theta, X)$, which depends on productivity $\theta$, other pre-treatment covariates $X$ (empirical element not modelled by the theory), for a given payroll tax regime (characterised by parameters $\kappa_c$ and $\tau_c$ in the model but abstracted from here). According to a standard Heckman (1979) selection rule, a firm enters as an employer if the latent profit exceeds an idiosyncratic noise term $\nu_c$ capturing random factors mainly of business cycle effects:

$$D_c = 1\left\{ \Pi^{\dagger}(\theta, X) + \nu_c \geq 0 \right\}, \qquad \nu_c \sim H_c(\cdot),$$

where $H_c$ denotes the cumulative distribution of the unobserved shocks specific to cohort $c$.

Operationally, the goal is to remove from the post-reform entrant population the set of marginal firms $\mathcal{M}$, which in our theoretical model corresponds to those with latent productivity $\theta$ falling below the pre-reform hiring threshold $\theta^{\dagger}$. Because $\theta$ is unobserved, we construct a pre-treatment hiring-propensity score $S$ based on observable firm characteristics, including pre-hiring business activities including sales and profits based on their Value-Added Tax (VAT) reports that proxy for productivity. Appendix E.3.1 provides the procedures for estimating such a hiring propensity. The model implies a monotone mapping between $\theta$ and $S$: more productive firms have higher $S$.

[26]Using monthly firm registry data, Cockx and Desiere (2024) estimate that the probability of hiring increased by 31%, implying a share of marginal entrants among post-reform firms of $0.31/(1 + 0.31) = 23.7\%$. Using quarterly employment data, Deng et al. (2026) find an increase in the number of entrants of 24%, corresponding to a marginal-entry share of $0.24/(1 + 0.24) = 19.4\%$. We therefore adopt an *ad-hoc* range of $[\underline{\lambda}, \overline{\lambda}] = [0.20, 0.30]$ to account for sampling and frequency differences across data sources. Because the first paper is dedicated to the effect on the induced entry quantity, whereas the second is only approximate and at lower frequency, we allow the upper bound to exceed the first estimate slightly to accommodate potential undercounting of the entrants in monthly data had researchers used higher-frequency data.

The associated hiring propensity (score) is then defined as

$$S_c(\theta, X) \;\equiv\; \Pr(D_c = 1 \mid \theta, X) \;=\; 1 - H_c\big(-\Pi^\dagger(\theta, X)\big).$$

The subscript $c$ indicates that both the tax environment and the noise distribution may vary across cohorts, so that the same $\theta$ may correspond to different probabilities of entry in different policy regimes.

Under our theory, $\Pi^\dagger(\cdot)$ is strictly increasing in $\theta$, implying that firms with higher productivity are more likely to enter. This yields a smooth, probabilistic generalisation of the sharp threshold $\theta^\dagger$: instead of a deterministic cut-off, we obtain a continuous mapping between productivity and the probability of entry. The following corollary, proved in Appendix E.2.1, establishes that this ordering is preserved:

**Corollary 1** (Identification via propensity ranks)**.** *If $\Pi^\dagger(\theta, X)$ is strictly increasing in $\theta$ and $H_c$ is continuous and strictly increasing, then $S_c(\cdot, X)$ is a strictly increasing bijection in $\theta$. Hence, within each cohort $c$, trimming by the rank of $S_c$ is equivalent to trimming by the unobserved ability $\theta$.*

We then iteratively trim the lowest-propensity entrants from the post-reform cohort by varying the trimming (deletion) rate $t$. This step removes those firms most plausibly induced by the reform, yielding a post-reform sample $\mathcal{R}(t)$ that approximates the counterfactual population of always-hiring firms $\mathcal{F}$. The trimming rate $t$ serves as the empirical counterpart to $\lambda$, and varying $t$ within the plausible interval [0.20, 0.30] provides a sensitivity range for the estimated incidence effect.

After trimming, we re-estimate the cohort-based difference-in-differences model. The first difference compares the post-reform entrant cohort with the pre-reform entrant cohort, while the second difference subtracts the contemporaneous change observed among similar but ineligible incumbent firms, thereby removing business-cycle effects common to both groups. Formally, the trimmed treatment effect is expressed as

$$\Big[\min_{t\in[\underline{\lambda},\overline{\lambda}]} \hat{\tau}_{\mathcal{F}}(t),\ \max_{t\in[\underline{\lambda},\overline{\lambda}]} \hat{\tau}_{\mathcal{F}}(t)\Big], \qquad \hat{\tau}_{\mathcal{F}}(t) = \big[\overline{Y}_1(\mathcal{R}(t)) - \overline{Y}_0(\mathcal{F})\big] - \big[\overline{Y}_1^{\text{inc}} - \overline{Y}_0^{\text{inc}}\big], \tag{6}$$

where $\overline{Y}_1(\mathcal{R}(t))$ and $\overline{Y}_0(\mathcal{F})$ denote, respectively, post- and pre-reform entrant means, and $\overline{Y}_1^{\text{inc}}$ and $\overline{Y}_0^{\text{inc}}$ are the corresponding averages for the incumbent control group. If the score rank $S$ correctly tracks $\theta$, trimming at $t$ at the correct $\lambda$ removes marginal firms $\mathcal{M}$ and aligns the post-reform composition with the counterfactual pre-reform population of inframarginals $\mathcal{F}$.

The approach is *heuristic* in nature: we do not attempt to identify the marginal firms precisely, but instead exploit the monotone relationship between $\theta$ and $S$ to approximate their exclusion. It thus rests on economic intuition rather than additional statistical asymptotic properties. The resulting estimates form an empirical interval that can be interpreted as *bounded* (intervals of) incidence effects for inframarginal firms, robust to composition changes driven by subsidy-induced entry.

### 6.3 Hiring propensity

We first evaluate the predictive content of the propensity score model for the pre-reform entrant cohort of 2014, which serves as our baseline for the trimming procedure. The probit specification includes four lags of VAT reporting, turnover, inputs, and investment, interacted with reporting status, as well as firm age (quadratic).[27] We also control for sector, legal form, quarter, and arrondissement fixed effects, which are omitted from the coefficient tables for brevity. The estimation sample comprises all eligible firms (not-yet-entrants) without paid employees in the preceding year, i.e. both solo self-employed and potential entrants. In Cohort 2014 (the training dataset), there were 2.56 million not-yet-entrants and

[27] We code VAT turnover, inputs, and investment as zero whenever no VAT return is filed, and include a reporting dummy in each lag. This 'zero plus dummy' approach is equivalent to a missing-indicator specification, and ensures that financial variables contribute to prediction only when reporting takes place. Non-reporters therefore enter the estimation consistently, without artificially attributing economic meaning to zeros.

0.8% of them entered by the end of their cohort time; in Cohort 2016 (the extrapolation dataset), there were 2.72 million not-yet-entrants and 0.90% entered. This illustrates that hiring the first employee was a very rare (and challenging) event for starting businesses. With this propensity score model estimated, we apply it and predict the hiring propensity scores to both Cohort 2014 (in-sample prediction) and Cohort 2016 (out-of-sample prediction).

To visualise the distribution of estimated scores, Figure 5 plots histograms of predicted hiring propensities for our main analyses of two cohorts, Cohort 2016 (the treated, post-reform cohort) and Cohort 2014 (the untreated, pre-reform one). The lower panel shows the distribution for Cohort 2014, and the upper applies the same fitted model to Cohort 2016. The 2016 distribution is visibly shifted towards lower values relative to 2014. This pattern is consistent with the reform inducing a set of low-propensity firms into hiring, thereby lowering the average predicted probability of hiring within the entrant pool. This distributional shift underscores the need for trimming: without adjustment, post-reform entrants include a disproportionate share of marginal firms, and comparisons to pre-reform entrants would be unbalanced. Appendix E.3.2 reports details for the estimation results.

## 6.4 Bounded effects and incidence parameters

This section implements the trimming strategy and presents bounded effects on wages and employment as well as recalculates the employee incidence share parameters. Throughout this section, we focus on the coefficient $\hat{\beta}_{2016}$, the treatment effect from the simple two-by-two DiD comparing Cohort 2016 with Cohort 2014. As established in the previous section, this is the valid baseline comparison, given the satisfactory pre-trend checks and the robustness of the results to alternative specifications. The trimming exercise therefore sharpens inference by examining how this core effect varies once the most marginal firms are progressively excluded.

Figure 6 plots the DiD estimates for the five outcomes (per-FTE wages, pay-

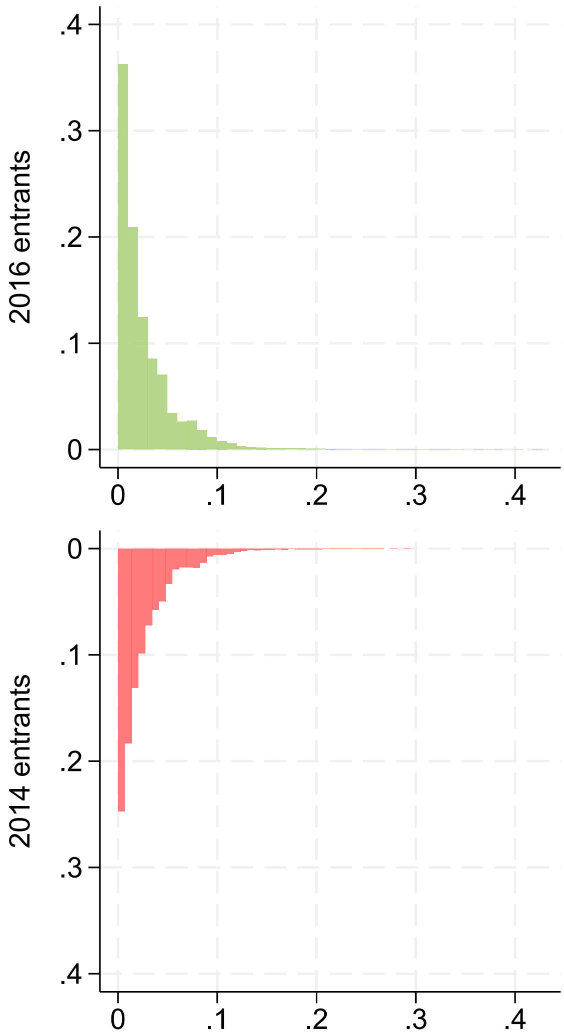



**Figure 5.** Distribution of predicted hiring propensities: Cohorts 2016 vs 2014

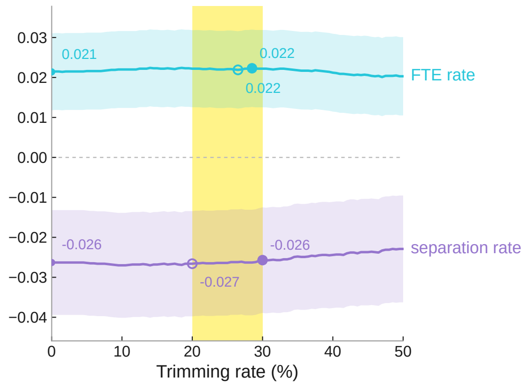
*Notes:* Predicted propensity scores (horizontal axis) from the probit model in Table A6. Vertical axis is the fraction of firms. Only Cohort 2014 is used for training the prediction model; the model is then applied for out-of-sample prediction for Cohort 2016 as well. The 2016 distribution is shifted towards lower values, consistent with induced entry of marginal firms after the reform.

roll taxes, labour costs; total earnings, payroll taxes, and labour costs) against the trimming rate, respectively in evaluation horizons of 1 year, 2 years, and 3 years. For each outcome and horizon, the solid curve shows the effect when progressively excluding firms with the lowest entry propensities estimated in Section 6.3. The yellow band highlights the interval between 20% and 30% trimming, which corresponds to the range of prior beliefs about the fraction of marginal entrants (Cockx and Desiere, 2024; Deng et al., 2026). The empirical bounds given by Eq. (6) are then given by the minimum (labelled with a hollow circle) and maximum (labelled with a solid circle) effects within this band.

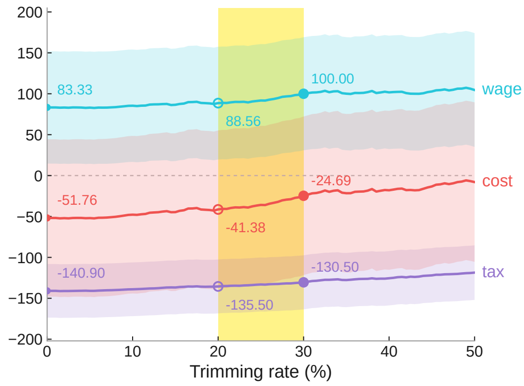
Overall, the bounded estimates demonstrate that the main results are not driven by extreme selection at the margin. The empirical bounds within the 20–30% range are narrow and close to the baseline without trimming.

First, trimming out low-propensity entrants has little effect on the estimated treatment effects. Across all working-time and per-FTE outcomes, the curves remain nearly flat as the trimming rate $t$ increases from 0 to 50 percent. Within the prior-belief band $t \in [0.20, 0.30]$, the estimated effects are essentially unchanged over one, two, or three years post hiring: FTE rates rise by about 0.022–0.025 (approximately two percentage points), separation rates fall by 2.2–2.5 pps, while per-FTE wages increase by €80–100 per quarter and payroll taxes fall by €130–175, leaving total labour costs slightly decreased. This confirms that composition effects from subsidy-induced excess entry do not drive the main results: wage, tax, and working-time responses are robust to the exclusion of marginal firms. The bounded effects on the per-employee total outcomes (working time $\times$ per-FTE measures) are in Figure A10. Similarly, trimming has little impact on the treatment effects.

