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Taxation on wages: evaluating reductions in employers' and employees' social security contributions

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To my parents

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Contents

1

Introduction

How agents, which can be business owners, jobseekers or individuals in salaried employment, respond to incentives set by the tax and benefit system lies at the heart of Labour Economics. One key tool used by governments to finance employees' entitlements to social security provisions—such as pensions, unemployment benefits, and disability benefits—is social security contributions (SSC). These contributions have legal incidence on both employees and employers. Specifically, employers pay SSC as a percentage of the gross wages they offer their employees, while employees, in turn, pay a percentage of the gross wages they receive as SSC.

Employers' and employees' social security contributions are sizable in most advanced economies. In Belgium, they typically amount to more than 20% of the wage paid by firms and around 13% of the wage received by employees. Consequently, it is not surprising that, over the past decades, discussions have arisen about the possibility of reducing these contributions for certain groups of firms or employees, making hiring less costly for firms or working time more remunerative for employees. In this thesis, we evaluate two policies implemented in Belgium—one in 2000 and another in 2016—that respectively reduced the social security contributions with legal incidence on employees and employers. Understanding whether and how these policies modify the agents' behaviour is crucial for ensuring the efficient use of public expenditures.

Chapter 2 and Chapter 3 of this thesis explore the 2016 reform, which permanently exempted new employers from paying social security contributions on the gross salary of one employee. The policymakers' motivation was that these payroll taxes posed a significant barrier to hiring, and a policy encouraging firms to hire their first employee would, in turn, foster a virtuous cycle of business growth. To create such a virtuous cycle of business growth, it is not sufficient for firms that were not profitable enough to cover these payroll taxes to simply hire their first employee thanks to the exemption. The reform must also reduce certain frictions—such as hiring or credit frictions—that previously prevented or slowed firms' growth.

Using a panel of firms with fewer than 15 employees from the National Bank of Belgium, Chapter 2 shows that the reform did not result in a noticeable increase in the number of firms employing two or more employees. Based on this observation, we construct a simple frictionless general equilibrium model of occupational choices and find that it reproduces the rise in firms with one employee remarkably well. Specifically, we demonstrate that, in order to account for the increase in the stock of firms with one employee observed in the data, it is sufficient to model the response of firms that were previously not productive enough to cover the reduced wage costs in a frictionless economy. We then leverage the model to quantify the general equilibrium effects of the policy on wages and output, and to assess its budgetary cost and distributional impact in the long run—i.e., when all firms employ a subsidised first employee. Although approximately 9% of employees will eventually be subsidised, our model predicts a modest wage increase of 0.5%. Intuitively, this wage increase is limited because the subsidy results in windfall profits for firms that would have employed more than one worker even without the subsidy, without affecting their optimal labour demand. Given the small wage increase, the impact on aggregate firms' output is negligible, with output decreasing by less than 0.01% in equilibrium.

Chapter 3 builds on these findings. It uses a directed search model with matching frictions to explain why, in the presence of hiring frictions, firms that began hiring due to the exemption did not grow beyond one employee, and why firms that were already hiring before the reform did not modify their hiring decisions. The model shows that approximately 20% of employers are encouraged by the exemption to hire their first employee. However, for these firms to achieve further growth, they need to experience a positive increase in their idiosyncratic productivity—an occurrence that happens for only a minority of these firms. Moreover, the exemption does not significantly alter the hiring behaviour of about 80% of firms which would have hired even in the absence of the reform. For these firms, the cost of attracting one more jobseeker is relatively high, and the social security contributions exemption offsets only a small part of this cost. We then assess whether these employers' responses would have been stronger if the benefit of attracting an additional jobseeker had been higher. To explore this, we assume that firms have a higher probability than estimated in the data of filling each of their vacancies for a given number of jobseekers. This implies that attracting, by posting higher wages, one additional jobseeker has a greater impact on the firm's probability of making an additional hire, thereby potentially driving faster employment growth due to the exemption. We find that even if the exemption had been introduced in this setting more favourable to employment growth, firms' responses are likely to remain small.

Finally, Chapter 4 investigates the impact of introducing the Social Work Bonus (SWB), a payroll tax reduction for low-wage salaried workers in Belgium. The SWB differs from other in-work benefits aimed at enhancing work incentives because it specifically targets individuals with a low wage rate rather than those with low labour earnings. This approach prevents part-time traps, where individuals may reduce their working time to qualify for in-work benefits or may not increase their working time to avoid losing the benefits. Using a panel dataset from the Crossroads Bank for Social Security of individuals in salaried employment, we implement a difference-in-differences approach to estimate the causal impact of the reform. Namely, we compare the evolution in working time between two groups: those with a low wage rate just before the reform—specifically, in 1999Q4—and those with a higher wage rate in 1999Q4. The former group acts as our treatment group because most of these individuals, conditional on remaining in salaried employment post-reform, continue to have a low wage rate and, as a result, receive the SWB. The latter group serves as our control group, as most of these employees still have a high wage rate in the post-reform period and, therefore, are not eligible for the SWB. We find a relatively large statistically significant increase in the quarterly number of days worked for young salaried employees with part-time contracts. No such effect is found for older employees or those on full-time contracts. The effect also appears to be present only for women and not for men, even though the number of men with part-time contracts is relatively low, and therefore, the results are not precisely estimated. These findings indicate that, for some subgroups of the population, in-work benefits targeted at the wage rate can lead to a significant increase in working time.

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Too much of a good thing? The macro implications of massive firm entry

*This chapter is co-authored with Sam Desiere and Gert Bijnens.**

Whether stimulating firm entry has the potential to increase aggregate output by addressing market failures or, conversely, distorts the optimal firm size distribution and reduces aggregate output remains an open question. This chapter offers a comprehensive evaluation of a unique policy in Belgium that reduces the labour cost of the first employee by up to 20%. Empirically, we find that the policy resulted in a surge in the number of firms employing exactly one employee, without a noticeable effect on the number of firms employing two or more employees. A simple frictionless general equilibrium model of occupational choices predicts the empirical facts remarkably well. Leveraging our model, we show that the general equilibrium effects on wages and aggregate output are likely to be small. However, the policy is expensive. Our findings support the traditional view that size-dependent subsidies distort the optimal allocation of resources.

*The views expressed in this chapter are those of the authors and do not necessarily reflect the views of the National Bank of Belgium.

2.1 Introduction

Policymakers across the political spectrum love small businesses. Barack Obama called small businesses *the backbone of our economy and the cornerstones of our communities* and supported them by lowering their taxes and providing subsidised loans. The Trump Administration also emphasised the critical role of small businesses and introduced the Paycheck Protection Program to protect small businesses during the COVID-19 pandemic. Support for small businesses is not restricted to the US. Policies favouring small over medium-sized businesses are prevalent across both developed (e.g., OECD (2021)) and developing countries (e.g., Martin et al. (2017); Bachas et al. (2023)).

Economists are generally sceptical of size-dependent policies (Guner et al., 2008; Restuccia and Rogerson, 2008; Buera et al., 2013; Acs et al., 2016). A common assumption of economic models with heterogeneous firms is that larger firms are more productive than smaller ones. Given this assumption, policies favouring small firms over larger ones will reduce aggregate output, as labour will reallocate from large, high-productivity firms to small, low-productivity firms. Economists, therefore, argue that policies favouring small businesses can only be justified by market failures that impede the entry of new firms or hinder small firms from growing, such as liquidity and credit constraints (Evans and Jovanovic, 1989; Hurst and Lusardi, 2004; Cagetti and De Nardi, 2006; Moll, 2014; Siemer, 2019), the uninsurable risk of starting a business (Hombert et al., 2020; Ando, 2021), market power of incumbent firms (De Loecker et al., 2020), or missing markets for innovation (Acemoglu et al., 2018). Then, it becomes fundamental to understand whether market failures always play a central role in shaping the effects of size-dependent policies, or whether there are setups where a simple neoclassical model without market failures is sufficient to predict their first-order effects.

We focus on a unique and expensive size-dependent policy in Belgium.[1]

[1] The Belgian policy is similar to the the long-standing *employment allowance* in the UK, which allows eligible employers to reduce their National Insurance Contributions (currently 13.8% of gross wages) by £5,000 a year (a recent government decision raised the tax rate to 15% and the allowance to £10,000). This allowance effectively exempts small firms from payroll taxes. To our knowledge, the impact of this allowance has not yet been evaluated. Like Belgium, Finland experimented with offering a subsidy to entrepreneurs hiring their first employee. However, unlike the Belgian and UK policy, the Finnish subsidy was temporary rather than permanent, and the take-up was extremely low (2-12%) (Nivala, 2024).

Firms that hired their first employee after 1 January 2016 are permanently exempt from paying payroll taxes for that employee, while paying the standard rate for subsequent employees. As a result, this policy reduces the labour cost for one employee by approximately 20% compared to that of other employees. While this policy initially favours new firms over those established before 2016, over time, all firms in Belgium will be created after its implementation and will have one employee exempt from payroll taxes. As a result, the policy will increasingly benefit smaller firms over larger ones, as the subsidy is, in relative terms, significantly more generous for the former, making it a textbook example of a size-dependent policy.

In this paper, we assess whether a frictionless model, in which all firms are exempt from paying payroll taxes for one employee post-reform, can predict the impact of this size-dependent policy. Given that the model is frictionless, it is expected to predict the impact of the policy only if the exemption does not alleviate market failures identified in the literature as crucial to firm entry and growth. We proceed in four steps. First, we document the policy's impact on the number of firms, a key statistic our model aims to predict. Second, we develop a frictionless model of occupational choice and calibrate it using only the pre-reform firm size distribution. Third, despite its simplicity, the model predicts the increase in the number of firms remarkably well, which we interpret as evidence in favour of the validity of our model. Finally, we leverage the model to explore the long-term impact of the policy on aggregate output, its budgetary cost and its distributional impact.

The first part of the paper presents empirical evidence on the evolution of the number of firms in the years following the reform. Using a difference-in-differences approach, we find that the number of firms with one employee increased by 7.2% three years after the reform, with little evidence of an increase in the number of firms with more than one employee. This finding, consistent with Cockx and Desiere (2024), illustrates that the policy led to a sudden and substantial rise in the number of firms with one employee. Additionally, the lack of an increase in the number of firms with two employees aligns with Deng et al. (2024), who show that new employers entering as a response to the subsidy are generally smaller and less productive than those who would have entered in the absence of the subsidy. Even three years after entry, most of the new employers who would not have hired without the subsidy appear to employ a single employee. These observations suggest that the policy did not spur new entrants to become highly successful firms.

2. Too much of a good thing? The macro implications of massive firm entry

The second part of the paper develops a simple general equilibrium model of occupational choice in the spirit of Lucas (1978), incorporating size-dependent payroll taxes following Braguinsky et al. (2011) and Garicano et al. (2016). Heterogeneous individuals have an individual-specific productivity level and choose between being a manager (and earning profits) or an employee (and earning the market wage). In equilibrium, the most productive individuals are managers, and the least productive ones are employees. The subsidy for the first employee makes it more attractive to become a manager, which induces some employees to become managers. However, these new managers are insufficiently productive to employ more than one employee. Consequently, the entry of new managers leads to an increase in the number of firms with at most one employee, without any effect on the number of firms with more than one employee, which is in line with the empirical findings. The subsequent decrease in labour supply (as some individuals switch from being an employee to being a manager) and increase in labour demand (by the new managers) increase equilibrium wages. The wage increase, in turn, reduces labour demand and leads to a reallocation of labour from firms with more than one employee to firms with at most one employee, resulting in a fall in aggregate output.

The third part of the paper shows that the calibrated model predicts a 10.2% increase in the number of firms with one employee, which is close to the 7.2% increase observed in the data. A straightforward extension of the baseline model, incorporating not only the exemption for the first hire but also a 3.3 percentage points reduction in the payroll tax rate for all other employees—a policy gradually implemented between 2016 and 2019—predicts an 8.9% increase in the number of firms with one employee, aligning the simulated effect more closely with the DiD estimate. The close match between the empirical DID estimate and the simulated effect is noteworthy, especially given that the model contains only three fundamental parameters and is calibrated on the firm size distribution and average firm size in the pre-reform period. This finding suggests that simple frictionless models can provide useful insights into the first-order effects of size-dependent policies. This is a particularly relevant for government institutions responsible for making ex-ante forecasts of the likely long-term impact of size-dependent policies under consideration by policymakers.

In the final part of the paper, we leverage the model to quantify the general equilibrium effects of the policy on wages and output, and to reflect on its budgetary cost and distributional impact in the long run, i.e. when all firms employ a subsidised first employee. Although approximately 9% of the employees will eventually be subsidised, our model predicts a modest wage increase of 0.5%. Intuitively, this wage increase is limited because the subsidy provides a windfall profit for firms that would have employed more than one employee absent the subsidy, without affecting their optimal labour demand. Given the small wage increase, the impact on aggregate output is negligible, decreasing by less than 0.01% in equilibrium.

However, the policy is expensive for the government, amounting to 0.36% of output. The reduction in government revenues does not benefit all individuals equally. Individuals who are employees in both periods experience modest gains due to the 0.5% wage rise. The profits of individuals who become managers following the reforms are 0.5% to 6.5% higher than their pre-reform wages. The main winners are managers running firms with at most one employee in the pre-reform period, who experience a 6.5% increase in profits. The increase in profits gradually decreases with the managers' productivity and, thus, with firm size. Managers running firms with at most 25 employees experience profit gains, while firms with more than 25 employees experience a slight reduction in profits of less than 1% due to higher wages. In this sense, the policy achieves its objective of supporting small businesses, albeit at the cost of lower government revenues and, to a much smaller extent, lower profits for larger firms. To further illustrate the policy's substantial budgetary cost, we show that the payroll tax rate must increase by 1.2 percentage points, or 8% in relative terms, if the government aims at maintaining revenues constant.

Like much of the literature, our model abstracts from the solo self-employed and the unemployed.[2] Consequently, we assume that the sum of the number of employees and managers—called the 'labour force' in our model—is fixed. This setup implies that the number of managers increases after the reform at the expense of the number of employees. Admittedly, this is a strong assumption, as it rules out the possibility that the self-employed become employers after the reform. For this reason, we also consider the opposite assumption of a perfectly elastic labour force, where the new managers and their employees come from outside the labour force, which rules out a wage increases and job losses in larger firms. This scenario yields similar predictions regarding the evolution of the number of firms and budgetary costs but results in a 0.75% increase in the labour force and a 0.25% increase in output produced by employers. An

[2]Modelling the hiring decisions of the self-employed is challenging, as many self-employed individuals do not intend to hire (e.g., Hurst and Pugsley (2011, 2017)).

intermediate case, where new managers come from outside the labour force but still compete with existing firms for employees, leads to a 0.24% increase in the labour force and a 0.08% increase in output.

The absence of market failures in our model implies that the policy cannot be rationalised on efficiency grounds because the allocation of resources is optimal without policy interventions. With a 'fixed' labour force, the policy always leads to the entry of low-productivity firms, results in job losses in larger, more productive firms, and, ultimately, reduces output. However, the policy could be justified for other reasons that are not explicitly modelled and may fall outside the realm of economics, such as a preference for small businesses. Our simulations of the cost of the policy in terms of reduced output or lower tax revenues can be used to support or dismiss the policy, depending on the value one attaches to small businesses.[3]

This paper contributes to the literature on size-dependent policies. The first strand of this literature, starting with the seminal work of Restuccia and Rogerson (2008) and Hsieh and Klenow (2009), finds that size-dependent policies can have significant negative effects on productivity and welfare. Our work is closely related to Guner et al. (2008) and Braguinsky et al. (2011). Guner et al. (2008) introduce size-dependent taxes or subsidies in a Lucas model calibrated to the US economy and simulate the impact of various potential policies. They consider both subsidizing capital and labour in small firms, and highlight that subsidising labour has a larger impact on the number of firms but leads to smaller welfare losses than subsidising capital. This prediction aligns with our findings, as we find a negligible impact on output (-0.01%), but a much larger impact on the total number of firms ($+2.8\%$), driven by the 10.2% increase in firms with one employee. Our findings are also consistent with Hopenhayn (2014) who argues that policies that distort the optimal firm size distribution but preserve the rank of the firms—such as the policy we study—have a limited impact on aggregate total factor productivity. Our modelling approach is similar to that of Braguinsky et al. (2011), who show that employment protection, modelled as a tax on labour in a Lucas model, reduces average firm size and leads to output losses.

[3] Dustmann et al. (2022) make a similar argument. They show that the introduction of a minimum wage in Germany led to a reallocation of labour from small (less productive) to large (more productive) firms, and argue that this reallocation does not necessarily increase welfare for all workers if small firms offer valuable job amenities. For instance, they demonstrate empirically that commuting time increased following the adoption of the minimum wage.

The second strand of the literature evaluates country-specific size-dependent policies. Gourio and Roys (2014) and Garicano et al. (2016) evaluate the cost of labour market regulations in France that apply only to firms with at least 50 employees. Cahuc et al. (2023) evaluate a policy that prevents large firms from using temporary contracts in Portugal. Akcigit et al. (2023) study the implications of employment targets during the privatisation of firms in East Germany, and Rotemberg (2019) investigates the reallocation effects of relaxing eligibility criteria for small business subsidies in India. The key difference between our paper and the existing literature is that the Belgian policy explicitly aimed to stimulate firm entry and to increase the number of firms, unlike the existing studies that focus on policies with different objectives but unintentionally distort the firm size distribution.

The paper is organised as follows. The next section discusses the policy and the institutional setting. Section 4.4 presents the data. Section 2.4 evaluates the impact of the policy on the number of firms with one employee. Section 2.5 presents the model, which is calibrated in Section 2.6. Section 2.7 simulates and validates the model, and leverages it to reflect on the policy's macro implications and distributional impact. Section 4.8 concludes.

2.2 The policy and the institutional setting

In Belgium, employers pay payroll taxes, called Social Security Contributions (SSC), on top of the wages they pay to the employees. These contributions are used to fund social security, including health care and retirement benefits. SSC are an important determinant of labour costs, with the current nominal payroll tax rate amounting to 25% of gross wages. On October 10, 2015, the Belgian government unexpectedly announced that new employers would receive a permanent exemption from payroll taxes for their first employee. This measure aimed to support employment and ensure the viability of businesses (Court of Audit, 2021). Some ministers at the time argued that payroll taxes were a significant barrier to starting hiring. Therefore, the policy was intended to encourage firms to hire their first employee, thereby fostering a virtuous cycle of business growth (Council of Ministers, 2015).[4]

The new policy went into effect for private sector firms that hired their

[4]Hiring rates among firms without employees are very low in Belgium (Cockx and Desiere, 2024), and elsewhere (see Fairlie and Miranda (2017) for the US).

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

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

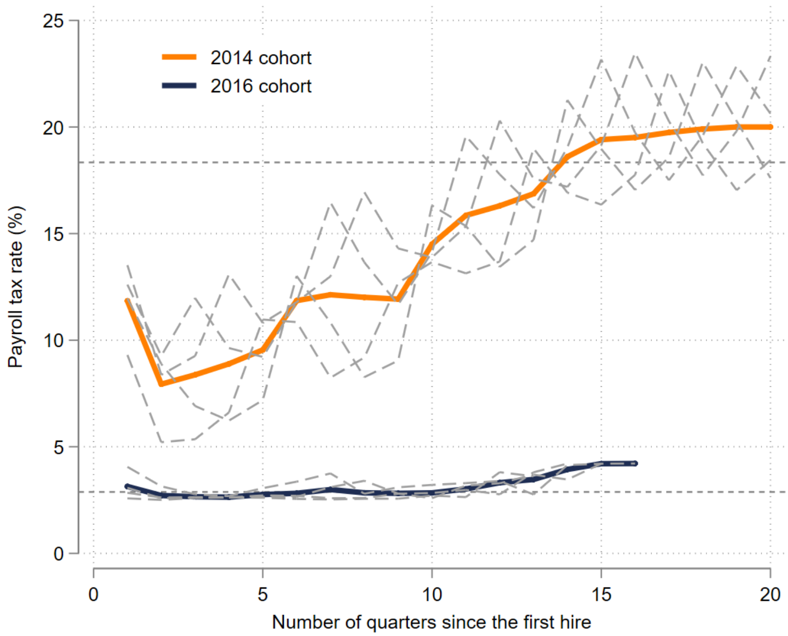
The 2016 reform replaced previously existing less generous temporary hiring subsidies for the first employee that had been in effect with some modifications since 2004. When new employers hired their first employee in 2015, they could claim a €1,550 quarterly payroll tax reduction for the first five quarters, €1,050 for the next four quarters, and €450 for the last four quarters.

To illustrate the generosity of the temporary pre-reform and permanent post-reform subsidy, we compute the median payroll tax rate for the first employee paid by quarterly cohorts of new employers who hired their first employee in 2014 (pre-refom cohorts) and 2016 (post-reform cohorts) in function of the number of elapsed quarters since their first hire (Figure 2.1).[5] The black and orange lines show the average annual payroll tax rate of the 2014 and 2016 cohorts.

The 2016 cohort benefits from the permanent payroll tax exemption and faces a constant payroll tax rate of 2.9% for the first employee. This rate is not exactly equal to zero because employers are only exempt from the base contribution, but they still pay specific contributions, such as contributions for short-time work or sector-specific training. The 2014 cohort benefits from temporary tax reductions for the first employee that are gradually phased out

[5]We do not show the payroll tax rate of the 2015 cohort because this cohort was exempt from SSC for their first employee from 2016Q1 for at most thirteen quarters. When announcing the 2016 reform in October 2015, the government granted this temporary SSC exemption to the 2015 cohort of new employers to reduce the differential treatment of the 2015 and 2016 cohorts. Importantly, at the time of hiring, the 2015 cohort could not anticipate future government decisions and expected to be treated as the 2014 cohort.

Figure 2.1. The payroll tax rate for the first employee over the firm's lifetime: 2014 vs. 2016 cohort of new employers



Notes: The figure shows the payroll tax rate for the first employee paid by new employers who hired their first employee in the first four quarters of 2014 and 2016 (grey dashed lines) and had not employed workers in the previous four quarters, in function of the number of elapsed quarters since their first hire. This population is eligible for temporary payroll tax reductions in the pre-reform period and the permanent payroll tax exemption in the post-reform period. The first quarter corresponds to the quarter in which the first employee is hired. To compute the payroll tax rate for the subsidised employee, the population of new employers is restricted to those that employ exactly one employee in a given quarter. The full black and red lines show the payroll tax rate averaged over the four quarterly cohorts of new employers who hired their first employee in 2014 and 2016. The horizontal dashed lines indicate the expected payroll tax rate, defined in the text, in the pre-reform period (18.3%) and post-reform period (2.9%).

over thirteen quarters. As a result, the payroll tax rate for the 2014 cohort gradually increases with the firm's age and reaches a stable level fourteen quarters after having hired their first employee.

Following Cockx and Desiere (2024), we compute the *expected* payroll tax rate for the first employee at the time of hiring in the pre-reform and post-reform period. The expected payroll tax rate is defined as the weighted average of the payroll tax rate for each quarter after hiring (shown in Figure 2.1), weighted by the firm's expected survival rate (i.e. the probability of still being an employer)[6] and a quarterly discount rate set at 1.275% (e.g., Kaas and Kircher (2015)). These computations reveal that the expected payroll tax rate for the first employee of new employers is 18.3% in the pre-reform period and decreases to 2.9% in the post-reform period.

The generosity of the tax exemption depends on the expected payroll rate for the first employee and the rate paid by firms that do not benefit from this exemption. Table 2.1 shows the payroll tax rate paid by firms with 7-15 employees[7], along with the expected payroll tax rate for the first employee before and after the reform. This table contains four payroll tax rates, depending on the firm size and period. In our model, we refer to the rates for the first employee in the pre-reform (*b*efore) and post-reform (*a*fter) period as $\tau_{1,b}$ and $\tau_{1,a}$, respectively, while $\tau_{2,b}$ and $\tau_{2,a}$ denote the rate paid for subsequent employees in the pre-reform and post-reform period.

Table 2.1. The (*expected*) payroll tax rate

	Pre-reform (b)	Post-reform (a)
1 employee (τ_1)	18.3%	2.9%
7-15 employees (τ_2)	26.9%	23.6%

Notes: The payroll tax rate for firms with 7-15 employees is computed as the SSC divided by the wage bill. We first computed the rate for each firm in each quarter and then computed the median rate across all firms. We report the average annual rate taking the average over four quarters in the pre-reform (2014Q4-2015Q3) and post-reform period (2018Q4-2019Q3). The payroll tax rate for the first employee is the *expected* payroll tax rate at the time of hiring over the firm's lifetime.

The table reveals two essential facts. First, in both periods, the (expected) payroll tax rate of firms with a single employee is lower than the rate paid by

[6]We compute the expected survival rate for the 2014 cohort of new employers for the first five years after hiring and assume that, after five years, 3% of the employers no longer employ workers in the following year, which corresponds to the Belgian average (Bijnens and Konings, 2020).

[7]We compute the median payroll tax rate for firms with 7-15 employees because (1) our data is restricted to firms with at most fifteen employees, and (2) only a tiny fraction of firms with at least seven employees receive payroll tax reductions.

firms with 7-15 employees. This implies that payroll taxes are size-dependent in both periods, with smaller firms paying lower taxes. Second, the payroll tax rate paid by firms with 7-15 employees decreased by 3.3 percentage points (pp), from 26.9% in the pre-reform to 23.6% in the post-reform period. This decrease is due to the so-called tax shift, which gradually decreased the payroll tax rate between 2016 and 2019. Importantly, the difference between the payroll tax rates for the first and subsequent employees is larger in the post-reform period. The gap is 20.7 pp in the post-reform period, up from 8.6 pp in the pre-reform period. Hence, despite the tax shift, the payroll exemption for the first employee remains a very generous subsidy.

Besides the permanent subsidy for the first employee, firms can also claim temporary subsidies for the second to the sixth employee. The temporary subsidies for the second to the fifth employee were reinforced in 2016, along with the introduction of a subsidy for the sixth employee. We make abstractions from these subsidies for three reasons. First, the permanent payroll tax exemption for the first employee is far more generous than the temporary payroll tax reductions for the subsequent five employees, which last at most thirteen quarters. Second, the temporary subsidies already existed with some modifications since 2004 and became only slightly more generous in 2016, which makes it unlikely that these subsidies substantially altered the firm size distribution after 2016.[8] Finally, the introduction of the temporary subsidy for the sixth employee in 2016 is unlikely to affect the decision to hire the first employee.

2.3 Data

We rely on a firm-level panel dataset of the population of employers employing at most fifteen employees provided by the National Bank of Belgium (NBB) to evaluate the short-term impact of the policy (Section 2.4) and to calibrate the firm model (Section 2.6).

The quarterly panel dataset covers the period from 2009Q1 to 2019Q4, encompassing employers with a maximum of fifteen employees in at least one quarter during this period. This comprises both new entrants who hired their first employee after 2009Q1, as well as employers that hired their first

[8]For instance, firms hiring their second employee could claim a payroll tax reduction over thirteen quarters of at most €8,850 before and €13,750 after the 2016 reform. The subsidy for subsequent hires are less generous than for the second hire.

employee before 2009Q1. According to this dataset, Belgium counted 152,999 employers with at most fifteen employees in 2015Q3, of which 53,001 employed one employee.

The NBB compiles data from various administrative sources. In the context of this paper, the most relevant data stems from the National Social Security Office (NSSO), which administers Social Security Contributions (SSC). The NSSO registers firm-level employment. For each employer, we observe the total number of full-time and part-time employees on the last day of the quarter. The exact number of days worked over a quarter is not observed.

The firm-level dataset includes information on the quarterly wage bill, SSC, and total SSC reductions. Total SSC reductions include the temporary or permanent SSC reductions for the first employee but also include other SSC reductions such as the reductions for low-wage workers. These variables enable us to compute the payroll tax rate by quarter and employer size. The payroll tax rate paid by a firm is defined as the SSC divided by the wage bill.

To calibrate the model, we need the average employer size and the proportion of employers with more than fifteen employees in 2015Q3. Since the NBB dataset is restricted to employers with fifteen employees, we obtained this information directly from the NSSO. In 2015Q3, the average employer has 10.84 employees and 11.8% of the employers have more than fifteen employees.

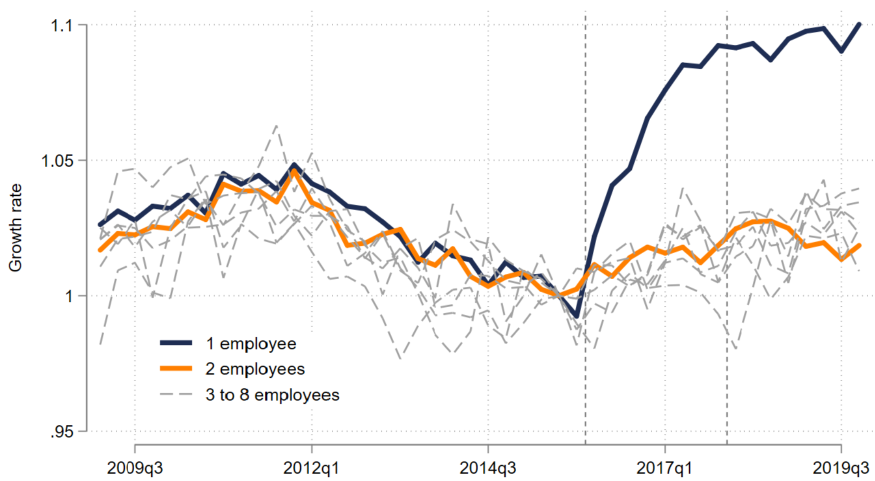
2.4 Evolution of employers

Using daily data on hiring decisions, Cockx and Desiere (2024) show that the labour cost reduction induced by the payroll tax exemption increases the monthly flow of new employers by 31% immediately after the implementation of the reform on January 1, 2016. Our quarterly data confirm the sharp jump in the quarterly flow of new employers.

In this section, we complement their study by examining the impact of the reform on the stock of firms, which is a key statistic our model aims to predict. More precisely, we examine whether the reform increased the number of firms employing exactly one or two employees relative to a control group of bigger firms. We first present graphical evidence and then quantify this evidence using a Difference-in-Differences (DiD) framework. In these analyses, firm size is defined as the headcount of full-time and part-time employees at the end of the quarter.

Graphical evidence. Figure 2.2 shows the number of firms employing one to eight employees for the quarters 2009Q1-2019Q4 relative to 2015Q3, the quarter preceding the announcement of the policy. The evolution of the number of firms with one and two employees is highlighted in black and orange. The grey dashed lines show the evolution of the number of firms with three to eight employees.

Figure 2.2. Growth in the number of firms



Notes: The figure shows the evolution of the number of firms by firm size relative to 2015Q3. The first dashed line indicates the start of the policy in 2016Q1. The second dashed line indicates 2018Q1, the point in time at which the number of firms with one employee stabilised. In 2015Q3, Belgium counted 53,001 firms with one employee, 28,672 firms with two employees, 18,163 firms with three employees, 12,381 employees with four employees, 9,037 firms with five employees, 6,823 firms with six employees, 5,251 firms with seven employees and 4,271 firms with eight employees.

The figure provides compelling evidence that (1) the number of firms by firm size evolved similarly in the pre-reform period; (2) the number of firms with one employee increased considerably after the implementation of the policy in 2016, continued to increase until the end of 2017, and stabilised thereafter; and (3) the number of firms with two employees evolved similarly as the number of firms with 3-8 employees in the post-reform period, suggesting that the reform did not affect the stock of firms with two employees.

DiD methodology. We use DiD regressions to quantify the graphical evidence. The DiD regressions contrast the evolution of the stock of firms with one or two employees (treatment group) to the evolution of the stock of firms with several employees (control group) in the pre-reform vs. the post-reform period.

We estimate the following DiD regression using aggregate quarterly data:

$$\frac{y_{s,t}}{y_{s,2015Q3}} = S_s + \beta_t + \sum_{i=0}^{3} \gamma_i I_s * T_i + \epsilon_{s,t}$$

where the outcome, $\frac{y_{s,t}}{y_{s,2015Q3}}$, indicates growth in the number of firms employing s employees at quarter t ($y_{s,t}$) relative to the number of firms of size s in 2015Q3. S_s denotes group dummies and β_t are quarterly time dummies. The indicator I_s equals 1 in the treatment group, and zero otherwise.

We compare the difference in the growth rate of the number of firms between the control and treatment groups relative to 2015Q3. We consider four time periods. The indicators T_0, T_1, T_2 are T_3 are equal to 1 in respectively 2009Q1-2015Q2, 2015Q4, 2016Q1-2017Q4 and 2018Q1-2019Q4. Consequently, the parameter γ_0 captures the difference between the treatment and control groups in the pre-reform period (2009Q1-2015Q2) and 2015Q3, and allows us to test whether the outcome evolved similarly in the treatment and control group in the pre-reform period. The parameter γ_1 captures anticipation effects in 2015Q4, during which the policy was already announced but not yet implemented; γ_2 captures the average impact of the reform from 2016Q1 to 2017Q4, that is the first two years after the reform, which is a transition period after which the number of firms with one employee stabilises, as evidenced by Figure 2.2; and γ_3 captures the average impact of the reform from 2018Q1 to 2019Q4.

We are only interested in γ_3 because our theoretical model captures the long-term impact on the number of firms but is silent about the dynamics leading to a new equilibrium. Hence, in Table 2.2, we only report γ_0, which allows to test the parallel trend in the pre-reform period, and γ_3, the parameter of interest.

We consider two control groups. First, we use firms with 7-8 employees as the control group. We can exclude that the exemption has helped firms achieve a size of 7–8 employees, as by 2019Q2, only 2% of the 30,000 full-time equivalent subsidised employees were employed in firms with 7–8 employees.[9]

[9]We do not include firms with more than eight employees in the control group because a visual inspection of the data reveals that the parallel trend does not hold for this group in the pre-reform period.

Next, we consider firms with 3–8 employees as the control group. These firms are more comparable to firms with one employee, which makes it more likely that the parallel trends assumption holds. Additionally, this control group has more observation, increasing the precision of our estimates of both the long-term impact on the number of firms and the evolution of the number of firms pre-reform. We cannot rule out a priori that the number of these firms was affected by the policy, as some received the subsidy and might have had fewer employees in a counterfactual scenario without it. However, if this were the case, we would expect a smaller increase in the stock of firms with 1 employee when using the control group of firms with 3-8 employees, compared to using the control group of firms with 7–8 employees.

DiD results. Table 2.2 show that the number of firms with one employee was, on average, 7.2% higher in 2018Q1-2019Q4 than 2015Q3, relative to firms with 7-8 employees (column 1) or 3-8 employees (column 2). The effect is precisely estimated in the second specification, with a 95% CI ranging from 6.6% to 7.7%. The nearly identical values in the two estimates for γ_3 also provide evidence that the reform has likely had no impact on the number of firms with more than three employees. In both specifications, the pre-reform estimate, γ_0, is close to 1% and statistically significant in the second specification where the coefficients are more precisely estimated. This indicates that the growth in the number of firms in the treatment and control groups did not perfectly coincide in the pre-reform period, raising concerns about the validity of the parallel trend assumption. Nonetheless, the post-reform point estimate, γ_3, is eight times larger than the pre-reform estimate, indicating that the policy had a substantial effect on the number of firms with one employee, even if the parallel trend assumption does not hold perfectly.

Columns (3) and (4) examine the evolution of the number of firms with two employees relative to firms with 7-8 and 3-8 employees. In both cases, there are no pre-trends in the pre-reform period, and the post-reform point estimate is small and statistically insignificant, implying that the subsidy did not affect the stock of firms with two employees. The statistically insignificant estimates for γ_3, when using firms with 7–8 employees and 3–8 employees as control groups, along with the fact that firms with 7–8 employees do not directly benefit from the policy within the three years following the reform, indicate that the policy only affected the number of firms with a single employee, without generating positive spillover effects on firms with more than one employee. These results align with Deng et al. (2024), who analyzed annual cohorts of new employers before and after the reform. Their findings indicate that, compared to the pre-reform cohort, the number of new employers in the post-reform cohort remained higher after three years. However, most of this increase was driven by a rise in firms with only one employee in the post-reform cohort.

Finally, column (5) contrasts the evolution of the stock of firms with one vs. those with two employees. This comparison is warranted because we do not observe an effect on firms with two employees and is relevant because firms with two employees are very similar to firms with one employee, alleviating concerns about the validity of the parallel trend assumption. Figure 2.2 demonstrates that the evolution of the stock of firms with one and two employees almost coincides in the pre-reform period, but diverges rapidly following the 2016 reform. This DiD regression indicates that the number of firms with one employee has increased by 7.2% in 2018Q1-2019Q4 relative to 2015Q3.

Deng et al. (2024) and Cockx and Desiere (2024) show that the policy led to a 24% to 31% increase in the flow of new employers, thereby gradually increasing the number of firms with one employee. At the same time, the 2016 reform might have increased the bankruptcy rate of existing firms with a single employee that did not benefit from the subsidy. Hence, the 7.1% increase should be interpreted as the net impact of the payroll tax exemption on the number of firms with one employee, accounting for both increased firm entry and exit.

One might be concerned that the policy increases market-level wages, which could lead to job losses in firms in the control group. These general equilibrium effects would violate the Stable Unit Treatment Value Assumption (SUTVA). A (substantial) wage increase is unlikely in the three years following the reform because by 2019 only about 1% of the employees in the private sector are subsidised. Additionally, even when all employers have a subsidised employee, our calibrated general equilibrium model predicts a wage increase of 0.5%, which is sufficiently small to ignore general equilibrium effects. A related concern is whether the tax shift, which lowered the payroll tax rate for firms in the control group by 3.3 percentage points between 2015 and 2018 (see Table 2.1), violates SUTVA. This modest tax reduction is unlikely to have a substantial positive effect on the number of firms in the control group. Additionally, the tax shift may have increased wages, further damping its impact on the number of firms.

A natural question for economists is whether the policy led to strategic behaviour, prompting existing firms to close and reopen to take advantage

Table 2.2. DiD estimates of the growth in the number of firms

Treatment group	1 EE	1 EE	2 EES	2 EES	1 EE
Control groups	7-8 EES	3-8 EES	7-8 EES	3-8 EES	2 EES
γ_0	0.0088	0.0095**	0.0039	0.0046	0.0049
	(0.015)	(0.0032)	(0.012)	(0.0032)	(0.014)
γ_3	0.0715***	0.0713***	−0.00079	−0.0011	0.072***
	(0.016)	(0.0028)	(0.013)	(0.0028)	(0.015)
N	132	308	308	132	88

Notes: The table shows the results of estimating DiD regressions, thereby quantifying the graphical evidence presented in Figure 2.2. EE(S) stands for employee(s). γ_0 and γ_3 capture the differences in the evolution of the number of firms in the treatment group vs. the control group in in, respectively, 2009Q1-2015Q2 and 2018Q1-2019Q4 relative to 2015Q3. Standard errors are clustered by firm size in column (2) and (4). Conventional standard errors are reported in all other columns. ***,**,** denote statistical significance at the 1%, 5% and 10%, respectively.

of the subsidy. Cockx and Desiere (2024) find no evidence of higher exit rates immediately following the reform. While we do not study this type of behaviour, the marked increase in the stock of firms with one employee demonstrates that the policy had a real impact, and did not merely increase firm entry and exit without affecting the total number of firms.

2.5 Theory

In this section, we adopt a frictionless static general equilibrium occupational choice model in the spirit of Lucas (1978). The model captures two key empirical findings: (1) the payroll tax exemption substantially increases the number of firms with one employee, and (2) low-productivity firms hire their first employee as a response to the policy, as shown by Deng et al. (2024). At the same time, the model allows us to go beyond these reduced-form estimates. Comparing the model equilibria before and after the introduction of the exemption, we capture the general equilibrium effects on wages and the macroeconomic implications of labour misallocation caused by the tax exemption.

We first present the model with *size-independent* payroll taxes, where all employers pay the payroll tax rate, $\tau > 0$, on top of employees' wages. This model mimics the economy in the pre-reform period, and will be calibrated in the next section using pre-reform data. Importantly, in our model, a *size-*

independent payroll tax does not distort the optimal labour allocation across firms.

We then introduce the payroll tax exemption by setting $\tau = 0$ for the first employee in each firm but keeping the payroll tax rate unaltered for subsequent employees. This makes the payroll tax rate *size-dependent*, as in Guner et al. (2008) and Braguinsky et al. (2011), and distorts the optimal labour allocation. We explore the general equilibrium effects of the policy on wages and derive the change in the number of firms, aggregate employment, and output from the pre-reform to the post-reform period. The model assumes that all firms employ a single employee exempt from payroll taxes, an outcome which only materialises in the long-run. Our static model is silent about the dynamics leading from the pre-reform to post-reform equilibrium.

To clarify the model's intuition and simplify notation, our baseline model assumes a single payroll tax rate, τ, paid by all firms. This assumption implies that (1) the payroll tax rate does not vary by firm size in the pre-reform period, and (2) the reform only affected the payroll tax rate for the first employee while keeping the rate for subsequent employees unaltered. In Appendix 2.C, we relax these assumptions by incorporating four payroll tax rates, differentiating between the rates paid firms with a single employee (τ_1) and those paid by larger firms (τ_2) in both periods, as reported in Table 2.1. This extension is insightful because the firm size distribution was already distorted in the pre-reform period—i.e. the tax rate was already 8.6pp lower for firms with a single employee than for larger firms before the reform. The extended model allows testing whether our calibrated parameters in the baseline model are sensitive to this distortion. Moreover, this extension clarifies how to define the payroll tax rate in the baseline model. Finally, it enables the simulation of the combined impact of the payroll tax exemption for the first employee and the payroll tax rate reduction for subsequent employees. The results of the simulation of the extend model are presented in Section 2.7.4.

2.5.1 A Lucas model with *size-independent* payroll taxes

Lucas (1978) introduces firm heterogeneity by assuming that individuals have a certain managerial ability, z, which follows a probability density function $\phi(z)$, with support in the range $[1, z_{max}]$. In equilibrium, individuals with managerial ability $z \geq z_{min}$ manage firms and employ workers, while individuals with $z < z_{min}$ are employees. The labour force is fixed, meaning that the sum of the number of managers and employees is constant. There is no unemployment.

To clearly distinguish between the pre and post-reform periods, we will respectively use the subscript b (before) and a (after).

Optimal labour demand of managers. The only input in production is labour provided by the employees, l. The labour input of the manager, while necessary to run the firm, does not contribute directly to production. Following Garicano et al. (2016), the production function of a manager with ability z is $y = zl^{\alpha}$. If an individual chooses to be a manager, she will choose l that maximises profits, $\pi(l, z) = zl^{\alpha} - w_b(1+\tau)l$, given the wage w_b and the payroll tax rate, $\tau > 0$.

The first-order condition determines optimal labour demand of managers with productivity z, $l_b^*(z)$:

$$l_b^*(z) = \left(\frac{\alpha z}{w_b(1+\tau)}\right)^{\frac{1}{1-\alpha}} \tag{2.1}$$

The profits of managers with optimal labour demand, $l_b^*(z)$, are equal to:

$$\pi_b^*(z) = (1-\alpha)\left(\frac{\alpha}{w_b(1+\tau)}\right)^{\frac{\alpha}{1-\alpha}} z^{\frac{1}{1-\alpha}} \tag{2.2}$$

Optimal employment and profits are strictly positive and increase in z for $\forall z \in [1, z_{max}]$, implying that more productive managers employ more workers and earn higher profits.

Occupational choice. Individuals choose between being a manager (and earning profits $\pi_b^*(z)$) or being an employee (and earning a gross wage, $w_b(1+\tau)$). Because profits increase with managerial ability while wages do not, there exists a minimum level of managerial ability, denoted by $z_{\min,b}$, at which the individual is indifferent between being a manager or an employee. Individuals with managerial ability greater than $z_{\min,b}$ are managers, whereas those with a lower ability are employees.

We assume that individuals care about gross wages, given by $w_b(1+\tau)$, rather than net wages, given by w_b. This assumption essentially rules out that some individuals prefer being managers to avoid payroll taxes. While some individuals may be managers for tax reasons (Parker, 2003), this is not the phenomenon we want to study in this paper. This assumption can also be motivated by the fact that payroll taxes offer real benefits to employees, such as a pension. In contrast, managers must save some of their profits to obtain similar benefits.

The indifference condition, $\pi_b^*(z) = w_b(1+\tau)$, pins down the productivity level and labour demand of the marginal manager:

$$z_{min,b} = \left(\frac{\alpha}{1-\alpha}\right)^{1-\alpha}(1+\tau)\frac{w_b}{\alpha} \tag{2.3}$$

$$l_{min,b} = \frac{\alpha}{1-\alpha} \tag{2.4}$$

Because about one-third of firms employ one employee (see Figure 2.2), $l_{min,b}$ should be smaller or equal to one. This requires $\alpha \leq 0.5$, implying strongly decreasing returns to scale.[10]

Labour market clearing. In equilibrium, labour demand equals labour supply:

$$\int_{z_{min,b}}^{z_{max}} l_b^*(z)\phi(z)dz = \int_1^{z_{min,b}} \phi(z)dz \tag{2.5}$$

The left-hand side of this condition expresses labour demand by the managers, i.e. individuals whose ability is greater than $z_{min,b}$. The right-hand side represents labour supplied by those individuals who are insufficiently productive to start their own firms. This condition determines the equilibrium wage, w_b.

Labour allocation across firms is independent of the payroll tax rate. Understanding the role of payroll taxes in this model is crucial. It turns out that the occupational choice and the allocation of labour across firms are independent of the payroll tax rate. As in the canonical tax incidence model with perfectly inelastic labour supply (Fullerton and Metcalf, 2002), an increase in the payroll tax rate is fully offset by a decrease in wages, leaving gross wages, occupational choice, employment and the allocation of labour across firms unchanged. In our model, this property follows directly from the indifference condition, which assumes that the marginal manager is indifferent between profits and gross wages.

The proof of this property is included in Appendix 2.B, but the intuition is straightforward. Given a certain wage, a payroll tax increase reduces labour demand among existing firms. Additionally, a higher payroll tax diminishes the attractiveness of being a manager because the profits of the marginal manager

[10] The literature on firm dynamics typically sets α in the range of 0.5 (Hsieh and Klenow, 2009) to 0.8 (Garicano et al., 2016) in order to match the firm size distribution and/or the labour share in the economy. By setting $\alpha = 0.8$, Garicano et al. (2016) implicitly assume that firms employ at least four employees (see their Appendix, equation (34)).

must equal $w_b(1+\tau)$, prompting the least productive managers to become employees. The subsequent decline in labour demand (among existing firms) and increase in labour supply (as some managers become employees) decreases equilibrium wages. The wage decrease, in turn, boosts labour demand among existing firms and induces some employees to become managers. In equilibrium, the wage decrease perfectly offsets the higher payroll tax rate and gross wages remain constant. Consequently, the share of managers in the labour force and labour allocation across firms are unaffected by the payroll tax.

2.5.2 A Lucas model with *size-dependent* payroll taxes

We now introduce the payroll tax exemption for the first employee by setting τ equal to zero for the first employee while keeping τ unaltered for subsequent employees. The exemption affects the profit function and makes it more attractive to be a manager rather than an employee. This, in turn, affects optimal labour demand and the firm size distribution.

Our set-up assumes that firms do not share the subsidy with their subsidised worker but pay the same market wage to each employee. This is a plausible assumption for at least three reasons. First, wages in Belgium are set by collective bargaining agreements, and firm-specific wage top-ups are uncommon (Du Caju et al., 2012).

Second, the subsidy is not tied to a specific employee, implying that the subsidised worker cannot threaten the employer to leave the firm. In addition, fairness considerations within firms make it unlikely that similar workers get a different wage (Benzarti, 2024).

Finally, several recent papers find no pass-through to wages of hiring subsidies and payroll tax cuts on the eligible population (e.g., Saez et al. (2019) in Sweden, Cahuc et al. (2019) in France), in particular when the payroll tax cut does not affect future benefits of the employees (Bozio et al., 2019), as in the case in our setting. Albanese et al. (2024) find no effect on wages of a generous hiring subsidy for lower-educated youth in Belgium.

Optimal labour demand of managers. The profit function of managers after the reform is:

$$\pi_a(z,l) = \begin{cases} zl^\alpha - w_a l & l \leq 1 \\ zl^\alpha - (\tau+1)w_a(l-1) - w_a & l > 1 \end{cases} \tag{2.6}$$

Then, the optimal labour demand of managers is given by the first order conditions:

$$l_a^*(z) = \begin{cases} \left(\frac{\alpha z}{w_a}\right)^{\frac{1}{1-\alpha}} & z < \underline{z}_{1,a} \\ 1 & z \in [\underline{z}_{1,a}; \overline{z}_{1,a}] \\ \left(\frac{\alpha z}{w_a(1+\tau)}\right)^{\frac{1}{1-\alpha}} & z > \overline{z}_{1,a} \end{cases} \tag{2.7}$$

Managers with productivity between $\underline{z}_{1,a} = w_a/\alpha$ and $\overline{z}_{1,a} = w_a(1+\tau)/\alpha$ employ exactly one employee. This means that labour demand bunches at one employee.

Optimal labour demand determines the profits, $\pi_a^*(z)$, that individuals with ability z will earn if they choose to be managers. Profits are strictly positive and are strictly increasing in z.

Occupational choice. As in the pre-reform period, individuals choose to be managers if profits exceed gross wages. The marginal manager is indifferent between being a manager or an employee if $\pi_a^*(z) = w_a(1+\tau)$. This condition pins down $z_{min,a}$ and $l_{min,a}$:

$$z_{min,a} = \left(\frac{\alpha}{1-\alpha}\right)^{1-\alpha} (1+\tau)^{1-\alpha} \left(\frac{w_a}{\alpha}\right) \tag{2.8}$$

$$l_{min,a} = \frac{(1+\tau)\alpha}{1-\alpha} \tag{2.9}$$

Individuals whose managerial ability exceeds $z_{min,a}$ are managers, while those with a lower ability level are employees.

Labour market clearing. In equilibrium, the following market clearing condition pins down the equilibrium wage, w_a:

$$\int_{z_{min,a}}^{\underline{z}_{1,a}} l_a^*(z)\phi(z)dz + \int_{\underline{z}_{1,a}}^{\overline{z}_{1,a}} \phi(z)dz + \int_{\overline{z}_{1,a}}^{z_{max}} l_a^*(z)\phi(z)dz = \int_1^{z_{min,a}} \phi(z)dz. \tag{2.10}$$

The terms on the left-hand side denote labour demand by managers who have (1) fewer than one employee, (2) exactly one employee; and (3) more than one employee. The term on the right-hand side is labour supplied by individuals who are not sufficiently productive to be managers.

2.5.3 The pre-reform vs. post-reform period

The key element to understand the impact of the reform is the relation between the minimum level of productivity required to be a manager in the pre-reform ($z_{min,b}$) and post-reform period ($z_{min,a}$). Combining equation (2.3) and (2.8) implies:

$$\frac{z_{min,a}}{z_{min,b}} = \frac{1}{(1+\tau)^{\alpha}} \frac{w_a}{w_b} \tag{2.11}$$

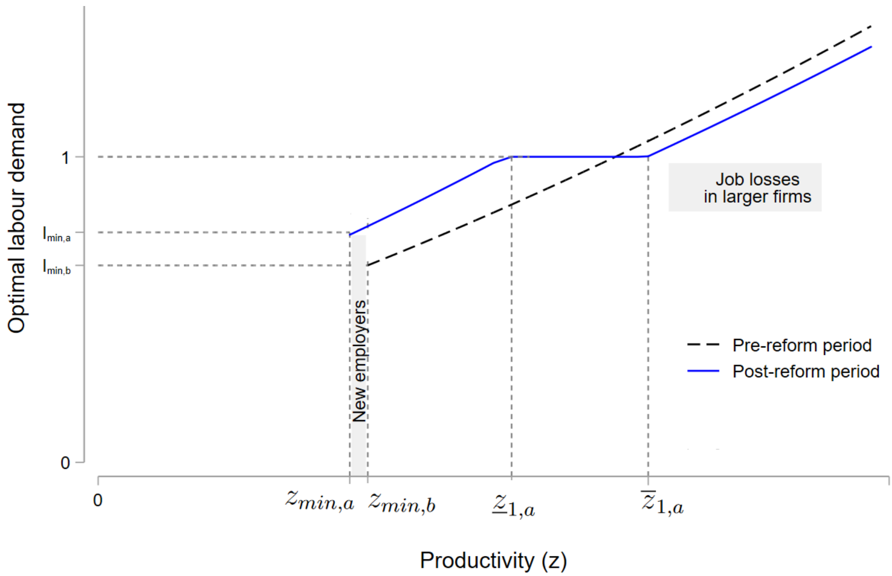
This equation shows that, if wages were to remain constant following the reform ($w_a = w_b$), the minimum productivity level required to be a manager would decrease. However, the entry of new firms increases labour demand and reduces labour supply, leading to a wage increase ($w_a > w_b$) to ensure that the labour market clears. The direct effect of τ on $z_{min,a}$ is stronger than the indirect effect through the wage increase, such that in equilibrium $z_{min,a} < z_{min,b}$.

Figure 2.3 helps to understand the key differences between the pre-reform and post-reform periods. It shows occupational choice and labour demand as a function of managerial ability in both periods, thereby illustrating that the precise response to the payroll tax exemption depends on someone's productivity level. While individuals with $z < z_{min,a}$ are employees in both periods, one can distinguish between three type of managers.

The first type are individuals who are just insufficiently productive to be managers in the pre-reform period, but become managers following the reform. This leads to an increase in the number of firms, as indicated by the grey bar in the figure. These managers recruit fewer than one full-time employee. The second type are managers who, pre-reform, employed fewer than one full-time employee. These managers adjust their labour demand following the reform, but remain insufficiently productive to employ more than one full-time employee after the reform. This explains why labour demand bunches at one full-time employee, a typical feature of models with kinked tax rates, as is the case in our setting (Saez, 2010). The third group are managers with more than one full-time employee in the pre-reform period. For these managers, the payroll tax exemption provides a windfall profit without directly affecting their optimal labour demand. However, due to general equilibrium effects driving up wages, there is a slight decrease in labour demand post-reform for this group, leading to job losses in these firms.

Effect on the number of firms. Using equation (2.11) allows us to compute the relative increase in the number of firms after the reform:

Figure 2.3. Occupational choice and optimal labour demand in the pre-reform and post-reform period



$$\frac{\text{New firms}}{\text{Number of firms pre-reform}} = \frac{\int_{z_{min,b}}^{z_{min,a}} \phi(z)dz}{\int_{z_{min,b}}^{z_{max}} \phi(z)dz} = C\left[\tau^{\alpha(\gamma-1)} \underbrace{\left(\frac{w_a}{w_b}\right)^{1-\gamma}}_{<1,\text{ general equilibrium effects}} -1\right] \tag{2.12}$$

The second equality is derived assuming that managerial ability follows a bounded Pareto distribution, $\phi(z) = [\frac{\gamma-1}{1-z_{max}^{1-\gamma}}]z^{-\gamma} = \delta z^{-\gamma}$, where $\gamma > 1$ and $z \in [1, z_{max}]$. Section 2.6 motivates this choice. $C = 1/(1-(\frac{z_{max}}{z_{min,b}})^{1-\gamma}) \approx 1$.[11]

Equation (2.12) demonstrates that the wage increase in general equilibrium ($w_a > w_b$) dampens the positive effect of the payroll tax exemption on the growth in the number of firms, compared to a scenario that assumes the absence of general equilibrium effects on wages.

[11]Equation (2.1) implies that $\frac{z_{max}}{z_{min,b}} = (\frac{l_{max,b}}{l_{min,b}})^{1-\alpha}$, where $l_{min,b}$ and $l_{max,b}$ are respectively the minimum and maximum firm size determined by the productivity of the marginal manager ($z_{min,b}$) and the productivity of the manager with the highest productivity level (z_{max}) in the pre-reform period. Hence, $(\frac{z_{max}}{z_{min,b}})^{1-\gamma} = (\frac{l_{max,b}}{l_{min,b}})^{1-K} \approx 0$, because $K = \gamma(1-\alpha) + \alpha > 1$ and $l_{max,b} >> l_{min,b}$.

Effect on employment and aggregate output. The effect on employment and aggregate output is determined by two opposite forces. On the one hand, labour demand and output increase in firms with at most one employee before the reform. On the other hand, higher wages reduce labour demand and output in larger, more productive firms (see Figure 2.3). Eventually, the reallocation of labour from high-productivity to low-productivity firms reduces aggregate output and employment.

More formally, let $z_{1,b}$ denote the productivity level of managers with exactly one employee and $L_{b,\text{large firms}}$ total employment in firms with more than one employee in the pre-reform period. Then, the difference in aggregate labour demand in the post-reform vs. the pre-reform period is:

$$
\begin{aligned}
L_a - L_b = & \underbrace{\int_{z_{min,a}}^{z_{min,b}} l_a^*(z)\phi(z)dz}_{\text{Job creation in new firms}} + \underbrace{\int_{z_{min,b}}^{z_{1,b}} [l_a^*(z) - l_b^*(z)]\phi(z)dz}_{\substack{\text{Job creation in existing firms} \\ \text{with less than one employee pre-reform}}} \\
& + \underbrace{\int_{z_{1,b}}^{z_{max}} [l_a^*(z) - l_b^*(z)]\phi(z)dz}_{\text{Job losses in larger firms}} \approx \underbrace{\int_{z_{min,a}}^{z_{min,b}} l_a^*(z)\phi(z)dz + \int_{z_{min,b}}^{z_{1,b}} (l_a^*(z) - l_b^*(z))\phi(z)dz}_{\text{Job creation in firms with at most one employee}} \\
& \underbrace{- \left[1 - \left(\frac{w_b}{w_a}\right)^{\frac{1}{1-\alpha}}\right] L_{b,\text{large firms}}}_{\text{Job losses in larger firms}} < 0
\end{aligned}
\tag{2.13}
$$

The first two terms capture the increased labour demand by new firms and by firms that would have employed less than one full-time employee in the pre-reform period but expand in the post-reform period. The third term captures the job losses in firms with more than one employee caused by the wage increase.[12] This term illustrates that a small wage increase can cause a decline in overall employment because firms with more than one employee account for the lion's share of total employment. Job losses in large firms exceeded job creation in new and small firms for the simple reason that the

[12] $\int_{z_{1,b}}^{z_{max}} (l_a^*(z) - l_b^*(z))\phi(z)dz \approx -[1 - (\frac{w_b}{w_a})^{\frac{1}{1-\alpha}}]L_{b,\text{large firms}}$ is derived assuming that managerial ability follows a bounded Pareto distribution. Because we neglect job losses in firms with productivity between $z_{1,b}$ and $\overline{z}_{1,a}$, i.e. firms whose labour demand is exactly equal to one in the post-reform period but slightly higher than one in the pre-reform period.

tax exemption induces some employees to become managers. Given that the labour force is fixed, the share of employees in the labour force must, therefore, decrease.

Similarly, the effect of the tax exemption on aggregate output in general equilibrium can also be decomposed into a positive effect on firms with one employee and a negative effect on larger firms:

$$\begin{aligned} Y_a - Y_b \approx & \underbrace{\int_{z_{min,a}}^{z_{min,b}} l_a^{*\alpha} z\phi(z)\,dz + \int_{z_{min,b}}^{z_{1,b}} \left(l_a^*(z)^\alpha - l_b^*(z)^\alpha\right) z\phi(z)\,dz}_{\text{Additional output in firms with one employee}} \\ & \underbrace{- \left[1 - \left(\frac{w_b}{w_a}\right)^{\frac{\alpha}{1-\alpha}}\right] Y_{b,\text{large firms}}}_{\text{Output loss in larger firms}} < 0 \end{aligned} \tag{2.14}$$

Because of decreasing return to scale, the reduction in output in larger firms is smaller than the reduction in employment. Again, there is no output loss in large firms if wages do not adjust.

2.6 Calibration

Following the literature, we assume that managerial ability follows a bounded Pareto distribution, $\phi(z) = [\frac{\gamma-1}{1-z_{max}^{1-\gamma}}]z^{-\gamma} = \delta z^{-\gamma}$, where $\gamma > 1$ and $z \in [1, z_{max}]$. This functional form allows us deriving the pre-reform firm size distribution, which is the key moment targeted in the calibration (see Appendix 2.D). The pre-reform firm size distribution is a bounded Pareto distribution with parameter $K = \gamma(1-\alpha) + \alpha$ and support $[l_{min,b}, l_{max,b}]$, where $l_{min,b}$ and $l_{max,b}$ are respectively the minimum and maximum firm size determined by the productivity of the marginal manager ($z_{min,b}$) and the productivity of the manager with the highest productivity level (z_{max}).

Therefore, the model contains four fundamental parameters: α, γ, z_{max} and τ. We set $\tau = \frac{1+\tau_{1,b}}{1+\tau_{1,a}} - 1 = 0.15$[13] to match the ratio of the expected payroll tax for the first employee in the pre-reform and the post-reform period. Recall that, in the baseline model, the level of τ does not affect the optimal value of α, γ and z_{max} because the firm size distribution is independent of τ.

[13]This choice is motivated by equation (2.22) of the extended model, and corresponds to assuming that the payroll tax rate only changed for the first employee in the post-reform period, but remained unaltered for subsequent employees.

We obtain α, γ, and z_{max} by matching the average firm size while minimizing the distance between the observed and simulated firm size distribution in 2015Q3.[14,15] More specifically, we minimize the following chi-square statistic:

$$\min_{\alpha,\gamma}\Big[\sum_{i=1}^{15}\frac{(observed_i - simulated_i)^2}{simulated_i} + \frac{(observed_{>15} - simulated_{>15})^2}{simulated_{>15}}\Big]$$

where $observed_i$ and $simulated_i$ denote the observed and simulated share of firms employing exactly i employees; and $observed_{>15}$ and $simulated_{>15}$ denote the observed and simulated share of firms with more than 15 employees.

Table 2.3 shows the optimal values of α, γ and z_{max} along with the minimum level of productivity required to start a firm and the minimum and maximum firm size, while Figure 2.4 compares the observed versus the simulated firm size distribution. Our parsimonious model matches the firm size distribution in the pre-reform period relatively well. The share of firms employing one employee (30.5%) is almost perfectly matched by the model (29.8%). The model overstates the share of firms employing exactly two employees (16.5% in reality vs. 20.3% in the model) and the share of firms employing more than 15 employees (11.8% vs. 16.0%).

Our calibrated parameters come with two caveats. First, the choice of a bounded Pareto distribution implies that one can either accurately match the distribution of small or large firms, but rarely both (Dewitte et al., 2022). We choose to match the distribution of firms with 15 employees because small firms are directly affected by the payroll tax exemption. In our setting, it is crucial to accurately match the share of firms with one employee, as these

---

[14] 2015Q3 is the quarter preceding the announcement of the policy. This choice is innocuous because the firm size distribution is remarkable stable from 2009Q1 to 2015Q3, with the share of firms with one (two) employee(s) out of firms with at most fifteen employees ranging from 34.6% (18.7%) to 35.1% (18.9%). The average firm size in the private sector in 2015Q3 was 10.84.

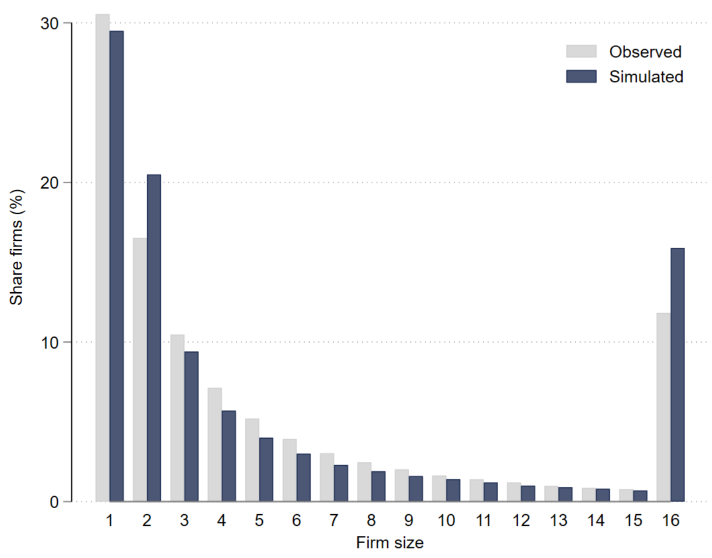
[15] The calibration follows a simple iterative procedure. In the first step, we choose starting values for γ and α, which pins down $l_{min,b}$. In the second step, we determine the equilibrium wage, w_b, so that labour demand equals labour supply, and determine z_{max} so that the simulated average firm size equals the observed average firm size. This step determines $l_{max,b}$. In the third step, we compute the firm size distribution, and compare the simulated and observed firm size distribution. We obtain the optimal values of α and γ by repeating steps 2 and 3 until the chi-square statistic reaches a minimum.

Table 2.3. Optimal parameter values

Fundamental parameters	
α	0.32
γ	1.63
z_{max}	2,417
Derived parameters	
$z_{min,b}$	44.2
$l_{min,b}$	0.47
$l_{max,b}$	172

firms benefit most from the subsidy. The implication of this choice is that the employment distribution in large firms is poorly matched by the model. For instance, our model predicts that firms with more than 100 employees employ 28.1% of the employees, while these firms actually employ 52.7% of the employees. The mismatch occurs because our model imposes a maximum firm size of 172, which rules out the existence of very large firms that account for a disproportionate share of employment.

Figure 2.4. Simulated versus observed firm size distribution in the pre-reform period



Second, while we do not explicitly impose the condition that $\alpha < 0.5$ when calibrating the model, this condition is naturally met. Firms with one employee only exist if $\alpha < 0.5$ (see eq. (2.4)), and since almost one-third of firms have one employee, the optimal value of α must be smaller than 0.5.