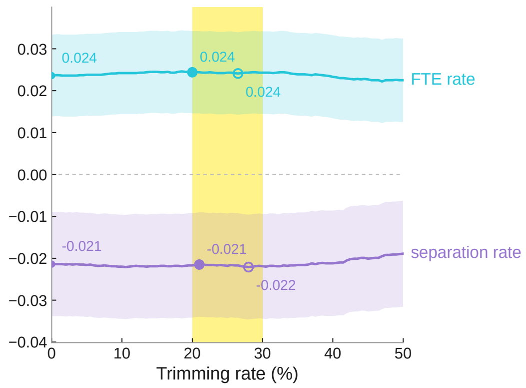
Second, the bounded incidence measures in Figure 7 show only mild upward shifts with higher trimming. On a per-FTE basis (Panel a), within one, two, or three years post hiring, the employee share of the payroll tax relief rises from 0.30–0.37 at zero trimming to about 0.36–0.43 within the prior-belief band; on a per-employee (earnings) basis (Panel b), it rises from 0.65–0.69 at zero trim-



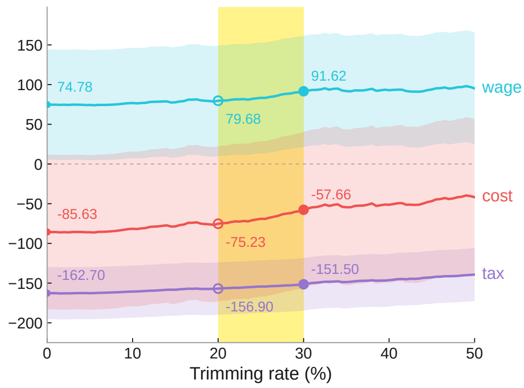

**(a)** Working time: 1 year



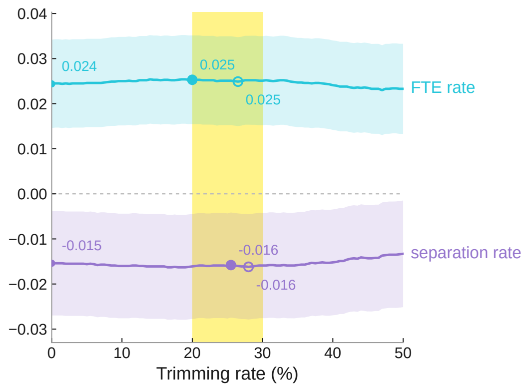

**(b)** Per-FTE measures: 1 year



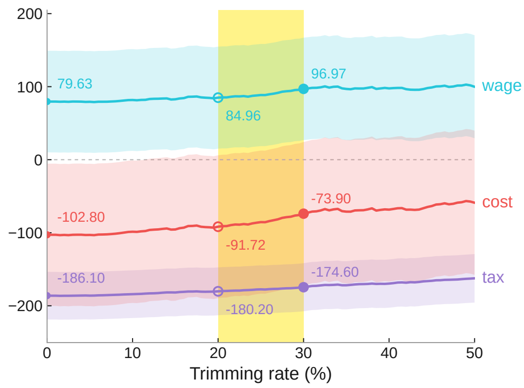

**(c)** Working time: 2 years




**(d)** Per-FTE measures: 2 years




**(e)** Working time: 3 years

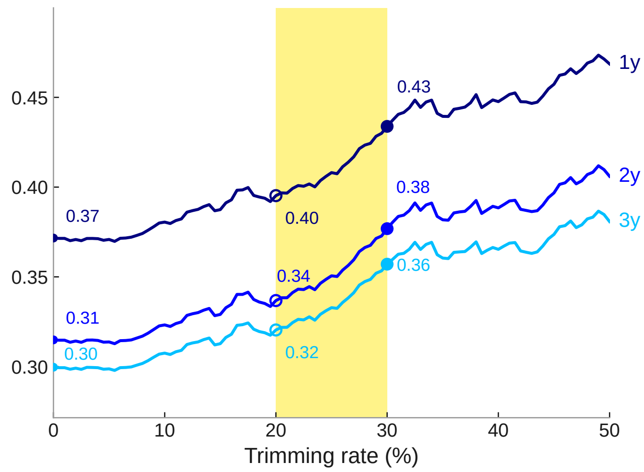



**(f)** Per-FTE measures: 3 years

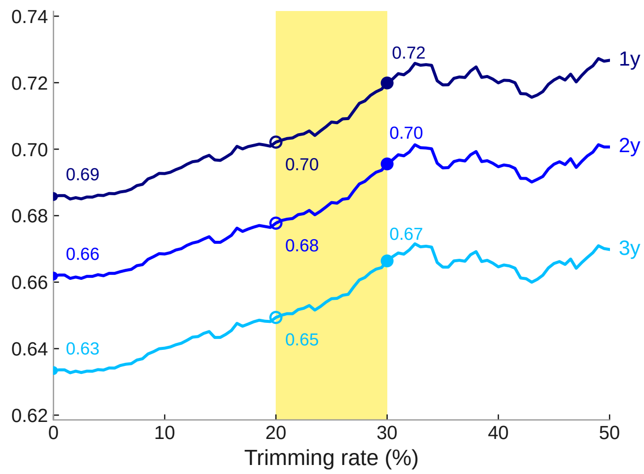
**Figure 6.** Bounded effects on core outcomes, within one, two, or three years

*Notes:* Estimates with 95% confidence intervals are plotted against progressive trimming rates ranging from $t \in [0, 50\%]$ with step size of 0.5%, by deleting entrants from Cohort 2016 with the lowest estimated hiring propensity scores below the $t$-th percentile. The true excess rate $\lambda$ is believed to be within the prior-belief band of $[20\%, 30\%]$ as indicated by the highlighted vertical region. For Panels a, c, and e, employee retention is replaced with employee separation=1−retention so it lies below the horizontal axis for clarity.

ming to about 0.67–0.72 within the prior-belief band. This pattern indicates that newly induced, low-productivity entrants—those dropped at higher $t$—shared slightly less of the fiscal gain with workers. Once excluded, the estimated employee incidence stabilises around one-third on the per-FTE basis and two-thirds on the earnings basis.




**(a)** Per-FTE basis




**(b)** Per-employee total basis

**Figure 7.** Bounded incidence measures, within one, two, or three years

*Notes:* Estimates (without confidence intervals) are plotted against progressive trimming rates ranging from $t \in [0, 50\%]$ with step size of 0.5%, by deleting entrants from Cohort 2016 with the lowest estimated hiring propensity scores below the $t$-th percentile. The true excess rate $\lambda$ is believed to be within the prior-belief band of $[20\%, 30\%]$ as indicated by the highlighted vertical region.

Theoretical interpretation aligns with these findings. In the competitive baseline with a single homogeneous labour market, all entrants face a common equilibrium wage $w^{\text{eq}}(\tau, \kappa)$; thus, by construction, incidence cannot vary across firms. Yet in reality, small firms differ in contract forms and working-time arrangements even when wages are institutionally compressed. Belgian joint-committee bargaining imposes sectoral minimum wages and fixed pay scales that constrain individual adjustments (Momferatou et al., 2008; OECD, 2020), while limited monopsony power (Card et al., 2018; Manning, 2021) and fixed hiring costs generate firm-specific frictions. Consequently, rather than altering posted wages alone, employers transmit part of the tax relief by expanding hours and improving retention—a broader, dynamic form of rent sharing consistent with our theoretical model and with the idea that incidence depends jointly on entry and wage-setting margins.

# 7 Conclusion

This paper asks a central question: who benefits when small firms receive a payroll tax exemption for hiring their first employee? We focus on a uniquely designed policy—the 2016 Belgian reform that permanently abolished employer social security contributions for the first hire.

Empirically, we document that at the first-hire threshold, roughly one third of the statutory relief accrues to employees on a per-FTE basis and about two thirds once adjustments in working time and retention are taken into account. While most tax incidence studies do not have the exact observation on working hours and thus only use total earnings, we document that whether adjusting for working time matters in deriving this incidence parameter. And thus, in terms of the classical incidence definition (on the FTE basis), one-third is our preferred interpretation. Our results thereby provide a micro-foundation for understanding why small-firm hiring subsidies can raise employment and wages jointly thereby benefitting both employers and employees.

To interpret its effects, we develop a theoretical framework that unifies two phenomena typically studied separately: the surge of excess entry following payroll tax cuts and the division of their incidence between firms and workers. Our model formalises how payroll tax reductions lower the productivity threshold for hiring, thereby linking firm entry decisions with tax incidence. It generalises standard managerial-ability models (Evans and Jovanovic, 1989; Jovanovic, 1982; Lucas, 1978) to the employer margin and shows that even when the labour market is competitive, the possibility of entry amplifies effective labour-demand elasticity, leading new employers to absorb a significant share of the tax burden.

Methodologically, we address an ontological challenge—identifying tax incidence for firms with a changing composition due to the reform. By combining pre-reform productivity proxies with a theory-guided sample-trimming correction, we make this counterfactual estimand empirically tractable. This approach isolates incidence among inframarginal entrants only by removing the marginal ones and thus retaining a balanced composition. Beyond its practical use, the method contributes conceptually by extending what can be identified in *treatment-at-birth* settings, where the treated entities themselves are endogenously created by the policy.

Taken together, the paper contributes to the literatures on tax incidence, entrepreneurship, and small firm dynamics by showing that who enters and how they share fiscal gains with workers are jointly determined outcomes of labour taxation.

Future work shall further strengthen the treatment to our theoretical model and the statistical causal-inference framework, bringing them into fully-fledged stand-alone research papers.

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# Appendix

# A Policy details

**Table A1.** Maximum tax cups, by employee order and hiring cohort

| Reform time | 1 January 2004 | 1 October 2012 | 1 January 2014 | 1 January 2015 | 1 January 2016 | 1 January 2017 |
|---|---|---|---|---|---|---|
| 1st employee | **€8,200**<br>**13 quarters**<br>5×€1,000<br>8×€400 | **€13,100**<br>**13 quarters**<br>5×€1,500<br>4×€1,000<br>4×€400 | **same** | **€13,750**<br>**13 quarters**<br>5×€1,550<br>4×€1,050<br>4×€450 | **Exemption**<br>**Permanent** | **same** |
| 2nd employee | **€5,200**<br>**13 quarters**<br>13×€400 | **€8,200**<br>**13 quarters**<br>5×€1,000<br>8×€400 | **same** | **€8,850**<br>**13 quarters**<br>5×€1,050<br>8×€450 | **€13,750**<br>**13 quarters**<br>5×€1,550<br>4×€1,050<br>4×€450 | **same** |
| 3rd employee | **€3,600**<br>**9 quarters**<br>9×€400 | **€6,600**<br>**9 quarters**<br>5×€1,000<br>4×€400 | **same** | **€7,050**<br>**9 quarters**<br>5×€1,050<br>4×€450 | **€8,850**<br>**13 quarters**<br>5×€1,050<br>8×€450 | **€11,250**<br>**13 quarters**<br>9×€1,050<br>4×€450 |
| 4th employee | **none** | | **€6,600**<br>**9 quarters**<br>5×€1,000<br>4×€400 | **same** | **€7,050**<br>**9 quarters**<br>5×€1,050<br>4×€450 | **€11,250**<br>**13 quarters**<br>9×€1,050<br>4×€450 |
| 5th employee | **none** | | **€6,600**<br>**9 quarters**<br>5×€1,000<br>4×€400 | **same** | **same** | **€11,250**<br>**13 quarters**<br>9×€1,050<br>4×€450 |
| 6th employee | **none** | | | | **€6,600**<br>**9 quarters**<br>5×€1,000<br>4×€400 | **€11,250**<br>**13 quarters**<br>9×€1,050<br>4×€450 |

*Notes:* In each cell, undiscounted sums of the subsidy caps are reported in the first row, the duration of the subsidy in the second, followed by a decomposition of different caps in each quarter. These caps apply to full-time jobs, and approximately proportionally to part-time jobs with respect to the FTE rate. 'None' means the subsidy for that employee has not been introduced yet. 'Same' means that the terms and caps of the employee rank did not change.

# B Data details

## B.1 Cohorts of interest

We aim for the payroll tax incidence for the first hire by identifying the effects of this 2016 payroll tax exemption for the first worker on wages and employment. We therefore divide entrant firms into four broad entry cohorts according to the quarter of first hire and the policy regime in effect. We are mainly interested in the comparison between pre-reform cohorts and post-reform cohorts.

Older-regime cohorts cover firms hiring their first employee before 2012Q4. These cohorts operated under earlier versions of the subsidy, which differed in both duration and cap levels. They are included in the descriptive and robustness analyses for completeness, but our main comparisons focus on later, more stable regimes.

**Pre-reform cohorts** include firms hiring their first employee between 2012Q4 and 2014Q4, when the capped subsidy schedule remained unchanged. We shall take these firms and their employees as the base cohorts for our analyses.

Transitional cohorts comprise firms hiring between 2015Q1 and 2015Q4. These firms initially faced the same capped schedule but later benefited from an unexpected adjustment when the 2016 reform introduced more generous exemptions. Deng et al. (2025) study the effect of this temporary tax cut and document a full tax incidence on employers in this context. However, this complex structure makes it difficult to use them as the base cohorts for the 2016 exemption.[28]

**Post-reform cohorts** include firms hiring from 2016Q1 onward, under the permanent exemption that eliminated both the cap and the time limit for the first employee. While this rule remained in place thereafter, the base payroll tax rates were modified again from 2017, as discussed earlier. Because 2016 was the

[28] Deng et al. (2025) analyse this transitional measure and find that the temporary exemption for the 2015 cohort reduced employers' labour costs but did not affect wages or employment. Hence, for wage outcomes, using pre-reform or transitional cohorts as the baseline yields similar results. But for other outcomes such as labour costs, payroll taxes, however, the choice of baseline remains crucial.

first cohort to experience the reform, and given that our data extend only to 2019 (before the COVID-19 shock), this cohort provides the longest observable post-reform horizon of up to three years. Thus, Cohort 2016 will be our main target treated group for the tax incidence study.

## B.2 Entrant sample selection details

A major concern in identifying entrant firms is the existence of *spurious entrants*—as called by Bijnens and Konings (2020); Deng et al. (2026); Geurts and Van Biesebroeck (2016)—firms that appear as new in the dataset with a new identifier. But their 'new' appearance happened solely due to administrative changes or corporate restructuring, while their owners and main businesses remained essentially the same.

We drop those whose employment jumped from zero to above ten immediately at entry, following Bijnens and Konings 2020; Geurts and Van Biesebroeck 2016. Using advanced corporate record–linking techniques, Geurts and Van Biesebroeck (2016) estimate that more than half of these firms immediately hiring more than ten employees were 'spurious' in this sense in Belgium.

Similarly, for the incumbent groups, we require that they cannot hire more than ten employees at the cohort time. Simultaneously, the incumbent firms must be growing in the employment size: if they fire someone at the same time, the net change in the number of employees must be increasing after the hiring event. For one, growing firms might exhibit different wage patterns than shrinking firms; second, this condition ensures that the hiring event does not give them the eligibility to benefit from additional subsidies shortly after.

## B.3 Additional tables for descriptive statistics

**Definitions for education and labour market position** For education in Panel A, We follow the International Standard Classification of Education (ISCED) to categorise employees' highest education attained by the time of hiring: high education is any type of tertiary education (short-cycle, bachelor's, master's, and

PhD); medium education includes upper-secondary and post-secondary non-tertiary education; low education includes lower-secondary education, primary education or below. Every employees is classified as either a blue-collar or a white-collar, and thus the proportion of white-collars equals to one minus that of blue-collars. 'Working' positions consist of employment by other firms and self-employment, which are not mutually exclusive, so the two do not add up to the fractions in 'Working'.