The low value of α implies that the labour share of output is not matched. Given the Cobb-Douglas production function, the share of output spent on labour equals α, while the share spent on the other input (here: the managers) equals $1 - \alpha$. In our specific setting, the labour share is split between the employees, who get α/τ, and the government, which gets $\alpha(\tau - 1)/\tau$. With the optimal value of α being 0.32, the labour share of output is 32%, split between employees (28%) and the government (4%). This is low compared to what is typically reported in the national accounts.[16]

The main explanation for the discrepancy between the simulated labour share and the one reported in the national accounts, is that, empirically, the labour share is lower in small firms than at the aggregate level. To illustrate this, we compute the labour share by firm size using our firm-level data on turnover, the wage bill (including payroll taxes), and intermediate inputs. We define the labour share at the firm-level as the 'wage bill' divided by 'turnover minus intermediate inputs' (Appendix 2.A). In our model, the labour share is always equal to α, but in the data, the labour share increases with firm size . The median labour share in firms with one employee is 21%, rising to 60% for firms with 10-15 employees. Since we match the size distribution of firms with fewer than 15 employees, it is expected that our estimate of α lies in-between the labour share for firms with one employee and those with 10-15 employees.

Appendix 2.F investigates the sensitivity of our results to (1) the poor match of employment in large firms, and (2) the low value of α. Following Guner et al. (2008), we assume that a small fraction of firms have a high productivity level, whereas the productivity level of the other (small) firms continues to follow a Pareto distribution. This additional assumption helps in matching the employment distribution and allows setting the value of α to 0.8 for large firms. Our main results remain unaffected.

[16] The labour share increases to 35% if the compensation managers receive for running the firm, about 3% of output (i.e., $w(1 + \tau)$ per manager), is added to the labour share, as is common in national accounts.

2.7 Simulation

We first test whether the predictions from the calibrated baseline model align with the empirical findings. To this end, we compare the simulated to the observed impact of the payroll tax exemption on the total number of firms. Additionally, we verify that the simulated performance of new employers that enter in response to the exemption is consistent with the empirical findings of Deng et al. (2024).

After scrutinizing the validity of our model, we leverage it to quantify the macroeconomic implications of the payroll tax exemption. Specifically, we examine its impact on wages, employment and aggregate output. Finally, we discuss its budgetary cost, and distributional implications.

Our general equilibrium model assumes that wages fully adjust to ensure equilibrium in the labour market. We also consider an alternative scenario, termed the fixed-wage scenario, in which wages remain constant at their pre-reform level. In this scenario, the labour force—defined as the sum of managers and employees—is perfectly elasticity rather than fixed, as in the general equilibrium scenario. This assumption holds if new managers come from outside the labour force, for example because they were self-employed in the pre-reform period, and if increased labour demand can be entirely met by attracting individuals who were previously outside the labour force (e.g., the unemployed or the inactive). Wages remain constant in this scenario because there is no competition among firms to attract employees. As a result, there are no job losses in larger firms in the fixed-wage scenario.

The general equilibrium and fixed-wage scenario are polar cases. Appendix 2.E considers an intermediate case, where new managers come from outside the labour market while there is still competition between firms to attract employees. This intermediate scenario still leads to a wage increase and job losses in larger firms, but to a smaller extent than in the general equilibrium scenario.

2.7.1 Model validation

Impact on the number of firms. To empirically support our model, we compare the DiD estimate of the impact of the exemption on the number of firms with one employee to the model's prediction. We expect the DiD estimate and model prediction to be comparable for three reasons.

First, our static model simulates the new equilibrium but remains silent about the dynamics leading from the pre-reform to post-reform equilibrium. The DiD estimate captures the impact on the number of firms after three years. Therefore, the DiD estimate is comparable to the model only if the number of firms with one employee has already reached the new equilibrium within this time frame. This is plausible because new managers (i.e. those with $z < z_{min,b}$) tend to hire their first employee almost immediately after the reform (Cockx and Desiere, 2024), a finding consistent with the existing literature on labour demand adjustment to large shocks in labour costs (Hamermesh, 1989). Equally important, Figure 2.2 shows that the number of firms with one employee increased rapidly until the end of 2017, but remained stable between 2018Q1 and 2019Q4, suggesting that the number of firms reached a new equilibrium two years after the reform.

Second, the wage increase predicted by our model affects the control group, leading to a violation of the SUTVA assumption in the DiD regressions. However, even if wages immediately increase by 0.5% following the reform rather than adjusting slowly to the new equilibrium, the impact on the control group will be limited. In the general equilibrium scenario, the number of firms with 1 employee increases by +10.18%, whereas the number of firms with 2, 3-8, and 7-8 employees decreases by -0.33% due to the rise in the equilibrium wage

Third, our model assumes that all firms have a subsidised first employee, which is not the case three years after the reform, as it takes time for existing firms to be replaced by subsidised ones. However, this does not cause our model to under or overstate the effect of the policy on the number of firms or wages. Our simulation reveals that the increase in labour demand among firms with at most one employee that would also have existed without the exemption only marginally contributes to the total change in employment and output post-reform. In other words, the wage increase is primarily caused by the entry of new employers rather than by increased labour demand among existing firms.[17]

Given that these three conditions are met, it is reassuring to observe that the DiD estimate (+7.2%) is remarkably close to the model prediction, as

[17] Assuming that only firms that would not have existed absent the subsidy (i.e. those with $z < z_{\min,b}$) hire after the reform and ignoring labour adjustments among firms that already employed one or fewer employees before the reform limits the increase in the equilibrium wage to 0.34% instead of 0.52%. This shows that the wage increase is primarily caused by the new employers.

reported in Table 2.4. The model predicts that the number of firms with one employee increases by 10.5% (fixed-wages) or 10.2% (general equilibrium). This demonstrates that our simple, frictionless model, which consists of only four fundamental parameters and is calibrated using pre-reform data only, matches a key empirical fact in the post-reform period surprisingly well. As we will discuss in Section 2.7.4, the simulated impact gets even closer to the DiD estimate when taking into account that the tax shift decreased the payroll tax rate for all employees.

Productivity of new employers. Our model is static and abstracts from firm growth, thereby assuming that firms immediately reach their optimal size. For this reason, our predictions relate to the stock of firms. This makes it challenging to compare empirical findings related to the flow of entrants to the model predictions. However, the predictions and the empirical findings of Deng et al. (2024), who study firm performance of cohorts of new entrants in the pre-reform and post-reform periods, are at least qualitatively similar. They show that the number of new entrants was 24% higher in 2016 than 2015, and report that the 2016 cohort of new entrants recruits fewer employees at entry and employs fewer employees in subsequent years than the previous cohort.

A key feature of our model is that the increase in the total number of firms is entirely driven by the entry of low-productivity firms with at most one employee, without any increase in the number of firms with more than one employee. In our model, a 24% increase in the number of entrants would reduce the share of entrants with at least two (three) employees at entry by 4.0 (2.5) percentage points.[18] This reduction is greater than but qualitatively comparable to the empirical estimates of Deng et al. (2024) who find a decline of 2.40 (1.10) percentage points in the share of entrants with at least two (three) employees at entry.

2.7.2 Macroeconomic implications

In this section, we leverage our model to reflect on the macroeconomic effects of the tax exemption when all firms employ a subsidised first employee. Specifically, we examine the impact of the tax exemption in the fixed-wage

[18] In the pre-reform period, 25.9% (13.0%) of the new entrants hired at least two (three) employees. A 24% increase of new entrants with at most one employee decreases the share of new entrants with least two or three employees to 20.9% (=29.9%/124%) and 10.5% (=13.0%/124%), respectively.

and general equilibrium scenarios on the following three macroeconomic outcomes: (1) number of employees, (2) average firm size, and (3) aggregate output.

Table 2.4 shows the impact of the tax exemption on these outcomes in the fixed-wage (column 1) and general equilibrium (column 2) scenarios. We report the relative change between the post-reform and pre-reform period. The main difference between both scenarios is that the fixed-wage scenario assumes wages to remain constant while the labour force expands, whereas the general equilibrium scenario assumes the labour force to be fixed while wages adjust. In the fixed-wage scenario, the labour force (defined as the sum of managers and employees) expands by 0.75%. This small expansion of the labour force is sufficient to ensure that the increase in managers and the additional labour demand by these new managers can be met. In general equilibrium, wages increase by 0.52%. This modest wage increase is sufficient to ensure that the additional labour demand from new managers is compensated by job losses in larger firms.

Table 2.4. Impact on the number of firms, the macro economy, and the government budget

	Fixed wages	General equilibrium
Defining characteristics (%Δ)		
Minimum productivity level	−4.04%	−3.91%
Wage rate	0.00%	+0.52%
Labour force	+0.75%	0.00%
Change number of firms (%Δ)		
Number of firms with one employee	+10.54%	+10.18%
Number of firms	+3.13%	+2.77%
Evolution of macro indicators (%Δ)		
Number of employees	+0.53%	−0.26%
Average firm size	−2.53%	−2.95%
Output	+0.25%	−0.01%
Budgetary cost (% output$_b$)	0.36%	0.36%

The implications of the policy are different depending on whether the labour force or wages adjust. In the fixed-wage scenario, the expansion of the labour force leads to a 0.53% increase in the number of employees, which in turn increases aggregate output by 0.25%. Because wages are assumed to remain constant, the policy has no negative spillover effect on firms with more than one employee and does not lead to labour reallocation between firms. This explains why the key economic indicators evolve favourably in this scenario.

By contrast, the wage increase in the general equilibrium scenario causes job losses in firms with more than one employee and leads to a small reduction in aggregate output. In this scenario, the 2.77% increase in the number of managers is compensated by a 0.26% decrease in the number of employees to ensure that the labour force remains constant. As a result, in the post-reform period, 91.3% of the labour force are employees and 8.7% are managers, compared to, respectively, 91.6% and 8.4% in the pre-reform period. At the same time, we observe a shift from employment in firms with more than one employee to employment in firms with at most one employee. The share of employees in firms with one employee increases from 1.9% in the pre-reform period to 2.4% in the post-reform period, offset by a corresponding decrease in employment in firms with more than one employee. The reallocation of labour from high-productivity to low-productivity firms results in a negligible loss in output (−0.01%).

2.7.3 The budgetary cost and the distributional impact

Budgetary cost. From a policy perspective, the main implication of the payroll tax exemption is the reduction in government revenues. Once all firms benefit from the tax exemption, both scenarios predict its budgetary cost at 0.36% of output. The budgetary cost is computed as the difference in the share of government revenues in the post-reform and pre-reform period, relative to pre-reform output. A back-on-the-envelope computation suggests that this is a realistic long-term prediction.[19]

Budget constraint in general equilibrium. To further illustrate the substantial loss in government revenues, we introduce a budget constraint in the baseline model. More specifically, we assume that the government increases the payroll tax rate for all employees except the first one to balance the books. This approach allows us to examine the extent to which the payroll tax rate has to increase in order to exempt the first employee in each firm from payroll taxes. In the baseline model, the payroll tax rate must increase by a sizeable

[19] According to the NSSO, the private sector in Belgium counted 216,450 firms in 2015Q3 and employed 2.22 million full-time equivalent employees. When all firms receive the subsidy, 9.8% of the employees will be exempt from payroll taxes, which is consistent with the model predictions of 8.9%. The average annual cost per subsidised employee in 2018-19 is €7,769, so the total cost is in the order of 1.69 billion euros, or 0.35% of GDP.

1.2 percentage points, from 15% to 16.2%, to balance the budget (Table 2.5, column 2).

Table 2.5. Effect of the policy without and with a budget constraint (general equilibrium)

	No budget constraint	Budget constraint
Number of firms with one employee	+10.18%	+11.31%
Wages	+0.52%	−1.11%
Output	−0.010%	−0.012%
Budgetary cost (% output)	0.36%	0.00%
Payroll tax rate	15%	16.2%

The tax increase makes the exemption for the first employee more valuable, leading to a further increase in the number of firms with at most one employee and reinforcing the reallocation of labour from large to small firms. The rise in the payroll tax rate reduces wages. Without a budget constraint, wages increase by 0.52%. With a budget constraint, wages decrease by 1.11%. Balancing the budget occurs at the expense of employees' wages.

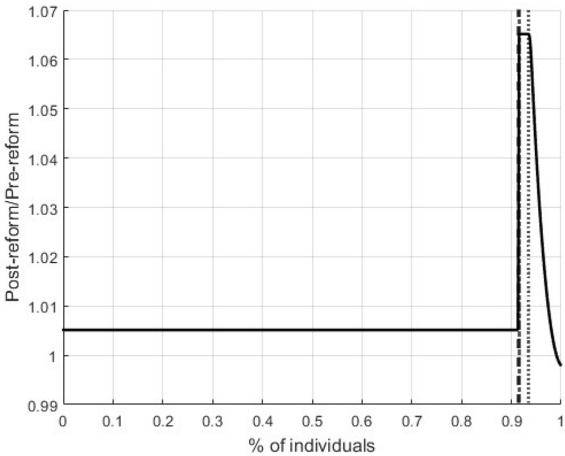
Distributional impact in general equilibrium without budget constraint. The distributional impact depends on someone's managerial ability. The left panel of Figure 2.5 shows the ratio of someone's income (either wages or profits) in the post-reform to the pre-reform period, ranking individuals by managerial productivity. The gains are modest for the large majority (90%) of individuals who are employees in both periods. This group experiences a 0.52% wage increase. By contrast, managers experience more substantial gains, although the 1% most productive managers lose out from the policy.

The right panel of Figure 2.5 zooms in on the managers, showing the gains or losses for those individuals who manage firms in the post-reform period. The figure illustrates that individuals with managerial ability between $z_{min,b}$ and $\underline{z}_{1,a}$, meaning those who were just sufficiently productive to be managers in the pre-reform period, benefit most from the policy. Following the reform, their profits increase by 6.5%. Profits increase less for larger firms, and even slightly decline for firms with more than 25 employees. For these larger firms, the reduction of profits due to the wage increase is not compensated by the tax exemption for the first employee.

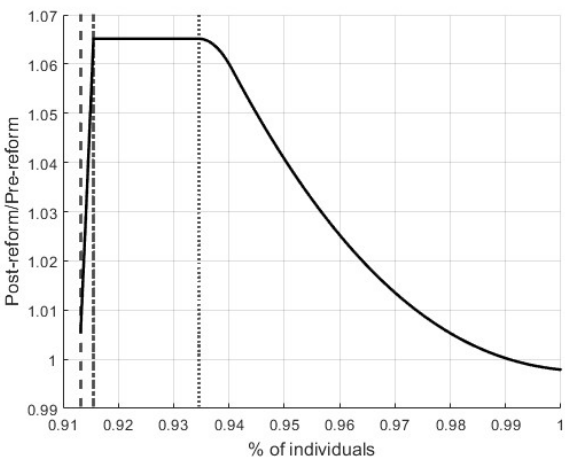
Individuals who become managers following the reform, i.e. those with productivity between $z_{min,a}$ and $z_{min,b}$, are also better off in the post-reform period, with post-reform profits being 0.5% to 6.5% higher than their pre-reform wages. The benefits for the post-reform marginal managers are exactly equal to those for individuals who are employees in both periods.

Figure 2.5. Income ratio in the post-reform vs pre-reform period

(a) All individuals



(b) Managers



Notes: The dashed line identifies $z_{min,a}$, the dash-dotted line identifies $z_{min,b}$, while the dotted line identifies $\underline{z}_{1,a}$.

2.7.4 The extended model: impact of the tax shift

The extended model, discussed in Appendix 2.C, allows for size-dependent payroll taxes in the pre-reform and post-reform periods. One advantage is that we can account that firms with only one employee were paying a lower payroll tax rate than larger firms even before the reform (18.3% vs 26.9%). We find that the calibrated values of α and γ (see Table 2.C.1) are largely identical in this model compared to the baseline model, indicating that not accounting for this wedge in the payroll tax rate before the reform does not significantly bias the value of the calibrated parameters. Then, we simulated the exemption while we assume that payroll taxes for firms with more than one employee did not change from the pre-reform to the post-reform period (Table 2.6, column 2). This choice implies that $\tau_{2,b} = \tau_{2,a} = 27\%$, while the payroll tax rate for the first employee changes from $\tau_{1,b} = 18\%$ in the pre-reform to $\tau_{1,a} = 3\%$ in the post-reform period. In general equilibrium, this extended model leads to similar conclusions as our baseline model (Table 2.6, column 1), illustrating that our results are robust to having size-dependent payroll taxes also in the pre-reform period.

Additionally, the extended model enables us to simulate the impact of the payroll tax exemption in combination with the tax shift, which reduced the payroll tax rate for firms with more than one employee by 3.3 percentage points, from 26.9% in 2015 to 23.6% in 2019 (see Table 2.1). The model reveals that the combination of both policies leads to a smaller increase in the number of firms with one employee (+8.9%) than the tax exemption alone (+10.9%) (Table 2.6, column 2) . This brings our empirical DiD estimate of the evolution of the number of firms with one employee between 2015 and 2019 even closer to the model prediction. Intuitively, the tax shift reduces the relative value of the tax exemption, which makes it less attractive to hire a first employee, and dampens the increase in the number of firms. The main effect of the tax shift is to increase wages by 3.1%. This effect is expected because, even in a model with size-dependent taxes, general payroll tax reductions primarily increase wages, with limited effects on the allocation of labour across firms.

2.8 Conclusion

This paper exploits a unique policy in Belgium that exempts new employers from payroll taxes for their first employee to understand the macro

Table 2.6. Impact of the SSC exemption and the tax shift in the extended model

	Baseline model	Extended model	
		Tax exemption	Tax exemption & tax shift
Evolution of:			
Wages	+0.52%	+0.52%	+3.10%
Firms with 1 EE	+10.18%	+10.93%	+8.85%
Output	−0.01%	−0.02%	−0.02%

Notes: The baseline model sets the payroll tax rate at 15%. The extended model contains four tax rates. In column (2), we set the payroll tax rate for the first employee at 18.3% and 2.9% in the pre-reform and post-reform period, respectively, and assume that the tax rate for subsequent employees is 26.9% in both periods. In column (3), we model the impact of the tax shift by changing the payroll tax rate in the post-reform for firms with more than one employee to 23.6%. EE stands for employee.

implications of massive firm entry. We found empirically that the policy increased the number of firms with one employee by 7.2% three years after its implementation, without much evidence of an increase in the number of firms with more than one employee.

We then developed and calibrated a frictionless occupational choice model. A key take-away from our paper is that a simple static model calibrated using pre-refom data accurately predicts the actual increase in the number of firms with one employee after three years. This suggests that, at least in our setting, modelling market failures is not necessary to predict the first-order effects of size-dependent policies. Additionally, it indicates that the impact of size-dependent policies can be predicted ex-ante using simple models.

The fact that our frictionless model accurately predicts the impact of this size-dependent policy does not mean that frictions are irrelevant for firms hiring their first employee. Our results simply show that the exemption did not address the challenges firms face when trying to grow and that the cost of hiring the first employee may not be the primary obstacle for firms aiming to expand. Furthermore, as Acemoglu et al. (2018) and Haas et al. (2024) emphasise, effective subsidies for start-ups should target high-potential firms while discouraging low-productivity entrants. However, the exemption does the opposite, disproportionately benefiting firms with a single employee rather than those aiming for rapid growth.

Our model does not address the dynamics leading to the new equilibrium, implying that our simulations reveal the effect in the long run, i.e. when all firms employ a subsidised worker. This is not a cause for concern for the predicted impact on the number of firms, as evidence points out that new firms are quickly established following the reform. Similarly, the predicted impact on market wages, is primarily driven by increased labour demand among these new firms. However, the cost of the policy, estimated at 0.36% of output, will only fully materialise in the long run, as it takes time for all incumbent firms to be replaced by new ones, in particular given the low entry and exit rates in Belgium.[20]

The model enables us to consider the long-term macro implications of the policy. In general equilibrium, where the labour force is assumed to be fixed, labour demand by new employers is entirely offset by job losses in larger firms. The policy's only effects are a modest wage increase (+0.5%) and a reallocation from large, high-productivity firms to low-productivity firms, leading to a negligible reduction in output (−0.01%). The main winners of the policy are managers of firms with a single employee, who experience an income gain of up to 6.5%.

Our evaluation does not provide economic arguments to justify this policy. However, it could still be supported for other reasons, such as a preference for small businesses. In this sense, the limited negative impact on output may be reassuring. Nevertheless, the budgetary cost remains a concern, raising difficult questions about who will ultimately bear the budgetary cost of the subsidy for the first employee.

[20] Given an annual exit rate of about 3% reported by Bijnens and Konings (2020), it takes roughly 23 years for half of the incumbents to exit and be replaced by new firms. The (slow) increase in the budgetary cost is evident in the official statistics. In 2023, the budgetary cost amounted to €488 million (about 0.1% of GDP), up from €428 million in 2022 and €372 million in 2021, and continues to increase.

Appendix

2.A Labour share in firm-level data

Using the NBB firm-level data, Table 2.A.1 reports annual turnover, the wage bill (including payroll taxes), and inputs by firm size for the period 2014Q4-2015Q3. We first computed the median outcome by quarter, and then summed the medians over the four quarters.

The 'labour share', reported in the last column, is defined as the ratio of the wage bill to value added, defined as turnover minus inputs. We computed the labour share of the median firm by quarter, and report the average over these four quarters. The labour share increases gradually with firm size, from 21% for firms with one employee to 60% for firms with 15 employees.

Table 2.A.1. Annual turnover, wage bill, inputs and labour share (2014Q4-2015Q3), by firm size

Firm size	Turnover	Wage bill	Inputs	Labour share
1	194,817	22,521	109,574	21%
2	310,609	50,468	180,530	34%
3	427,513	81,695	249,964	41%
4	554,598	115,450	316,838	45%
5	684,585	152,650	393,689	48%
6	824,945	190,928	470,151	51%
7	978,367	228,923	572,118	53%
8	1,156,398	271,276	674,677	54%
9	1,307,846	313,843	777,627	55%
10	1,441,123	353,528	866,435	56%
11	1,657,691	396,784	982,107	58%
12	1,781,542	443,269	1,093,195	59%
13	1,969,981	478,187	1,232,854	60%
14	2,102,829	520,201	1,311,362	61%
15	2,413,063	563,978	1,500,153	60%

2.B The equilibrium is independent of the payroll tax rate

It is straightforward to prove that the payroll tax rate does not affect labour allocation across firms in the model with size-independent payroll taxes. Using the indifference condition of the marginal manager (eq. (2.3)), we can write optimal labour demand in function of $z_{min,b}$ and α:

$$l_b^*(z) = \frac{\alpha}{1-\alpha}\left(\frac{z}{z_{min,b}}\right)^{\frac{1}{1-\alpha}}, z > z_{min,b} \tag{2.15}$$

Substituting this expression in the equilibrium condition (eq. (2.5)) gives:

$$\int_{z_{min,b}}^{z_{max}} \frac{\alpha}{1-\alpha}\left(\frac{z}{z_{min,b}}\right)^{\frac{1}{1-\alpha}} \phi(z)dz = \int_{1}^{z_{min,b}} \phi(z)dz \tag{2.16}$$

This equation explicitly determines $z_{min,b}$ as a function of the parameter of the production function, α, and the parameters of the probability density function, $\phi(z)$. This proves that, in equilibrium, $z_{min,b}$ does not depend on τ.

Combining this finding with the definition of $z_{min,b}$ (eq. (2.3)), implies that:

$$\frac{dz_{min,b}}{d(1+\tau)} = \frac{d}{d(1+\tau)}\left[\left(\frac{\alpha}{1-\alpha}\right)^{1-\alpha} w(1+\tau)\right] = 0 \Rightarrow \frac{d[w(1+\tau)]}{d(1+\tau)} = 0$$

$$\Rightarrow \epsilon_{1+\tau}^{w} = \frac{d[log(w)]}{d[log(1+\tau)]} = -1 \tag{2.17}$$

This last expression shows that the elasticity of the wage with respect to the change in the payroll tax is equal to -1. In other words, a change in the payroll tax rate is fully absorbed by the wage.

2.C Size-dependent payroll taxes in both periods

The baseline model assumes that (1) the payroll tax rate in the pre-reform period does not depend on the firm size; (2) the payroll tax rate is the same in the pre-reform and post-reform period, except for the first employee; and (3) the payroll tax rate for the first employee is exactly equal to zero in the post-reform period.

As documented in Table 2.1, these simplifying assumptions do not hold perfectly because firms hiring their first employee received temporary payroll tax reductions in the pre-reform period, the tax shift reduced the payroll tax rate for all firms in the post-reform period, and the payroll tax rate for the first employee in the post-reform period is not exactly equal to zero.

It is straightforward to relax these assumptions. To this end, we distinguish between the payroll tax rate for the first employee, τ_1, and the rate for subsequent employees, τ_2, in the pre-reform and post-reform periods (with $\tau_2 > \tau_1$ in both periods). Consequently, the extended model contains four payroll tax rates, depending on the firm size and the period. The introduction of two payroll tax rates leads to the following profit function:

$$\pi(z,l) = \begin{cases} zl^\alpha - (1+\tau_1)wl & l \leq 1 \\ zl^\alpha - (1+\tau_2)w(l-1) - (1+\tau_1)w & l > 1 \end{cases} \tag{2.18}$$

The first-order conditions determine optimal labour demand:

$$l^*(z) = \begin{cases} 0 & z < z_{min} \\ \left(\frac{\alpha z}{(1+\tau_1)w}\right)^{\frac{1}{1-\alpha}} & z \in [z_{min}; z_{1,\tau_1}[\\ 1 & z \in [z_{1,\tau_1}; z_{1,\tau_2}] \\ \left(\frac{\alpha z}{w(1+\tau_2)}\right)^{\frac{1}{1-\alpha}} & z \geq z_{1,\tau_2} \end{cases} \tag{2.19}$$

Labour demand bunches at one full-time employee because firms with a productivity between $z_{1,\tau_1} = w(1+\tau_1)/\alpha$ and $z_{1,\tau_2} = w(1+\tau_2)/\alpha$ are sufficiently productive to employ a single employee, but are not sufficiently productive to pay the higher payroll tax rate for the second employee.

The assumption that the marginal manager is indifferent between earning the gross wage, $w(1+\tau_2)$, or profits, $\pi(z_{min})$, pins down z_{min} and l_{min}:

$$z_{min} = \left(\frac{\alpha}{1-\alpha}\right)^{1-\alpha} \left(\frac{w(1+\tau_2)}{\alpha}\right) \left(\frac{1+\tau_1}{1+\tau_2}\right)^{\alpha} \tag{2.20}$$

$$l_{min} = \frac{\alpha}{1-\alpha}\frac{1+\tau_2}{1+\tau_1} \tag{2.21}$$

These equations encompass equation (2.3) of the baseline model, which determined $z_{min,b}$ in the pre-reform period assuming that $\tau_1 = \tau_2 = \tau$, and equation (2.8) of the baseline model, which determined $z_{min,a}$ in the post-reform period assuming that $\tau_1 = 0$ and $\tau_2 = \tau$.

As in the baseline model, the condition that labour supply equals labour demand closes the model.

The extended model contains three new insights. First, the relation between the minimum level of productivity required to be a manager in the pre-reform and post-reform period—which plays a key role in understanding general equilibrium effects—is determined by the four payroll tax rates, and the wage response:

$$\frac{z_{min,a}}{z_{min,b}} = \frac{w_a}{w_b}\left(\frac{1+\tau_{1,a}}{1+\tau_{1,b}}\right)^{\alpha}\left(\frac{1+\tau_{2,a}}{1+\tau_{2,b}}\right)^{1-\alpha} \tag{2.22}$$

This equation simplifies to equation (2.11) if the three aforementioned assumptions hold, i.e. $\tau = \tau_{2,b} = \tau_{2,a} = \tau_{1,b}$ and $\tau_{1,a} = 0$.

The second insight from the extended model is that the firm size distribution (derived in Appendix 2.D) is already distorted in the pre-reform period. Even in the pre-reform period, some firms will bunch at one employee because the payroll tax rate kinks at one employee. The payroll tax exemption in the post-reform period reinforces bunching at one employee, as the payroll tax rate for the first employee further decreases. One concern is that the distortion in the pre-reform period biases the calibration of α, γ, and z_{max} in the undistorted baseline model. To verify this, we also calibrated the extended model (Table 2.C.1). The values of α and γ in the baseline and extended models are similar, and the simulated firm size distribution is identical, indicating that failing to account for the empirical fact that payroll taxes are size-dependent in the pre-reform period only leads to a small bias in α and γ.

Table 2.C.1. Optimal parameter values - baseline vs. extended model

	Baseline model	Extended model
Fundamental parameters		
α	0.32	0.33
γ	1.63	1.65
z_{max}	2,417	2,027
Derived parameters		
$z_{min,b}$	44.2	40.14
$l_{min,b}$	0.47	0.54
$l_{max,b}$	172	173

Notes: The baseline models sets $\tau = 15\%$. The extended model sets $\tau_{1,b} = 2.9\%$ and $\tau_{2,b} = 18.3\%$.

The third insight is that equation (2.22) justifies the choice for setting the single payroll tax, $1+\tau$, in the baseline model, equal to $\frac{1+\tau_{1,b}}{1+\tau_{1,a}}$. To see this, note that the baseline model assumes that the payroll tax rate only changed for the first employee, but remained constant for subsequent employees ($\tau_{2,a} = \tau_{2,b}$). This assumption simplifies equation (2.22) to $\frac{z_{min,a}}{z_{min,b}} = \frac{w_a}{w_b}\left(\frac{1+\tau_{1,a}}{1+\tau_{1,b}}\right)^{\alpha}$. Comparing this expression to equation (2.11) reveals that $1+\tau$ in the baseline model corresponds to $\frac{1+\tau_{1,b}}{1+\tau_{1,a}}$ in the extended model.

2.D The firm size distribution

In this section, we derive the firm size distribution for the extended model. The extended model encompasses all other models discussed in the main text. The baseline model with size-independent payroll taxes corresponds to $\tau_1 = \tau_2 = \tau$, while the baseline model with size-dependent payroll taxes corresponds to $\tau_1 = 0; \tau_2 = \tau$. Hence, the firm size distribution of the extended model also pins down the firm size distribution of the other models.

The derivation of the firm size distribution follows Garicano et al. (2016). Given optimal labour demand in function of managerial productivity z, the firm size distribution $g(l)$ can be derived as:

$$g(l) = \frac{\phi(z(l))}{p}\frac{dz(l)}{dl} \tag{2.23}$$

where p is the share of individuals who are managers.

Using this change-of-variable formula in combination with optimal labour demand given by equation (2.19) gives:

$$g(l) = \begin{cases} \frac{\delta(1-\alpha)}{p}\left(\frac{w(1+\tau_1)}{\alpha}\right)^{1-\gamma} l^{-K} & l < 1 \\ \frac{1}{p}\int_{z_{1,\tau_1}}^{z_{1,\tau_2}} \phi(z)dz & l = 1 \\ \frac{\delta(1-\alpha)}{p}\left(\frac{w(1+\tau_2)}{\alpha}\right)^{1-\gamma} l^{-K} & l > 1 \end{cases} \tag{2.24}$$

where p denotes the share of individuals who are managers, and $K = \gamma(1-\alpha)+\alpha > 1$.

This expression can be simplified. We first compute the share of firms employing exactly one employee:

$$\begin{aligned}
\frac{1}{p}\int_{z_{1,\tau_1}}^{z_{1,\tau_2}} \delta z^{-\gamma} dz &= \frac{1}{p}\int_{(1+\tau_1)w/\alpha}^{(1+\tau_2)w/\alpha} \delta z^{-\gamma} dz \\
&= \frac{\delta}{p(1-\gamma)}\left(\frac{w}{\alpha}\right)^{1-\gamma}[(1+\tau_2)^{1-\gamma}-(1+\tau_1)^{1-\gamma}] \\
&= \underbrace{\frac{\delta}{p(1-K)}\left(\frac{w}{\alpha}\right)^{1-\gamma}(1-\alpha)}_{C}[(1+\tau_2)^{1-\gamma}-(1+\tau_1)^{1-\gamma}] \\
&= C[(1+\tau_2)^{1-\gamma}-(1+\tau_1)^{1-\gamma}]
\end{aligned}$$

In the last step, we use the fact that $1-K=(1-\gamma)(1-\alpha)$.

We then exploit that $\int_{l_{min}}^{l_{max}} g(l)dl = 1$:

$$\begin{aligned}
\int_{l_{min}}^{1} g(l)dl + \frac{1}{p}\int_{z_{1,\tau_1}}^{z_{1,\tau_2}} \phi(z)dz + \int_{1}^{l_{max}} g(l)dl = 1 \\
\Rightarrow C(1-l_{min}^{1-K})(1+\tau_1)^{1-\gamma} + C[(1+\tau_2)^{1-\gamma}-(1+\tau_1)^{1-\gamma}] \\
+C(1+\tau_2)^{1-\gamma}(l_{max}^{-K+1}-1) = 1 \\
\Rightarrow C = -\frac{l_{min}^{K-1}(1+\tau_1)^{\gamma-1}}{1-(\frac{1+\tau_2}{1+\tau_1})^{1-\gamma}(\frac{l_{max}}{l_{min}})^{1-K}} < 0
\end{aligned}$$

Using the previous expressions, we obtain the following probability density function of the firm size distribution in the extended model:

$$g(l) = \begin{cases} (1+\tau_1)^{1-\gamma}C(1-K)l^{-K} & l<1 \\ C[(1+\tau_2)^{1-\gamma}-(1+\tau_1)^{1-\gamma}] & l=1 \\ (1+\tau_2)^{1-\gamma}C(1-K)l^{-K} & l>1. \end{cases} \tag{2.25}$$

Without size-dependent payroll taxes ($\tau=\tau_1=\tau_2$), the firm size distribution simplifies to:

$$g(l) = \frac{(K-1)l_{min}^{K-1}}{1-(\frac{l_{min}}{l_{max}})^{K-1}}l^{-K}. \tag{2.26}$$

The calibration of the baseline model exploits this equation to estimate α, γ and z_{max}.

2.E The self-employed

The main text considers two scenarios: the general equilibrium, which assumes a fixed labour force allowing wages to adjust, and a fixed-wage scenario, which assumes a perfectly elastic labour force so that wages remain constant at their pre-reform level.

These two scenarios represent polar cases. This appendix discusses an intermediate scenario, which assumes an imperfectly elastic labour force. Specifically, this scenario assumes that, following the reform, new managers come from outside the labour force (as in the fixed-wage scenario), leading to an expansion of the labour force. Meanwhile, employees are considered part of the labour force in both periods (as in the general equilibrium scenario), leading to a wage increase due to competition among firms for a fixed number of employees. Our preferred interpretation of this scenario is that new managers were already self-employed in the pre-reform period and become employers following the reform, while the wage increase is too modest to convince inactive individuals to join the labour market.

Table 2.E.1 compares the macroeconomic implications of this intermediate scenario (column 2) to the fixed-wage (column 1) and general equilibrium scenario (column 3). The intermediate scenario predicts a 0.34% wage increase and a 0.24% expansion of the labour force. This results in a similar increase in the number of firms with one employee (+10.4%) and budgetary cost (0.36% of output) as in the two other scenarios. However, in contrast to the general equilibrium scenario, the labour force expansion results in a small increase in output produced by employers (+0.08%).

Table 2.E.1. Implications for the number of firms, the macro economy, and the government budget

	Fixed wages	Intermediate scenario	General equilibrium
Defining characteristics (%Δ)			
Minimum productivity level	−4.04%	−4.00%	−3.91%
Wage rate	0.00%	+0.34%	+0.52%
Labour force	+0.75%	+0.24%	0.00%
Change number of firms (%Δ)			
Number of firms with one employee	+10.54%	+10.37%	+10.18%
Number of firms	+3.13%	+2.89%	+2.77%
Evolution of macro indicators (%Δ)			
Number of employees	+0.53%	+0.00%	−0.26%
Average firm size	−2.53%	−2.81%	−2.95%
Output	+0.25%	+0.08%	−0.01%
Budgetary cost (% output$_b$)	+0.36%	+0.36%	+0.36%

2.F Matching the employment distribution

One might be concerned that the poor approximation of the employment distribution in large firms could bias our results. To address this, we follow Guner et al. (2008) and assume that productivity in firms below a certain size—specifically, those with fewer than 100 employees—follows a Pareto distribution. Meanwhile, a small proportion of firms——approximately 2%—is assumed to have a high productivity. This approach allows us to match the number of small firms relatively well, while also replicating the employment distribution in larger firms more precisely. Moreover, it enables us to test the sensitivity of our results to the low value of α in the baseline model, as we can use a different value of α for the small proportion of firms with the high productivity level. Indeed, descriptive evidence reported in Appendix 2.A demonstrates that small firms have a lower labour share than larger ones.

We first assume a common output elasticity of labour, α, for firms with productivity following the Pareto distribution and those with the high productivity level. Next, we exogenously set α to 0.8 for the small fraction of highly productive firms, a value commonly used in the literature (e.g., Guner et al. (2008); Bilal et al. (2022); Schaal (2017)). Both specifications yield results consistent with our baseline model, indicating that the baseline results are not driven by the poor approximation of the employment distribution of relatively large firms or by imposing a low α for large firms.

Our NBB data includes the number of employees in firms with up to 15 employees. To derive the firm size and employment distribution for the whole economy, we supplement these statistics with publicly available data from the National Social Security Office (NSSO) for 2015Q3 (NSSO, 2015). The NSSO provides data on the number of firms with fewer than 5 employees, 5–9, 10–19, 20–49, 50–99, 100–199, 200–499, 500–999, and over 1,000 employees, along with the total number of employees in each group. Thus, by merging the NBB and NSSO data, we can compute the moments we aim to match (Table 2.F.1). To calibrate the model, we also assume that each highly productive firm employs exactly 524 employees, which is the average number of employees in firms with more than 100 employees. Therefore, the employment share in firms with more than 100 employees is only implicitly targeted in our estimation.

Table 2.F.1. Targeted moments

N of firms by size over the total N of firms	
1	0.295
2	0.160
3	0.101
4	0.069
5	0.050
6	0.038
7	0.029
8	0.024
9	0.019
[10, 19] employees	0.104
[20, 49] employees	0.070
[50, 99] employees	0.021
more than 100	0.020

N of employees by size over the total N of employees	
[10, 19] employees	0.078
[20, 49] employees	0.120
[50, 99] employees	0.080

Formally, we assume that z follows a bounded Pareto distribution with support $[0, z_{max}]$, but differently from our baseline model, we set z_{max} such that $l_b^*(z_{max}) = 99$. Moreover, we assume that there is a mass of firms, called Ω that takes productivity z_Ω, where z_Ω is such that $l_b^*(z_\Omega) = 524$. We search for the value of α, γ and Ω that minimize the distance between the moments presented in table 2.F.1, identified with the letter i, and the corresponding ones computed in the model:

$$\min_{\alpha,\gamma,\Omega} \Big[\sum_{i=1}^{16} \frac{(observed_i - simulated_i)^2}{simulated_i} \Big] \tag{2.27}$$

The model matches the distribution of small firms well. This is illustrated in Figure 2.F.1, where we plot the simulated distributions against the observed firm size distribution in the pre-reform period. However, the advantage of Guner et al. (2008)'s approach lies in its ability to match the employment distribution in relatively larger firms, as shown in Figure 2.F.2.

The estimated value of α remains largely unchanged compared to the baseline model, at 0.30 (vs 0.32). However, γ decreases to 1.34 (from 1.63), leading to a distribution of firms that is less left-skewed than in our baseline estimation. Note that setting α to 0.8 for firms with more than 100 employees reduces the productivity level needed for large firms to employ 524 workers, but does not affect the estimated values of the estimated parameters, nor the simulated firm size and employment distribution.

Table 2.F.2 compares the policy's macroeconomic implications in general equilibrium for the baseline model (column 1) vs. the alternative models (columns 2 and 3). The main message of the baseline model is retained. Indeed, all statistics change by a similar magnitude in the three simulations. Interestingly, the simulated impact on the number of firms in the alternative models (+7.5%) is closer to the DiD estimate (+7.2%) than in the baseline model. The wage increase is lower than in the baseline model, presumably because more individuals work in large firms so that a small wage increase leads to sufficient job losses in these larger firms to offset increased labour demand in new firms.

Imposing $\alpha = 0.8$ for large firms (column 3) only marginal changes the results, indicating that imposing a low value for α for large firms in our baseline estimation is unlikely to drive the results in the baseline model.

Figure 2.F.1. Simulated versus observed firm size distribution in the pre-reform period

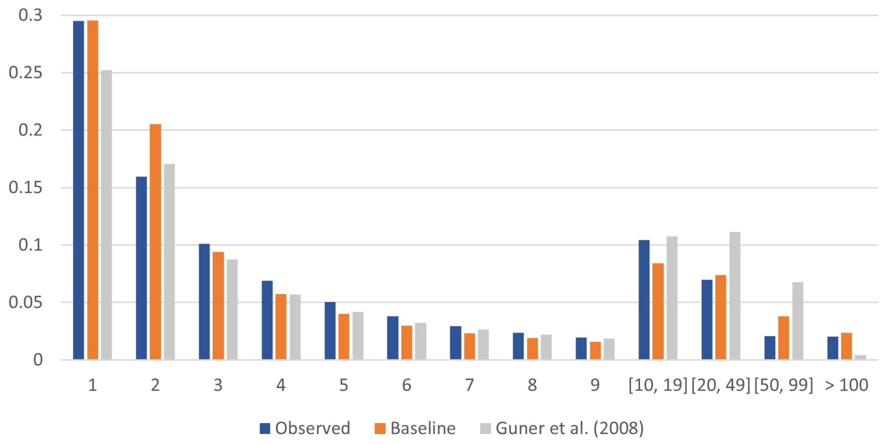


Figure 2.F.2. Simulated versus observed employment distribution in the pre-reform period

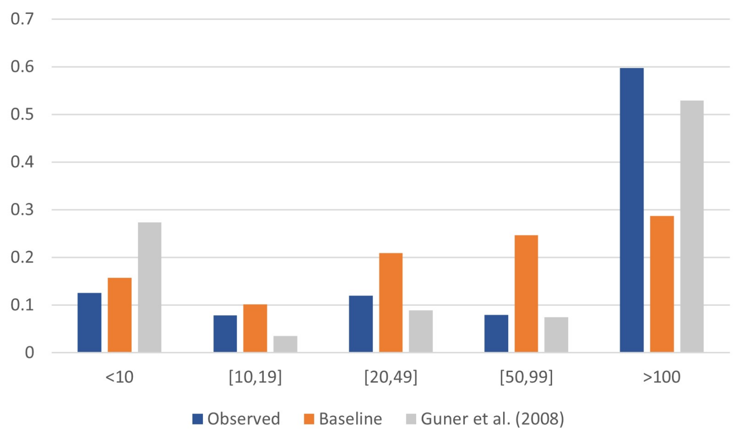


Table 2.F.2. Impact on the number of firms, the macro economy, and the distributional impact - Guner et al. (2008)'s approach vs baseline

	General equilibrium	Guner et al.	Guner et al. & $\alpha = 0.8$
Defining characteristics (%Δ)			
Minimum productivity level	-3.91%	-3.72%	-3.88%
Wage rate	$+0.52\%$	$+0.30\%$	$+0.21\%$
Labour force	0.00%	$+0.00\%$	$+0.00\%$
Change number of firms (%Δ)			
Number of firms with one employee	$+10.18\%$	$+7.54\%$	$+7.57\%$
Number of firms	$+2.77\%$	$+1.79\%$	$+1.83\%$
Evolution of macro indicators (%Δ)			
Number of employees	-0.26%	-0.13%	-0.13%
Output	-0.01%	-0.01%	-0.01%
Budgettary cost (% output$_b$)	-0.36%	-0.26%	-0.30%

3

Permanent exemption from payroll taxes: The role of hiring frictions

*This chapter is co-authored with Sam Desiere, Rigas Oikonomou, Bruno Van der Linden, and Gert Bijnens.**

Since 2016, Belgium has permanently exempted new employers from paying payroll taxes on the gross wage of one of their employees. This paper explains why the reform has resulted in a substantial increase in firms with only one employee but has had a much more limited impact on the stock of larger firms. By building a directed search model, we find that the exemption helps firms that were not hiring before the reform to become employers. However, unless their productivity increases over time – which occurs only for a minority of these firms – hiring more than one employee is not profitable for them. Similarly, few agents who would have hired even without the exemption respond to the policy, as the benefit of attracting more jobseekers, thanks to the exemption, is relatively minor compared to the additional wage costs. We also find that the wage increase in general equilibrium caused by the policy is limited and has had only a marginal impact on firms' employment decisions.

*The views expressed in this chapter are those of the authors and do not necessarily reflect the views of the National Bank of Belgium.

3.1 Introduction

In many advanced economies, the persistent problem of high unemployment rates, coupled with anemic economic growth, has led policymakers to prioritise policies that encourage job creation (Criscuolo et al., 2014). While most industrial policies offering subsidies target large, established firms (Acemoglu et al., 2018), there is also increasing awareness among policymakers that young firms play a crucial role in job creation. As a result, government aid/subsidy programs should also be designed for such firms (Bijnens and Konings, 2020; Decker et al., 2014; Criscuolo et al., 2014; Haltiwanger et al., 2013; Wong et al., 2005). Indeed, while many new (young) firms face a substantial risk of exiting the market, a significant portion of them experience rapid growth, contributing substantially to the job creation rate in the economy (Decker et al., 2014). Thus, it may be more effective to direct subsidies toward new employers.

To assess the effectiveness of government policies designed to stimulate job creation, we need as much evidence as possible from instances where governments intervened in the labour market through subsidy or tax policies. In this paper, we contribute to the existing body of research by examining the effects of a reform introduced in Belgium in 2016, where the Belgian government permanently exempted new employers from paying payroll taxes on the gross salary of one employee. The objective of this reform was to increase the number of new employers and, in turn, promote the creation of new jobs.

Empirical evidence using Belgian data indicates that the policy primarily resulted in an increase in the number of firms with a single employee (Cockx and Desiere, 2024; Deng et al., 2024). The aim of this paper is to disentangle the increase in the number of firms with one employee by analysing the reactions of two groups of firms. The first group comprises firms that would have hired at least one employee even in the absence of the exemption, with the lower payroll taxes allowing them to offer higher wages and attract more applicants—we refer to these new employers as the *inframarginal* employers. The second group consists of firms that would not have hired without the reform, for whom lower taxes tilt the optimal decision towards hiring one employee—we refer to these new employers as the *marginal* employers.

Both channels can explain the short-term effects we observe, but their implications for long-term productivity differ. If most hiring is done by inframarginal firms (those that ultimately grow to have multiple workers), then the policy promotes faster growth of productive companies, thereby increasing their share of overall employment and boosting average productivity within the economy. In contrast, if growth stems primarily from marginal firms, then productivity gains will be modest, and a larger fraction of aggregate employment will be concentrated in firms with one worker.

To investigate these margins, we rely on a structurally estimated model of the Belgian economy. Our model is a directed search model, broadly similar to that of Kaas and Kircher (2015), in which firms, differing in productivity and workforce size, can hire workers in a frictional labour market by posting multiple vacancies. Heterogeneous firms post vacancies in different segments of the market, offering applicants a wage and retention probability tuple. Workers are assumed to be homogeneous. As in the baseline directed search model, applying for a job offering a higher wage is compensated by a lower job-finding probability and a longer duration of unemployment. Drawing on models of occupational choice and entrepreneurship (e.g., Lucas (1978); Cagetti and De Nardi (2006)), we endogenise entrepreneurship within the directed search model, assuming that firms arise not from a pool of potential entrepreneurs but from unemployed individuals who choose to start their own businesses rather than search for a job. We introduce this assumption to ensure that, if the reform leads to an increase in the number of employers, these employers will come from agents who are already in the labour force. We believe these agents are more likely to start a business and hire employees, or, if they already own a business, to make their first hire thanks to the exemption than individuals outside the labour force.

Solving this directed search model, while allowing multi-worker firms to post vacancies sequentially, is not an easy task. Since firms that grow over time and experience productivity shocks are likely to create vacancies in different segments of the labour market, the wages offered to their workers may vary depending on when the vacancies were filled. In theory, one would need to track the entire distribution of promised values offered to workers as a state variable in the firm's program in order to solve for optimal hiring and firing policies. This makes solving the firm's value function essentially intractable. We circumvent this problem by demonstrating that the optimal policies under commitment in our model can be found under a front-loaded wage contract, where wage payments are concentrated at the beginning of the employment spell. Since workers are assumed to be risk-neutral, they are indifferent between a front-loaded wage path, a flat wage path, and (generally) any timing of payments. Moreover, we explicitly show that the timing of wages does not distort the firm's optimal hiring or layoff decisions. Thus, we prove that our value functions can be made tractable with only two state variables: firm employment and productivity level. This makes our model a powerful tool for analysing the impact of policies on multi-worker firms.

We estimate our model to match statistics from a panel dataset of small firms in Belgium, provided by the National Bank of Belgium (NBB). When we simulate the reform, we find that most of the response comes from marginal employers. Nevertheless, these low-productivity new firms are unlikely to grow beyond a single employee. This is because, unless their productivity increases over time—which happens for only a minority of these firms—hiring more than one employee is not profitable for them. Moreover, few inframarginal employers respond to the exemption, despite constituting the majority of firms receiving it. This is because, while the expected wage cost needed to attract one more jobseeker is proportional to the number of jobseekers, the resulting increase in the probability of filling a vacancy is relatively small and decreases as the number of jobseekers rises.

The exemption increases the overall profitability of running a business. Given that unemployed individuals face an arbitrage between creating a firm and searching for a job, the present value that firms need to offer to job seekers increases. However, we find that these general equilibrium effects are relatively small. In a counterfactual simulation, where the value of unemployment is held constant at its pre-reform steady-state level, we obtain similar results. We also observe that the new steady state is reached relatively quickly. Indeed, we compute the increase in the stock of firms with one employee between the pre and post-reform steady states. We can then observe that our economy reaches half of this increase after 11 quarters, and three-quarters of the increase after 24 quarters.

Finally, we test a scenario in which the matching efficiency is 50% higher than the value estimated for our economy. In this case, firms experience a greater increase in their vacancy-filling rate when attracting additional jobseekers thanks to the exemption, potentially leading to faster growth of the inframarginal firms. However, we find that if the labour force post-reform is fixed at the pre-reform level (with the labour force consisting of employees, jobseekers, and business owners), the number of employers increases only slightly more than in our baseline scenario. Indeed, if inframarginal firms hire more employees due to the exemption compared to our baseline, these additional hires must be offset by a reduction in hiring by larger firms in order for the labour force to remain the same.

Our paper complements two evaluations of this policy, which used Belgian data to empirically assess its impact. Cockx and Desiere (2024) find a 31% increase in the number of new employers created immediately after the reform's implementation. Comparing a cohort of new employers created just before and after the reform's implementation, Deng et al. (2024) confirm the increase in the number of new employers post-reform and find that employers created after the reform announcement are less productive on average than those created before. This suggests that some firms that were not profitable to hire before the reform, began hiring thanks to the exemption.[1] Our paper complements this research by presenting a structural model that offers insights into the compositional and general equilibrium effects and allows us to disentangle the impact of labour market frictions, which are not directly observable in the data.

Our model contributes to the literature on size-dependent policies, which target firms above (or below) a certain threshold. There is an overall consensus that these policies result in the misallocation of labour, thereby reducing output and productivity. However, while some studies find significant negative effects (Guner et al., 2008; Braguinsky et al., 2011; Gourio and Roys, 2014; Garicano et al., 2016)[2], others find that the spillover effects on firms not affected by a size-dependent reform may mostly compensate for the negative effects from misallocation (Cahuc et al., 2023)[3]

[1] Deng et al. (2024) find that the cohort of employers established after the policy implementation employ, on average, fewer employees, and have lower turnover and profits.

[2] Guner et al. (2008) develop a growth model with an endogenous size distribution of production units and find that policies that reduce the average size of establishments by 20% lead to reductions in output and output per establishment up to 8.1% and 25.6% respectively, as well as large increases in the number of establishments (23.5%). Braguinsky et al. (2011) shows that the high levels of employment protection in Portugal operate as a tax on wages, and can produce a shift in the firm size distribution, relative to the distortion-free benchmark. Their results suggest that Portugal could achieve first-order productivity gains by moving to a less distorted labour market. In France, firms with 50 employees or more face substantially more regulation than firms that have less than 50. Gourio and Roys (2014) show that removing the regulation improves labour allocation across firms, leading in steady state to an increase in output per worker slightly less than 0.3%, holding the number of firms fixed. Looking at the same setup, Garicano et al. (2016) find that the welfare costs of the regulations are 3.4 (1.3) percent of GDP under partial real wage inflexibility (real wages perfectly flexible downward).

[3] The paper exploits a labour law reform implemented in Portugal in 2009 which restricted

or that crowding-out effects reduce but do not completely cancel out the positive effects of targeting small firms (Rotemberg, 2019)[4]. Our policy, by design, directly influences the decision to become an employer, rather than the choice of hiring more or fewer employees: The constraints preventing a firm from becoming an employer likely differ from those hindering the hiring of more employees. Moreover, we not only differentiate between the impact on eligible firms—i.e. firms with zero employees at the time of the reform implementation or established thereafter—and ineligible firms—i.e. employers at the time of the reform implementation—but also, among eligible firms, distinguish between inframarginal and marginal new employers. Understanding how the former large proportion of firms that would have hired even without the reform respond to a generous payroll tax reduction is essential for evaluating the policy's effectiveness.

This paper also contributes to the literature about the employment effects of payroll tax reductions while taking into account spillover effects and labour market frictions by equilibrium job search models (Shephard, 2017; Bíró et al., 2022; Breda et al., 2024; Cahuc et al., 2019, 2023).[5] These papers find that, although reallocation from non-eligible to eligible firms and spillover effects are often modest, they still need to be assessed to avoid biased estimates for the policy interventions. Our paper differs from these studies by evaluating

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the use of fixed-term contracts for large firms. Accounting for spillovers on small firms yields an almost negligible employment impact of the reform.

[4]The paper studies the loosening of eligibility requirements for various subsidies for small firms in India and shows that these subsidies predict large gains in firm output. However, crowding-out effects absorbed approximately two-thirds of the direct effects. Properly estimating crowding-out is therefore crucial for understanding the aggregate effects of firm-level shocks.

[5]Shephard (2017) examines a series of reforms in the UK, implemented between 1997 and 2002, to increase in-work support for families with children, finding relatively modest equilibrium effects. They find reallocation and spillover effects to be moderate. Bíró et al. (2022) look at the general equilibrium effects of an employers' payroll tax cut for both younger and older workers in Hungary, finding an increase in wages at high-productivity firms. Breda et al. (2024) quantify the unintended effects of a substantial reduction payroll taxes for low-wage jobs in France. They find that while the reform leads to an overall improvement in economic efficiency, it also causes minor negative spillover and reallocation effects due to congestion. Cahuc et al. (2019) analyze a temporary hiring credit tied to monthly wages for firms with less than 10 employees in France: due to the short-term nature of the policy, they exclude the existence of general equilibrium effects. Cahuc et al. (2023) analyses a policy that restricted the use of fixed-term contracts for companies with more than 750 employees in Portugal, showing significant spillover effects on smaller firms.

a payroll tax reduction that has three distinctive features: it is permanent, it can be received by firms regardless of the employees' characteristics, and its eligibility depends on the firm's characteristics—specifically, firms must be new employers.

This paper complements the companion analysis presented in Chapter 2, which, by examining the same policy analysed in this paper and constructing a simple, frictionless general equilibrium model of occupational choices, demonstrates that the change in the stock of firms observed in the data is consistent with firms of low productivity that were not hiring prior to the reform, responding to the policy. We explain why inframarginal firms adjust their hiring decisions only to a limited extent, and thus, why the response of marginal firms—the focus of Chapter 2—approximates the policy effects observed in the data. Additionally, this directed search model allows us to simulate the dynamics after the policy implementation and determine how quickly the economy reaches its new steady state.

This paper is organized as follows: Section 2 discusses the policy under consideration. Section 3 presents the model used in our analysis. Section 4 describes the data we use. In Section 5, we detail the estimation procedure and the model's fit. Moving forward, Section 6 presents the firms' optimal policy pre-reform. Section 7 is dedicated to simulating and discussing the policy's effects. Section 8 concludes.

3.2 The policy and some descriptive evidence

In October 2015, Belgium implemented an unforeseen and permanent exemption from payroll taxes for private sector firms that recruited their first employee after January 1st, 2016. Starting from that date onward, any firm, including new firms, that hires workers and has no employees in the preceding four quarters is permanently exempt from paying the employers' payroll taxes linked to the gross salary of one employee. The exemption is not granted to existing employers or to new firms created by existing employers[6]. The National Social Security Office (NSSO) is responsible for enforcing the correct application of the policy (Court of Audit, 2021).

[6]In Belgium, an individual running a business is classified as self-employed. As such, they are subject to a different payroll taxes regime from employees and cannot claim the exemption.

This exemption stands out for several reasons. Firms keep the exemption even if they continue hiring, and firms with multiple employees can decide to whom to assign the exemption at the beginning of each quarter. Moreover, if the subsidized worker leaves, the firm can reassign the exemption to another worker. This means that once the firm claims the exemption, it is permanent as long as the firm keeps at least one employee. Lastly, until 2022, the exemption remained uncapped, enabling employers to maximize payroll tax savings by assigning it to the employee with the highest gross remuneration.

The tax savings resulting from the exemption can be measured by the difference in the expected rate of payroll taxes that a new employer would have paid on the gross wages of the first employee pre and post-reform. Post-reform, new employers consistently paid a 2.9% rate of payroll taxes on the wage of one employee. In the pre-reform period, calculating the expected rate of payroll taxes on the first employee is more complex. Indeed, the 2016 reform replaced less generous temporary hiring subsidies for the first employee, which had been in effect, with modifications since 2004.[7] Therefore, in the pre-reform period, the rate of payroll taxes was lower at the start of the employment spell than later on. In Chapter 2, we compute the expected payroll tax rate when hiring the first employee pre-reform, by calculating the weighted average of the payroll taxes paid each quarter after hiring—more details in Appendix 3.C. This resulted in a 18.3% payroll taxe rate pre-reform, meaning that the exemption reduced the rate of payroll taxes by about 15pp. Given the scale of this change, it is unsurprising that around 72% of new employers in 2016 requested this exemption within two quarters of hiring their first employee (López-Novella, 2021).

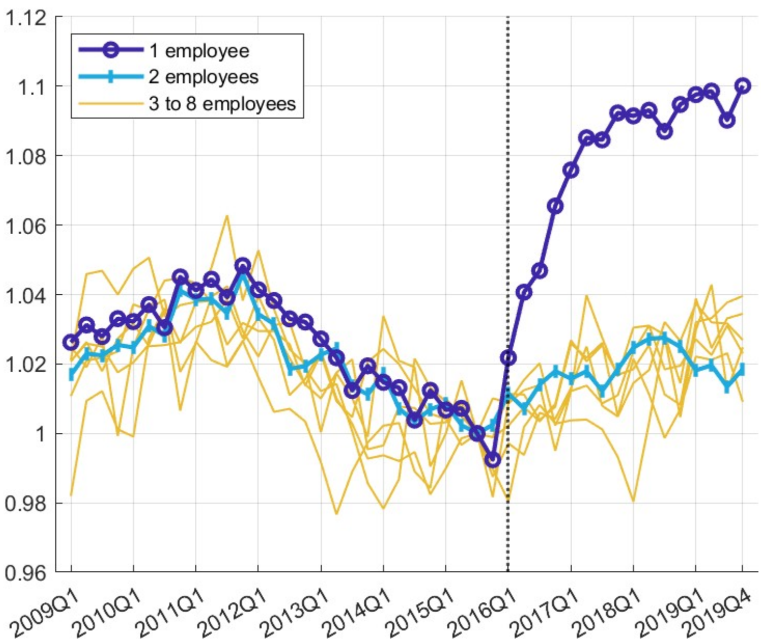
We can assess the first-order impact of the policy on firms' behaviour by plotting the evolution of the stock of firms pre and post-reform. Figure 3.1 illustrates the changes in the number of firms employing 1, 2, and between 3 and 8 employees over time.[8] Firm size is defined as the number of employees in

[7] In 2015, new employers hiring their first employee could claim a €1,550 quarterly payroll tax reduction for the first five quarters, €1,050 for the next four quarters, and €450 for the last four quarters.

[8] We plot only firms with 8 employees or fewer because firms with 3 to 8 employees follow the same trend pre-reform as firms with 1 or 2 employees, while this is not the case for firms with 9 to 15 employees. Similarly, the number of firms with 0 employees has been increasing throughout the entire period of our observation—see Figure 3.A.1. This paper does not study or attempt to explain this trend in firms with 0 employees. In the model estimation, we assume that the number of such firms in the pre-reform period is equal to their average

the firm on the last day of the quarter, regardless of whether they are part-time or full-time. Each quarter, we calculate the ratio of firms employing 1 employee (and likewise for those between 2 and 8 employees) to the number of firms with 1 employee in 2015Q3—we use 2015Q3 as the pre-reform quarter because, as demonstrated by Cockx and Desiere (2024), when the reform was announced in 2015Q4, some firms decided to postpone hirings from 2015Q4 to 2016Q1.

Figure 3.1. Evolution stock of firms



Notes: The figure illustrates the changes in the stock of firms with 1 to 8 employees. For each firms' group, we normalize the number of firms to 1 in 2015Q3. The vertical dotted line marks the beginning of the policy in 2016.

Figure 3.1 illustrates that, prior to the reform, the number of firms with one employee and the number of larger firms evolved in a similar manner. However, the 2016 reform had an immediate effect on the stock of firms with one employee, which increased by 8 to 10% compared to the level in 2015Q3. In contrast, the stock of larger firms did not experience the same magnitude of change but followed a smaller, upward trend.

number over the two years preceding the reform.

The exemption was part of broader reforms aimed at increasing hiring by reducing employers' payroll taxes. Temporary subsidies for hiring the 2nd to the 5th employee have also been in place since 2004. In 2016, the government further increased these subsidies and introduced an additional temporary subsidy for hiring the 6th employee. However, these increases were modest compared to the introduction of the permanent exemption, which is why it is the only change in the rate of payroll taxes simulated in the paper.[9]

In addition, starting in 2016, the government began gradually reducing the nominal rate of employers' payroll taxes for all private sector employers, from 32.4% to around 25% by 2018. At the same time, the government adjusted the reductions in employers' payroll taxes available to firms that employed both low-wage and high-wage employees.[10] From 2018 onwards, these reforms affected the rate of payroll taxes paid for employees beyond the first one. However, we leave the analysis of these additional reductions in payroll taxes for further research.

3.3 The model

This paper is inspired by the Kaas and Kircher (2015)'s directed search modell; however, it departs from it in three ways. First, we introduce employers' payroll taxation, assuming that, pre-reform, employers pay a tax rate of $\tau > 0$ on the wages of all their workers. After the policy is implemented, new employers pay a tax rate of $0 \leq \tau_1 < \tau$ on the gross earnings of a single employee, while continuing to pay τ on the wages of the other employees. Secondly, we assume that firms arise from unemployed individuals who choose to start their own businesses rather than search for a job. Thirdly, since this paper focuses on small and medium-sized firms, the number of workers, L, vacancies posted by firms, V, and jobseekers per vacancy, λ, are treated as natural numbers. Hence, the probabilities of various events are computed taking this discrete feature into account.

[9]For example, firms hiring their second employee could claim a payroll tax reduction over thirteen quarters, amounting to a maximum of €8,850 before the 2016 reform and €13,750 afterwards. The subsidies for subsequent hires are less generous than for the second hire.

[10]Specifically, on 1 April 2016, the government increased the maximum wage limit for firms to benefit from reduced payroll taxes when hiring low-wage employees. In 2018, this threshold was further raised, and the reduction for employing high-wage employees was removed.

There is a finite, constant mass of individuals who are infinitely lived, risk-neutral, and discount future income with a factor of $\beta < 1$. Time is discrete, and capital markets are perfect. At the beginning of each period, each agent can be employed, searching for a job, or running a firm. Firms produce a homogeneous good using only homogeneous labour, and the good market is perfectly competitive.

All employed agents provide one unit of labour per period and cannot search for a job in other firms while employed. Each period, each firm generates revenues $R(L, x)$, where $x \in X = [x_1, x_2, ..., x_n]$ is the firm's idiosyncratic productivity at the start of the period. The function R increases at a decreasing rate in L, $L \in \mathbb{N}_0$. Moreover, we assume that $R(L = 0, x) > 0$. Solo-entrepreneurs are business owners who run their firms without any employees. Each period, the firm incurs certain non-pay expenditures, $c(L)$, which represent the costs associated with managing the employee-employer relationship. These costs increase weakly at a decreasing rate as the number of employees rises, with $c(L = 0) = 0$.

At the beginning of the period, a firm's owner must choose between exiting the market, terminating S employees, $0 \leq S \leq L$, or posting vacancies. These decisions will not impact the current period's workforce and production but will determine whether the firm is active and/or their number of employees at the start of the next period. If the business owner closes the firm, all employees lose their jobs at the end of the period. The (former) business owner must then decide whether to start a new business or look for a job at the beginning of the next period. If the business owner chooses to fire employees, we assume that each of the L employees has an equal probability S/L of being fired. This assumption reduces the numerical complexity of our problem. The business owner does not pay firing costs for any of the employees fired. Finally, if the business owner decides to hire, they compete for jobseekers by posting vacancies. Following Kaas and Kircher (2015), each vacancy represents a long-term contract. Each long-term contract defines for each future period a state-contingent wage and a state-contingent retention probability. Firms are perfectly committed to the contract they offer, meaning they cannot change the proposed state-dependent wages and retention probabilities after hiring. In the pre-reform period, firms pay the rate of payroll taxes $\tau > 0$ on the wages offered. As a result, τ increases the cost of offering a given wage to employees. We assume that neither firms, employees, nor jobseekers benefit from the collected payroll taxes.