**Table A2.** Descriptive statistics of new hires by annual cohort and group (Cohorts 2013–2016)

| | Entrant cohorts | | | | Incumbent cohorts | | | |
|---|---|---|---|---|---|---|---|---|
| | 2013 | 2014 | 2015 | 2016 | 2013 | 2014 | 2015 | 2016 |
| *N* employees | 30,102 | 31,469 | 30,045 | 36,084 | 33,952 | 33,059 | 34,600 | 38,268 |
| *N* firms | 19,838 | 20,400 | 19,767 | 24,349 | 18,425 | 18,451 | 19,101 | 20,098 |
| Hires per firm | 1.56 | 1.62 | 1.60 | 1.54 | 1.93 | 1.97 | 1.99 | 2.06 |
| **A. Population characteristics** | | | | | | | | |
| Age | 34.8 | 34.8 | 34.8 | 35.0 | 32.8 | 33.0 | 33.2 | 33.4 |
| | (11.9) | (12.0) | (12.0) | (11.8) | (11.4) | (11.5) | (11.6) | (11.7) |
| Education | | | | | | | | |
| High | 19.5% | 18.8% | 18.5% | 18.4% | 20.0% | 19.3% | 18.1% | 16.7% |
| Medium | 43.8% | 43.9% | 43.0% | 42.8% | 44.7% | 43.9% | 44.1% | 43.7% |
| Low | 36.7% | 37.3% | 38.4% | 38.7% | 35.3% | 36.8% | 37.7% | 39.6% |
| Nationality | | | | | | | | |
| Belgium | 83.0% | 81.9% | 81.2% | 80.2% | 84.8% | 83.6% | 83.5% | 82.5% |
| EU | 94.3% | 94.4% | 94.1% | 93.0% | 96.0% | 96.0% | 95.7% | 94.9% |
| Male | 58.3% | 57.0% | 58.2% | 58.3% | 61.4% | 60.2% | 60.2% | 61.0% |
| Blue-collar | 58.7% | 59.4% | 59.8% | 58.6% | 63.4% | 62.6% | 61.4% | 62.0% |
| **B. Labour market positions: 1 quarter before hiring** | | | | | | | | |
| Working | 51.0% | 51.7% | 51.1% | 52.9% | 43.6% | 43.3% | 44.5% | 47.2% |
| Employed | 46.0% | 47.2% | 47.0% | 47.4% | 39.9% | 39.3% | 41.1% | 43.7% |
| Self-employed | 8.0% | 7.7% | 7.1% | 9.0% | 6.0% | 6.3% | 5.8% | 6.2% |
| **C. Employee separation** | | | | | | | | |
| 1 quarter | 24.1% | 24.0% | 24.7% | 23.6% | 28.7% | 28.3% | 29.1% | 29.9% |
| 1 year | 48.8% | 48.2% | 49.3% | 48.1% | 52.4% | 51.8% | 53.1% | 54.3% |
| 3 years | 72.6% | 72.3% | 72.8% | 73.7% | 73.6% | 73.3% | 74.3% | 75.8% |
| **D. Employment and wage records: within 1 year** | | | | | | | | |
| Full-time | 20.3% | 21.3% | 21.6% | 23.2% | 20.5% | 21.7% | 22.4% | 22.1% |
| FTE rate | 0.56 | 0.56 | 0.56 | 0.59 | 0.57 | 0.56 | 0.57 | 0.57 |
| | (0.30) | (0.31) | (0.31) | (0.31) | (0.32) | (0.32) | (0.32) | (0.33) |
| Earnings | 3,885 | 4,006 | 4,023 | 4,125 | 4,083 | 4,266 | 4,267 | 4,180 |
| | (2,717) | (2,762) | (2,780) | (2,778) | (2,833) | (2,845) | (2,862) | (2,862) |
| FTE gross wage | 6,667 | 6,716 | 6,720 | 6,681 | 6,822 | 6,857 | 6,830 | 6,738 |
| | (2,324) | (2,379) | (2,400) | (2,332) | (2,227) | (2,380) | (2,420) | (2,303) |
| FTE payroll tax | 950 | 952 | 921 | 545 | 1,606 | 1,612 | 1,582 | 1,347 |
| | (1,060) | (1,080) | (1,067) | (910) | (1,024) | (1,070) | (1,075) | (1,007) |
| FTE labour cost | 7,619 | 7,668 | 7,642 | 7,232 | 8,431 | 8,473 | 8,414 | 8,088 |
| | (3,168) | (3,235) | (3,225) | (2,879) | (3,152) | (3,340) | (3,383) | (3,187) |
| FTE subsidy | 569 | 578 | 614 | 788 | 3 | 4 | 11 | 23 |
| | (662) | (662) | (692) | (819) | (18) | (22) | (62) | (105) |
| Eff payroll tax rate | 0.119 | 0.117 | 0.114 | 0.068 | 0.213 | 0.211 | 0.207 | 0.175 |
| | (0.131) | (0.132) | (0.133) | (0.123) | (0.111) | (0.116) | (0.118) | (0.116) |

**Table A2**. Descriptive statistics of new hires by annual cohort and group (Cohorts 2013–2016) (continued)

| | Entrant cohorts | | | | Incumbent cohorts | | | |
|---|---|---|---|---|---|---|---|---|
| | 2013 | 2014 | 2015 | 2016 | 2013 | 2014 | 2015 | 2016 |
| **E. Employment and wage records: within 3 years** | | | | | | | | |
| Full-time | 22.6% | 23.3% | 23.8% | 25.9% | 23.5% | 24.7% | 25.2% | 25.0% |
| FTE rate | 0.58 | 0.58 | 0.58 | 0.60 | 0.59 | 0.58 | 0.59 | 0.59 |
| | (0.31) | (0.31) | (0.32) | (0.32) | (0.33) | (0.33) | (0.33) | (0.34) |
| Earnings | 4,059 | 4,149 | 4,173 | 4,314 | 4,283 | 4,435 | 4,440 | 4,377 |
| | (2,866) | (2,880) | (2,905) | (2,939) | (2,999) | (2,999) | (3,007) | (3,017) |
| FTE gross wage | 6,748 | 6,765 | 6,765 | 6,772 | 6,889 | 6,906 | 6,886 | 6,833 |
| | (2,358) | (2,402) | (2,427) | (2,375) | (2,286) | (2,411) | (2,433) | (2,335) |
| FTE payroll tax | 1,039 | 1,012 | 904 | 591 | 1,663 | 1,639 | 1,583 | 1,405 |
| | (1,068) | (1,076) | (1,044) | (918) | (1,035) | (1,066) | (1,063) | (1,007) |
| FTE labour cost | 7,787 | 7,777 | 7,673 | 7,367 | 8,553 | 8,548 | 8,471 | 8,240 |
| | (3,233) | (3,283) | (3,229) | (2,938) | (3,229) | (3,378) | (3,392) | (3,231) |
| FTE subsidy | 525 | 533 | 609 | 786 | 3 | 3 | 9 | 19 |
| | (598) | (601) | (687) | (812) | (15) | (21) | (48) | (86) |
| Eff payroll tax rate | 0.130 | 0.125 | 0.112 | 0.074 | 0.219 | 0.214 | 0.206 | 0.182 |
| | (0.127) | (0.128) | (0.129) | (0.121) | (0.108) | (0.112) | (0.114) | (0.112) |
| **F. Employment and wage records: previous job** | | | | | | | | |
| Full-time | 28.7% | 28.7% | 29.3% | 30.8% | 26.6% | 27.5% | 28.5% | 29.4% |
| FTE rate | 0.66 | 0.67 | 0.67 | 0.68 | 0.64 | 0.65 | 0.65 | 0.66 |
| | (0.30) | (0.30) | (0.30) | (0.30) | (0.32) | (0.32) | (0.32) | (0.32) |
| Earnings | 4,833 | 4,961 | 4,979 | 5,053 | 4,669 | 4,913 | 4,881 | 4,850 |
| | (3,217) | (3,265) | (3,298) | (3,243) | (3,169) | (3,256) | (3,269) | (3,158) |
| FTE gross wage | 7,026 | 7,135 | 7,142 | 7,134 | 7,030 | 7,211 | 7,184 | 7,071 |
| | (2,573) | (2,593) | (2,639) | (2,588) | (2,354) | (2,485) | (2,529) | (2,379) |
| FTE payroll tax | 1,668 | 1,680 | 1,648 | 1,522 | 1,752 | 1,791 | 1,759 | 1,614 |
| | (1,161) | (1,183) | (1,203) | (1,178) | (1,109) | (1,141) | (1,161) | (1,104) |
| FTE labour cost | 8,699 | 8,820 | 8,803 | 8,666 | 8,789 | 9,007 | 8,961 | 8,693 |
| | (3,639) | (3,677) | (3,744) | (3,650) | (3,379) | (3,534) | (3,613) | (3,370) |
| FTE subsidy | 70 | 102 | 127 | 153 | 29 | 52 | 62 | 75 |
| | (211) | (291) | (329) | (368) | (118) | (199) | (226) | (250) |
| Eff payroll tax rate | 0.211 | 0.209 | 0.203 | 0.187 | 0.226 | 0.226 | 0.221 | 0.205 |
| | (0.116) | (0.117) | (0.119) | (0.120) | (0.112) | (0.112) | (0.115) | (0.116) |

*Notes:* Refer to the footnote of Table 1 for variable definitions.

**Table A3.** Sector and joint committee distribution of new hires by cohort and group

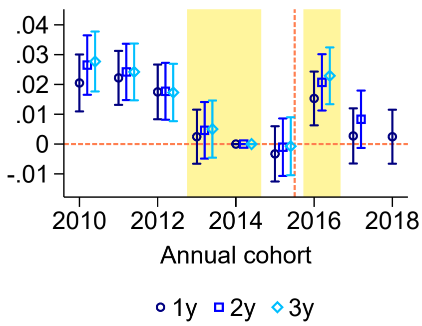
| | Entrant cohorts | | | | Incumbent cohorts | | | |
|---|---|---|---|---|---|---|---|---|
| | 2013 | 2014 | 2015 | 2016 | 2013 | 2014 | 2015 | 2016 |
| *N* employees | 30,102 | 31,469 | 30,045 | 36,084 | 33,952 | 33,059 | 34,600 | 38,268 |
| **A. Sectors** | | | | | | | | |
| Food and beverages | 20.2% | 21.5% | 22.1% | 21.6% | 21.2% | 22.8% | 23.4% | 24.9% |
| Retail trade | 14.9% | 14.5% | 14.6% | 13.9% | 11.7% | 11.7% | 11.9% | 11.7% |
| Specialised construction | 10.9% | 10.5% | 9.9% | 10.0% | 10.1% | 8.8% | 8.9% | 8.3% |
| Wholesale trade | 5.7% | 5.0% | 5.2% | 5.1% | 7.6% | 7.5% | 7.3% | 7.1% |
| Head offices | 4.2% | 4.4% | 4.4% | 5.5% | 2.0% | 2.4% | 2.1% | 2.4% |
| Others | 44.1% | 44.1% | 43.8% | 43.9% | 47.4% | 46.8% | 46.4% | 45.7% |
| **B. Joint committees** | | | | | | | | |
| Hotel industry | 22.87% | 24.59% | 25.45% | 25.51% | 23.22% | 25.10% | 25.50% | 27.26% |
| Indepent retailers | 12.17% | 11.86% | 11.63% | 11.36% | 9.11% | 9.09% | 8.96% | 9.06% |
| Construction industry | 12.87% | 11.84% | 10.99% | 11.00% | 10.86% | 9.12% | 8.97% | 8.45% |
| Transport and logistics | 2.72% | 2.92% | 2.93% | 3.57% | 4.36% | 4.57% | 4.07% | 4.38% |
| Hairdressing and beauty industry | 3.12% | 3.77% | 3.41% | 3.38% | 1.60% | 1.50% | 1.50% | 1.25% |
| Liberal professions | 2.95% | 3.59% | 2.65% | 2.58% | 1.59% | 1.40% | 1.61% | 1.31% |
| Horticultural industry | 3.14% | 2.56% | 3.51% | 2.56% | 3.93% | 3.99% | 3.69% | 3.37% |
| Sectors related to metal, mechanical and electrical construction | 2.75% | 2.47% | 2.54% | 2.34% | 2.69% | 2.57% | 2.53% | 2.53% |
| Garage company | 1.48% | 1.59% | 1.87% | 1.89% | 1.40% | 1.32% | 1.37% | 1.40% |
| Cleaning | 1.54% | 1.54% | 1.52% | 1.52% | 1.57% | 1.69% | 1.63% | 1.38% |
| Others | 34.40% | 33.26% | 33.50% | 34.28% | 39.67% | 39.66% | 40.16% | 39.61% |

*Notes:* Sectors in Panel A refer to the 2-digit NACE (Statistical Classification of Economic Activities in the European Community) codes of firms' main businesses. The five most common industries are: Food and beverage service activities (code 56); Retail trade, except of motor vehicles and motorcycles (47); Specialised construction activities (43); Wholesale trade, except of motor vehicles and motorcycles (46); and Activities of head offices; management consultancy activities (70). In Panel B, the names and codes of joint committee (*commission paritaire*) can be found on the data warehouse website of the Crossroads Bank for Social Security.

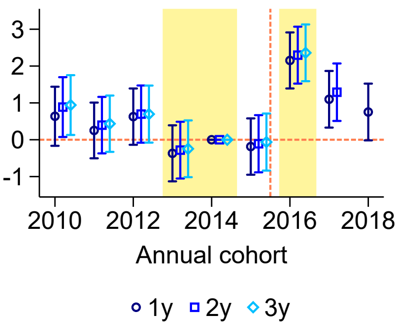
# C Additional simple DiD results

## C.1 Quantity effects (additional outcomes)

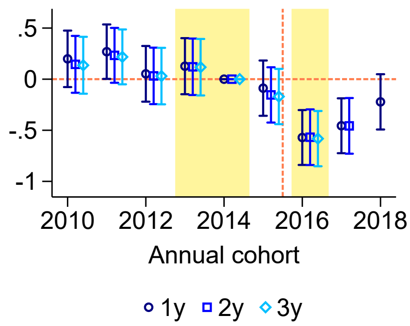
This subsection (Figure A1) focuses on three additional outcomes capturing the intensive margin of working time. Panel (a): an employee is classified as a full-time one if their quarterly FTE rate is 100%; otherwise they are part-time ones. Panels (b) and (c) plot employees' number of days working under full-time schedules (full hours a day), and under part-time schedules (less than the full hours), multiplied by the corresponding rate converting part-time into an FTE in that day.




**(a)** Full-time employee share




**(b)** Days worked as full-time



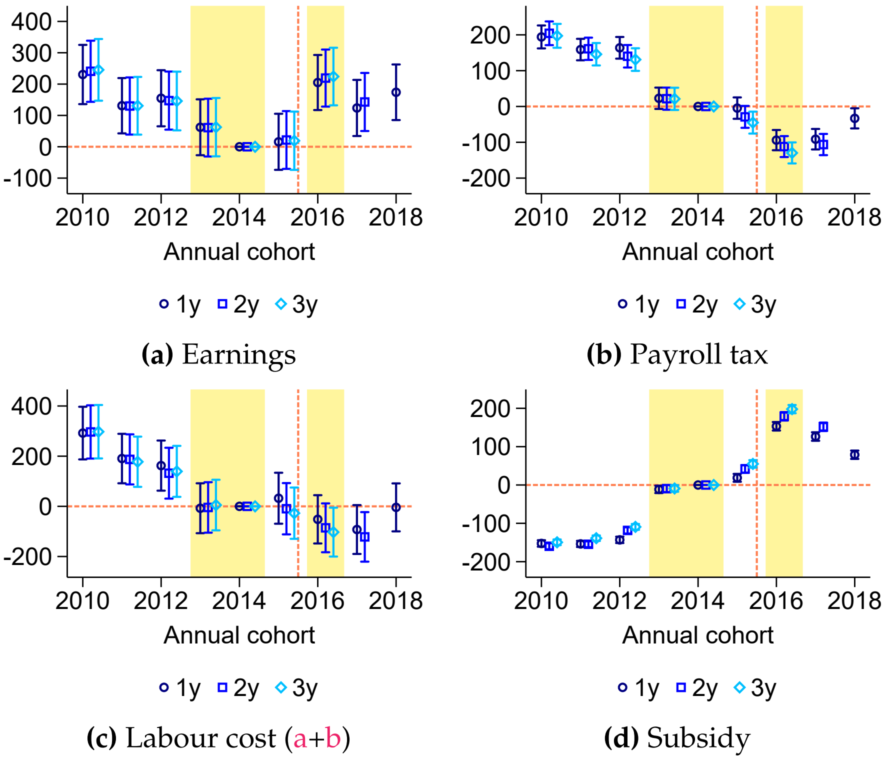

**(c)** Days worked as part-time

**Figure A1.** Additional effects on working time: Simple DiD estimates

*Notes:* Simple DiD estimates using Eq (1) on employer–employee matched level outcomes. Three annual cohorts (2013, 2014, and 2016) are within a stable period for the DiD design as indicated by the highlighted shade. No covariates or fixed effects. Coefficients show entrants versus incumbents; base cohort is 2014. Horizontal axis is the redefined annual cohort of hiring. Vertical dashed line between 2015 and 2016; horizontal dashed line at 0. For each annual cohort, the outcome is evaluated in three different horizons: 1 year (circle), 2 years (square), and 3 years (diamond). Spikes show the 95% confidence intervals with standard errors clustered at the firm level. All defined as a cumulative average within the respective evaluation period.