The number of jobseekers, λ, $\lambda \in \mathbb{N}_0$, that each vacancy attracts determines the probability, $m(\lambda)$, that the vacancy will be filled. The function $m(\lambda)$ increases at a decreasing rate as λ increases, with $m(\lambda = 0) = 0$ and $m(\lambda = +\infty) = 1$. The worker's probability of being hired is determined by $\frac{m(\lambda)}{\lambda}$, which strictly decreases as λ increases. In this framework, the number of jobseekers applying to a vacancy is directly tied to the expected payoff they get if hired. Assuming that jobseekers can view all available vacancies at no cost, a vacancy will attract more applicants the higher the expected payoff the jobseeker would receive if hired. Therefore, firms can increase $m(\lambda)$ by offering higher wages. By offering higher wages, they will also pay a higher amount of payroll taxes. As a vacancy becomes more appealing and attracts more applicants, the likelihood of any jobseeker being hired for that vacancy decreases. This ensures that, at equilibrium, the expected payoff from applying to any vacancy is the same.

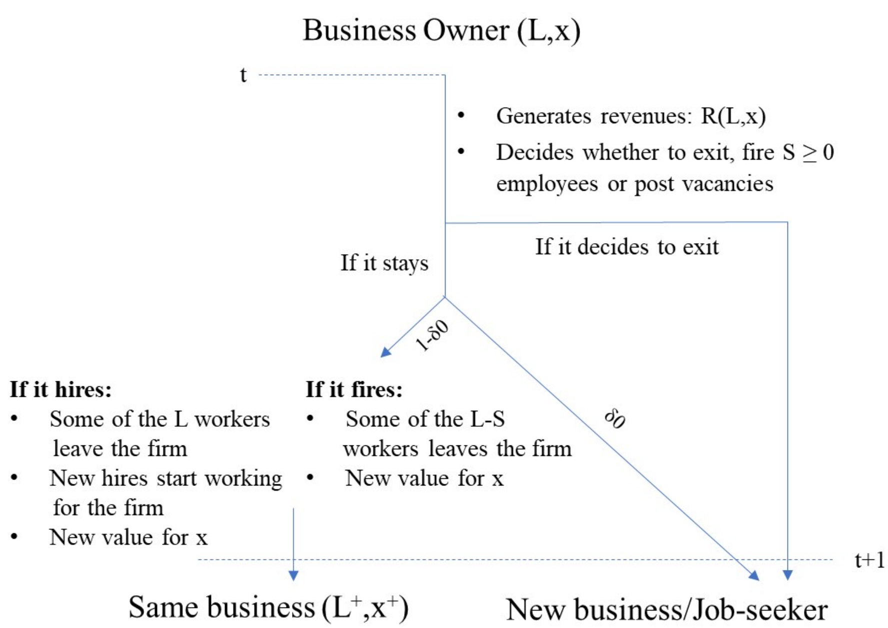
We assume that searching for new hires is costly. Following Kaas and Kircher (2015), the cost of posting vacancies, $C(V, L)$, is strictly convex in the number of vacancies, V, $V \in \mathbb{N}$. This prevents firms from instantaneously growing large by posting numerous vacancies at a fixed cost. Moreover, for any V, $C(V, L)$ decreases with L. This recognises that posting a given number of vacancies is less expensive for firms with many employees than for smaller firms.

Since the probability of filling a vacancy increases at a decreasing rate as the number of jobseekers rises and the cost of posting vacancies is convex in the number of vacancies, a firm that wants to increase hiring will both post more vacancies and attract more jobseekers per vacancy. This standard result from the directed search literature is supported by empirical work. Davis et al. (2010) shows for the US that firms that expand faster not only post more vacancies, but also fill these vacancies at a higher rate. He et al. (2023), with a field experiment conducted on a Chinese job board, find that higher wage offers raise application rates.

Even if the business owner chooses not to close the firm but instead to fire or hire employees, the firm still faces an exogenous probability $\delta_0 > 0$ of shutting down at the end of each period. In this case, all employees lose their jobs. Furthermore, if the firm decides to fire $0 \leq S \leq L$ employees, each of the remaining $L-S \geq 0$ employees may quit their job with an exogenous probability $s_0 \geq 0$ at the end of the period. We assume that the probabilities of the firm exogenously closing and a worker exogenously leaving are independent. For brevity, let ϕ represent the probability that an employee will continue working for the firm at the start of the next period. Specifically, $\phi = 0$ if the firm decides to shut down; $\phi = (1-\delta_0)(1-s_0)(1-S/L)$ if the firm chooses to fire $0 \leq S \leq L$ employees; and $\phi = (1-\delta_0)(1-s_0)$ if the firm decides to hire.

At the beginning of each period, individuals who were not in salaried employment or running a business activity during the previous period must either seek employment from available job vacancies, in which case they will receive the out-of-work income, denoted as b, for the current period, and work for a firm in the next period if they get the job. Alternatively, they can decide to start a firm. If an agent decides to start a firm, they will draw an initial idiosyncratic productivity value x from the discrete set X with probability σ_x. Following this decision, they will immediately need to decide whether to exit (and therefore have a firm only in the current period), stay without employees or hire employees. If a firm does not exit the market, its idiosyncratic productivity x follows a Markov process on the finite state space X, characterized by known transition probabilities $\pi(x^+|x)$, where superscript $+$ designates the next period's value. Figure 3.2 represents the timing of the event for the business owners.

Figure 3.2. Timing of events for business owners between time t and t+1



3.3.1 Simplifying property

Proposition 1. *If the tax rate τ on the stream of wages is independent of the firm's size, individuals are risk neutral and the capital market is perfect, then business owners would make the same hiring/firing/exit decisions, and unemployed agents would make the same employment decisions whether firms post i) long-term contracts that specify a state-contingent wage and retention probability for each future period, or ii) contracts that only provide a hiring bonus, $\bar{w}$, when the jobseeker is hired and a continuation wage, $\underline{w}$, for each period the employee produces output. The continuation wage is set so that the value of remaining in the current job is equal to the present value of being unemployed.*

Based on Proposition 1's result—see Appendix 3.B for the proof—we assume firms offer contracts specifying a hiring bonus ($\bar{w}$) and a continuation wage ($\underline{w}$) instead of long-term state-contingent contracts. This makes our model numerically tractable because the value employees receive from the moment they provide one unit of labour is the same across all firms. Therefore, when comparing different job vacancies, jobseekers solely compare the offered hiring bonus. Thus, λ depends solely on $\bar{w}$. If a firm cannot pay employees at least the amount they would receive in unemployment, it will either lay them off or exit the market.

We also assume that each firm offers the same hiring bonus to all individuals hired at the same time. This assumption is mild. Indeed, $\mu(\lambda)$ increases at a decreasing rate in λ. Therefore, it will become relatively more costly to increase the filling rate of a vacancy as the number of jobseekers for that vacancy increases. As a result, firms have little[11] incentive to increase the filling rate of one vacancy while leaving the filling rate of other vacancies unchanged.

3.3.2 Workers' problem in the pre-reform period

Let $\mathcal{I} = \{(L, x) | L \in \mathbb{N}_0, x \in X\}$. We define with $i \in \mathcal{I}$ all firms that share the same pair (L, x). Furthermore, let $\bar{\mathcal{I}}$ the subset of $\mathcal{I}$ where firms post at least one vacancy.

[11] Since we assume that the number of jobseekers and vacancies are natural numbers, there could be a situation in which a firm would like to have a vacancy with one more jobseeker than another vacancy, hence the need for this assumption.

At the beginning of each period, agents in unemployment (i.e. not in salaried employment and not running a business from the previous period) can either apply for a vacancy or start their own business. Let's define with S_i the expected value an agent obtains by applying to a vacancy posted by firm i. The present value obtained by an agent deciding to open a business and receiving the initial idiosyncratic productivity draw x is denoted $J(L=0,x)$.

Agents can observe all vacancies at no cost. When applying for a vacancy, they select the one offering the highest expected value, denoted as $\max_{i\in\bar{\mathcal{I}}}[S_i]$. As a result, in equilibrium, all vacancies will offer the same expected value. Similarly, agents will only apply for a vacancy if the expected value of doing so is higher than the expected value of starting a business net of the sunk cost s_b to start a business (and vice versa). This sunk cost serves as a proxy for the investment in time and/or resources necessary to open a business, such as completing administrative procedures, learning how to file taxes, etc. In equilibrium, the expected value of searching for a job and the net expected value of opening a business take the same value, denoted as U. Therefore, the present value in unemployment is given by:

$$U = \max[\sum_{x\in X} \sigma_x J(L=0,x) - s_b, \max_{i\in\bar{\mathcal{I}}}[S_i]]. \tag{3.1}$$

The expected lifetime value that an agent receives when employed by firm i, denoted as W_i, is the sum of three components. Firstly, it consists of the continuation wage $\underline{w_i}$ the agent receives in the current period. Secondly, it consists of the expected value the agent receives if they remain employed in the next period, denoted as $E_{x^+}[W_i^+]$—in the current period, the agent only knows the transition matrices $\phi(x^+|x)$. $E_{x^+}[W_i^+]$ is weighted by the known probability that the agent might retain their job, ϕ_i. If an agent loses their job, they will become unemployed in the following period and will receive the value in unemployment U^+. Therefore, if employed in firm i, the agent gets:

$$W_i = \underline{w_i} + \beta\phi_i E_{x^+}[W_i^+] + \beta(1-\phi_i)U^+. \tag{3.2}$$

S_i is also the weighted sum of three components: the out-of-work income b the individual obtains during the job search, the expected value the individual obtains if they remained unemployed at the end of the period, U^+; and the expected value if they were hired. This latter is determined by two factors: the hiring bonus, $\bar{w}_i$, and the expected value while employed, $E_{x^+}[W_i^+]$. Therefore, the value of applying to a vacancy posted by firm i is given by:

$$S_i = b + \beta(1-\delta_0)\frac{m(\lambda)}{\lambda(\bar{w}_i)}[E_{x^+}[W_i^+] + \bar{w}_i] + \beta(1-(1-\delta_0)\frac{m(\lambda)}{\lambda(\bar{w}_i)})U^+. \quad (3.3)$$

Given that, in equilibrium, $S_i = U$, $\forall i \in \bar{\mathcal{I}}$, and $\underline{w_i}$ is such that $W_i = U$, we can rewrite equation (3.3) and equation (3.2) as follows:

$$U = b + \beta(1-\delta_0)\frac{m(\lambda)}{\lambda(\bar{w}_i)}[U^+ + \bar{w}_i] + \beta(1-(1-\delta_0)\frac{m(\lambda)}{\lambda(\bar{w}_i)})U^+ \quad (3.4)$$

and

$$U = \underline{w_i} + \beta\phi_i U^+ + \beta(1-\phi_i)U^+. \quad (3.5)$$

By rearranging these two equations, we determine the continuation wage,

$$\underline{w_i} = U - \beta U^+, \quad (3.6)$$

and the hiring bonus posted by firms,

$$\bar{w}_i = \frac{U - \beta U^+ - b}{\beta(1-\delta_0)\frac{m(\lambda)}{\lambda(\bar{w}_i)}}. \quad (3.7)$$

To find the steady state value for $\underline{w_i}$ and $\bar{w}_i$ we set $U = U^+$. In equilibrium, $\bar{w}_i \in \overline{W} = [\bar{w}(\lambda = 1), \bar{w}(\lambda = 2), ...]$.

3.3.3 Firms' problem in the pre-reform period

Let's consider a firm that, at the beginning of the period, has the pair (L, x), where $L \in \mathbb{N}_0$ and $x \in X$—for brevity, we omit the subscript i. Then, this firm chooses among three exclusive alternatives: exiting the market (e), firing workers (f), or hiring workers (h). The firm's payoff at the beginning of the period is:

$$J(L, x) = \max[e(L, x), f(L, x), h(L, x)]. \quad (3.8)$$

The firm's payoff from exiting the market, $e(L, x)$, is determined by two components. First, the current net revenues, which are the gross revenues minus wages and the non-pay expenditures. Second, the next period's payoff which is the value of entering unemployment if an agent decides to close the firm.

$$e(L,x) = R(L,x) - (1+\tau)\cdot \underline{w} \cdot L - c(L) + \beta \cdot U^{+}, \tag{3.9}$$

where $\underline{w} = U - \beta U^{+}$ comes from the workers' problem. The first component of the firm's payoff when firing is still the current net revenues specified for the exit decision. Let's define with S, $0 \leq S \leq L$, the number of workers a firm wants to fire, and with $Pr(L^{+}, L-S)$ the probability of having $L^{+} \in [0, ..., L-S]$ workers next period, given that each of the remaining $L - S \geq 0$ workers has an exogenous probability s_0 to leave the firm. The next period's payoff from firing is equivalent to the payoff from operating a firm with L^{+} employees after firing S employees. If the business owner decides not to exit, they will still be forced to exit with exogenous probability δ_0. Therefore, the payoff the firm obtains if it decides to fire is[12]:

$$\begin{aligned} f(L,x) = \max_{S\in\{0,1,..,L\}} \Big[& R(L,x) - (1+\tau)\cdot \underline{w} \cdot L - c(L) \\ & + \beta \cdot (1-\delta_0) \cdot E_{x^{+}}[\sum_{L^{+}\in\{0,...,L-S\}} Pr(L^{+}, L-S) \cdot J(L^{+}, x^{+})] + \beta \cdot \delta_0 \cdot U^{+} \Big]. \end{aligned} \tag{3.10}$$

The first component of the firm's payoff when hiring remains the current net revenues, as outlined for the exit and firing decision. However, since the firm post vacancies to hire, it incurs the current extra cost $C(V, L)$. We define H, $H \in [0, \ldots, V]$, as the number of hired employees. The probability of hiring H employees when posting $V \in \mathbb{N}_0$ vacancies and choosing a hiring bonus $\bar{w}$ is given by $Pr(H, m(\bar{w}), V)$, where $m(\bar{w})$ is a short notation for $m(\lambda(\bar{w}))$. Each of the $L \geq 0$ workers has an exogenous probability s_0 of leaving the firm. We define $Pr(L_R, L)$ as the probability that L_R workers, $L_R \leq L$, remain among the initial L workers next period. Therefore, the next period's payoff from hiring is the value of running a firm with L^{+} employees, where $L^{+} = H + L_R$, minus the hiring bonus paid for the newly hired employees. As before, even if the business owner decides to hire, they will still be forced to close the firm with exogenous probability δ_0. Then, the payoff the firm obtains if it decides to hire is:

[12] For all maximisations with respect to natural numbers, we compute the objective function for each possible combination of these natural numbers and then select the combination that maximises the objective function.

$$h(L,x) = \max_{V\in\mathbb{N},\bar{w}\in\overline{W}} \Bigg[R(L,x) - (1+\tau)\cdot \underline{w}\cdot L - c(L) - C(V,L)$$
$$+\beta\cdot(1-\delta_0)\cdot \sum_{H\in\{0,\ldots,V\}} Pr\big(H, m(\bar{w}), V\big)\Big[\sum_{L_R\in\{0,\ldots,L\}} Pr\big(L_R, L\big)\cdot \big[E_{x^+} J(L^+ = L_R + H, x^+)$$
$$-H\cdot(1+\tau)\cdot\bar{w}\big]\Big] + \beta\cdot\delta_0\cdot U^+\Bigg], \tag{3.11}$$

From Equation (3.11), it is also clear that the expected total hiring bonuses a firm needs to pay to attract a given number of jobseekers for each of the V vacancies, $\beta\cdot(1-\delta_0)\cdot\sum_{H\in\{0,\ldots,V\}} Pr\big(H, m(\bar{w}), V\big)\cdot H\cdot(1+\tau)\cdot\bar{w}$, is proportional to the number of jobseekers per vacancy, to the number of vacancies, and it is linear in τ. In other words, if a firm, given V, wants to increase the number of jobseekers per vacancy from 1 to 2, it is expected to pay an amount of hiring bonuses that is twice the initial amount. However, it should also be remembered that the expected number of hires will not double, as the probability of filling a vacancy increases at a decreasing rate with the number of jobseekers. This makes it relatively more expensive to fill a given number of vacancies at a higher rate.

3.3.4 Resource constraint

We assume that the total number of agents—i.e. the sum of the number of jobseekers, employees, and business owners—remains constant over time. In the pre-reform steady state, we standardize the total number of business owners to 1. Then, we find the optimal number of job seekers and employed individuals by solving the workers' and firms' problems. The resulting total number of agents is denoted by K, where $K > 1$.

When we introduce the exemption, we assume that the total number of agents remains equal to K. In other words, we assume that offering an exemption for the first hire does not incentivise individuals who are inactive to start seeking jobs or initiate their own businesses.

3.3.5 Introducing the exemption for the first hire

Pre-reform, all firms pay the rate of payroll taxes $\tau > 0$. Post-reform, firms eligible for the exemption pay the reduced rate $0 \leq \tau_1 < \tau$ on the wage of a single employee. Eligible firms are those with zero employees at the time of the reform or those established post-reform's implementation. Once the economy reaches its post-reform steady state, all firms will have been created post-reform. Therefore, all firms are eligible for the exemption in the post-reform steady state. Firms that are not eligible, namely those with one or more employees at the time of reform's implementation, continue solving the optimisation problem outlined in Subsection 3.3.3. We assume that these firms never become eligible for the exemption, even if they experience periods without employees after the reform. While this assumption simplifies our analysis, it is also motivated by the fact that, by law, a firm with zero employees, which was previously an employer, would need to remain without employees for one year in order to become eligible for the exemption. Similarly, the workers' decisions remain unaffected, as the parameter τ does not directly influence either the value of being employed or the value of searching for a job. Moreover, note that in our model, we assume the government does not need to balance any budget. This means that we allow the amount of SSC collected from firms to decrease due to the exemption, without this being compensated by an increase in the SSC levied on other workers or, more generally, by other adjustments in the government tax/benefit system.

We have assume risk neutrality and perfect capital markets. This means that, for jobseekers, only the present value of wage commitments matters, rather than the distribution of wages and retention probabilities over time. Pre-reform, when firms paid the same payroll tax rate for all employees, these assumptions did not create any incentive for firms to favour one wage profile over another. However, this changes when firms become eligible for the exemption. Without further restrictions on how firms set wages or transfer the exemption between employees, firms could optimise tax savings by manipulating wages and retention probabilities. For example, a firm might assign the exemption to each employee for a limited number of quarters, setting a high wage when the employee is exempt from payroll taxes and a low (or zero) wage when the exemption is transferred to another worker. However, in practice, employees care about how wages are distributed over time, making this tax-saving maximising strategy unfeasible.

To avoid unrealistic fluctuations in the wage paid to each employee under the assumption of risk neutrality and perfect capital markets, we impose three additional assumptions on the wage-setting behaviour in the post-reform period. First, we assume that once an exemption is assigned to an employee, the firm cannot reassign the exemption to another until the employee leaves the firm.

Secondly, if the subsidised employee exogenously leaves the firm, the firm will randomly reassign the exemption to one of the remaining employees at the beginning of the next quarter. Moreover, if a firm endogenously fires employees, we assume it cannot choose whom to dismiss, and, therefore, each employee has an equal probability of being fired. If the subsidized employee is fired, the exemption is randomly reassigned among the remaining employees at the beginning of the next quarter.

Thirdly, we assume that firms must offer the same contract—i.e., the same future wages and retention probability—to individuals hired at the same time. Consider a firm with zero employees posting two vacancies at the start of a given quarter. Without this assumption, before posting the wages, the firm could decide which potential hire would receive the exemption. Knowing that it would not have to pay payroll taxes for that employee, the firm could offer higher wages for that vacancy only in order to fill it more rapidly. With this assumption, the firm can still offer higher wages, but this applies to both vacancies, not just one. An implicit consequence of this assumption is that firms with zero employees hiring more than one employee in a given quarter, will randomly assign the exemption to one of the new hires.

These three assumptions are made to keep the model tractable, as they eliminate the need to track individual wages or to keep track to whom the exemption is assigned. Therefore, without loss of generality, we can continue to assume that firms eligible for the exemption set the hiring bonus and continuation wage as described for the pre-reform period. However, these assumptions reduce the tax savings a firm can achieve through the exemption. In practice, firms may reassign the exemption to the highest-paid employee or decide in advance which employees will receive the exemption during the hiring process. In other words, our modelling choices likely reproduce a lower bound of the financial benefit a firm can obtain from the exemption.

Based on these results, we can rewrite the firms' problem for those eligible for the exemption post-reform. For such firms, the exemption modifies the employers' payroll tax rate applied to the continuation wages. Indeed, all eligible firms will pay $(1+\tau)\cdot\underline{w}\cdot\max[0,L-1]+(1+\tau_1)\cdot\underline{w}\cdot\min[1,L]$ instead of $(1+\tau)\cdot\underline{w}\cdot L$. Therefore, the primary direct effect of the exemption is to lower the total wage cost per employee, allowing less productive firms, previously unable to afford hiring, to do so.

Then, if the firm decides to hire employees, the hiring bonus of one of these hires may be exempt from payroll taxes. Consequently, the policy has a second direct effect: it lowers the cost of increasing the vacancy-filling rate through higher wages. Specifically, assume the firm starts with zero employees and hires $H \geq 0$ employees. One of these hires will be subject to a payroll tax rate of τ_1 on the hiring bonus, while the rest will pay the rate τ. As per our third assumption, the rate τ_1 is assigned randomly among the H employees. Moreover, even if the firm already has $L > 0$ employees at the start, with probability s_0, the individual benefiting from the exemption will leave the firm at the end of the quarter. As a result, the exemption will be randomly reassigned to one of the $L_R + H$ employees the firm has at the beginning of the next quarter. The payoff of an eligible firm that decides to hire is:

$$
\begin{aligned}
h(L,x) = \max_{V \in \mathbb{N}, \bar{w} \in \overline{W}} \Bigg[& R(L,x) - c(L) - C(V,L) \\
& - (1+\tau) \cdot \underline{w} \cdot \max[0, L-1] - (1+\tau_1) \cdot \underline{w} \cdot \min[1, L] \\
& + \beta \cdot (1-\delta_0) \cdot \sum_{H \in \{0,\ldots,V\}} Pr\big(H, m(\bar{w}), V\big) \\
& \Bigg[\sum_{L_R \in \{0,\ldots,L\}} Pr\big(L_R, L\big) \Big[E_{x^+} J(L^+ = L_R + H, x^+) \\
& - 1_{L=0} \cdot H \cdot \bar{w}[1 + \frac{1}{H} \cdot \tau_1 + \frac{(H-1)}{H} \cdot \tau] \\
& - 1_{L>0} \cdot H \cdot \bar{w}\big[s_0[1 + \frac{1}{(L_R+H)} \cdot \tau_1 + \frac{(L_R+H-1)}{(L_R+H)} \cdot \tau] + (1-s_0) \cdot (1+\tau)\big]\Big]\Bigg] \\
& + \beta \cdot \delta_0 \cdot U^+ \Bigg]
\end{aligned}
\tag{3.12}
$$

3.4 Data

We estimate the model parameters such that the model: i) closely reflects the employment patterns of small firms observed in the data; ii) matches the proportion of employees in larger firms in the economy, without precisely replicating the observed distribution of employment. We chose this approach because the policy is more relevant to very small firms than to large ones, and to reduce the model's computational time.

To calibrate the model, we primarily rely on a panel of firms provided by the National Bank of Belgium (NBB). This panel includes all firms that had 15 or fewer employees, including zero, at the end of at least one quarter between 2009Q1 and 2019Q4. The NBB considers a firm to encompass all entities that engage in business activities, namely, sole proprietorships, companies, and non-profit organisations. All these entities may or may not have salaried employees.[13] For each of these firms, the NBB provides data on the number of paid employees with a part-time and a full-time contract at the end of each quarter between 2009Q1 and 2019Q5. For our analysis, we determine the firm's size based on its total number of employees, regardless of the contract type.[14] The NBB data also includes information on the quarterly total firms' revenues, gross wage bill, payroll taxes gross of any reduction, and payroll tax reductions received. We supplement the NBB data with statistics from the National Employment Office (ONEM) and the Statistical Office of the European Union (EUROSTAT).

3.5 Estimation

We start with some functional form assumptions. Then, we describe the parameters that are externally set or standardized. Subsequently, we outline the procedure to internally estimate the remaining parameters and the moments used in the estimation. Finally, we discuss the estimation results and the model's fit.

[13]Self-employed individuals can run their business either as a sole proprietorship or as a company. If self-employed individuals decide to create a company, they will become shareholders and active partners/directors. Normally, the active partner is classified as self-employed, meaning they are not considered salaried workers of the company. The director, although also considered self-employed, may hold dual status as a salaried worker if the salaried activity involves technical functions distinct from those performed in their capacity as director. In such cases, the existence of a relationship of subordination must be proven.

[14]In Belgium, to be considered a part-time worker, it is sufficient for the individual to be required to work less than the normal amount of hours required by a full-time contract. Therefore, it is possible to have part-time jobs that require roughly the same amount of work as full-time jobs.

3.5.1 Functional form assumptions

Each model's period corresponds to a quarter—we chose this frequency because this is the frequency of our NBB data. The matching function is Cobb–Douglas (e.g. Bilal et al. (2022) and Cahuc et al. (2023)). Defining with γ_m the elasticity of the vacancy filling rate with respect to the number of jobseekers, and with μ_m the matching efficiency, the probability of filling a vacancy is $q(\lambda) = \mu_m \cdot (\frac{1}{\lambda})^{-\gamma_m}$, and the probability of a jobseeker being hired is $p(\lambda) = \mu_m \cdot (\frac{1}{\lambda})^{1-\gamma_m}$, where λ is the number of jobseekers per vacancy.

The firm's productivity takes the value x from the set $X = [x_1, x_2, ..., x_n]$, where all values between x_1 and x_n are equally spaced. Let's call Δx the difference between two consecutive values of x. The probability an agent obtains a certain value of $x \in X$ at entry is given by $\sigma_x \in \Sigma = [\sigma_{x_1}, \sigma_{x_2}, ..., \sigma_{x_n}]$. As in Bilal et al. (2022), the values of $\sigma_x \in \Sigma$ are derived from a bounded Pareto distribution with shape parameter σ_P—the pdf of the bounded Pareto distribution for $z \in [x_1, x_n]$ is $\frac{\sigma_P \cdot x_1 \cdot^{\sigma_P} \cdot z^{-\sigma_P - 1}}{1-(\frac{x_n}{x_1})^{\sigma_P}}$. Then, $\sigma_{x_1} = Pr(x \leq x_1 + \frac{1}{2} \cdot \Delta x)$, $\sigma_{x_2} = Pr(x \leq x_2 + \frac{1}{2} \cdot \Delta x) - Pr(x \leq x_2 - \frac{1}{2} \cdot \Delta x)$ and so on, with $\sum_{x=x_1}^{x_n} \sigma_x = 1$. We selected this distribution from those commonly used in the literature – see Dewitte et al. (2022) for a review – because of its simplicity and flexibility.

Once the productivity at entry is assigned, it follows a random walk process where $x^+ = x + \epsilon$, with $\epsilon \sim \mathcal{N}(0, \sigma_{\mathcal{N}})$. Based on this process, we compute $\phi(x^+|x)$. We discretise the random walk process so that x^+ can only take values $x \in X$.

Firms produce output with labour only. We assume that there is perfect competition in the good market, and we normalize the good's price to one, such that the firm's revenue are $R(L, x) = x \cdot (L + 1)^\alpha$, $L \geq 0$ - this specification implies that solo-entrepreneurs generate positive revenues. The per-period non-pay expenditures are set to $c(L) = \mu_o \cdot L^{\gamma_o}$, $L \geq 0$, $\mu_o \geq 0$ and $\gamma_o \geq 0$—i.e. the solo-entrepreneurs do not have these non-pay expenditures, while they are weakly increasing for employers.

The cost of posting vacancies $C(V, L) = \mu_C \frac{V^{(1+\gamma_C)}}{(L+1)^{\gamma_C}}$, where $\mu_C > 0$ and $\gamma_C > 0$. This specification, adopted by Kaas and Kircher (2015), implies that $C(V, L)$ is convex with respect to the number of vacancies and that, for any V, $C(V, L)$ decreases with L.

3.5.2 Parameters that are externally set/standardized or internally estimated

Following the literature, we set the quarterly discount rate $\beta = 1.3\%$—which is equivalent to a yearly discount rate of about 5%. For the pre-reform period, we set the rate of employers' payroll taxes, τ, equal to 18.3% for all workers. For the post-reform period, we set the rate of employers' payroll taxes to 2.9% for one employee for eligible firms—details on how we have computed these rates are in Appendix 3.C. We set the number of levels for the idiosyncratic productivity n equal to 75. The choice of 75 has been made to have a fast code but enough "nodes" for the idiosyncratic productivity to match the firms' employment dynamics. We standardize x_1 to 1. We assume that the quarterly exogenous exit rate is size-dependent and takes the values $\delta_0 \in [0.0230, 0.0120, 0.0113, 0.0109, 0.0106]$ for $L \in [0, 1, 2, 3, 4]$. For $L \geq 5$, $\delta_0 = 0.0103$. These exit rates are imputed from the NBB data—further details in Appendix 3.D. Finally, we set the exogenous separation rate, s_0, to zero. Indeed, in the data for Belgium, we lack the information needed to estimate the separation of workers not caused by negative shocks to the idiosyncratic productivity of firms. In Appendix 3.F, we test the robustness of our results by setting the quarterly exogenous separation rate to $s_0 = 0.02$—we find that the overall match between the simulated and observed moments improves, suggesting that there is a level of exogenous separation in the economy. However, the simulated impact of the reform remains the same. We set the elasticity matching function with respect to the number of jobseekers, γ_m, to 0.5, which is the value commonly estimated by the literature (Petrongolo and Pissarides, 2001), as our data did not allow us to estimate this parameter. Table 3.2 reports the externally set or standardized parameters.

The 12 parameters left to be estimated are the highest value of the idiosyncratic productivity x_{75}; the shape parameter of the bounded Pareto distribution σ_P; the standard deviation of the shocks of the random walk process $\sigma_{\mathcal{N}}$; the production function elasticity α; the scale parameters μ_C and the elasticity parameter γ_C of the cost of posting vacancy function; the scale parameter of the matching function μ_m; the scale parameter of the per-period non-pay expenditures μ_o and the elasticity of the per-period non-pay expenditures wrt the number of employees γ_o; the out-of-work income b; the one-time sunk cost to open a firm s_b.

3.5.3 Empirical and simulated moments

We jointly estimate the parameters of our model to minimize the weighted sum of the squared distance between some empirical and respective simulated moments, where the empirical moments are derived from pre-reform data, and the simulated moments are obtained from the model's pre-reform steady state. If θ is the vector of empirical moments, and $\hat{\theta}$ the model simulated counterparts, we minimize:

$$(\hat{\theta} - \theta)' W^{-1} (\hat{\theta} - \theta) \quad \text{s. to} \quad L \leq 50 \tag{3.13}$$

The matrix W contains the square of θ on the main diagonal, with zeros elsewhere. In other words, for each moment, we compute the square of the percentage change between the observed and simulated values. This weighting matrix is commonly used in cases where it is not feasible to calculate the variance and covariances of certain moments employed in the estimation, while ensuring that the units of measurement do not influence the weight assigned to each moment (Low and Meghir, 2017; Bilal et al., 2022). This situation applies to our analysis, as some of our moments are sourced from ONEM and EUROSTAT. For these moments, the standard deviations are unknown, and it is not possible to calculate the correlations with the moments derived from our NBB panel data. Moreover, to reduce the numerical time required to solve the model, we impose that firms cannot have more than 50 employees.

We now outline the moments targeted in our estimation, with a detailed description of each provided in Appendix 3.E. We also explain how each parameter of our model is informed by the selected moments. Indeed, although each parameter can potentially influence all moments, certain parameters are particularly informed by specific moments.

From the NBB data, we calculate the proportion of firms with 0, 1, 2, 3, 4, and 5 employees relative to the total number of firms with zero or more employees. These moments primarily provide information on the maximum value of idiosyncratic productivity (x_n), the production function elasticity (α), and the scale parameter of the per-period non-pay expenditures (μ_o). As the productivity gap between the most productive, x_n, and least productive, $x_1 = 1$, agents widens, the economy has fewer firms with zero employees but more employers. Similarly, when α increases, it becomes more profitable to hire additional employees. We assume that the per-period non-pay expenditures are zero for firms with zero employees but increases for employers with L. As a result, a rise in μ_o leads to a reduction in the number of employers and an increase in the number of solo-entrepreneurs.

We target the ratio of employees in firms with five or fewer employees to the total number of employees in the economy. As with the first set of moments, α, x_n, and μ_o have a significant influence on this statistic. Additionally, the elasticity of the cost of posting vacancies (γ_C) also plays a role. Specifically, when this elasticity increases, firms expand more slowly, which ultimately leads to fewer employees being hired. This, in turn, results in a higher proportion of employees working in firms with five or fewer employees.

We target the ratio of average revenues between firms with zero employees and firms with one employee. This moment ensures that we have firms that remain without employees because they generate positive revenue, rather than because they expect positive productivity shocks. By increasing the output from an additional hire or the productivity gap between the most and least productive firms, α and x_n affect the revenue difference between solo-entrepreneurs and firms with one employee.

The fourth set of moments represents the proportion of new firms in a given quarter that have 0, 1, 2, or 3 employees, relative to the total number of new firms. New firms are defined as those that did not exist in the previous quarter. This moment primarily provides information about the shape parameter of the bounded Pareto distribution, σ_P. It is not surprising that, if there are more agents with relatively low productivity at entry, there will also be more new firms with zero employees. An increase in the sunk cost of opening a firm, s_b, also impacts this moment. If s_b increases, firms must be more productive at entry to cover this cost. With higher productivity, they will also hire more employees upon entry.

We take firms with zero employees in a given quarter and compute the proportion that either remains with zero employees or grows to one or two employees after four quarters. Similarly, we consider firms with one employee in a given quarter and compute the proportion that either shrinks to zero employees, remains with one employee, or grows to two employees after four quarters. Finally, we take firms with two employees in a given quarter and consider the proportion that either shrinks to zero employees, shrinks to one employee, or remains with two employees after four quarters. This set of moments is influenced by the scale parameter of the per-period non-pay expenditures. Specifically, if μ_o increases, there will be fewer employers and more solo entrepreneurs within one year. Furthermore, these moments are affected by the variance of the idiosyncratic shocks ($\sigma_{\mathcal{N}}$). If $\sigma_{\mathcal{N}}$ increases, a higher proportion of firms will change size, including shrinking, from year to year. Lastly, the scale (μ_C) and elasticity parameters (γ_C) of the cost of posting vacancies, as well as the production function elasticity, affect the proportion of firms that grow.

We target the unemployment rate, computed by ONEM as the proportion of individuals receiving unemployment benefits to the total number of individuals eligible for these benefits. The primary factor affecting this statistic is the scale parameter of matching efficiency (μ_m). In particular, an improvement in matching efficiency – and, consequently, an increase in the job-finding rate – results in a reduction in the unemployment rate.

We target the job-finding rate, which we also compute using statistics from ONEM. Specifically, we consider individuals who were unemployed in quarter $t-1$ and are either in salaried employment (A) or still unemployed (B) in quarter t. The job-finding rate is calculated as the ratio A/(A+B). As in the previous case, this moment informs the scale parameter of matching efficiency, but it is also influenced by the scale parameter of the cost of posting vacancies. Indeed, the higher this parameter, the more firms prefer to fill vacancies quickly rather than post new ones. To fill vacancies faster, firms need to offer higher wages to attract more jobseekers; consequently, the job-finding rate decreases.

We then target the ratio of the job vacancy rate (JVR) for firms with 9 or fewer employees to the JVR for firms with 10 or more employees, as provided by EUROSTAT. This metric identifies the elasticity parameter of the cost of posting vacancies. This parameter influences the cost of posting a given number of vacancies as firm's size increases, thus influencing the number of vacancies posted by firms of different sizes. The last moment we target is the ratio of the average unemployment allowance to the average gross wage in Belgium. This statistic determines the out-of-work income b.

3.5.4 Results of estimation and model fit

Table 3.1 compares the actual data with the simulated moments. Overall, the match between the observed and simulated moments is satisfactory. However, we underestimate the proportion of firms with zero employees. This discrepancy may arise because, in the NBB data, firms with zero employees could include inactive firms that have not deregistered from official statistics or firms that may never intend to become employers, regardless of their idiosyncratic productivity. In contrast, our model assumes that all firms produce some output and will hire if their productivity is sufficiently high. To address this concern, in Appendix 3.G, we recompute the target moments with the ad hoc assumption that the number of firms with zero employees in each quarter is half of what is reported in the NBB data. This determines a change in two targeted sets of moments, namely the proportion of firms with 0, 1, 2, 3, 4, and 5 employees relative to the total number of firms with zero or more employees, and the proportion of firms with zero employees that either remain with zero employees or grow to one or two employees after four quarters. When we re-estimate the model with these new target moments, we find that the model reproduces the firms' distribution slightly better. This points towards the presence, among the firms with zero employees, of some inactive firms or firms that may never intend to become employers regardless of their idiosyncratic productivity. Reassuringly, the simulated impact of the reform, when the model is estimated under this ad hoc assumption, remains similar to the results obtained when we do not modify the number of firms with zero employees.

The estimated model also implies that firms with one or two employees experience higher year-on-year employment growth than is observed in the data. Indeed, for firms with one employee, the proportion that remains at 1 or fewer employees is 82.32% (64.91 + 17.91%), compared to 81.25% (75.09 + 6.16%) in the model. Meanwhile, for firms with two employees, the proportion that remains at 2 or fewer employees is 77.81% (54.67 + 17.02 + 6.12%), compared to 72.65% (66.85 + 3.33 + 2.47%). This discrepancy arises because we have set a cap on the number of employees a firm can have to 50. As a result, to match the proportions of employment in firms with more than five employees, the model requires many firms with around 50 employees, rather than a few firms employing hundreds of employees, as in the data. In other words, the model needs more small firms growing in size than is currently the case.

We also notice that the model underestimates the proportion of firms with 1 or 2 employees that shrink in size from quarter to quarter. This may be related to our assumption that the quarterly exogenous separation rate of employees, s_0, is zero. Indeed, we see in the Appendix 3.F that when we make the ad hoc assumption that $s_0 = 0.02$, these transitions are more precisely matched. Moreover, this difference may also imply that the idiosyncratic productivity shocks affecting small firms may be larger than the shocks hitting other firms, while we assume that they do not vary with firm size.

Table 3.1. Comparison between observed and simulated moments

		From the data	Simulated
Firms with 0, 1, 2, 3, 4, and 5 over the total number of firms	N_0/N	79.09%	68.22%
	N_1/N	6.50%	6.73%
	N_2/N	3.51%	3.04%
	N_3/N	2.21%	1.93%
	N_4/N	1.52%	1.45%
	N_5/N	1.08%	1.54%
Employment in firms with ≤ 5 EES over total employment		11.63%	6.23%
Ratio of avg. revenues for firms with 0 EES and 1 EE		25.74%	26.69%
Number of new firms with 0, 1, 2, and 3 employees at entry	$N_{\text{new},0}$	95.55%	96.27%
	$N_{\text{new},1}$	2.30%	2.17%
	$N_{\text{new},2}$	0.93%	0.92%
	$N_{\text{new},3}$	0.44%	0.41%
Firms with 0 EES that have 0, 1, and 2 EES in one year	$0 \to 0$	88.88%	88.77%
	$0 \to 1$	1.62%	1.98%
	$0 \to 2$	0.31%	0.26%
Firms with 1 EE that have 0, 1, and 2 EES in one year	$1 \to 0$	17.91%	6.16%
	$1 \to 1$	64.41%	75.09%
	$1 \to 2$	9.98%	9.94%
Firms with 2 EES that have 0, 1, and 2 EES in one year	$2 \to 0$	6.12%	2.47%
	$2 \to 1$	17.02%	3.33%
	$2 \to 2$	54.67%	66.85%
Unemployment rate		10.75%	10.91%
Qtly job finding rate		8.90%	10.00%
Ratio of the JVR for firms with ≤ 9 and > 9 EES		1.60	2.02
Ratio of the avg. unemployment allowance over the avg. gross wage		20.32%	20.21%

Notes: EES stands for employees, and JVR for the job vacancy rate. N represents the total number of firms, N_0 denotes the number of firms with zero employees, and $N_{\text{new},0}$ refers to the number of new firms with zero employees.

Table 3.2 summarises the estimated parameters. To interpret the estimated values, we need to either compare the parameters with one another or with the values estimated in the existing literature. In more detail, the production elasticity $\alpha = 0.77$ is only slightly lower than the estimated value of 0.817 reported by Bilal et al. (2022), or the value of 0.85 by Schaal (2017), both for the US. The difference is likely due to the fact that we matched the distribution and employment dynamics of very small firms. Our estimated value for the scale parameter of the matching function, 0.11, is lower than the estimate of 0.195 reported by Bilal et al. (2022), who used the same matching function and quarterly time period. However this estimate was derived for the US, where matching efficiency is likely higher than in Belgium. Our value for the matching efficiency, combined with a matching function elasticity of 0.5, results in a vacancy filling rate of $q(\lambda) = [0.11, 0.15, 0.19, 0.36, 0.22, \dots]$ and a job finding rate of $p(\lambda) = [0.11, 0.08, 0.06, 0.05, 0.05, \dots]$ for a number of jobseekers per vacancy $\lambda = [1, 2, 3, 4, 5, \dots]$.

Regrettably, no reference value for the sunk costs of opening a firm exists in the literature. Our estimated sunk costs are approximately 50% of the expected value of opening a firm, $\sum_{x \in X} \sigma_x J(L = 0, x)$, suggesting that these costs are substantial. The shape parameter of the Pareto distribution, 1.22, implies that for $X = [x_1, x_2, x_3, ..., x_n] = [1.00, 1.23, 1.45, ..., 17.73]$, $\sigma_x = [0.25, 0.16, 0.11, ..., 0.0001]$. This indicates that most firms in our model start with low idiosyncratic productivity.

As this paper examines a policy that has already been implemented, we can assess the quality of our estimation by checking whether our model replicates some key changes observed in the data in the post-reform period. In Subsection 3.7.2, we compare the observed and simulated evolution of the stock of firms with 1, 2, and [3,8] employees after the policy implementation, finding that the model satisfactorily replicates the observed changes.

3.6 Firm's optimal policy pre-reform

In this section, we describe the firm's optimal policy for any pair (L, x) in the pre-reform period. The presented statistic will help understand the model mechanics and, in the following sub-sections, will be used as a benchmark to understand how the exemption modifies the firms' behaviour.

Figure 3.3 shows whether a firm, which starts the period with the pair (x, L), decides to exit the market, reduce its workforce—i.e., set $0 < S \leq L$—maintain the same workforce—i.e., set $S = 0$—or hire employees during the period. To illustrate, for a firm that begins a period with $(x = x_{60}, L = 0)$, the

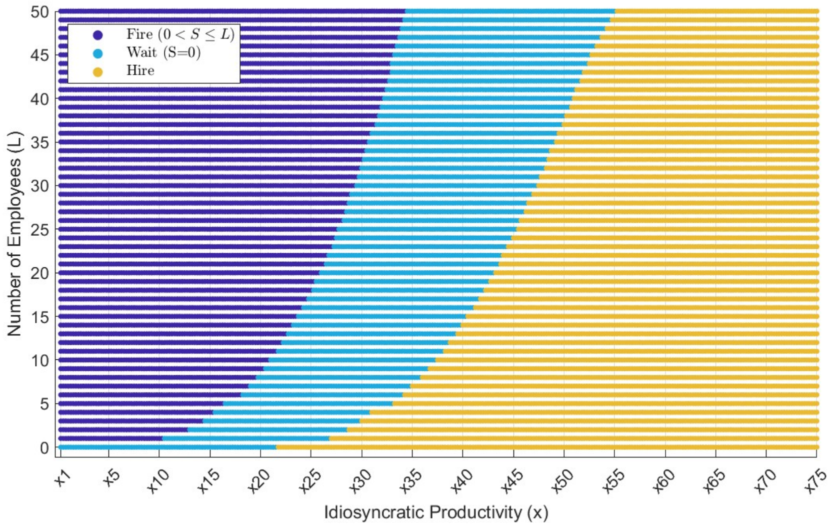
Table 3.2. externally/standardized/internally estimated parameters

Parameter		Value
Externally set/normalized parameters		
β	quarterly discount rate	0.987
τ	payroll tax rate pre-reform	0.183
τ_1	payroll tax rate post-reform	0.029
n	number of levels idiosyncratic productivity	75
x_1	lowest idiosyncratic productivity	1
δ_0	quarterly exogenous exit rate	[0.023,...]
γ_m	elasticity matching function wrt λ	0.5
Internally set parameters		
x_n	highest value X	18.1
σ_P	shape parameter Pareto distribution	1.22
α	production function elasticity	0.77
$\sigma_{\mathcal{N}}$	standard deviation shocks random walk	0.52
μ_C	scale parameter cost of posting vacancies	1.61
γ_C	elasticity cost of posting vacancies	0.67
μ_m	scale parameter matching function	0.11
μ_o	scale parameter non-pay expenditures	1.68
γ_o	elasticity non-pay expenditures wrt L	0.33
b	quarterly out-of-work income	0.61
s_b	sunk cost to open a firm	116

optimal policy is to hire. If it manages to make at least one hire and does not exogenously exit the market due to the shock δ_0, the firm will start the next period with some $L > 0$ (an upper line in the graph). It will also have a new value of idiosyncratic productivity, which will then determine its new location (L, x). Then, the firm will implement the best policy indicated in the figure for this specific new pair (L, x). Notice that in our estimation of the model, there are no pairs (x, L) for which the firms endogenously decide to exit the market.[15]

[15] This outcome arises because there are two opposing forces in the estimation process. On the one hand, we want many small firms in our economy (in the NBB data, 94% of firms have 5 or fewer employees). This requires the model to have many firms with low productivity at entry (for instance, the model predicts that 25% of firms at entry have $x = x_1$). On the other hand, if we have endogenous exit, the first firms to exit would be the least productive ones. However, if the least productive firms are numerous due to the high number of firms at

Figure 3.3. Firms' optimal policy pre-reform



Notes: The figure represents the pairs of idiosyncratic productivity (x-axis) and the number of employees (y-axis) at which agents reduce the number of employees, wait, or hire more employees.

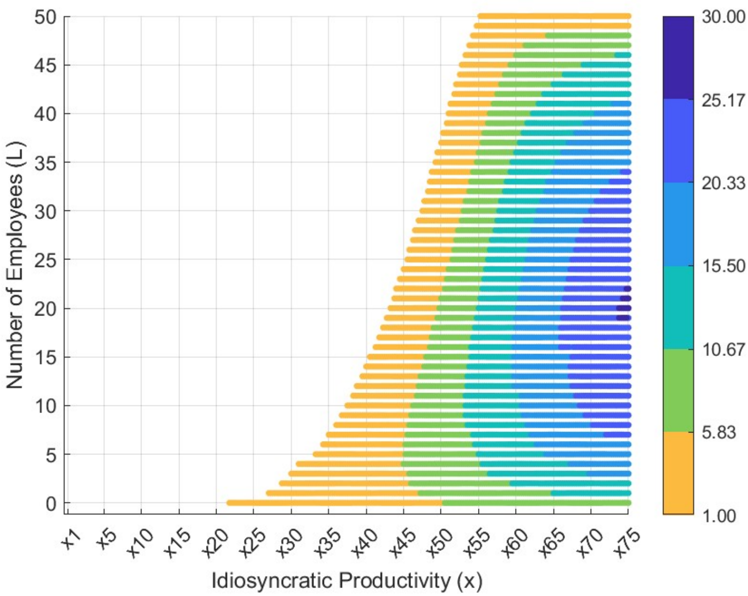
Within the hiring region, firms with different pairs of (L, x) hire at varying rates. Specifically, when hiring, a firm has two decisions to make: how many vacancies V to post and how quickly to fill each vacancy. To fill a vacancy faster, the firm needs to attract more jobseekers for that vacancy, and it does so by posting higher wages. The expected number of individuals hired is then given by the product of the number of vacancies and the chosen vacancy-filling rate (remember, we assume that the job-filling rate is the same for all vacancies posted in the same period).

Figure 3.4 shows the number of vacancies posted by a firm with a given pair (x, L) at the beginning of the period. We find that, given L, the firm posts

entry with low productivity, we would then predict exit rates that exceed the observed exit rates. To balance these opposing forces, we have assumed that the firms' exits we observe in the data are due to δ_0, and therefore eliminated the need to get endogenous exits in the estimation. This has allowed us to focus the model on estimating the correct firm's distribution. Another possibility for obtaining endogenous exits while having many small firms would have been to increase the number of levels for the idiosyncratic productivity, thereby reducing the number of firms that would exit after a negative idiosyncratic shock. However, this would have increased the computational time of the model.

more vacancies as its idiosyncratic productivity increases. This is because more productive firms have more to gain by hiring employees, as the revenues they can generate with a given number of employees are proportional to their idiosyncratic productivity. At the same time, for a given value of x, the number of vacancies first increases and then decreases with the number of employees. This occurs because, on the one hand, the revenues of a firm increase at a decreasing rate in L, while, on the other hand, the cost of posting vacancies, $C(V, L)$, decreases in L given V.

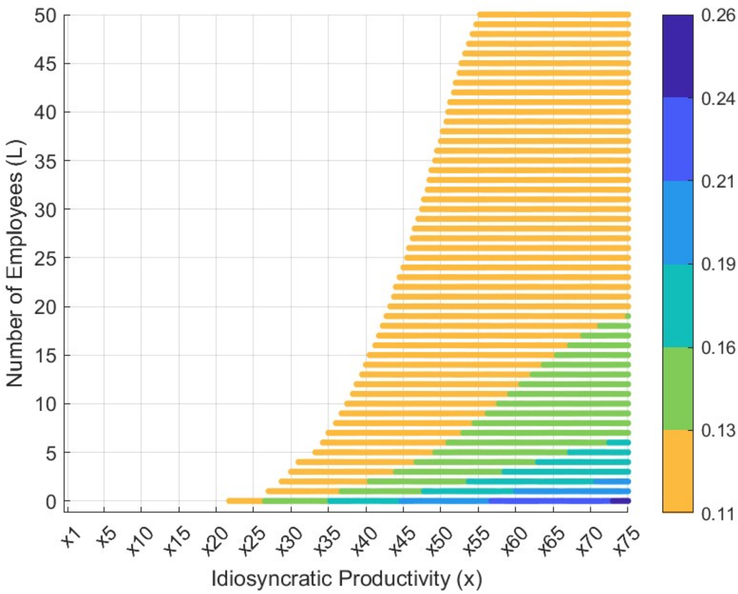
Figure 3.4. Number of posted vacancies pre-reform



Notes: The graph shows the number of vacancies posted by firms for each pair (L, x).

Figure 3.5 shows the rate at which firms fill each of their vacancies. Similar to the previous figure, given L, the job-filling rate increases with x (because, for a given L, $R(L, x)$ is proportional to x). Moreover, for a given x, the job-filling rate decreases with L (since, for a given x, $R(L, x)$ increases at a decreasing rate with L). Indeed, it is the most productive firms, conditional on being small, that benefit the most from filling their vacancies at a higher rate and, consequently, grow in size at a faster rate. To fill the vacancies faster, firms need to attract more jobseekers per vacancy; this is shown in Figure 3.H.1, which provides the same insights as Figure 3.5.

Figure 3.5. Vacancy-filling rate pre-reform

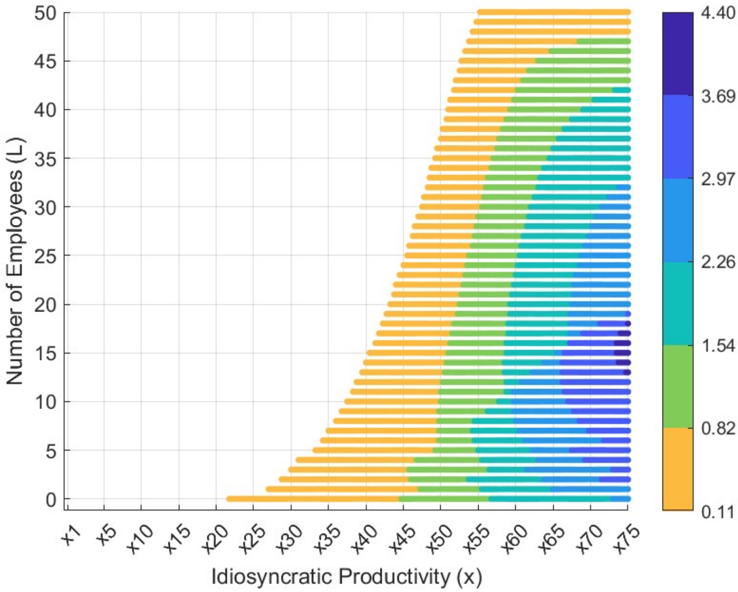


Notes: The graph shows the vacancy-filling chosen by firms for each pair (L, x).

In Figure 3.6, for firms that want to hire, we plot the expected number of hires for each pair (L, x). This statistic effectively summarises the hiring decisions of firms. Given that the expected number of hires is the product of the number of vacancies and the vacancy-filling rate, it is not surprising that the values it takes as a function of (L, x) depend on the factors described in Figures 3.5 and 3.4.

Similarly, within the firing region, firms with different pairs of (L, x) fire at different rates. Figure 3.H.2 shows the number of individuals fired for each pair (L, x). It shows that the number of individuals fired decreases with x given L and increases with L given x, as the revenues obtained from each worker are proportional to x, but total revenues increase at a decreasing rate in L.

Figure 3.6. Firms' expected number of hires pre-reform



Notes: The graph shows the expected number of hires for each pair (L, x).

3.7 Simulated impact of the reform

All else being equal, the exemption raises the payoff, $h(L, x)$, that eligible firms obtain by hiring. As a result, the expected value of opening a business, $\sum_{x \in X} \sigma_x J(L = 0, x)$, increases. If agents get a higher value from opening a business, employers must offer a greater present value to perfectly informed unemployed individuals to convince them to become job seekers: $\max_{\bar{i} \in \bar{\mathcal{I}}}[S_i]$ must increase to match $\sum_{x \in X} \sigma_x J(L = 0, x) - s_b$. Therefore, the present value of being in unemployment U increases by the same amount.

All firms, whether eligible—those without employees or established after the policy's enactment—or not eligible for the exemption—those with one or more employees at the time of enactment—must provide job seekers with the new, higher present value U. Additionally, they are required to pay higher continuation wages to ensure employees remain indifferent between continuing their work and searching for a new job. Firms not eligible for the exemption may dismiss employees if this proves more profitable than paying the increased continuation wages.

Over time, companies that were active in the market when the policy was introduced—whether eligible for the exemption or not—gradually exit due to external shocks or voluntary departures. Eventually, the economy reaches a new post-reform steady state, where all firms qualify for the exemption. Even if non-eligible firms exit the market gradually, if the number of agents choosing to become business owners is strictly positive in each post-reform period, the present value U the firms need to offer to jobseekers jumps from its pre-reform steady-state value to its new post-reform steady-state value upon the reform announcement. Formally, when new firms enter the market in any given period, the equilibrium value of U for that period is determined by solving these firms' optimal problem, $J(L = 0, x)$. Since new firms face the same optimisation problem every period after the reform, U, which is determined by Equation (3.1), jumps to its new steady-state value when the policy is introduced. We have numerically verified that the number of agents deciding to become business owners is strictly positive in every post-reform period.

This property greatly simplifies the transition dynamics from the pre-reform steady state—where no firms are eligible for the exemption—to the post-reform steady state—where all firms are eligible. Specifically, firms that are not eligible for the exemption determine their optimal exit, firing, and hiring decisions for each pair (L, x), given the new higher present value U, by solving the problem in Subsection 3.3.3. Since U immediately jumps to its new steady-state value, the optimal policy for these non-eligible firms is the same in each post-reform period. The same applies to firms eligible for the exemption: their optimal policy is determined by solving the problem in Subsection 3.3.5, and remains the same in each post-reform period.

To describe how the number of firms for each pair (L, x) evolves from the moment the policy is implemented, we proceed in four steps (from the number of firms for each pair (L, x), we can then easily derive the changes over time in the total number of firms, the number of firms categorised by their number of employees, the total number of employees, the number of job-seekers, the average job-filling rate and job-finding rate, for example). First, using the pre-reform model, we calculate the steady-state total number of firms and their distribution over (L, x), classifying them into two groups: firms with zero employees—referred to as incumbent solo-entrepreneurs—and firms with a strictly positive number of employees—referred to as incumbent employers. Second, we calculate the post-reform steady-state value of U. Third, starting with the distribution of incumbent employers obtained in the pre-reform steady state, we track how the number and distribution of these firms over (L, x) change from quarter to quarter by applying their optimal policy post-reform. The same process is implemented for the incumbent solo-entrepreneurs. Fourth, we calculate the number of new agents becoming business owners each quarter after the reform as a residual, ensuring that the total number of agents in the economy remains constant over time and equals the pre-reform level.

In the next sub-section, we describe how firms' optimal policies change in the post-reform periods compared to the pre-reform steady-state. As mentioned, the optimal policy differs between eligible and ineligible firms, but for each of these two groups, it remains the same across all quarters following the reform. However, for firms created before 2016 (independently of whether they have zero or more employees at the moment of the reform implementation), the optimal policy in the post-reform steady state is undefined, as these firms are assumed to have exited the market in the very long-run. So far, we have emphasised that the reform directly affects the hiring behaviour of eligible firms. However, by increasing U, it also indirectly influences the optimal policy of all firms. To isolate the positive first-order effect of making firms eligible for the exemption from the reduction in hiring caused by the increase in the equilibrium value of U, we also present an alternative scenario where, despite the eligible firms receiving the exemption, we assume that the firms solve their optimal problem using the pre-reform value of U instead of the new increased value. In Subsection 3.7.3, we describe the ad-hoc assumptions required to obtain meaningful statistics on the change in the number and distribution of firms, employees and job-seekers.

Then, we illustrate how the number and distribution of firms, the number of employees, and jobseekers evolve between the pre and post-reform steady states. Finally, we compare how these quantities change in the post-reform steady state compared to the pre-reform steady state.

3.7.1 Changes in the firms' optimal policy

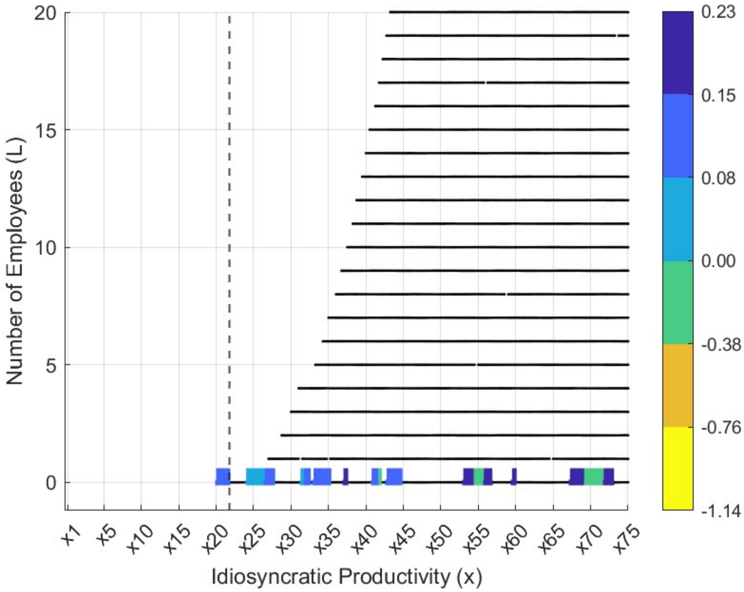
Figure 3.7 illustrates how the expected optimal number of hires per eligible firm changes for any pair $(L,\ x)$ post-reform, assuming that the present value firms must offer to job seekers remains fixed at its pre-reform level. The black horizontal interrupted lines represent the $(L,\ x)$ pairs where firms hire the same expected number of individuals pre and post-reform. The rectangles represent pairs where firms hire a different expected number of employees post-reform, with colour coding indicating the magnitude of the difference. The black vertical dashed line indicates the minimum productivity level at which firms with zero employees were hiring pre-reform.

In accordance with the discussion of Subsection 3.3.5, for some pairs ($L = 0$, x), firms that were not hiring pre-reform start hiring afterwards—the minimum productivity for a firm with 0 employees to start hiring decreases by 5.7%. These pairs are identified by the dark blue rectangles to the left of the vertical dashed line. Meanwhile, for some pairs ($L = 0$, x) where firms were already hiring prior to the reform, firms now hire at a faster rate. The exemption makes it more convenient to offer a higher wage to attract more jobseekers, as they no longer need to pay payroll taxes on this wage. These firms are represented by the blue squares to the right of the vertical dashed line. Not all firms with zero employees to the right of the dashed line modify their hiring behaviour. This is because we assume that the number of jobseekers a firm can attract is a natural number. Therefore, while it may be profitable to attract one (or more) additional jobseeker(s) for certain values of x thanks to the exemption, this may not be the case for other values of x. For example, among firms with zero employees, only firms with productivity between x_{46} and x_{58} were attracting 4 jobseekers per vacancy pre-reform. Post-reform, among the firms with productivity between x_{46} and x_{58}, it is only profitable for those with productivity between x_{54} and x_{58} to attract an additional jobseeker, namely 5 jobseekers, while firms with productivity between x_{46} and x_{54} continue to attract 4 jobseekers.

Additionally, we observe that for some values of x, there is a decrease in hiring among firms with zero employees. As explained when presenting the model, to increase the number of individuals hired, firms have two instruments, namely posting more vacancies or attracting more jobseekers per vacancy. Normally, firms have an incentive to use both margins, since $C(V, L)$ is convex in V. However, if attracting more jobseekers becomes less expensive thanks to the exemption, some firms could find it profitable to reduce the total number of vacancies while increasing the number of jobseekers per vacancy for the remaining vacancies. This happens post-reform for some values of x, leading to a negative net effect on the hiring of these firms.

By definition, if we keep U fixed, the expected number of hires remains unchanged for any pair (L,x) for firms that are not eligible for the exemption (i.e., incumbent employers). While they do not benefit from the exemption,

Figure 3.7. Change in the post-reform expected number of hires for eligible firms - fixed U

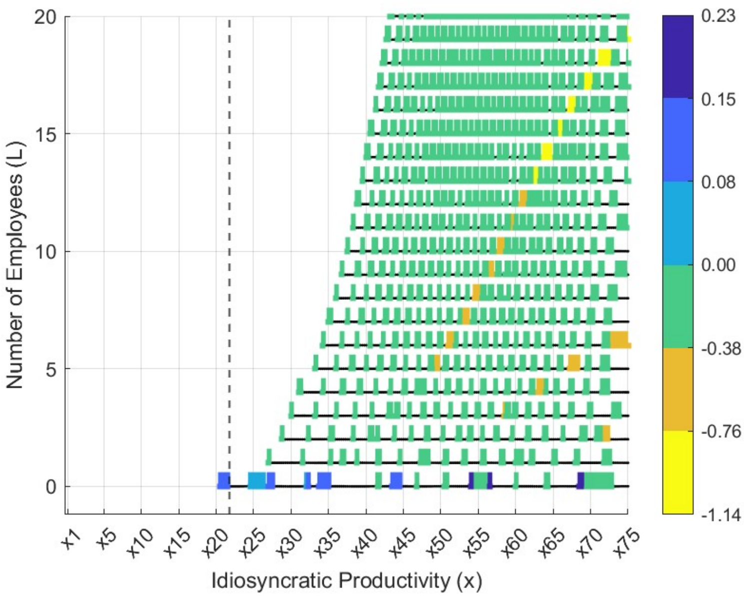


Notes: The black horizontal interrupted lines represents the (L, x) pairs where firms hire the same expected number of individuals both pre and post-reform. The rectangles represent pairs where firms hire a different expected number of employees post-reform, with the colour coding indicating the magnitude of the difference. The black dashed line represents the minimum productivity level at which firms with zero employees were hiring before the reform's implementation.

they also incur no additional costs, so they behave as before the exemption implementation.

Now, we present the firms' optimal hiring decisions when we allow U to adjust to its new equilibrium value, which is 0.94% higher than the pre-reform value. Figure 3.8 illustrates how the expected optimal number of hires per firm varies for any pair (L, x) for eligible firms post-reform. The colour coding previously defined applies. While some firms with zero employees still adjust their hiring decisions, the increase in U raises hiring costs. As a result, some firms reduce their hiring rates, indicated by green and ochre squares. In detail, the ochre squares represent pairs (L, x) where firms are reducing the number of jobseekers per vacancy, while the green squares represent pairs (L, x) where firms are only reducing the number of vacancies. Although these squares may appear to be randomly distributed, they correspond to the boundary of areas where firms in the pre-reform steady state were either adjusting the number of vacancies or the number of attracted jobseekers. These negative effects may offset the direct positive effect of the exemption, making the overall impact on the number of firms and their distribution post-reform unclear.

Figure 3.8. Change in the post-reform expected number of hires for eligible firms - increased U



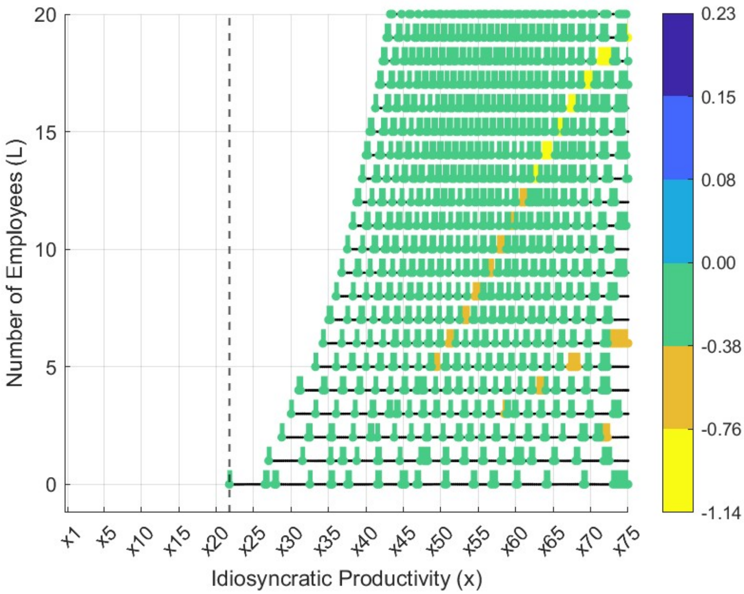
Notes: See the legend under Figure 3.7.