## C.2 Total effects on earnings and costs

Outcomes in this subsection are defined as price (per-FTE measures in Section 4.2) $\times$ FTE rate as working time (Figure 3b in Section 4.1).




**(a)** Earnings

**(b)** Payroll tax

**(c)** Labour cost (a+b)

**(d)** Subsidy

**Figure A2.** Effects on (total) earnings and costs: Simple DiD estimates

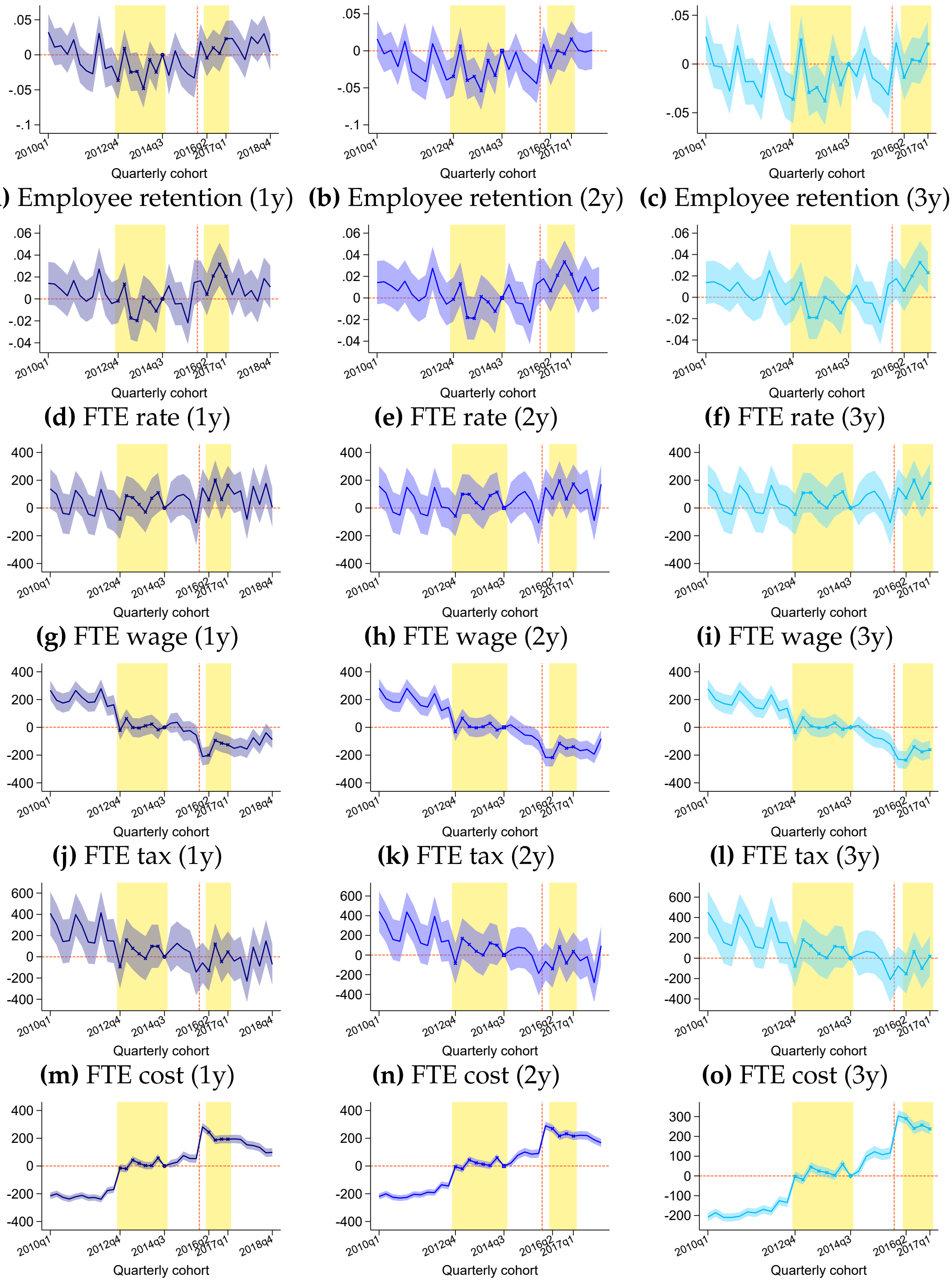
*Notes:* Simple DiD estimates using Eq (1) on employer–employee matched level outcomes. Three annual cohorts (2013, 2014, and 2016) are within a stable period for the DiD design as indicated by the highlighted shade. No covariates or fixed effects. Coefficients show entrants versus incumbents; base cohort is 2014. Horizontal axis is the redefined annual cohort of hiring. Vertical dashed line between 2015 and 2016; horizontal dashed line at 0. For each annual cohort, the outcome is evaluated in three different horizons: 1 year (circle), 2 years (square), and 3 years (diamond). Spikes show the 95% confidence intervals with standard errors clustered at the firm level. All outcomes defined as a cumulative average within the respective evaluation period.

## C.3 Robustness checks

### C.3.1 Quarterly cohort regressions

Figure A3 repeats the DiD analyses but at the quarterly cohort level of the six outcome variables (at three horizons—1 year, 2 years, 3 years) studied in Sections 4.1 and 4.2. Rows 1 and 2 correspond to the working time as in Figures 3 and

Rows 4–6 to the per-FTE wage, payroll tax, labour cost, and subsidy as in Figure 4.




**(a)** Employee retention (1y) **(b)** Employee retention (2y) **(c)** Employee retention (3y)

**(d)** FTE rate (1y) **(e)** FTE rate (2y) **(f)** FTE rate (3y)

**(g)** FTE wage (1y) **(h)** FTE wage (2y) **(i)** FTE wage (3y)

**(j)** FTE tax (1y) **(k)** FTE tax (2y) **(l)** FTE tax (3y)

**(m)** FTE cost (1y) **(n)** FTE cost (2y) **(o)** FTE cost (3y)

**(p)** FTE subsidy (1y) **(q)** FTE subsidy (2y) **(r)** FTE subsidy (3y)

**Figure A3.** Quarterly cohort specification: Simple DiD estimates

*Notes:* Simple DiD estimates using the quarterly-cohort specification Eq (1) on employer-employee matched level outcomes. 12 quarterly cohorts (2012Q4–2014Q3, 2016Q2–2017Q1), or equivalently, three annual cohorts (2013, 2014, and 2016) are within a stable period for the DiD design as indicated by the highlighted shade. No covariates or fixed effects. Coefficients show entrants versus incumbents; base cohort is 2014Q3. Horizontal axis is the quarterly cohort of hiring. Vertical dashed line between 2015Q4 and 2016Q1; horizontal dashed line at 0. Each row is the same outcome, and each column is one evaluation horizon: left for 1 year, middle for 2 years, and right for 3 years. (a)–(c) Employee retention is the end-of-period value, and the others are cumulative averages. Shaded areas show the 95% confidence intervals using standard errors clustered at the firm level.

### C.3.2 Controlling for fixed effects

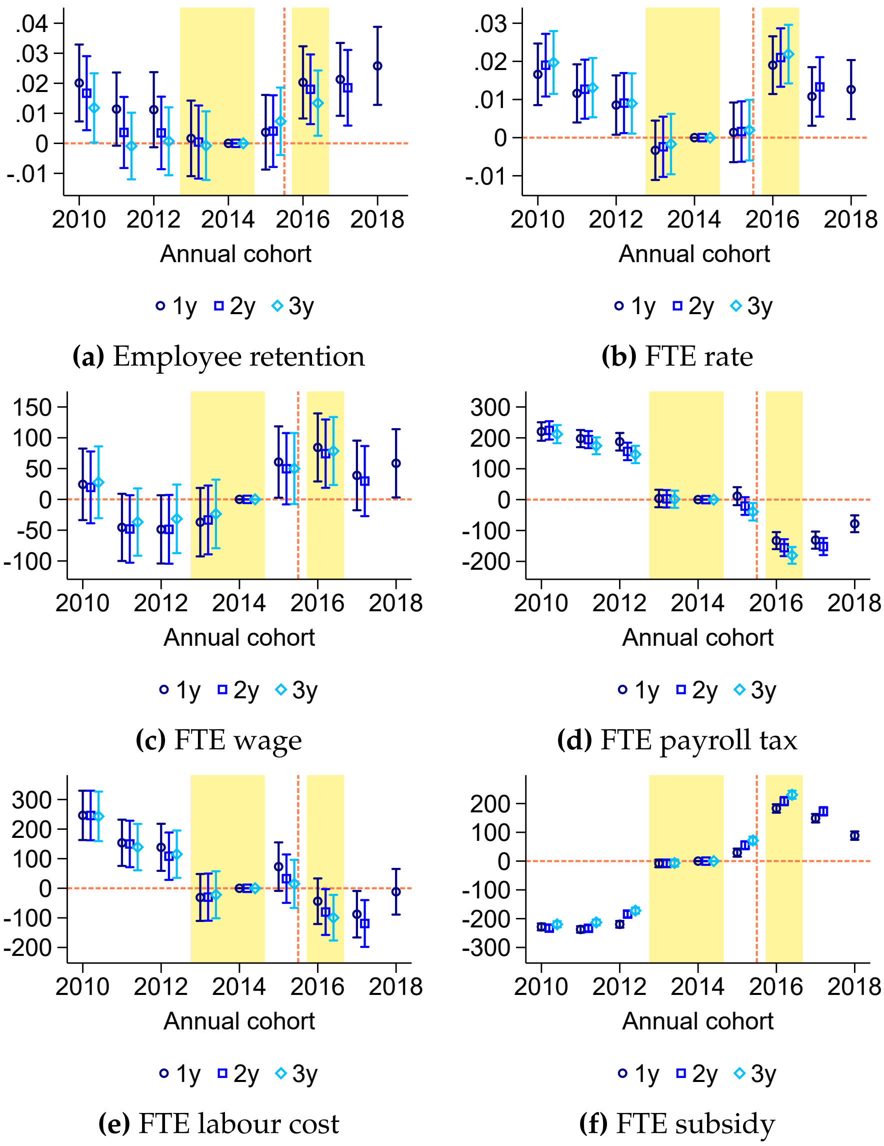
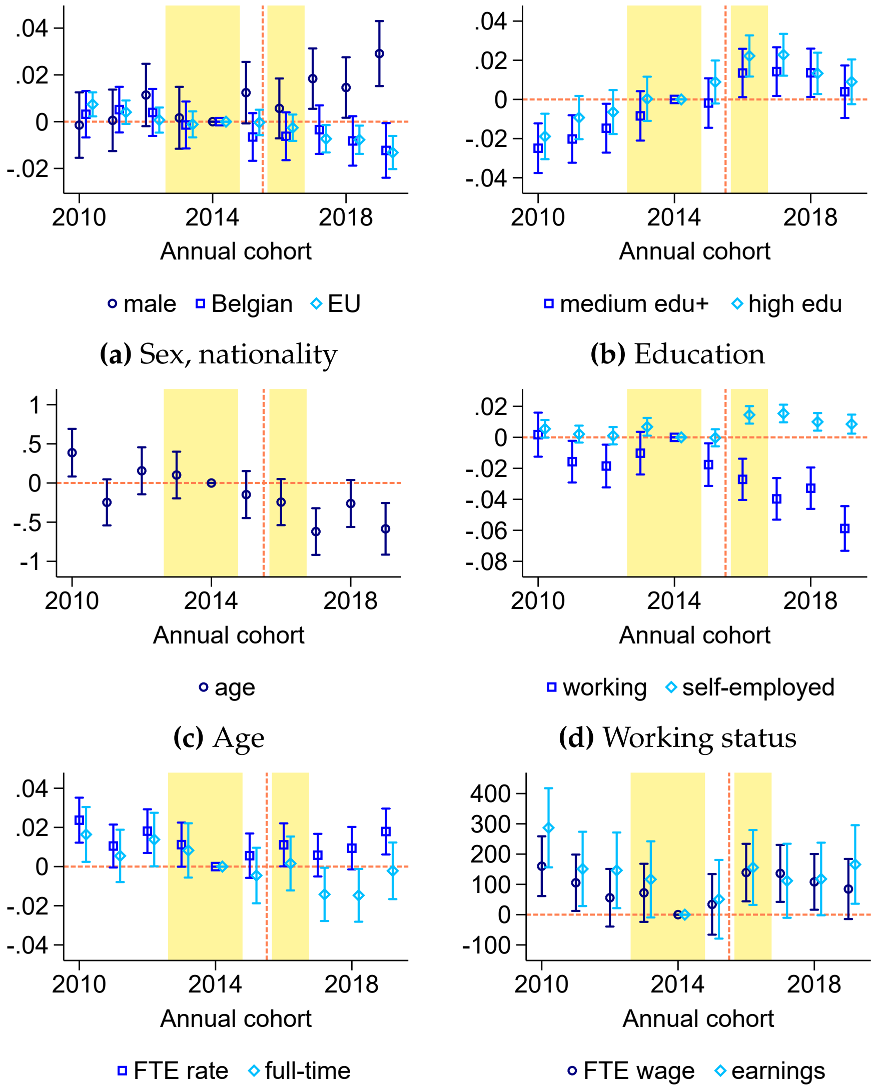



**Figure A4.** Effects on working time and per-FTE wages and costs: Simple DiD estimates with fixed effects

*Notes:* Simple DiD estimates using Eq (1) on employer–employee matched level outcomes. Three annual cohorts (2013, 2014, and 2016) are within a stable period for the DiD design as indicated by the highlighted shade. Fixed effects of joint committees, arrondissements, and NACE 2-digit sectors are controlled for. No covariates are controlled for. Coefficients show entrants versus incumbents; base cohort is 2014. Horizontal axis is the redefined annual cohort of hiring. Vertical dashed line between 2015 and 2016; horizontal dashed line at 0. For each annual cohort, the outcome is evaluated in three different horizons: 1 year (circle), 2 years (square), and 3 years (diamond). Spikes show the 95% confidence intervals with standard errors clustered at the firm level. (a) Employee retention is end-of-period value; all others are defined as a cumulative average within the respective evaluation period.