Firms with at least one employee when the reform was announced are ineligible for the exemption and must pay the higher U for all their employees. As a result, it is unsurprising that, for these firms, the expected number of hires either decreases or remains the same, but never increases—see Figure 3.9.

3.7.2 Changes during the transitions

By following the steps outlined at the start of this section, we can simulate the evolution of the stock of firms with 1, 2, and $[3, 8]$ employees post-reform, when U jumps to its new steady-state value following the reform's announcement. In Figure 3.10, we compare the simulated changes with the observed data. Both

Figure 3.9. Change in the post-reform expected number of hires for non-eligible firms - increased U

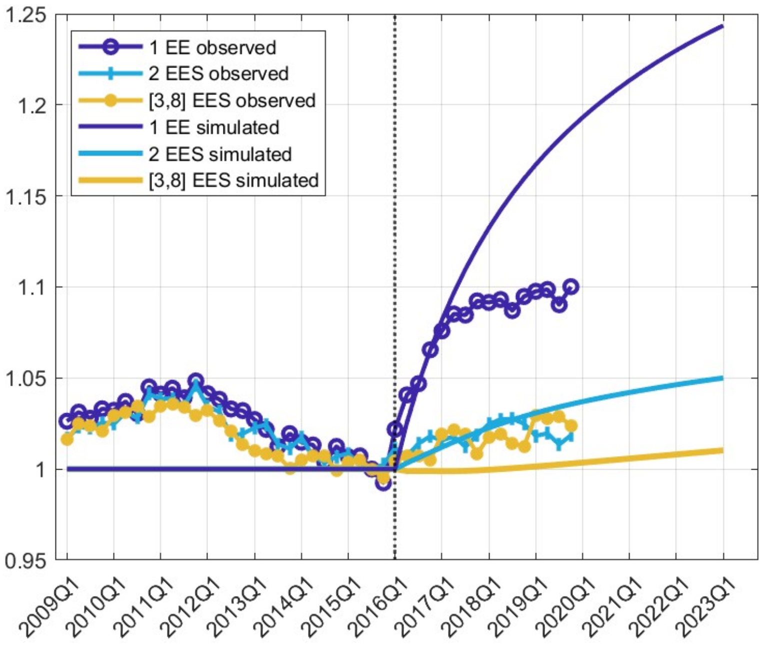


Notes: See the legend under Figure 3.7.

the data and the model show a similar sharp increase in the stock of firms with one employee following the reform's announcement, which then stabilises within a few quarters. However, the model overestimates this change. This could be due to the assumption that all agents in our model respond to the changes in monetary incentives offered by the exemption. In reality, some solo-entrepreneurs may prefer to work alone and choose not to hire employees, regardless of the generosity of the monetary incentives. In this regard, Hurst and Pugsley (2017) and Levine and Rubinstein (2018) show that many business owners prefer self-employment and have no desire to grow their business. This argument is also supported by the results in Appendix 3.G, where we show that the increase in the number of firms with one employee is smaller post-reform when we estimate the model with the ad hoc assumption that the number of firms with zero employees in each quarter is half of what is reported in the NBB data. Figure 3.10 also indicates a relatively modest change in the number of firms with two and $[3, 8]$ employees, consistent with the data.

Beyond the change in the stock of firms, we can also simulate the variation in the total number of employees and jobseekers over time. In Figure 3.H.3, we can see that the number of agents in each of these groups remains largely unchanged in the post-reform period. This is because, without the reform, most new employers would have been solo-entrepreneurs, and the employees hired by the growing number of firms with only one employee are largely offset by a decrease in employment in larger firms. This will become more apparent when we examine the changes in the number of firms, employees, and jobseekers by comparing the pre and post-steady state.

Figure 3.10. Evolution of the stock of firms



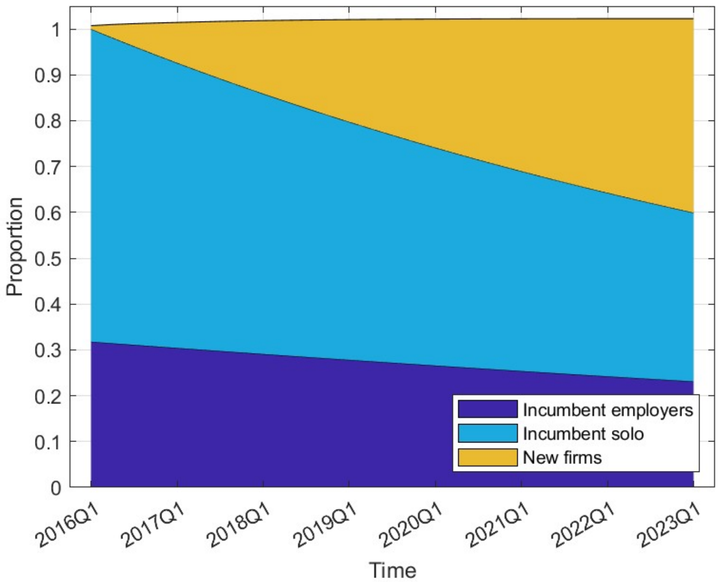
Notes: The figure illustrates the changes in the stock of firms with 1, 2, and [3,8] employees in the data and simulated by the model. For each group of firms, we normalise the number of firms to 1 in 2015Q3. The vertical dotted line marks the beginning of the policy in 2016.

Finally, the transition dynamics we simulate in the model reflect the combined effects of the behaviour of incumbent employers, incumbent solo-entrepreneurs, and new employers. To illustrate how the relative importance of each group evolves over time, we first plot the changes in the number of firms within each of the three groups, normalising the number of firms in the pre-reform period to 1—see Figure 3.11. Unsurprisingly, the number of incumbent firms (both employers and solo-entrepreneurs) decreases over time. The decline in the number of firms that were solo-entrepreneurs just before the reform is faster than that of incumbent employers, as the former have, on average, a higher exogenous exit rate—remember that we have assumed a higher exogenous exit rate for smaller firms. Figure 3.12 depicts how total employment for each group evolves over time, with total employment in the pre-reform period normalised to 1. As anticipated, employment decreases for existing employers, while it increases for both incumbent solo-entrepreneurs and new firms. Although this is not apparent from the graph, employment for incumbent solo-entrepreneurs will eventually decline as these firms exit the market. Finally, Figure 3.13 illustrates how the total number of jobseekers (namely, the total number of jobseeker per vacancy times the number of vacancies) for each group changes over time. Interestingly, the number of jobseekers in new firms becomes significant after a few quarters. This is because new firms, including small but highly productive ones, attract a high number of jobseekers per vacancy as they aim for rapid employment growth.

3.7.3 Changes in the pre and post-reform steady state

If we attempt to solve the model with the assumption that U is fixed at the pre-reform level, we encounter two main issues. Thanks to the exemption, some firms would be incentivised to start hiring or hire at a higher rate. However, given that the total number of agents is fixed, the demand for jobseekers would exceed the supply. This would raise the problem of how to allocate the available jobseekers to the different firms that want to hire. To simplify the analysis, we make two ad hoc assumptions. First, this extra labour demand by firms is met by individuals coming from outside the labour force. Secondly, to prevent all agents in our economy from deciding to create a firm instead of searching for a job—since U does not increase, but the firm's value increases, the value firms offer to jobseekers is lower than the expected value of opening a firm—we assume that the change in the number of firms in this scenario compared to the pre-reform situation is equal to the increase in the number of firms we observe when U is free to change.

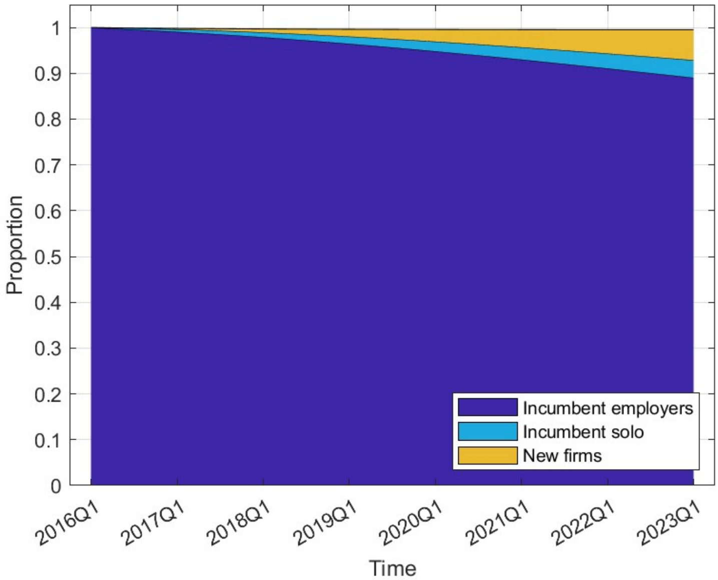
Figure 3.11. Evolution of the stock of firms by firm type



Notes: This figure shows the changes in the number of firms classified as incumbent employers, incumbent solo-entrepreneurs, and new employers during the post-reform period—we normalize to one the number of firms in the pre-reform period.

In Figure 3.14, the black bars represent the change in the number of firms, grouped by their number of employees, between the pre-reform and post-reform steady states when the present value of the job search, U, jumps to its new steady-state value. Instead, the dark-grey bars show the change when U remains fixed at its pre-reform value. As observed during the transition period, both scenarios indicate an increase in firms with one employee at the expense of solo-entrepreneurs, with minimal impact on larger firms. In both scenarios, the number of employers increases, consistent with the evidence of Cockx and Desiere (2024) and Deng et al. (2024). When U is fixed at the per-reform level, there is a more pronounced increase in the number of employers of each size compared to when U is at its new steady-state level. This is because, in the former case, the negative effect of the increase in U on hiring is absent. Finally, by comparing the steady-state change in the number of firms when U in free to increase with the transition dynamics, we can observe that the economy reaches the new steady state relatively quickly. For example, consider

Figure 3.12. Evolution of the stock of employees by firm type

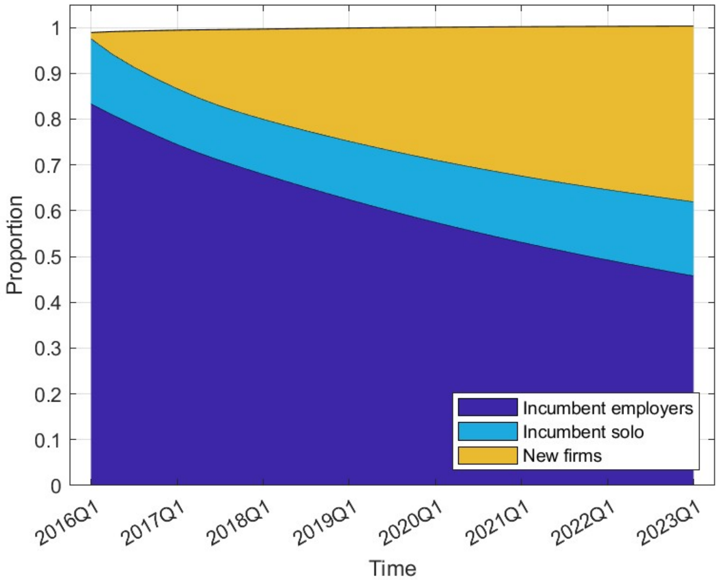


Notes: This figure shows the changes in the number of employees in incumbent employers, incumbent solo-entrepreneurs, and new employers during the post-reform period—we normalize to one the number of employees in the pre-reform period.

the increase in the stock of firms with one employee between the pre and post-reform steady state. We can then observe that our economy reaches half of this increase after 11 quarters, and three-quarters of the increase after 24 quarters.

In Figure 3.15, we show the overall changes in the total number of agents, employees, job seekers, and firms between the pre-reform and post-reform steady states when U is at its new steady-state level (black bars) and when U is fixed to the pre-reform level (grey bars). When U is at its new, higher level, total employment decreases slightly. This occurs because the rise in employment due to the increased number of firms with one employee is offset by the reduction in employment in larger firms. By construction, the total number of agents remains constant. If U is fixed, larger firms do not reduce their hiring, and therefore, the increase in employers leads to a rise in both job seekers and employees. Finally, we see that the payroll taxes collected by the government reduces by a sizable 4.94% when U is let free to increase and by 3.79% when U is fixed to the pre-reform level. Therefore, in both cases,

Figure 3.13. Evolution of the stock of jobseekers by firm type



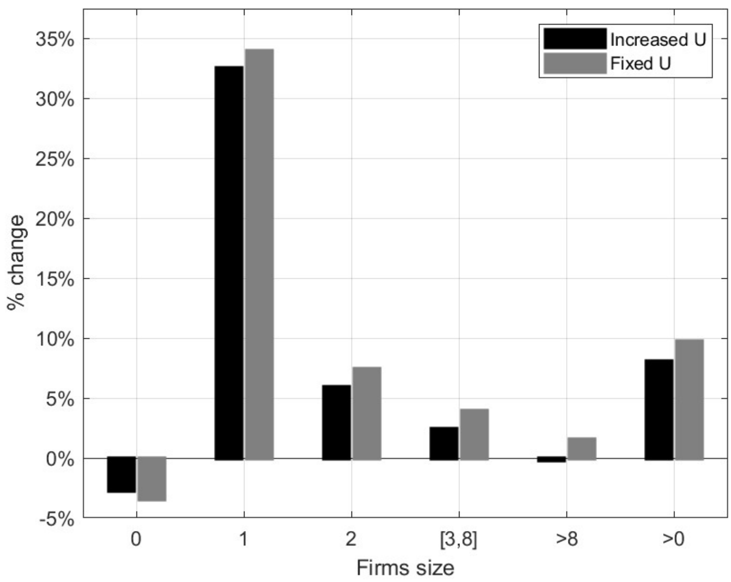
Notes: This figure shows the changes in the number of jobseekers in incumbent employers, incumbent solo-entrepreneurs, and new employers during the post-reform period—we normalize to one the number of jobseekers in the pre-reform period.

the policy imposes significant costs on the government budget. If we look at the revenues generated by the firms, we see that total revenues increase by a modest 0.27% in our preferred scenario when U is free to increase and by 1.67% when U is kept fixed.

To understand why the reform primarily increases the number of firms with more than one employee, rather than significantly affecting the total stock of firms with two or more employees, we separately analyse how two groups of agents have responded to the policy: those who, in the absence of the reform, would not have been productive enough to hire employees – the agents to the left of the dashed line in Figure 3.7 and 3.8 – and those who would have hired employees regardless – the agents to the right of the dashed line. We refer to these agents as the marginal and inframarginal firms, respectively.

Indeed, marginal firms are low-productivity firms which, even if they hire due to the exemption, would have no incentive to employ more than one employee unless they also experience an increase in their idiosyncratic productivity. That is, if their productivity does not change, they would remain with one employee, setting $S = 0$. Conversely, inframarginal firms, after hiring their first employee more quickly due to the exemption, would continue to hire additional employees, even if their productivity remains unchanged (i.e. their productivity is likely high enough to continue hiring, even if it does not increase). Before proceeding with this analysis, we should remember that these two groups of agents have markedly different sizes. In the post-reform steady state, employers with productivity below (above) the level at which they would not have hired pre-reform account for 18% (82%) of the total number of employers.

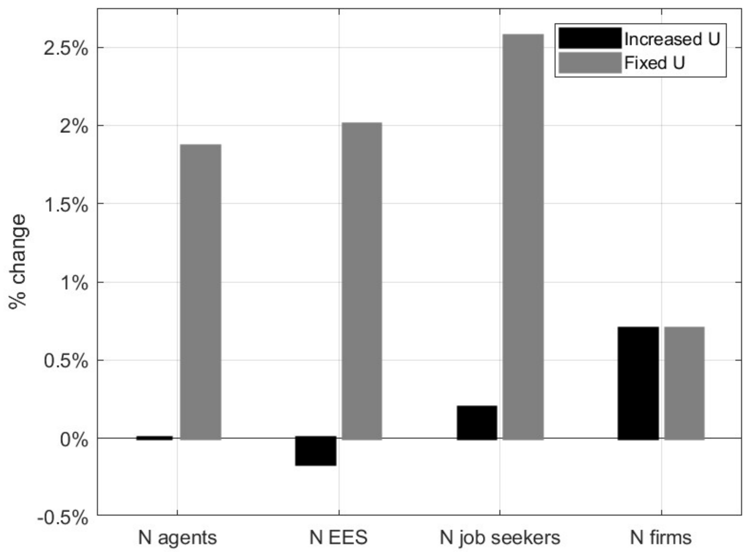
Figure 3.14. Change (in %) in the stock of firms



Notes: This figure shows the change in the stock of firms with 0, 1, 2, [3,8], 9 or more employees, and 1 or more employee between the pre and post-reform steady state.

First, we assume that the policymaker can perfectly observe the productivity x of each agent and grants the exemption solely to the marginal firms. In Figure 3.16, light blue bars, we plot the change in the number of firms by their number of employee when U jumps to its new steady-state level. Compared to the baseline (dark blue bars) we observe that agents who were not productive to hire pre-reform are responsible for the majority of the

Figure 3.15. Change (in %) in the number of agents by type

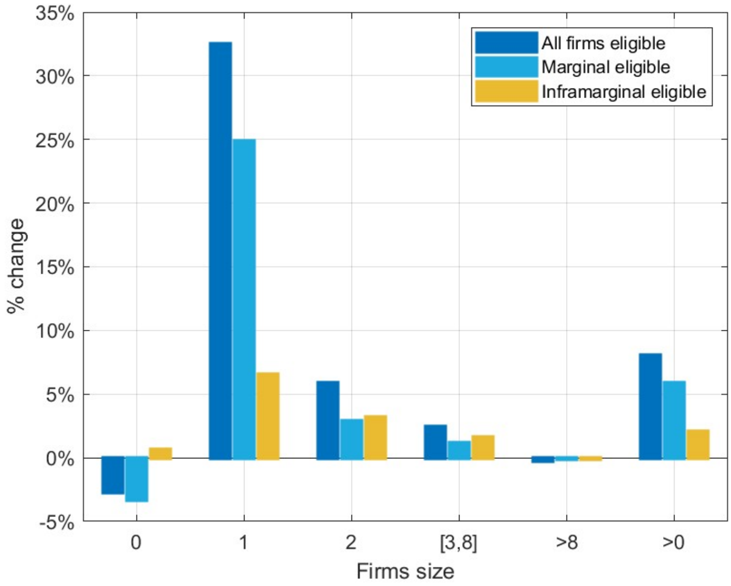


Notes: This figure shows the change in the total number of agents in the labour force, total number of employees, job seekers, and firms between the pre and post-reform steady state.

changes in the stock of firms with one employee. However, only a limited number of these new employers created thanks to the exemption grow above one employee. This confirms our intuition that these new low-productivity firms, with just one employee and created due to the exemption, mostly remain in the policy region where they do not hire additional employees. Only a few of them, those who experience a positive productivity shock, hire more employees. When only marginal employers are eligible for the exemption, U increases by 0.15% in steady state, compared to the 0.94% increase when all firms are eligible. This is not surprising, as marginal employers make up only around 18% of the agents receiving the exemption in our baseline analysis. This indicates that the response of marginal employers has only a minimal impact on the overall cost of hiring and, consequently, on the hiring decisions of incumbent non-eligible employers.

Next, we assume that the policymaker grants the exemption only to the inframarginal firms. In Figure 3.16, the ochre bars illustrate the steady-state

Figure 3.16. Change (in %) in the stock of firms in steady state - decomposition



Notes: This figure shows the change in the stock of firms with 0, 1, 2, [3,8], 9 or more employees, and 1 or more employee between the pre and post-reform steady state. It describes three scenarios: the situation where all firms are eligible for the exemption; when only firms that were not productive enough to hire pre-reform are eligible (marginal firms); the situation where only firms that were productive enough to hire pre-reform are eligible (inframarginal firms). In all scenarios, the labour force is fixed, and therefore, U increases post-reform.

change in the number of firms for this scenario when U jumps to its new steady-state level. The increase in the hiring rate by inframarginal firms has only a limited impact on the change in the stock of firms. In other words, too few of these firms that receive the exemption modify their hiring behaviour—i.e., by offering higher wages to attract more jobseekers—to have a sizable impact on the firm's distribution. This is because, firstly, as seen in Equation 3.11, the expected cost of attracting a given number of jobseekers per vacancy is proportional to the number of jobseekers per vacancy. Therefore, if a firm decides, for example, to attract 2 jobseekers instead of 1, it will, on average, pay double the amount in hiring bonuses. This implies that even if the firm pays a rate of SSC that is 15 percentage points lower on these hiring bonuses (remember that this is the maximum reduction, only obtained by firms with 0 employees that post 1 vacancy, while this reduction decreases with the number of vacancies and becomes zero if a firm has more than one employee and $s_0 = 0$), the increase in wages the firm needs to cover to attract an additional jobseeker is still substantial. Secondly, the vacancy-filling rate increases to a much lesser extent as the number of jobseekers per vacancy rises, limiting the benefit of attracting more jobseekers. When only inframarginal employers are eligible for the exemption, U increases by 0.79% in steady state, compared to the 0.94% increase when all firms are eligible. This is because, although inframarginal employers contribute only minimally to changes in the distribution of firms, they represent the majority of firms in the economy that receive the exemption.

The finding that most of the increase in firms with one employee is caused by agents who were not productive enough to hire before the reform, and that neither these agents nor the inframarginal employers significantly contributed to the increase in the stock of larger employers, aligns with the results of Deng et al. (2024). Deng et al. (2024), comparing a cohort of employers created after the reform with a cohort of employers created before, show that employers created after the reform announcement are, on average, less productive than those created before. They also find that the number of new employers in the post-reform cohort remained higher after three years than in the pre-reform cohort, but this is primarily due to an increase in firms with only one employee in the post-reform cohort. Moreover, by explaining why inframarginal employers did not react to the policy, our paper clarifies why the reaction of marginal firms modelled in Chapter 2 matches the effects of the reform on the stock of firms.

3.7.4 Exploring alternative parametrizations

In the previous subsection, we observed that inframarginal employers did not significantly alter their hiring behaviour in response to the policy. As a result, only a small number of additional firms hired two, three, or more employees. This raises the question of whether, under certain model parameterisations, the exemption could result in faster employment growth among inframarginal employers. As noted in the previous section, while the expected cost of attracting an additional jobseeker is proportional to the number of jobseekers, the benefit of doing so—namely, the increase in the probability of filling a vacancy—grows at a much slower rate in the number of jobseekers. This limits the benefit of attracting additional jobseekers, even if the exemption reduces the expected cost.

Suppose the matching efficiency, μ_m, is 50% higher than in the baseline scenario both pre and post-reform. In this case, if a firm decides to attract an additional jobseeker, the absolute change in the probability of filling the vacancy would be 50% higher than in the baseline. Therefore, attracting an additional jobseeker due to the exemption would have a significantly larger impact on firms' employment growth.

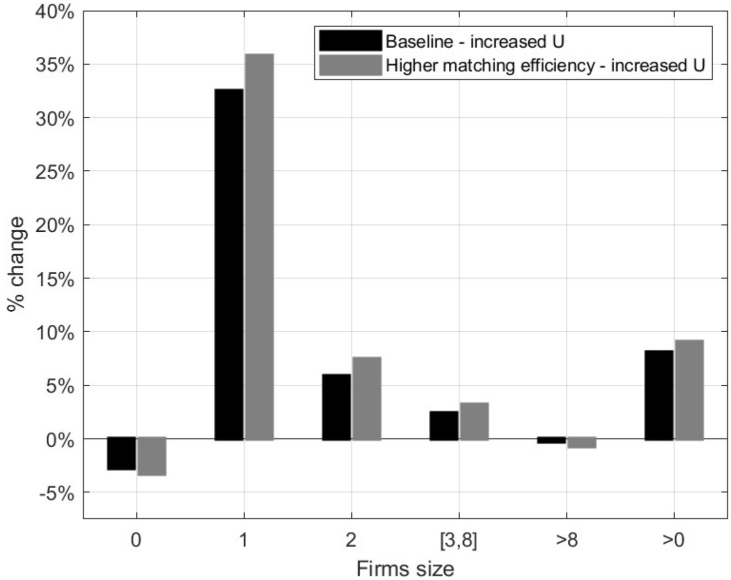
Figure 3.17 illustrates the change in firm size between the pre-reform and post-reform steady states for the baseline scenario (black bars) and for the scenario with a 50% increase in matching efficiency (grey bars) when U is free to adjust. When matching efficiency is higher, there is a more pronounced increase in the number of firms with 1, 2, and [3,8] employees. However, if firms expand more than in the baseline scenario due to the exemption, U must rise to maintain the equilibrium between the labour demand of firms and the number of individuals seeking employment. The increase in U leads to a reduction in the hiring rate across all firms, resulting in a more significant decrease in firms with more than 8 employees compared to the baseline. Overall, when the matching efficiency is higher, the number of employers increases more than in the baseline, albeit by only a small margin. This finding raises doubts about the potential for a substantial policy impact on the number of large firms, even in a parameterisation where the policy is more effective in fostering employment growth among inframarginal employers.

3.8 Conclusion

This paper examines the impact of exempting new employers from payroll taxes for one employee. Specifically, it determines how the observed increase in the number of firms with one employee, and the relatively modest change in the stock of larger firms observed in the data, is driven by the responses of two types of firms: those that would have hired at least one employee regardless of the tax exemption (inframarginal employers) and those that would not have hired without the reform (marginal employers).

To answer this question, we develop a directed search model inspired by Kaas and Kircher (2015), in which multi-worker firms differ in their idiosyncratic productivity and hire employees in a frictional labour market. We depart from their model by introducing employers' payroll taxation, and by assuming that firms arise from unemployed individuals who choose to start

Figure 3.17. Change (in %) in the stock of firms in steady state - increased matching efficiency and increased U



Notes: This figure shows the change in the stock of firms with 0, 1, 2, [3,8], 9 or more employees, and 1 or more employee between the pre and post-reform steady state. It compares the baseline scenario with the scenario where matching efficiency is 25% higher. In both cases, U adjusts to the respective new steady-state value.

their own businesses rather than search for a job. Additionally, we consider the number of workers, the vacancies posted by firms, and the number of jobseekers per vacancy a firm can attract as natural numbers. We show that the optimal firm's policy under commitment can be made tractable with only two state variables: firm employment and productivity level. We estimate the model using data from a panel of small firms provided by the National Bank of Belgium. Finally, we use the model to simulate the impact of payroll tax exemptions on the behaviour of firms and employees.

In accordance with the empirical evidence from Cockx and Desiere (2024) and Deng et al. (2024), we find that the reform led to an overall increase in the number of employers. Moreover, while it primarily increased the number of firms with one employee, it did not significantly affect the stock of larger firms. Furthermore, we observe that the new steady state is reached relatively quickly. For example, consider the increase in the stock of firms with one employee between the pre and post-reform steady state: our economy reaches half of this increase after 11 quarters, and three-quarters of the increase after 24 quarters. The sharp simulated increase in the stock of firms with one employee aligns with the increase observed in the data. We also find that, due to the increase in the value of the employees' outside options —i.e. starting a business has become more profitable thanks to the exemption—firms need to offer higher wages to attract jobseekers and retain employees. However, the overall rise in wages has a modest impact on the firms' behaviour.

While marginal employers contribute to the increase in the number of firms with one employee, only a small proportion of these firms grow beyond one employee. We attribute the limited growth of these new employers to the fact that they are low-productivity agents: The exemption encourages them to hire their first employee, but to grow further, they need to experience a positive increase in their idiosyncratic productivity. This finding suggests that tax and benefit policies encouraging low-productive firms to hire should be accompanied by interventions to make these micro-firms more productive. Otherwise, a spiral of further employment growth is relatively unlikely.

The exemption does not significantly alter the hiring behaviour of inframarginal employers. Indeed, for these individuals, the cost required to attract one more jobseeker is relatively large compared to the benefit, which is the resulting increase in the probability of filling a vacancy. In our model, inframarginal employers account for around 82% of the firms receiving the exemption, and their limited reaction to the exemption raises concerns about the justification for allocating financial resources to these agents. However, it remains true that distinguishing between those who would be able to hire and those who would not in the absence of the policy is not a straightforward task, and it could raise equity concerns or lead to opportunistic behaviour among firms.

We then assess whether the reaction of inframarginal employers would have been stronger if the benefit of attracting an additional jobseeker had been higher. To do this, we increase the matching efficiency of the economy by 50%, meaning that attracting one additional jobseeker has a greater impact on the probability of making an additional hire, thus driving faster employment growth. We find that, if the total labour force is fixed, much of the faster growth among inframarginal employers is offset by a reduction in hiring by larger firms due to the general equilibrium effects on the equilibrium wages.

This result highlights that the response of inframarginal firms is likely to remain small, even if the exemption had been introduced in a setting potentially more favourable to their employment growth.

The results are obtained without imposing any government's balanced budget condition. One may believe that some of the changes observed in the firms' behaviour could be attenuated if the government were to introduce other taxes/compulsory contributions to finance the reduction in SSC due to the exemption for the first employee, for example, an increase in the SSC on employees other than the first.

Our model overestimates the increase in the number of firms with one employee observed in the data. We provide some evidence suggesting that this may be due to some agents having a strong preference for remaining solo entrepreneurs, despite changes in payroll taxation. To investigate this possibility further, one would need to incorporate into the model the fact that agents differ in the non-pecuniary benefits they derive from operating a business on their own, as in Hurst and Pugsley (2017) and Levine and Rubinstein (2018). We leave this extension for future research. Similarly, in our model, we do not account for other frictions that could limit firms' growth, such as credit constraints. For example, we cannot exclude the possibility that some agents are credit-constrained and, as a result, do not hire employees because the cost of a worker, including payroll taxes, may come before the associated benefits. The policy may help alleviate this constraint. We also leave the investigation of how significant this channel might have been in shaping the response of firms for future research.

Appendix

3.A Evolution stock of firms

Figure 3.A.1. Evolution stock of firms

Notes: The Figure illustrates the changes in the number of firms with 0, 1, 2, [3,8], [9,15] employees. For each firms' group, we normalize the number of firms to 1 in 2015Q3. The vertical dotted line marks the beginning of the policy in 2016. Notice that our dataset only has information for firms with 15 employees or less.

3.B Proposition' proof

Proposition 2. *If the tax rate τ on the stream of wages is independent of the firm's size, individuals are risk neutral and the capital market is perfect, then business owners would make the same hiring/firing/exit decisions, and unemployed agents would make the same employment decisions whether firms post i) long-term contracts that specify a state-contingent wage and retention* *probability for each future period, or ii) contracts that only provide a hiring bonus, $\bar{w}$, when the jobseeker is hired and a continuation wage, $\underline{w}$, for each period the employee produces output. The continuation wage is set so that the value of remaining in the current job is equal to the present value of being unemployed.*

Let's denote by $\{W_{i,t}\}_{i=1}^{L}$ the value in employment that a firm with L employees and productivity x has promised its employees for the current period t. Let's consider a contract for individual j that pays the wage $w_{j,t}$ at time t. Then, the current promised utility to agent j is equal to $W_{j,t} = w_{j,t} + \beta\phi_{j,t}Ex_{t+1}[W_{i,t+1}] + \beta(1-\phi_{j,t})U_{t+1}$. For simplicity, we assume that the firm's productivity is fixed over time, but the same reasoning applies when x changes according to a known Markov process. Let's also assume that U is at its steady-state value.[16]

We proceed by demonstrating that the cost incurred when either retaining or firing worker j depends solely on the current promised utility $W_{j,t}$ and the value of unemployment U, rather than on the wages $w_{j,\cdot}$ over time. To illustrate this, we consider two scenarios: one where the firm fires individual j at time t, and the other where the firm never fires individual j from t to $+\infty$. It is important to note that the worker can still lose their job due to the exogenous exit of the firm, denoted by δ_0, or due to the exogenous separation rate s_0. As a result, the probability that the worker retains their job from one period to the next, even if the firm does not fire them, is $\phi_0 = (1-\delta_0)\cdot(1-s_0)$.

Let us set $t = 0$. If the firm fires individual j in the current period, then the firm needs to pay the current wage $w_{j,0} = W_{j,0} - \beta U$ to provide the promised utility $W_{j,0}$. Not surprisingly, the cost of firing only depends on the current promised utility and U. If the firm never fires the worker, it must provide $W_{j,0} = w_{j,0} + \beta\phi_0 W_{j,1} - \beta(1-\phi_0)U$ for the current period, $W_{j,1} = w_{j,1} + \beta\phi_0 W_{j,2} - \beta(1-\phi_0)U$ for the next period etc. If we recursively substitute the values of $W_{i,1}$, $W_{i,2}$, etc, we get that the total amount of wages the firm needs to pay for this individual j is equal to $\sum_{t=0}^{\infty}\beta^t\phi_0^t w_{j,t} = W_{j,0} - \frac{\beta(1-\phi_0)U}{1-\beta\phi_0}$. Again, the cost of retaining the worker does not depend on the value of future wages but only on the current value of $W_{i,0}$ and on U. This ensures that how wages are distributed does not impact the cost of retaining a worker or firing it. A similar reasoning applies to hiring.