### C.3.3 Employee characteristics

**Employee characteristics as outcomes** Panel A5a plots the evolution of employees' sex (male=1, female=0), dummies indicating whether they are Belgian or EU nationals. Panel A5b analyses dummies indicating whether they have obtained at least medium education or higher education. Panel A5c analyses their age (in years) when entering this firm. Panel A5d analyses their past employment status, in the preceding quarter before joining this firm. 'Working' means either employed (by others) or self-employed. Panel A5e analyses the working time in their preceding job (only applicable to those who worked in the previous quarter), focusing on the FTE rate and a dummy indicating whether they worked full-time (FTE rate=100%) in that quarter. Panel A5f analyses their wage rate (per-FTE measure) and earnings in their past job in the preceding quarter (also only applicable to those who worked).




**(a)** Sex, nationality **(b)** Education

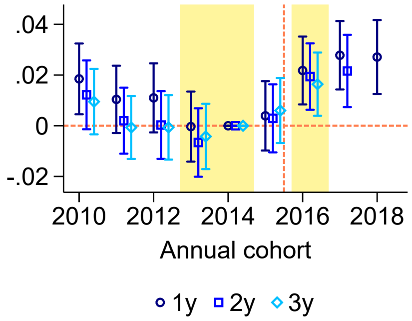
**(c)** Age **(d)** Working status

**(e)** Working time **(f)** Wage and earnings

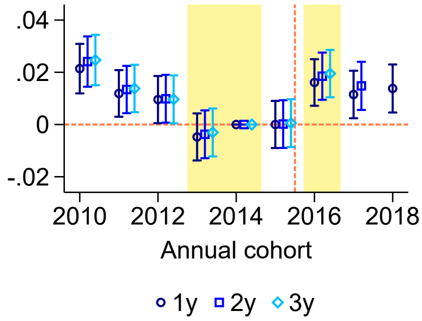
**Figure A5.** Effects on employee composition: Simple DiD estimates

*Notes:* Simple DiD estimates using Eq (1) on employee characteristics as outcomes. Three annual cohorts (2013, 2014, and 2016) are within a stable period for the DiD design as indicated by the highlighted shade. No fixed effects. Coefficients show entrants versus incumbents; base cohort is 2014. Horizontal axis is the redefined annual cohort of hiring. Vertical dashed line between 2015 and 2016; horizontal dashed line at 0. Spikes show the 95% confidence intervals with standard errors clustered at the firm level.

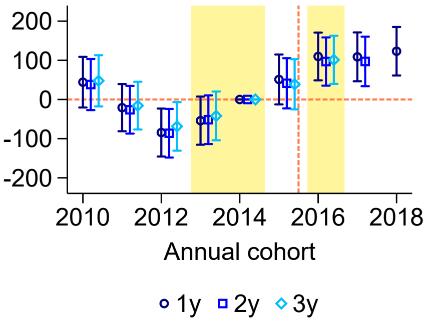
## Controlling for employees' covariates



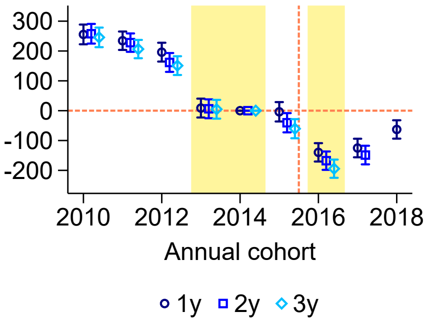

**(a)** Employee retention



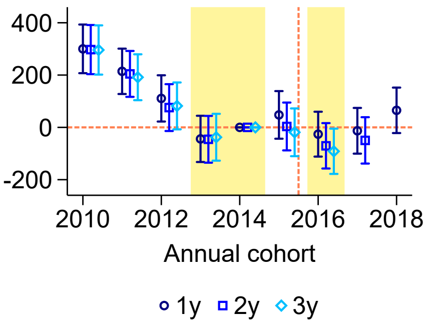

**(b)** FTE rate



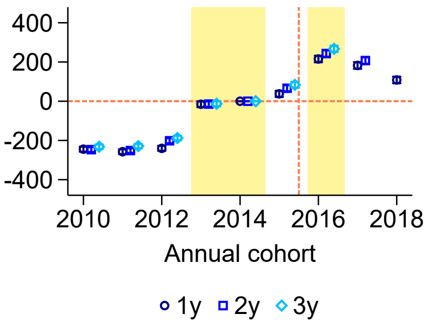

**(c)** FTE wage




**(d)** FTE payroll tax




**(e)** FTE labour cost

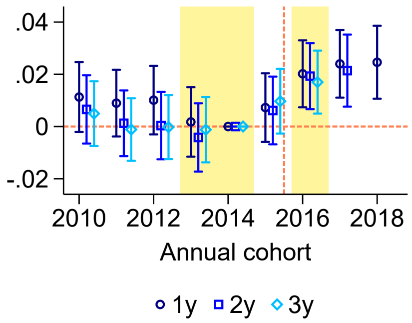



**(f)** FTE subsidy

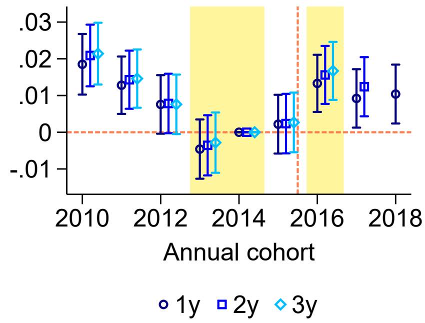
**Figure A6.** Effects on working time and per-FTE wages and costs: Simple DiD estimates controlling for employee covariates

*Notes:* Simple DiD estimates using Eq (1) on employer–employee matched level outcomes. Three annual cohorts (2013, 2014, and 2016) are within a stable period for the DiD design as indicated by the highlighted shade. No fixed effects. Employees' covariates (age, education, nationality, gender, past employment status, blue- or white-collar) are controlled for. Coefficients show entrants versus incumbents; base cohort is 2014. Horizontal axis is the redefined annual cohort of hiring. Vertical dashed line between 2015 and 2016; horizontal dashed line at 0. For each annual cohort, the outcome is evaluated in three different horizons: 1 year (circle), 2 years (square), and 3 years (diamond). Spikes show the 95% confidence intervals with standard errors clustered at the firm level. (a) Employee retention is end-of-period value; all others are defined as a cumulative average within the respective evaluation period.

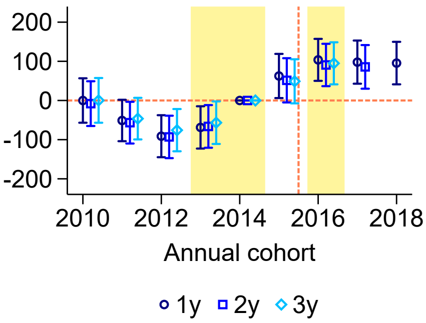
### Controlling for both fixed effects and employees' covariates



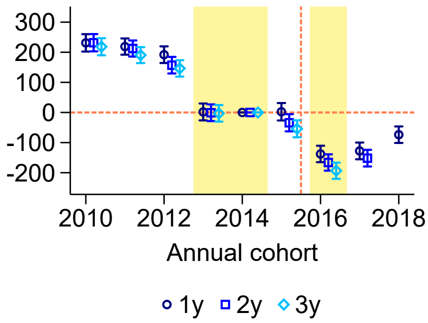

**(a)** Employee retention



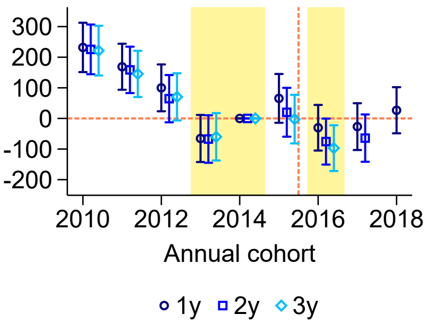

**(b)** FTE rate



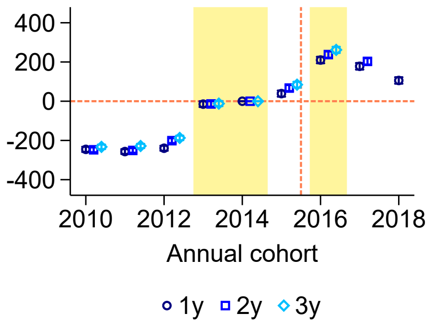

**(c)** FTE wage




**(d)** FTE payroll tax




**(e)** FTE labour cost

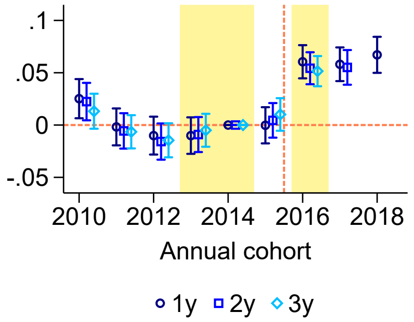



**(f)** FTE subsidy

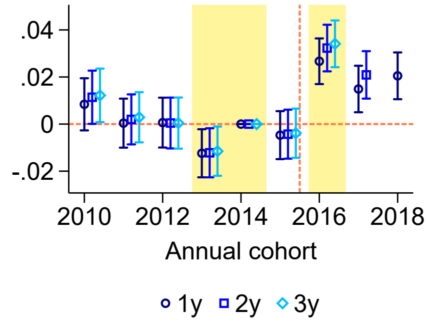
**Figure A7.** Effects on working time and per-FTE wages and costs: Simple DiD estimates controlling for fixed effects and employee covariates

*Notes:* Simple DiD estimates using Eq (1) on employer–employee matched level outcomes. Three annual cohorts (2013, 2014, and 2016) are within a stable period for the DiD design as indicated by the highlighted shade. Fixed effects of joint committees, arrondissements, and NACE 2-digit sectors are controlled for. Employees' covariates (age, education, nationality, gender, past employment status, blue- or white-collar) are controlled for. Coefficients show entrants versus incumbents; base cohort is 2014. Horizontal axis is the redefined annual cohort of hiring. Vertical dashed line between 2015 and 2016; horizontal dashed line at 0. For each annual cohort, the outcome is evaluated in three different horizons: 1 year (circle), 2 years (square), and 3 years (diamond). Spikes show the 95% confidence intervals with standard errors clustered at the firm level. (a) Employee retention is end-of-period value; all others are defined as a cumulative average within the respective evaluation period.

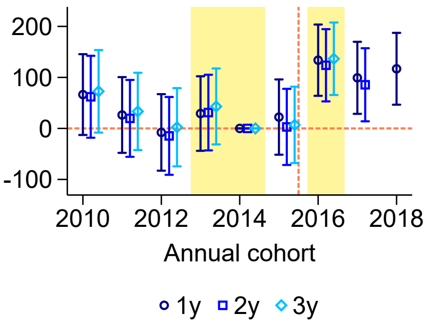
### C.3.4 Effects on only first-employees labelled by the subsidy



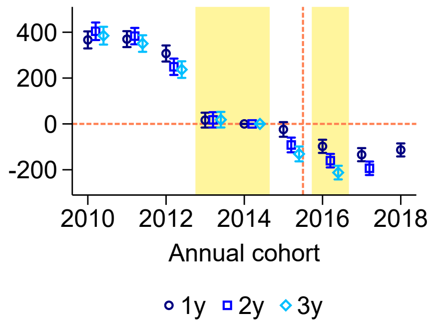

**(a)** Employee retention



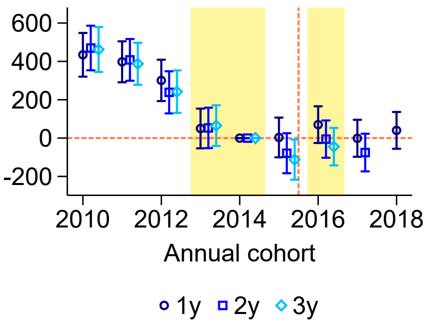

**(b)** FTE rate



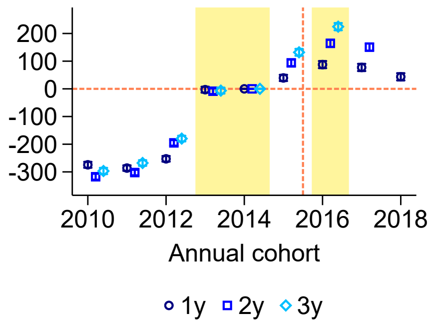

**(c)** FTE wage




**(d)** FTE payroll tax




**(e)** FTE labour cost




**(f)** FTE subsidy

**Figure A8.** Effects on working time and per-FTE wages and costs: Simple DiD estimates, first-employees only

*Notes:* Simple DiD estimates using Eq (1) on employer–employee matched level outcomes. Only employees labelled as the first employees by taker-up firms are included into the entrant group; the incumbent group is unaffected. Three annual cohorts (2013, 2014, and 2016) are within a stable period for the DiD design as indicated by the highlighted shade. No fixed effects. Employees' covariates (age, education, nationality, gender, past employment status, blue- or white-collar) are controlled for. Coefficients show entrants versus incumbents; base cohort is 2014. Horizontal axis is the redefined annual cohort of hiring. Vertical dashed line between 2015 and 2016; horizontal dashed line at 0. For each annual cohort, the outcome is evaluated in three different horizons: 1 year (circle), 2 years (square), and 3 years (diamond). Spikes show the 95% confidence intervals with standard errors clustered at the firm level. (a) Employee retention is end-of-period value; all others are defined as a cumulative average within the respective evaluation period.

Table A4 summarises the treatment effects and their relative effects (compared to the base cohort 2014 entrants' means) of outcome variables we have discussed. Since confidence intervals can be directly found in previous figures, standard errors are omitted. All coefficients are significant at the 5% level except per-FTE labour costs and per-employee labour costs.

**Table A4.** Treatment effects and relative effects of the reform

| | | All the initial hires | | | Only first employees | | |
|---|---|---|---|---|---|---|---|
| | | 1 year | 2 years | 3 years | 1 year | 2 years | 3 years |
| **Panel A. Working time** | | | | | | | |
| Employee retention (pps) | $\hat{\beta}_{2016}$ | 2.63 | 2.14 | 1.54 | 6.05 | 5.43 | 5.15 |
| | $\hat{\mu}_{E,2014}$ | 51.8 | 36.6 | 29.5 | 55.24 | 38.53 | 30.77 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | 5.1% | 5.8% | 5.2% | 11.0% | 14.1% | 16.7% |
| FTE rate (pps) | $\hat{\beta}_{2016}$ | 2.14 | 2.36 | 2.44 | 2.67 | 3.23 | 3.41 |
| | $\hat{\mu}_{E,2014}$ | 56.4 | 57.7 | 57.9 | 65.58 | 66.8 | 67.05 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | 3.8% | 4.1% | 4.2% | 4.1% | 4.8% | 5.1% |
| Full-time share (pps) | $\hat{\beta}_{2016}$ | 1.53 | 2.07 | 2.29 | 2.99 | 3.83 | 4.20 |
| | $\hat{\mu}_{E,2014}$ | 21.38 | 23.04 | 23.46 | 24.94 | 27.1 | 27.65 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | 7.2% | 9.0% | 9.8% | 12.0% | 14.1% | 15.2% |
| **Panel B. Unit price per FTE** | | | | | | | |
| FTE wage (2018€) | $\hat{\beta}_{2016}$ | 83.3 | 74.8 | 79.6 | 133.8 | 123.8 | 136.6 |
| | $\hat{\mu}_{E,2014}$ | 6716 | 6749 | 6765 | 7137.7 | 7168.9 | 7179.6 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | 1.2% | 1.1% | 1.2% | 1.9% | 1.7% | 1.9% |
| FTE payroll tax (2018€) | $\hat{\beta}_{2016}$ | -140.9 | -162.7 | -186.1 | -97.79 | -160.4 | -212.4 |
| | $\hat{\mu}_{E,2014}$ | 951.7 | 995.6 | 1012 | 394 | 492 | 544 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | -14.8% | -16.3% | -18.4% | -24.8% | -32.6% | -39.0% |
| FTE labour cost (2018€) | $\hat{\beta}_{2016}$ | -51.8 | -85.6 | -102.8 | 70.2 | -5.3 | -44.7 |
| | $\hat{\mu}_{E,2014}$ | 7668 | 7745 | 7777 | 7535.3 | 7662.4 | 7724.7 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | -0.7% | -1.1% | -1.3% | 0.9% | -0.1% | -0.6% |
| FTE subsidy (2018€) | $\hat{\beta}_{2016}$ | 190.1 | 215.4 | 237.6 | 87.63 | 164.4 | 224.7 |
| | $\hat{\mu}_{E,2014}$ | 581.6 | 560.0 | 535.2 | 1363.6 | 1284.0 | 1216.5 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | 32.7% | 38.5% | 44.4% | 6.4% | 12.8% | 18.5% |
| **Panel C. Per-employee totals** | | | | | | | |
| Earnings (2018€) | $\hat{\beta}_{2016}$ | 205 | 219.2 | 224.1 | 298.5 | 336.9 | 353.8 |
| | $\hat{\mu}_{E,2014}$ | 4006 | 4120 | 4149 | 4789.3 | 4901.7 | 4924.8 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | 5.1% | 5.3% | 5.4% | 6.2% | 6.9% | 7.2% |
| Payroll tax (2018€) | $\hat{\beta}_{2016}$ | -93.94 | -112 | -129.7 | -109.9 | -158.1 | -197.3 |
| | $\hat{\mu}_{E,2014}$ | 556.8 | 604.1 | 619.9 | 301.5 | 383.1 | 426.4 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | -16.9% | -18.5% | -20.9% | -36.5% | -41.3% | -46.3% |
| Labour cost (2018€) | $\hat{\beta}_{2016}$ | 118.6 | 115.4 | 101.7 | 220.6 | 211.2 | 187.4 |
| | $\hat{\mu}_{E,2014}$ | 4560 | 4721 | 4767 | 5091 | 5285.2 | 5351.1 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | 2.6% | 2.4% | 2.1% | 4.3% | 4.0% | 3.5% |
| Subsidy (2018€) | $\hat{\beta}_{2016}$ | 153.1 | 179.1 | 197.9 | 119.6 | 194.6 | 246.9 |
| | $\hat{\mu}_{E,2014}$ | 372.4 | 362.0 | 343.6 | 913.4 | 861.9 | 807.9 |
| | $\hat{\beta}_{2016}/\hat{\mu}_{E,2014}$ | 41.1% | 49.5% | 57.6% | 13.1% | 22.6% | 30.6% |

*Notes:* This table is a summary of the treatment effects ($\hat{\beta}_{2016}$, the coefficient on Cohort 2016), the base level mean of the outcome ($\hat{\mu}_{E,2014}$, the mean for Cohort 2014 entrants' employees), and the relative effects by dividing the two numbers. Cols 1–3 report our baseline estimation including all initial hires by the entrant firms, and Cols 4–6 only the first employees labelled by the subsidy. The treatment effects ($\hat{\beta}_{2016}$) in Panel A: Cols 1–3 are found in Figures 3 and A1a; Rows 1–2, Cols 4–6 are found in Row 1 of Figure A8. The treatment effects ($\hat{\beta}_{2016}$) in Panel B: Cols 1–3 are found in Figure 4; Cols 4–6 in Rows 2–3 of Figure A8. The treatment effects ($\hat{\beta}_{2016}$) in Panel C: Cols 1–3 are found in Figure A2; Cols 4–6 are not plotted.

# D Theory details

## D.1 Microfoundation for the labour-supply schedule

This appendix provides a minimal microfoundation for the labour-supply curve used in the partial-equilibrium analysis. We present two equivalent constructions—one based on intensive-margin hours with isoelastic disutility, and one based on extensive-margin participation with heterogeneous reservation costs. Both deliver an aggregate Marshallian supply of the form $S(w) = A\, w^{\varepsilon_s}$, where $A > 0$ and $\varepsilon_s > 0$ are primitives.

**A. Intensive-margin microfoundation (isoelastic hours).** Consider a representative household (or a unit mass $N$ of identical workers) that chooses hours $\ell \geq 0$ to maximise

$$u(c, \ell) = c - \frac{\phi}{1 + 1/\varepsilon_s}\, \ell^{1+1/\varepsilon_s}, \qquad \phi > 0,\ \varepsilon_s > 0,$$

subject to the budget constraint $c = \bar{y} + w\,\ell$, where $\bar{y}$ is non-labour income. The first-order condition $u_\ell = 0$ gives $w = \phi\, \ell^{1/\varepsilon_s}$, hence $\ell = (w/\phi)^{\varepsilon_s}$. Aggregating across $N$ identical workers yields the Marshallian supply

$$S(w) = N\,\ell = N\,\phi^{-\varepsilon_s}\, w^{\varepsilon_s}.$$

Defining $A \equiv N\,\phi^{-\varepsilon_s}$ recovers $S(w) = A\, w^{\varepsilon_s}$ with elasticity $\varepsilon_s$.

**B. Extensive-margin microfoundation (participation with heterogeneity).** Let workers supply one unit of labour if and only if their idiosyncratic participation cost $z$ does not exceed the offered wage $w$. Suppose $z$ has cdf $G(z) = \min\{(z/b)^{\varepsilon_s}, 1\}$ on $[0, \infty)$, with $b > 0$ a scale parameter and $\varepsilon_s > 0$. The participation rate is $G(w) = (w/b)^{\varepsilon_s}$ for $w < b$, so total supplied labour is

$$S(w) = N\,G(w) = N\,b^{-\varepsilon_s}\, w^{\varepsilon_s}.$$

Setting $A \equiv N\, b^{-\varepsilon_s}$ again yields $S(w) = A\, w^{\varepsilon_s}$ with elasticity $\varepsilon_s$.

**C. Remarks for the main text.** (i) In the partial-equilibrium model, workers perceive and respond to the pre-tax wage $w$; the employer payroll tax $\tau$ loads on the firm's unit labour cost and does not enter workers' utility. (ii) Either microfoundation justifies the reduced-form supply $S(w) = A\, w^{\varepsilon_s}$; therefore the wage-incidence formula derived in the main text is unaffected by the chosen microfoundation. (iii) A combined model with both hours choice and participation heterogeneity also aggregates to $S(w) = A\, w^{\varepsilon_s}$, with the constant $A$ absorbing the composition across margins.

## D.2 Proofs for entry with exogenous wage

**Preliminaries.** Throughout, $w$ is fixed and $g$ satisfies the maintained assumptions in the main text ($g' > 0$, $g'' < 0$). Let $L^*(\theta; w, \tau)$ and $\Pi^*(\theta; w, \tau, \kappa)$ be the optimiser and optimised value defined in the text. By the envelope theorem, at interior $L^*$ (see Milgrom and Segal, 2002),

$$\Pi^*_\theta(\theta; w, \tau, \kappa) = g(L^*) > 0, \qquad \Pi^*_\tau(\theta; w, \tau, \kappa) = -w\, L^* \leq 0, \qquad \Pi^*_\kappa(\theta; w, \tau, \kappa) = -1 < 0.$$

Because $g'' < 0$, the FOC $\theta g'(L^*) = w(1+\tau)$ implies $L^*$ increases in $\theta$ and decreases in $\tau$ by implicit differentiation; equivalently, monotone comparative statics apply since the objective has increasing differences in $(\theta, L)$ and decreasing differences in $(\tau, L)$ (Milgrom and Shannon, 1994; Topkis, 1998).

**Proof of Lemma 1.** Define $V(\theta) \equiv \Pi^*(\theta; w, \tau, \kappa)$. By continuity and $\Pi^*_\theta > 0$, $V$ is strictly increasing in $\theta$. At $\theta = 0$, $V(0) = \max_{L \geq 0}\{-w(1+\tau)L - \kappa\} = -\kappa < 0$. As $\theta \to \infty$, for any fixed $L > 0$ we have $\theta g(L) - w(1+\tau)L - \kappa \to \infty$, hence $V(\theta) \to \infty$. By the intermediate value theorem and strict monotonicity, there exists a unique $\theta^*$ with $V(\theta^*) = 0$ and $\{\theta : V(\theta) \geq 0\} = [\theta^*, \infty)$. Monotonicity of $\theta^*$ in $\tau$ and $\kappa$ follows since $V$ is (weakly) decreasing in each (envelope derivatives above) and upper contour sets shift monotonically in parameters; see Milgrom and Shannon (1994). □

**Proof of Proposition 1.** (a) For a fixed-cost cut $\kappa_1 < \kappa_0$ with $\tau$ held fixed, Lemma 1 gives $\theta^*(w, \tau, \kappa_1) \le \theta^*(w, \tau, \kappa_0)$. For a per-unit tax cut $\tau_1 < \tau_0$ with $\kappa$ fixed, the same lemma yields $\theta^*(w, \tau_1, \kappa) \le \theta^*(w, \tau_0, \kappa)$. Hence entry (weakly) increases in both cases. (b) Let $\mu(t) \equiv \mathbb{E}[\theta \mid \theta \ge t]$ for the ability distribution used in the text. If $f$ is log-concave on $[0, \infty)$, then $\mu(t)$ is strictly increasing in $t$ (Proposition 1 in Bagnoli and Bergstrom, 2005). Therefore lowering the threshold $t = \theta^*$ after either tax cut weakly reduces the average ability among entrants (strictly so under log-concavity unless the added mass is null). The marginal and inframarginal sets defined in the text coincide with the intervals between and above the ordered thresholds. □

#### D.2.1 Cobb–Douglas illustration (fixed wage)

Let $g(L) = L^\alpha$ with $\alpha \in (0, 1)$ and $c \equiv w(1 + \tau)$. The firm's optimal employment is $L^*(\theta; w, \tau) = \big(\alpha\theta/c\big)^{1/(1-\alpha)}$ and the zero-profit cutoff solves $\Pi^*(\theta; w, \tau, \kappa) = 0$, yielding

$$\theta^*(w, \tau, \kappa) = \Big(\tfrac{\kappa}{(1-\alpha)\,\alpha^{\alpha/(1-\alpha)}}\Big)^{1-\alpha} [w(1 + \tau)]^\alpha.$$

Comparative statics (with $w$ held fixed) follow immediately:

$$\frac{\partial\theta^*}{\partial\tau} = \frac{\alpha}{1 + \tau}\,\theta^* \ge 0, \qquad \frac{\partial\theta^*}{\partial\kappa} = \frac{1 - \alpha}{\kappa}\,\theta^* > 0.$$

Both derivatives are proportional to $\theta^*$, confirming the (weak) increase of the ability threshold with either cost component in this benchmark.

### D.3 Proofs for entry and incidence with endogenous wage

#### D.3.1 Proof of Lemma 2 (endogenous-wage threshold)

**Setup and notation.** Market clearing is $A\, w^{\varepsilon_s} = L^D(w, \tau, \kappa)$ with $\varepsilon_s > 0$. Let $w^{\mathrm{eq}}(\tau, \kappa)$ denote the unique solution (proved below). Define the *effective unit cost* $C(\tau, \kappa) \equiv w^{\mathrm{eq}}(\tau, \kappa)\,(1 + \tau)$ and the endogenous threshold $\theta^\dagger(\tau, \kappa) \equiv \theta^*(w^{\mathrm{eq}}(\tau, \kappa), \tau, \kappa)$.

As in the fixed-$w$ case, write the optimised value as $\Pi^*(\theta; w, \tau, \kappa) = \max_{L \geq 0}\{\theta g(L) - w(1+\tau)L - \kappa\}$ with unique $L^*$. Then this yields the optimised profit $\Pi^\dagger(\theta; \tau, \kappa)$ when wage is endogenised.

**Step 1: Existence and uniqueness of $w^{\text{eq}}(\tau, \kappa)$.** For fixed $(\tau, \kappa)$, the LHS $A\, w^{\varepsilon_s}$ is strictly increasing and continuous in $w > 0$. The RHS $L^D(w, \tau, \kappa) = \int_{\theta \geq \theta^*(w,\tau,\kappa)} L^*(\theta; w, \tau)\, f(\theta)\, d$ is continuous and (weakly) *decreasing* in $w$: increasing $w$ raises unit cost $w(1+\tau)$, which lowers $L^*$ and (weakly) raises the cutoff $\theta^*$, shrinking the integral. Limits are $L^D(w, \cdot) \to \infty$ as $w \downarrow 0$ and $L^D(w, \cdot) \to 0$ as $w \uparrow \infty$. By the intermediate value theorem a unique $w^{\text{eq}}(\tau, \kappa)$ exists; continuity in $(\tau, \kappa)$ follows by standard arguments.

**Step 2: Comparative statics of $\theta^*$ in $(C, \kappa)$.** Consider $\Pi^*(\theta; C, \kappa)$ as a function of $(C, \kappa)$ holding $C = w(1+\tau)$. The threshold $\theta^*(C, \kappa)$ is the unique $\theta$ with $\Pi^*(\theta; C, \kappa) = 0$ (upper contour set $\{\theta : \Pi^* \geq 0\}$ is an interval by single crossing). Envelope derivatives at interior $L^*$ are

$$\Pi^*_\theta = g(L^*) > 0, \qquad \Pi^*_C = -\, L^* \leq 0, \qquad \Pi^*_\kappa = -1 < 0.$$

By the implicit function theorem,

$$\frac{\partial \theta^*}{\partial C} = -\frac{\Pi^*_C}{\Pi^*_\theta} = \frac{L^*}{g(L^*)} \;\geq\; 0, \qquad \frac{\partial \theta^*}{\partial \kappa} = -\frac{\Pi^*_\kappa}{\Pi^*_\theta} = \frac{1}{g(L^*)} \;>\; 0.$$

Thus $\theta^*$ is (weakly) increasing in C and strictly increasing in $\kappa$.

**Step 3: Monotonicity in $\tau$ (endogenous $w$).** Totally differentiate market clearing in logs:

$$\varepsilon_s\, d\ln w^{\text{eq}} = \frac{\partial \ln L^D}{\partial \ln w}\, d\ln w^{\text{eq}} + \frac{\partial \ln L^D}{\partial \ln[w(1+\tau)]}\, d\ln(1+\tau),$$

and define the *effective demand elasticity* $\mathcal{E}_D(\tau, \kappa) \equiv -\,\partial \ln L^D / \partial \ln[w(1+\tau)] \geq 0$ (it includes intensive and entry terms). Rearranging gives the standard competitive pass-through form

$$d \ln w^{\text{eq}} = -\,\Phi(\tau,\kappa)\, d\ln(1+\tau), \qquad \Phi(\tau,\kappa) \equiv \frac{\mathcal{E}_D}{\varepsilon_s + \mathcal{E}_D}.$$

Hence $d\ln C = d\ln[w^{\text{eq}}(1+\tau)] = (1-\Phi)\, d\ln(1+\tau) \ \geq 0$ for $d\tau \geq 0$. By Step 2, $\partial\theta^*/\partial C \geq 0$, so composing yields

$$\frac{\partial\theta^\dagger}{\partial\tau} = \frac{\partial\theta^*}{\partial C} \cdot \frac{\partial C}{\partial\tau} \ \geq \ 0,$$

i.e., the endogenous threshold is (weakly) increasing in $\tau$.

**Step 4: Monotonicity in $\kappa$ (endogenous $w$).** A change in $\kappa$ leaves the per-unit rate $\tau$ unchanged but moves $w^{\text{eq}}$ via the entry channel. Using $C = w^{\text{eq}}(1+\tau)$ and Step 2,

$$d\theta^\dagger = \frac{L^*}{g(L^*)}\, dC + \frac{1}{g(L^*)}\, d\kappa = \frac{L^*}{g(L^*)}\Big[d\kappa + \big((1+\tau)\, dw^{\text{eq}}\big)\Big].$$

Log-differentiating market clearing w.r.t. $\ln\kappa$ gives $d\ln w^{\text{eq}} = -\,\Psi(\tau,\kappa)\, d\ln\kappa$, where $\Psi(\tau,\kappa)$ is the wage elasticity to the fee (purely entry-driven and nonnegative). Hence $(1+\tau)\, dw^{\text{eq}} = -(1+\tau)\, w^{\text{eq}}\, \Psi\, d\kappa/\kappa$, and

$$d\theta^\dagger = \frac{L^*}{g(L^*)}\, d\kappa \left[1 - \Psi(\tau,\kappa) \cdot \frac{(1+\tau)w^{\text{eq}}}{\kappa}\right].$$

Therefore $\partial\theta^\dagger/\partial\kappa \geq 0$ whenever

$$\boxed{\Psi(\tau,\kappa) \ \leq \ \frac{\kappa}{(1+\tau)\, w^{\text{eq}}(\tau,\kappa)}} \tag{$\star$}$$

i.e., when the (endogenous) wage reduction induced by a higher fee is not large enough to offset the direct increase in the fixed cost at the margin. A sufficient condition for ($\star$) is that labour supply be not too elastic and the *marginal-employment* share induced by the fee be small (both shrink $\Psi$). Economically, raising a fixed entry cost should not lower the unit wage bill so much that it makes entry *easier*. Under ($\star$), $\theta^\dagger$ is (weakly) increasing in $\kappa$.

**Summary.** Existence/uniqueness and continuity of $w^{\text{eq}}$ and $\theta^{\dagger}$ follow from monotonicity of the clearing equation. The threshold is (weakly) increasing in $\tau$ unconditionally, and in $\kappa$ under the mild no-overcompensation condition ($\star$), which is satisfied whenever the fee-induced entry response is limited relative to supply and the incumbent wage bill. $\square$

### D.3.2 Effective elasticities and the role of entry

Let entrants be $E(\tau,\kappa) = \{\theta \geq \theta^{\dagger}(\tau,\kappa)\}$ and aggregate labour demand

$$L^D(w,\tau,\kappa) \;=\; \int_{\theta\in E(\tau,\kappa)} L^*(\theta;w,\tau)\, f(\theta)\, d\theta, \qquad s(\theta) \;\equiv\; \frac{L^*(\theta;w,\tau)}{L^D(w,\tau,\kappa)}.$$

Define the *effective cost elasticity of demand* (a positive number)

$$\mathcal{E}_D(\tau,\kappa) \;\equiv\; -\frac{\partial \ln L^D(w,\tau,\kappa)}{\partial \ln[w(1+\tau)]}\bigg|_{w=w^{\text{eq}}(\tau,\kappa)}.$$

Leibniz's rule yields the decomposition

$$\mathcal{E}_D(\tau,\kappa) \;=\; \underbrace{\int_{\theta\in E(\tau,\kappa)} \eta_{\text{int}}(\theta;w,\tau)\, s(\theta)\, d\theta}_{\text{intensive margin}} \;+\; \underbrace{\frac{L^*(\theta^{\dagger};w,\tau)\, f(\theta^{\dagger})}{L^D(w,\tau,\kappa)} \cdot \Big(-\frac{\partial \theta^{\dagger}}{\partial \ln[w(1+\tau)]}\Big)}_{\text{entry margin}},$$

where $\eta_{\text{int}}(\theta;w,\tau) \equiv -\partial \ln L^*(\theta;w,\tau)/\partial \ln[w(1+\tau)] \geq 0$. Since $\partial\theta^{\dagger}/\partial \ln[w(1+\tau)] \geq 0$, the entry term is nonnegative. This is the channel through which selection (movement in $\theta^{\dagger}$) *raises* $\mathcal{E}_D$ and thus increases $\Phi = \mathcal{E}_D/(\varepsilon_s + \mathcal{E}_D)$, while keeping $\Phi < 1$.

For the fixed fee, define the (equilibrium) demand elasticity w.r.t. $\kappa$ as

$$\Gamma_\kappa(\tau,\kappa) \;\equiv\; -\frac{\partial \ln L^D(w,\tau,\kappa)}{\partial \ln \kappa}\bigg|_{w=w^{\text{eq}}(\tau,\kappa)}.$$

Because $L^*(\theta;w,\tau)$ does not depend on $\kappa$, only the lower limit moves:

$$\Gamma_\kappa(\tau,\kappa) \;=\; \frac{L^*(\theta^{\dagger};w,\tau)\, f(\theta^{\dagger})}{L^D(w,\tau,\kappa)} \cdot \frac{\partial\theta^{\dagger}}{\partial \ln \kappa} \;\geq\; 0.$$

Log-differentiating market clearing $Aw^{\varepsilon_s} = L^D$ then gives the wage elasticities used in the text:

$$\Phi(\tau,\kappa) = \frac{\mathcal{E}_D(\tau,\kappa)}{\varepsilon_s + \mathcal{E}_D(\tau,\kappa)}, \qquad \Psi(\tau,\kappa) = \frac{\Gamma_\kappa(\tau,\kappa)}{\varepsilon_s + \mathcal{E}_D(\tau,\kappa)}.$$

#### D.3.3 Composition ('excess entry') and contamination of naïve wage changes

Let $\mathcal{F}$ be inframarginals (hire in both high- and low-tax regimes) and $\mathcal{M}$ the newly-entering marginals only in the low-tax regime (with the tax cut). Let the post-reform entrant share of $\mathcal{M}$ be $\lambda_{\mathcal{M}} \in [0,1]$ (so $\lambda_{\mathcal{F}} = 1 - \lambda_{\mathcal{M}}$). Define population averages:

$$\overline{w}_{\text{pre}} \equiv \mathbb{E}[w_0(\theta) \mid \theta \in \mathcal{F}], \qquad \overline{w}_{\text{post}} \equiv \lambda_{\mathcal{F}}\, \mathbb{E}[w_1(\theta) \mid \mathcal{F}] + \lambda_{\mathcal{M}}\, \mathbb{E}[w_1(\theta) \mid \mathcal{M}].$$

The naïve mean wage change among all post-reform entrants decomposes as

$$\overline{\Delta w} \equiv \overline{w}_{\text{post}} - \overline{w}_{\text{pre}} = \underbrace{\mathbb{E}[w_1(\theta) - w_0(\theta) \mid \mathcal{F}]}_{\text{inframarginal incidence}} + \underbrace{\lambda_{\mathcal{M}}\Big(\mathbb{E}[w_1(\theta) \mid \mathcal{M}] - \mathbb{E}[w_0(\theta) \mid \mathcal{F}]\Big)}_{\text{composition (excess entry) term}}.$$

The second term is *not* an incidence object (pre-reform $w_0$ for $\mathcal{M}$ does not exist). Trimming that removes $\mathcal{M}$ sets $\lambda_{\mathcal{M}} \to 0$, recovering the inframarginal incidence. This formalises the remark in the main text.

# E Sample-trimming method: details and results

## E.1 Implementation of Lee bounds in a difference-in-differences design

To address differential sample selection between treated and control cohorts, we combine the bounding procedure of Lee (2009) with a difference-in-differences (DiD) design. Within the affected cohort, we apply Lee's trimming procedure to recover lower and upper bounds on the mean post-treatment outcome, then compare these bounds to the evolution of the same outcome in control cohorts. This yields Lee-style lower and upper bounds on the treatment effect net of business-cycle variation.

**Estimator** Let $Y$ denote the outcome, `post` a post-reform indicator, and `group` an indicator for belonging to the treated cohort (`group` $= 1$) or to control cohorts (`group` $= 0$). The procedure is as follows: (i) estimate lower and upper Lee bounds for the coefficient on `post` using only the treated group (`group` $= 1$); (ii) estimate the coefficient on `post` in the control cohorts (`group` $= 0$) by ordinary least squares; (iii) form the DiD-type bounds as

$$\widehat{\Delta}^{\ell} = \widehat{\beta}^{\ell}_{\text{treated}} - \widehat{\beta}_{\text{control}}, \quad \widehat{\Delta}^{u} = \widehat{\beta}^{u}_{\text{treated}} - \widehat{\beta}_{\text{control}},$$

where $\widehat{\beta}^{\ell}_{\text{treated}}$ and $\widehat{\beta}^{u}_{\text{treated}}$ denote, respectively, the Lee lower and upper bounds in the treated cohort.

**Practical implementation** We implemented this estimator in `Stata` as an `eclass` program called `leedid`.

```
* Lee Bounds + DiD
cap program drop leedid
program def leedid , eclass
 args y post group
 qui{
```

```
 * entrant group
 leebounds ‘y’ ‘post’ if ‘group’==1
 scalar lb=e(b)[1,1]
 scalar ub=e(b)[1,2]

 * incumbent group
 reg ‘y’ ‘post’ if ‘group’==0
 scalar b0 = _b[‘post’]

 * Difference
 scalar lb=lb-b0
 scalar ub=ub-b0

 tempname b
 matrix ‘b’ = (lb,ub)
 matrix coln ‘b’ = DiD_lb DiD_ub

 ereturn post ‘b’

 }
end
```

The program returns the pair of coefficients (`DiD_lb`, `DiD_ub`) as an estimation result, allowing seamless post-estimation routines. Standard errors are obtained by nonparametric bootstrap, resampling the estimation sample (with clustering at the firm level when appropriate).

**Justification** The bounding procedure ensures that treatment and control are compared at common selection rates, eliminating bias from differential survival or entry into the sample. Embedding the Lee bounds into a DiD estimator allows us to control for common business-cycle shocks by contrasting the treated cohort against adjacent cohorts. Bootstrapping provides valid inference given the nonstandard distribution of bound estimators.

**Lee–DiD results** Table A5 reports Lee and Lee–DiD estimates for a set of key outcomes. Panel A compares Cohorts 2016 and 2014 among entrants only, effectively treating the timing of entry as random across cohorts. We apply the Lee bounding algorithm to inspect what the implied treatment effects would be under such an agnostic procedure. For employee retention, the one- and two-year outcomes cannot be identified, likely because the algorithm fails to compute a stable trimming rate for that variable. For the other outcomes, the program runs but yields implausibly small trimming rates of about one percent, far below the empirically observed excess-entry rate of 20–30%. As a result, the estimated bounds are extremely wide: the FTE wage effects always include zero, and the payroll tax effects, while consistently negative, span a range of roughly €150 to €450 per quarter.

Panel B extends the analysis by accounting for business-cycle evolution in incumbent firms through a cohort-level difference-in-differences specification. This adjustment slightly narrows some intervals, yet the main results remain unchanged. The estimated effects on the FTE wage rate and three-year retention continue to include zero, while payroll tax reductions persist as the only robust finding.

Taken together, these results show that the agnostic Lee procedure performs poorly in this context. The algorithm trims on *realised outcomes*—typically wages or employment—whereas firm participation in the reform depends on a latent profitability condition rather than on any single observed variable. Because wages, hours, and retention adjust jointly and none is monotone in firms' hiring incentives, outcome-based trimming fails to identify the correct selection margin. The wide and unstable bounds therefore provide little information about the true treatment effect. These findings motivate the theory-guided trimming approach developed in the main text, which uses pre-reform hiring propensities and externally informed trimming rates to recover inframarginal incidence.

**Table A5.** Lee bound and Lee–DiD bound results

| | Employee | Per-FTE | | |
|---|---|---|---|---|
| | retention | Gross wage | Payroll tax | Labour cost |
| **Panel A: Lee bounds: Cohort 2016 vs 2014 entrants, without incumbents** | | | | |
| *One year post entry* | | | | |
| trim rate | *cannot* | 1.20% | 1.20% | 1.20% |
| 95 CI Low | *identify* | -49.55 | -193.1 | -583.9 |
| 95 CI High | | 200.1 | -107 | 86.11 |
| *Two years post entry* | | | | |
| trim rate | *cannot* | 1.16% | 1.16% | 1.16% |
| 95 CI Low | *identify* | -128.7 | -465.3 | -253.6 |
| 95 CI High | | 190.9 | -394.7 | -312.7 |
| *Three years post entry* | | | | |
| trim rate | 28.40% | 1.15% | 1.15% | 1.15% |
| 95 CI Low | -0.131 | -53.33 | -237.7 | -556.7 |
| 95 CI High | 0.268 | 195.9 | -151.7 | 35.91 |
| **Panel B: Lee-DiD: Cohort 2016 vs 2014, entrants vs incumbents** | | | | |
| *One year post entry* | | | | |
| 95 CI Low | *cannot* | -154 | -451.1 | -219.1 |
| 95 CI High | *identify* | 69.51 | -379.3 | -316.9 |
| *Two years post entry* | | | | |
| 95 CI Low | *cannot* | -58.15 | -214.5 | -576.5 |
| 95 CI High | *identify* | 91.88 | -128.6 | 52.27 |
| *Three years post entry* | | | | |
| 95 CI Low | -0.296 | -111.1 | -463.5 | -271.5 |
| 95 CI High | 0.110 | 109.6 | -393.2 | -292.2 |

*Notes:* Panel A reports the *pure* Lee bounds obtained by comparing Cohort 2016 and Cohort 2014 entrants only, without adjusting for contemporaneous changes among incumbent firms; these estimates are used to determine the appropriate trimming rate for the entrant group. Panel B combines the entrant-based Lee bounds with the difference in outcomes between entrants and incumbents, thereby netting out business-cycle effects and yielding the Lee–DiD bounds. Only the 95% confidence intervals corresponding to the lower and upper bounds are reported.

## E.2 Primers for the sample-trimming framework

### E.2.1 Identification via propensity ranks and cross-cohort comparability

**Proof of Corollary 1.** Because $\Pi^{\dagger}(\theta, X)$ is strictly increasing in $\theta$ and $H_c(\cdot)$ is continuous and strictly increasing, $S_c(\theta, X) = 1 - H_c[-\Pi^{\dagger}(\theta, X]$ is itself a strictly increasing, continuous function of $\theta$. Quantiles are invariant under monotone transformations: for any strictly increasing function $h$, $Q_u^h = h(Q_u^X)$. Hence, quantiles (and therefore ranks) of $S_c$ coincide with those of $\theta$. Trimming or stratifying by the rank of $S_c$ is therefore equivalent to trimming or stratifying by the unobserved ability $\theta$ within each cohort. □

**Remark 1** (Cross-cohort comparability)**.** If the latent index is cohort-specific,

$$S_c(\theta, X) = 1 - H_c\big[ - \Pi^{\dagger}(\theta, X)\big],$$

then the mapping between $\theta$ and $S_c$ remains strictly increasing within each cohort as long as $\Pi^{\dagger}(\cdot)$ is increasing in $\theta$ and each $H_c$ is continuous and strictly increasing. This ensures *ordinal comparability* of ranks within a cohort: higher $S_c$ corresponds to higher $\theta$.

However, *cardinal comparability* across cohorts generally fails because the same $\theta$ may correspond to different propensity levels when the selection function $H_c$ differs across cohorts because of differential business cycles.

$$S_1(\theta, X) - S_0(\theta, X) = H_0\big( - \Pi^{\dagger}(\theta, X)\big) - H_1\big( - \Pi^{\dagger}(\theta, X)\big),$$

which varies with $\theta$ whenever $H_1 \neq H_0$. Hence, cross-cohort analyses should rely on within-cohort *ranks* (or monotone-shift designs such as trimming) rather than assume equality of cardinal propensity levels.

**Why matching on propensity *levels* fails.** Matching on cardinal levels of $\widehat{S_c}$ assumes that two firms with the same predicted probability of entry are comparable across cohorts. Yet when the intercept or slope of the latent index shifts—due to macroeconomic, informational, or institutional changes—the same value of $\widehat{S}$ reflects different underlying abilities $\theta$. This violates the common-support assumption and can align units that are off-support. In our context, 2014 and 2016 cohorts operated under distinct tax regimes and informational environments, making level-based matching inappropriate. Rank-based trimming avoids this by preserving the ordinal relationship between $\widehat{S}$ and $\theta$ within each cohort.

### E.2.2 Second-layer bounds: Imperfect proxies and misclassification

In the main text, we use the first (empirical) layer of bounds based on uncertainty in the induced-entry share $\lambda$. Here we formalise the second (theoretical) layer that accounts for the imperfection of the observed propensity $\widehat{S}$ as a proxy for the latent rank $S^*$. This misclassification of the marginal–inframarginal split can be serious especially when the propensity score function has limited prediction power, or alternatively when the available covariates $X$ are imperfect proxies for the underlying heterogeneity $\theta$.

Let $\epsilon \in [0, 1)$ denote the misclassification rate between $\mathcal{F}$ (inframarginals) and $\mathcal{M}$ (marginals): at most an $\epsilon$ share of inframarginals are mistakenly trimmed and at most an $\epsilon$ share of marginals are mistakenly retained. This induces contamination of the retained $\mathcal{R}(t)$ and discarded $\mathcal{D}(t)$ sets.

Define contamination bounds:

$$\pi_{M|R}(t, \lambda, \epsilon) \le \min\left\{\frac{\epsilon\lambda}{1-t}, \frac{\lambda}{1-t}\right\}, \qquad \pi_{F|D}(t, \lambda, \epsilon) \le \min\left\{\frac{\epsilon(1-\lambda)}{t}, \frac{1-\lambda}{t}\right\}.$$

Writing $\overline{Y}_1(\mathcal{R})$ and $\overline{Y}_1(\mathcal{D})$ for post-reform means of the retained and discarded groups, a sensitivity bound on the expected post-outcome of inframarginals is:

$$\left|\,\mathbb{E}[Y(1) \mid i \in \mathcal{F}] - \overline{Y}_1(\mathcal{R}(t))\,\right| \;\le\; \zeta(t, \lambda, \epsilon)\left|\,\overline{Y}_1(\mathcal{R}(t)) - \overline{Y}_1(\mathcal{D}(t))\,\right|, \tag{7}$$

where

$$\zeta(t, \lambda, \epsilon) = \max\left\{\frac{\pi_{M|R}(t, \lambda, \epsilon)}{1 - \pi_{M|R}(t, \lambda, \epsilon)}, \frac{\pi_{F|D}(t, \lambda, \epsilon)}{1 - \pi_{F|D}(t, \lambda, \epsilon)}\right\}.$$

Intuitively, if only a small fraction of marginals leaks into $\mathcal{R}(t)$ (and vice versa), the mean outcome for $\mathcal{F}$ cannot deviate much from the retained mean, with the discrepancy bounded by the contamination multiplier $\zeta$ times the observable mean gap between retained and discarded groups.

This bound shows that even with imperfect scores, our trimming delivers conservative and interpretable estimates. In principle, $\epsilon$ could be estimated statistically if measurement errors in $\widehat{S}$ were observable (e.g. through validation data or repeated measures). A full statistical treatment of this second-layer bound is beyond the scope of this paper, but our sample-trimming framework provides a foundation for such refinements.

## E.3 Propensity score estimation

### E.3.1 Estimation procedure

The trimming procedure requires a ranking of firms by their likelihood of hiring absent the reform. We construct this ranking through a propensity score estimated on pre-reform cohorts only. The goal is not to predict entry levels across cohorts, but to order firms within a cohort by their relative propensity to hire. This distinction is important: the intercept of the entry model may shift across years due to macroeconomic changes, so cardinal propensity levels *across years* are not comparable. What matters is simply that, *within a year*, firms with higher scores are systematically more likely to hire than those with lower scores.

Formally, let $H_i$ be an indicator for whether firm $i$ hires in the pre-reform regime: $H_i = 1$ for entrants, and 0 for those who eventually chose to stay solo self-employed, among all the firms that were previously running as non-employers ('not-yet-entered') eligible for the hiring subsidy. We estimate a binary response model among these non-employers in Cohort 2014,

$$\Pr(H_i = 1 \mid X_i) = \Phi(\alpha^H + X_i\beta^H),$$

where $\Phi$ is a probit link, and $X_i$ includes pre-hiring firm characteristics observed in the administrative data. These features include firm age, sector, region, sales and other VAT activity in the quarters before entry, and interactions that capture heterogeneous hiring propensities across sectors and business cycles.

The estimated linear predictor $\hat{s}_i = \hat{\alpha}^H + X_i\hat{\beta}^H$ (or its link transformation, the choice of which does not matter) is then applied to Cohort 2016 entrants, used as the ranking score $S_i$ for trimming. Because the score is estimated only on pre-reform entrants, it reflects the pre-existing relationship between observables and hiring, uncontaminated by the reform. When applied to the post cohort, $\hat{s}_i$ is interpreted purely as a ranking device.

Empirically, these models have only modest predictive power: observable firm characteristics explain some, but not all, of the variation in hiring. This is expected, since profiles of firm owners themselves (which do not show up in administrative data) and unobserved productivity shocks play a large role in hiring decisions. For the bounding strategy, however, only a positive correlation between the score and true latent hiring propensity is needed. A more predictive score would yield tighter bounds, but even a weakly informative score can meaningfully reduce the influence of induced entrants by trimming from the lower end of the distribution.

In robustness checks, we vary the specification of $X_i$, adding richer sectoral interactions and VAT-based measures of activity, to test whether the bounds depend on the details of the score. As later results show, the main conclusions are not sensitive to these alternatives, indicating that the trimming bounds are robust to score specification.

### E.3.2 Estimation results

For the 2014 cohort, the model achieves a log-likelihood of -105624.4, compared to -119084.9 for the null, corresponding to a McFadden pseudo-$R^2$ of 0.113. The Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC) are 211554.8 and 213506.2, based on these 2.56 million observations. These values indicate a substantial improvement in fit relative to the null, confirming that the score separates firms with higher and lower hiring propensities. Full coefficient estimates for using 2014 cohort as the data training set are reported in Table A6, Col 1.

To assess whether the propensity score levels are comparable across cohorts, we also perform a placebo exercise. The model is estimated on Cohort 2013 and then applied to Cohort 2014 for out-of-sample prediction, and the predicted scores are plotted by Figure A9. The distribution of predicted scores for 2014 is shifted towards higher values relative to 2013, with a lower density peak around 0. This rightward shift illustrates that the absolute scale of predicted values is not stable across cohorts, even in the absence of policy change. Consequently, matching based on propensity score levels would be inappropriate, as it risks absorbing such baseline shifts due to business cycle fluctuations that are unrelated to treatment. By contrast, trimming relies only on the within-cohort rank ordering of scores, which remains meaningful and comparable. This makes trimming the more reliable adjustment method in our setting.[29]

Fit statistics for the placebo prediction exercise are highly similar (log-likelihood $-102{,}941$ vs. $-115{,}734$ for the null, pseudo-$R^2 = 0.11$, AIC 206,183, BIC 208,091, based on 2.48 million observations). This stability suggests that the mapping from observables to hiring propensity is not sensitive to year-specific shocks before the reform. Full coefficient estimates for using the 2013 cohort as the data training set are reported in Table A6, Col 2.

[29]For the placebo cohorts, only Cohort 2013 is used for training the prediction model; the model is then applied for out-of-sample prediction for Cohort 2014 as well. The higher-centred distribution in 2014 indicates that absolute levels of predicted values may not be reliably comparable across cohorts, further motivating trimming instead of matching.

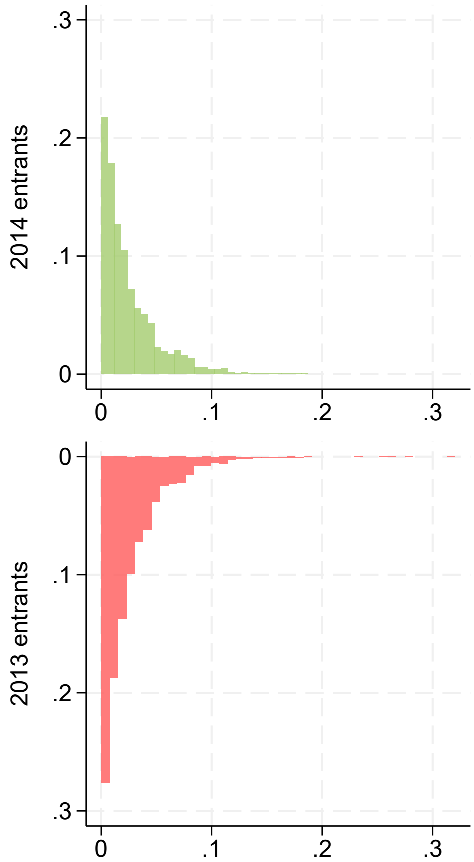



**Figure A9.** Distribution of predicted hiring propensities (Placebo): Cohorts 2014 vs 2013

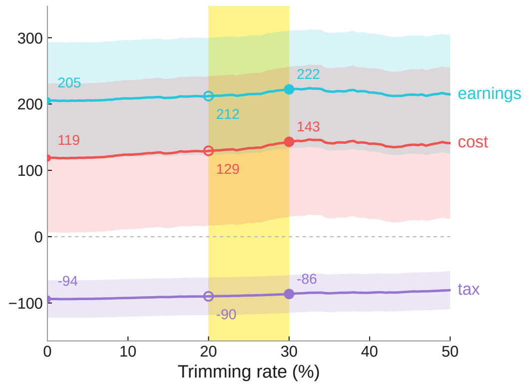
*Notes:* Predicted propensity scores (horizontal axis) from the probit model in Table A6. Vertical axis is the fraction of firms. Only Cohort 2013 is used for training the prediction model; the model is then applied for out-of-sample prediction for Cohort 2014 as well. The 2014 distribution is shifted towards higher values, which means that the levels of predicted values might not be reliably comparable across cohorts.

**Table A6.** Probit estimates of hiring propensity, pre-reform cohorts

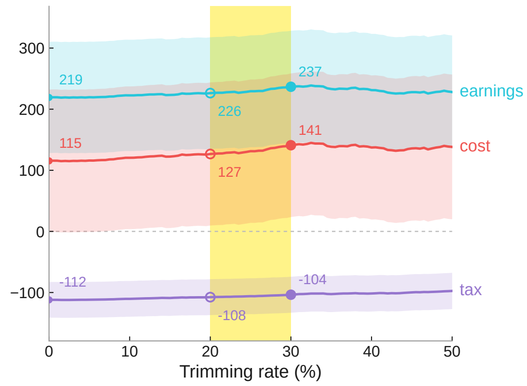
| | Dependent variable: Hire (0/1) | |
|---|---|---|
| | 2014 cohort | 2013 cohort |
| VAT reported ($t-1$) | 0.120*** | 0.130*** |
| | (0.010) | (0.011) |
| Turnover ($t-1$) | 0.242*** | 0.157*** |
| | (0.060) | (0.060) |
| Inputs ($t-1$) | 0.941*** | 0.869*** |
| | (0.077) | (0.077) |
| Investment ($t-1$) | 9.270*** | 8.721*** |
| | (0.364) | (0.366) |
| VAT reported ($t-2$) | -0.100*** | -0.093*** |
| | (0.012) | (0.013) |
| Turnover ($t-2$) | 0.072 | 0.071 |
| | (0.066) | (0.065) |
| Inputs ($t-2$) | 0.067 | 0.053 |
| | (0.091) | (0.090) |
| Investment ($t-2$) | 3.635*** | 2.993*** |
| | (0.424) | (0.420) |
| VAT reported ($t-3$) | -0.065*** | -0.079*** |
| | (0.013) | (0.013) |
| Turnover ($t-3$) | 0.008 | 0.019 |
| | (0.069) | (0.067) |
| Inputs ($t-3$) | -0.056 | -0.057 |
| | (0.095) | (0.093) |
| Investment ($t-3$) | 2.338*** | 2.391*** |
| | (0.450) | (0.433) |
| VAT reported ($t-4$) | -0.090*** | -0.110*** |
| | (0.011) | (0.011) |
| Turnover ($t-4$) | -0.012 | -0.008 |
| | (0.068) | (0.066) |
| Inputs ($t-4$) | -0.426*** | -0.228** |
| | (0.095) | (0.091) |
| Investment ($t-4$) | 2.068*** | 2.335*** |
| | (0.452) | (0.430) |
| Firm age | -0.013*** | -0.012*** |
| | (0.000) | (0.000) |
| Firm age squared | 0.00006*** | 0.00006*** |
| | (0.000) | (0.000) |
| Fixed effects | Sector, legal form, quarter, arrondissement | |
| Observations | 2,557,705 | 2,477,146 |

Notes: Probit coefficients with robust standard errors in parentheses. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

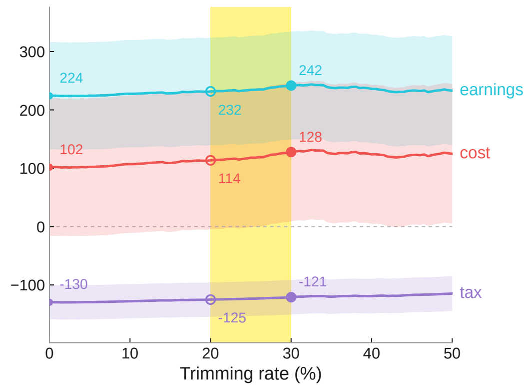
## E.4 More outcomes of the bounding strategy




**(a)** 1 year




**(b)** 2 years




**(c)** 3 years

**Figure A10.** Bounded effects on per-employee total measure outcomes, within one, two, or three years

*Notes:* Estimates with 95% confidence intervals are plotted against progressive trimming rates ranging from $t \in [0, 50\%]$ with step size of 0.5%, by deleting entrants from Cohort 2016 with the lowest estimated hiring propensity scores below the $t$-th percentile. The true excess rate $\lambda$ is believed to be within the prior-belief band of $[20\%, 30\%]$ as indicated by the highlighted vertical region.