[16] In search theory, even along the transitional dynamics, U immediately jumps to its new steady state following an unanticipated shock.

3.C Payroll tax rates pre and post-reform

To illustrate the generosity of the temporary payroll reductions (pre-reform) and the permanent payroll exemption (post-reform) for the first hire, we refer to the statistics presented in Chapter 2. In that Chapter, we calculate the expected payroll tax rate for the first employee at the time of hiring for two cohorts of new employers: one who hired their first employee pre-reform, and another who hired their first employee post-reform. We define the expected payroll tax rate as the weighted average of the payroll tax rate for each quarter after hiring, weighted by the firm's probability of still being an employer and a quarterly discount rate set at 1.275% (e.g., Kaas and Kircher, 2015). These computations reveal that the expected payroll tax rate for the first employee of new employers is 18.3% in the pre-reform period and decreases to 2.9% in the post-reform period.

3.D Firms' exit rate

In this section, we explain how we calculate the proportion of firms that exit the market within a year, grouped by the number of employees they have in a given quarter.

To compute the exit rates of these firms, we follow a two-step procedure. Let N_i, where $i \in [0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15]$, represent the number of firms with i employees in the market during a given quarter. First, we count how many of these firms are no longer registered in the NBB dataset four quarters later. The second column of Table 3.D.1 presents the average de-registration rates computed over four quarters: 2013Q1/2014Q1, 2013Q2/2014Q2, 2013Q3/2014Q3, and 2013Q4/2014Q4. We selected this pre-reform year because, in 2015Q4, some firms altered their behaviour in anticipation of the policy (Cockx and Desiere, 2024). The de-registration rates are very low, as firms do not always de-register from the NBB dataset if they are no longer active. Therefore, these rates are not a good proxy for the firms' exit rate.

To address this issue, in the third column of the same table, we calculate the proportion of firms with i employees in a given quarter that have 0 employees one year later. Interestingly, about 3.1-3.9% of firms with 10, 11, 12, 13, 14, and 15 employees see their workforce drop to 0 within a year. We have decided to include these transitions in the exit rate. Therefore, we make the conservative assumption that, in addition to the de-registration rate from the NBB dataset, an extra 3.6% of firms exit the market each period—where 3.6% is the average proportion of firms with 10–15 employees that have 0 employees one year later. These imputed yearly exit rates are shown in the last column of Table 3.D.1. Since our model is quarterly, we derive the quarterly exit rate, q, by applying the following formula to the yearly rates (y): $q = 1-(1-y)^{1/4}$. In our estimation procedure, we set δ_0 equal to these imputed quarterly exit rates.

Table 3.D.1. Yearly transitions

N EES	No longer in the dataset	With 0 EES	Imputed exit rate
0	5.33%	92.45%	8.89%
1	1.16%	21.48%	4.73%
2	0.87%	9.68%	4.43%
3	0.73%	6.54%	4.30%
4	0.62%	5.67%	4.18%
5	0.46%	4.93%	4.03%
6	0.52%	4.48%	4.08%
7	0.47%	4.05%	4.04%
8	0.39%	4.03%	3.95%
9	0.39%	4.16%	3.96%
10	0.42%	3.73%	3.99%
11	0.54%	3.96%	4.10%
12	0.52%	3.62%	4.08%
13	0.48%	3.68%	4.05%
14	0.49%	3.29%	4.05%
15	0.40%	3.11%	3.96%

Notes: This table reports the proportion of firms with 1, 2, etc. employees in a given quarter that have deregistered from the NBB datasets within one year (second column) and those with zero employees after one year (third column). The fourth column combines this information to provide the proportion of firms that have exited the market within one year.

3.E Targeted moments

3.E.1 Firms' and employment distribution

From the NBB data, we calculate the average number of firms with 1, 2, 3, 4, 5, 6, 7, 8, and 9 employees for 2013 and 2014. The NBB data does not include information about larger firms; therefore, we supplement these statistics with publicly available data from Statbel, the Belgian statistical office. Statbel (2022b) provides annual statistics on the number of active firms and their corresponding number of paid employees. These statistics are limited to for-profit firms in the industry and services sectors—sections B to N of the NACE codes (rev 2) - excluding holding companies (NACE code K64.2). Further details about the firms included in the Statbel statistics are available at Statbel (2022a). Table 3.E.1 illustrates the distribution of firms and employment, derived by combining our granular NBB data for small firms with the Statbel data for larger firms.

Table 3.E.1. Firms' and employment distribution

Firm size	N firms	N firms (%)	N EES	N EES (%)
0	655,083	79.09%		
1	53,829	6.50%	53,829	2.39%
2	29,055	3.51%	58,110	2.58%
3	18,296	2.21%	54,888	2.44%
4	12,567	1.52%	50,268	2.23%
5	8,962	1.08%	44,808	1.99%
6	6,834	0.83%	41,003	1.82%
7	5,311	0.64%	37,177	1.65%
8	4,267	0.52%	34,135	1.52%
9	3,480	0.42%	31,320	1.39%
[**6**,+∞]	30,557	3.69%	1,846,300	81.99%
total	828,240	100.00%	2,251,837	100.00%

Notes: These statistics are derived from our NBB data and publicly available Statbel statistics. They represent the averages for the period between 2013 and 2014.

3.E.2 Turnover

Using NBB data, we calculate the average quarterly turnover for firms with 0 and 1 employee between 2013Q1 and 2014Q4, at €14,711 and €47,510, respectively. However, turnover is reported only for firms subject to VAT. In Belgium, most firms can opt not to be subject to VAT if their annual turnover is below €25,000. As a result, the NBB does not record the turnover data for 29.32% (4.20%) of firms with 0 (1) employees. To account for this significant proportion of firms with 0 employees whose turnover is unknown, we make the conservative assumption that all such firms have an annual turnover of €25,000, corresponding to a quarterly turnover of €6,250. Under this assumption, the average quarterly turnover of firms with 0 employees decreases to €12,230. Consequently, the ratio of the average turnover of firms with 0 employees to that of firms with 1 employee is 25.74%.

3.E.3 Distribution of new firms

To measure the size distribution of new firms, we proceed as follows. Using the NBB data, we compute for each quarter between 2013Q1 and 2014Q4 the number of firms that did not exist in a previous quarter and report how many of these firms have 0, 1, 2, 3, 4, 5, 6, 7, 8, and 9 employees. Notice that when a firm changes its identification number (CBE number), it will appear as a new firm in the NBB data. Therefore, we may mistake firms that change CBE number as new firms. For this reason, as in Bijnens and Konings (2020), we assume that all firms which have 10 employees or more in the quarter they are created are not really new firms and, therefore, are not included in this statistic.[17] In Table 3.E.2, we report the employment distribution of new firms.

3.E.4 Transition dynamics

To compute the proportion of firms with zero employees that have 0, 1, or 2 employees within one year is relatively straightforward. First, we identify all firms with zero employees in 2013Q1 (and similarly for 2013Q2, 2013Q3, and 2013Q4). Then, we determine how many of these firms have 0, 1, and 2 employees at the end of 2014Q1. When calculating the number of firms with zero employees, we must subtract the estimated number of firms with

[17] Geurts and Van Biesebroeck (2016) estimate that already 57% of new entrants between 10–19 employees are spurious, with this percentage increasing with the firm's size at entry

Table 3.E.2. Employment distribution of new firms

size	N firms	N firms (%)
0	12,821	95.55%
1	308	2.30%
2	125	0.93%
3	59	0.44%
4	40	0.30%
5	24	0.18%
6	16	0.12%
7	11	0.08%
8	8	0.06%
9	6	0.04%
Total [0,9]	13,418	100.00%

Notes: These statistics are computed from our NBB data. They present the averages between 2013Q1 and 2014Q4.

zero employees that are no longer active following the procedure explained in Appendix 3.D. Finally, we average the results across all four quarters. The same process is repeated for firms with 1 and 2 employees in 2013Q1 (and similarly for 2013Q2, 2013Q3, and 2013Q4).

3.E.5 Statistics from ONEM and Statbel

The unemployment rate is derived from ONEM's annual reports. The average unemployment rate for 2013 and 2014 is 10.75% (the average unemployment rate for 2013 is 10.8% and for 2014, 10.7%). ONEM computes the unemployment rate as the ratio between the number of individuals who receive unemployment allowances and are seeking a job, and the number of individuals insured against unemployment.

Similarly, using the ONEM public statistics on transitions into and out of unemployment for each quarter between 2013Q1 and 2014Q4, we determine the number of individuals who were unemployed (receiving unemployment allowances and seeking a job) in quarter $t-1$ and who are either in salaried employment (A) or still unemployed (B) in quarter t. Next, we compute the job-finding rate for each quarter as the ratio A/(A+B). Finally, we calculate the average over these eight quarters, which is 8.90%.

EUROSTAT provides the job vacancy rate (JVR), which is the ratio of the number of vacant jobs to the total number of vacant jobs and occupied posts for firms with 10 or more employees, as well as for firms with fewer than 10 employees. A 'job vacancy' is defined as a paid post that is newly created, unoccupied, or about to become vacant: (a) for which the employer is taking active steps and is prepared to take further steps to find a suitable candidate from outside the enterprise concerned; and (b) which the employer intends to fill either immediately or within a specific period of time; An occupied post means a paid post within the organisation to which an employee has been assigned (Eurostat, 2022). For 2013 and 2014, the JVR for small firms was 3.78% and 3.40%, while for big firms was 2.40% and 2.12% (Statbel, 2022c). Then, the average ratio of the job vacancy rate between small and large firms is 1.60.

Finally, the ratio of the average unemployment allowance to the average gross wage is calculated as follows. Based on ONEM's annual reports for 2013 and 2014, the average monthly unemployment allowance paid to jobseekers during this period is €963 (€960 in 2013 and €966 in 2014). The average number of individuals receiving an unemployment allowance is 445,563 (432,484 individuals in 2013 and 458,642 in 2014). Additionally, the average number of jobseekers not receiving any unemployment allowance is 183,997 (193,275 in 2013 and 174,719 in 2014). Thus, the average unemployment allowance received by unemployed individuals in Belgium is calculated as follows: $445,563/(445,563 + 183,997) \cdot €963 = 0.71 \cdot €963 = €682$. The average gross wage in Belgium, as reported by Statbel (2022d), is 3,357 euros (€3,300 in 2013 and €3,414 in 2014). Therefore, the average unemployment allowance to the average gross wage is €682/€3357 = 20.32%.

3.F Alternative estimation: Increase s_0

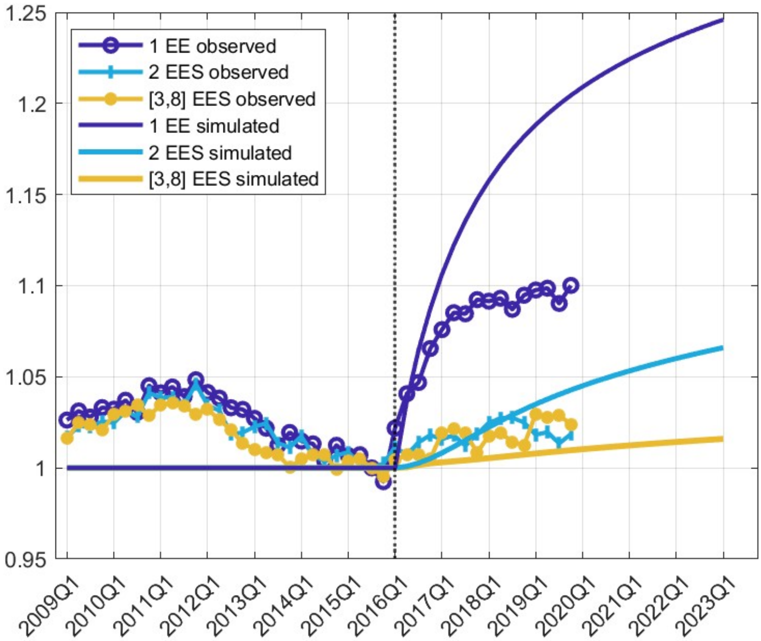
In this alternative estimation, we set the exogenous quarterly separation rate of employees to 2% instead of zero. We summarise the resulting match between the observed and simulated new moments in Table 3.F.1. In this scenario, the overall match between the simulated and observed moments improves compared to the baseline, indicating that a certain level of exogenous separation is likely present in the economy. However, despite the changes, the simulated impact of the reform is comparable to our baseline. In Figure 3.F.1, we simulate the effect of the reform on the stock of firms with 1, 2, and $[3, 8]$ employees post-reform, when U jumps to its new steady-state value following the reform's announcement. As in our baseline results in Figure 3.10, the model still simulates a sharp increase in the stock of firms with 1 employee, while there is a relatively limited change in the stock of larger firms.

Table 3.F.1. Comparison between observed and simulated moments

		From the data	Simulated
Firms with 0, 1, 2, 3, 4, and 5 over the total number of firms	N_0/N	79.09%	74.60%
	N_1/N	6.50%	5.33%
	N_2/N	3.51%	3.00%
	N_3/N	2.21%	1.99%
	N_4/N	1.52%	1.50%
	N_5/N	1.08%	1.27%
Employment in firms with ≤ 5 EES over total employment		11.63%	9.55%
Ratio of avg. revenues for firms with 0 EES and 1 EE		25.74%	25.09%
Number of new firms with 0, 1, 2, and 3 employees at entry	$N_{\text{new},0}$	95.55%	96.26%
	$N_{\text{new},1}$	2.30%	2.26%
	$N_{\text{new},2}$	0.93%	0.93%
	$N_{\text{new},3}$	0.44%	0.38%
Firms with 0 EES that have 0, 1, and 2 EES in one year	$0 \to 0$	88.88%	89.31%
	$0 \to 1$	1.62%	1.50%
	$0 \to 2$	0.31%	0.21%
Firms with 1 EE that have 0, 1, and 2 EES in one year	$1 \to 0$	17.91%	9.17%
	$1 \to 1$	64.41%	69.40%
	$1 \to 2$	9.98%	12.30%
Firms with 2 EES that have 0, 1, and 2 EES in one year	$2 \to 0$	6.12%	1.89%
	$2 \to 1$	17.02%	11.06%
	$2 \to 2$	54.67%	60.15%
Unemployment rate		10.75%	18.33%
Qtly job finding rate		8.90%	12.82%
Ratio of the JVR for firms with ≤ 9 and > 9 EES		1.60	1.95
Ratio of the avg. unemployment allowance over the avg. gross wage		20.32%	16.52%

Notes: EES stands for employees, and JVR for the job vacancy rate. N represents the total number of firms, N_0 denotes the number of firms with zero employees, and $N_{\text{new},0}$ refers to the number of new firms with zero employees. With respect to our baseline, we assume that s_0 is equal to 2% instead of zero.

Figure 3.F.1. Evolution of the stock of firms



Notes: The figure illustrates the changes in the stock of firms with 1, 2, and [3,8] employees in the data and simulated by the model. For each group of firms, we normalise the number of firms to 1 in 2015Q3. The vertical dotted line marks the beginning of the policy in 2016. With respect to our baseline, we assume that s_0 is equal to 2% instead of zero.

3.G Alternative estimation: Fewer firms with 0 employees

In this alternative estimation, we recompute the target moments based on the assumption that the number of firms with zero employees in each quarter is half of what is reported in the NBB data. This assumption leads to a change in two targeted sets of moments: first, the proportion of firms with 0, 1, 2, 3, 4, and 5 employees relative to the total number of firms with zero or more employees; and second, the proportion of firms with zero employees that either remain with zero employees or grow to one or two employees after four quarters. We then re-estimate the model and report the comparison between the observed and simulated new moments in Table 3.G.1. This change slightly improves the match in the distribution of firms and employment. Moreover, while the match of some moments worsens, the overall error between the simulated and observed moments slightly decreases.

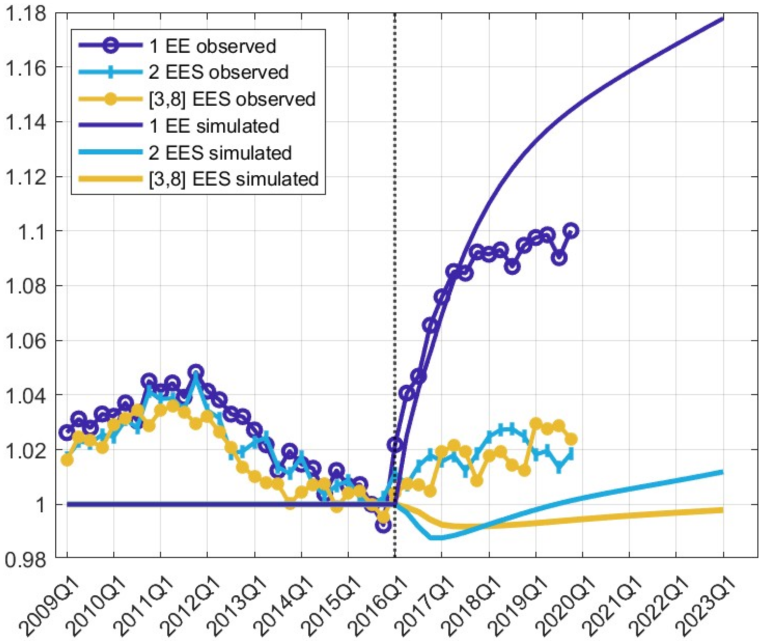
However, despite the changes, the simulated impact of the reform is comparable to our baseline. In Figure 3.G.1, we simulate the effect of the reform on the stock of firms with 1, 2, and $[3, 8]$ employees post-reform, when U jumps to its new steady-state value following the reform's announcement. As in our baseline results in Figure 3.10, the model still predicts a sharp increase in the number of firms with 1 employee (although this increase is smaller than in our baseline results), while the stock of larger firms changes only slightly.

Table 3.G.1. Comparison between observed and simulated moments

		From the data	Simulated
Firms with 0, 1, 2, 3, 4, and 5 over the total number of firms	N_0/N	65.42 %	57.39%
	N_1/N	10.75%	8.48%
	N_2/N	5.80%	4.29%
	N_3/N	3.65%	2.81%
	N_4/N	2.51%	2.21%
	N_5/N	1.79%	2.34%
Employment in firms with ≤ 5 EES over total employment		11.63%	7.64%
Ratio of avg. revenues for firms with 0 EES and 1 EE		25.74%	30.66%
Number of new firms with 0, 1, 2, and 3 employees at entry	$N_{\text{new},0}$	95.55%	96.14%
	$N_{\text{new},1}$	2.30%	2.18%
	$N_{\text{new},2}$	0.93%	0.94%
	$N_{\text{new},3}$	0.44%	0.45%
Firms with 0 EES that have 0, 1, and 2 EES in one year	$0 \rightarrow 0$	88.88%	86.70%
	$0 \rightarrow 1$	1.62%	3.68%
	$0 \rightarrow 2$	0.31%	0.58%
Firms with 1 EE that have 0, 1, and 2 EES in one year	$1 \rightarrow 0$	17.91%	8.18%
	$1 \rightarrow 1$	64.41%	70.11%
	$1 \rightarrow 2$	9.98%	11.53%
Firms with 2 EES that have 0, 1, and 2 EES in one year	$2 \rightarrow 0$	6.12%	4.19%
	$2 \rightarrow 1$	17.02%	3.38%
	$2 \rightarrow 2$	54.67%	63.62%
Unemployment rate		10.75%	10.83%
Qtly job finding rate		8.90%	10.98%
Ratio of the JVR for firms with ≤ 9 and > 9 EES		1.60	2.39
Ratio of the avg. unemployment allowance over the avg. gross wage		20.32%	23.76%

Notes: EES stands for employees, and JVR for the job vacancy rate. N represents the total number of firms, N_0 denotes the number of firms with zero employees, and $N_{\text{new},0}$ refers to the number of new firms with zero employees. With respect to our baseline, we assume that 50% of firms with zero employees are inactive.

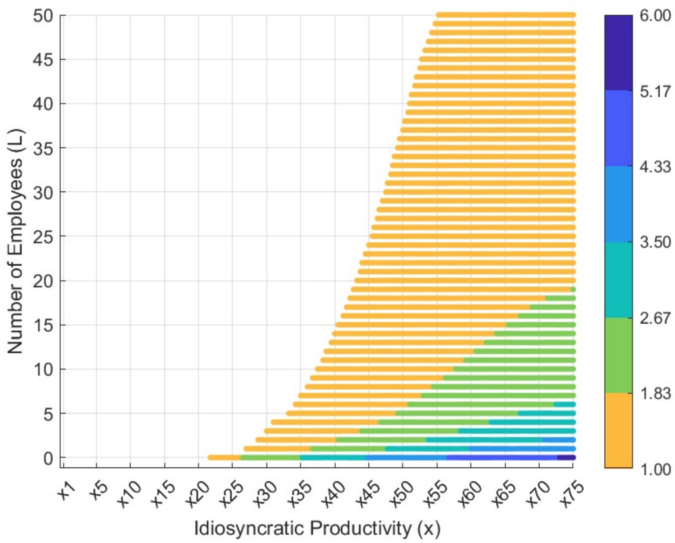
Figure 3.G.1. Evolution of the stock of firms



Notes: The figure illustrates the changes in the stock of firms with 1, 2, and [3,8] employees in the data and simulated by the model. For each group of firms, we normalise the number of firms to 1 in 2015Q3. The vertical dotted line marks the beginning of the policy in 2016. With respect to our baseline, we assume that 50% of firms with zero employees are inactive.

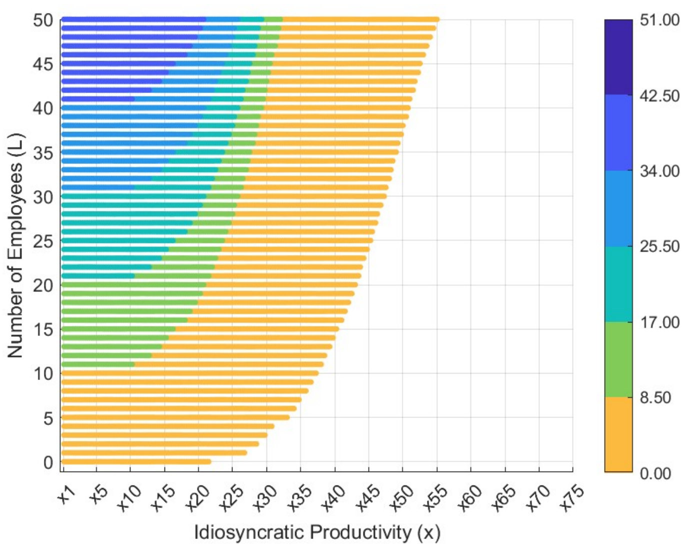
3.H Additional graphs

Figure 3.H.1. Number of jobseekers per vacancy pre-reform



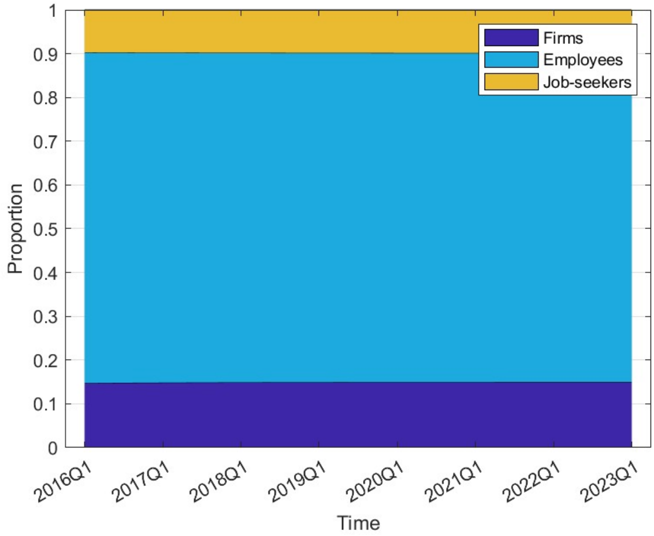
Notes: The graph shows the jobseekers per vacancy attracted by firms for each pair (L, x).

Figure 3.H.2. Number of fired employees pre-reform



Notes: The graph shows the number of fired employees by firms for each pair (L, x).

Figure 3.H.3. Evolution proportion firms, employees, and jobseekers



Notes: This figure illustrates how the proportions of firms, employees, and jobseekers evolve over time in the post-reform period. Note that the total number of agents is assumed to remain constant across all periods and is equal to the pre-reform total number of agents.

4

In-work benefits and eligibility based on wage rates: the effects on time worked

This paper investigates the impact of introducing the Social Work Bonus (SWB), a payroll tax reduction for low-wage individuals in salaried employment in Belgium. The SWB differs from other in-work benefits aimed at enhancing work incentives because it specifically targets individuals with a low-wage rate rather than those with low labour earnings. This approach prevents part-time traps, where individuals may reduce their working time to qualify for in-work benefits or may not increase their working time to avoid losing the benefits. Using individual administrative panel data and a difference-in-differences estimation strategy, we observe a relatively large increase in the quarterly number of days worked for young salaried employees with a part-time contract. No such effect is found for older employees or those on full-time contracts. These findings indicate that, for certain subgroups of the population, in-work benefits targeted at the wage rate can lead to a significant increase in working time.

4.1 Introduction

In the 1990s, the persistent labour market hurdles faced by low-skilled/low-earning individuals revived the debate about reducing taxation on labour to make salaried employment more financially attractive and to alleviate or prevent in-work poverty (Immervoll and Pearson, 2009). This debate led to the introduction of employment-conditional labour earnings subsidies in most advanced economies, commonly referred to as in-work benefits.

When looking at the effects of in-work benefits on labour supply, the literature generally finds that in-work benefits have a modest positive effect on employment participation, with minimal impact on the working hours of those already employed (Immervoll and Pearson, 2009; OECD, 2011; Laun, 2019; Brewer and Hoynes, 2019; Kleven, 2024). However, in-work benefits can vary across several dimensions, such as whether eligibility is determined by individual or household income, whether they are provided only when children are present in a household, whether an individual's working time affects eligibility, and whether the benefits are permanent or temporary (Kenworthy et al., 2015; McKnight et al., 2016; Laun, 2019; Van der Linden, 2021).

While each design serves different objectives for policymakers, different designs can also have different effects on labour supply (Liebman, 2002; Vandelannoote and Verbist, 2020). Specifically, when eligibility is based on individual labour earnings—where labour earnings are the product of the wage rate and working time—individuals in salaried employment may reduce their working time—and consequently their earnings—to qualify for in-work benefits. Similarly, individuals have little incentive to increase their working time, as a rise in earnings could result in the loss of these benefits (Saez, 2002; OECD, 2011; de Mahieu, 2021).

In Belgium, private-sector employees contribute 13.07% of their gross remuneration to fund their entitlements to pension, sickness and disability benefits, work-related accident coverage, occupational disease protection, and unemployment support. These contributions are commonly referred to as Social Security Contributions (SSC). In January 2000, the government reduced these SSC for individuals in salaried employment with a low-wage *rate* while maintaining the benefits financed by these contributions. This reduction, known as the Social Work Bonus (SWB), resulted in an increase in the wage rate net of SSC for these employees. Since its introduction, the

SWB has gradually expanded over time—its generosity and the number of eligible individuals have increased—and remains in effect today.

The SWB is unique among in-work benefits because it targets individuals with a low-wage rate, rather than those with low labour earnings. As a result, it is an in-work benefit that minimises the unintended disincentives to labour supply. This paper investigates the impact of this in-work benefit on labour supply at the intensive margin. Setting eligibility based on the wage rate rather than on earnings may reduce the effectiveness of the SWB in alleviating poverty, as the instrument will equally treat individuals with the same wage rate, regardless of whether one of them works a limited amount of time and is, therefore, more at risk of being in poverty. However, this paper does not examine the impact of the policy on poverty.

To conduct this study, we use an individual panel dataset from the Crossroads Bank for Social Security (CBSS), which gathers extensive administrative data from Belgian social security institutions, including individuals' working time, labour earnings, gender, and family composition. To estimate the reform's effect on time worked by individuals in salaried employment, we employ a difference-in-differences (DiD) approach. We compare the evolution in working time between two groups: those with a low-wage rate just before the reform—specifically, in 1999Q4—and those with a higher wage rate in 1999Q4. The former group acts as our treatment group because most of these individuals, conditional on remaining in salaried employment post-reform, continue to have a low-wage rate and, as a result, receive the SWB. The latter group serves as our control group, as most of these employees still have a high-wage rate in the post-reform period and, therefore, are not eligible for the SWB.

We find that the reform increased the quarterly number of days worked post-reform for individuals with a part-time job in 1999Q4 by approximately 0.86 days. This corresponds to a substantial elasticity of days worked to the wage rate, net of SSC, of about 0.62. In contrast, we do not observe a change in days worked post-reform for individuals with full-time contracts in 1999Q4. However, for full-time employees, our findings are not robust across all our robustness checks and heterogeneity analyses. We can interpret the positive net effect on working time as a substitution effect that is larger than the income effect at the intensive margin for individuals with a part-time contract. When examining whether the effect of the policy varies based on certain demographic characteristics of the beneficiaries, we find that the policy's impact is present only among young individuals with part-time contracts, but not among the oldest individuals. No heterogeneity by age is observed among individuals with full-time employment. Similarly, the impact is present among part-time female employees, but does not appear among males. However, the estimates for males are particularly imprecise due to the low number of observations in this group.

One caveat of our findings is that we are unable to disentangle whether the observed effect is due to the change in the amount of time worked by individuals who would have been in salaried employment in the absence of the reform, or whether it is because the reform has determined a reduction in the exit rate from salaried employment—where those who remained in salaried employment, thanks to the SWB, had relatively high working time. In other words, our intensive margin elasticity of days worked to the wage rate may be biased upwards. Nevertheless, this finding still indicates that for certain subgroups of the population, in-work benefits targeted at the wage rate can lead to a significant increase in working time, either by encouraging salaried employees to work more or by increasing their attachment to salaried employment.

Our paper contributes to the literature that evaluates labour market responses to in-work benefits, particularly those in-work benefits where eligibility depends on the working time.[1] The only other in-work benefit based on the wage rate was the French tax credit *Prime Pour l'Emploi.* The papers analyse this policy, assess its impact on the likelihood of being in salaried employment (i.e. extensive margin analysis), and find either small or statistically insignificant effects, or even negative effects for married women (Cochard et al., 2008; Sterdyniak, 2007; Stancanelli, 2008). However, they do not investigate the change in time worked conditional on being in salaried employment—intensive margin analysis—which is the primary focus of our paper. Moreover, the French credit was of low value, and there was a considerable delay (as much as 18 months) between the time an individual became eligible and the month they actually received it. By contrast, the SWB is paid monthly and can provide a significant reduction in the rate of SSC, up to 9 percentage points (pp).

While not based on the wage rate, a few other in-work benefits require a minimum number of weekly hours worked to be eligible. This applies to the UK Working Tax Credit, (Brewer et al., 2006; Brewer and Hoynes, 2019),

[1] Ooghe (2020) demonstrates that earnings subsidies are optimal only when targeted at low earners and therefore conditional on the individual's working hours.

the Working Family Payment (WFP) in Ireland, and the In-Work Tax Credit (IWTC) in New Zealand (OECD, 2011; Bargain and Doorley, 2011). However, all these instruments are primarily received by households with children and are contingent on the household's earnings. In contrast, the SWB is determined by the individual's wage rate, regardless of their family status. This individual targeting specifically reduces the risk of individuals lowering their working time to maintain the household's eligibility for the in-work benefit.

Our ex-post evaluation can be contrasted with the findings of the existing literature that assess SWB through ex-ante analyses, which are carried out using discrete choice labour supply models in combination with tax simulators. It is important to highlight that these studies do not precisely replicate the introduction of SWB in 2000. Notably, to calibrate their models, many of these simulations utilise EU-SILC data, which have only been available since 2004. Consequently, they primarily simulate modifications to the SWB rules, such as making them more generous or removing the SWB entirely. Orsini (2006), Decoster et al. (2010), Dagsvik et al. (2011), and de Mahieu (2021) find that, for individuals already in employment, the effects of the SWB are limited, namely a slight increase in working time or, in some cases, a decrease[2]. Similarly, Bargain et al. (2014) and de Mahieu (2021) report relatively small gross own-wage elasticities, ranging from 0 to 0.1, for working time at the intensive margin for both male and female employees, regardless of whether they are single or in a couple. Own-wage elasticities have also been estimated for countries other than Belgium. These estimates are derived from various sources, including structural models, long-term changes in wages, tax policy variations over extended periods, or policy reforms that serve as natural experiments (Blundell and MaCurdy, 1999; Blundell et al., 2007; Bargain and Peichl, 2016). Most evidence focuses on

[2]Orsini (2006) writes that the SWB creates a strong incentive to take up a job and finds a moderate decrease of hours worked of males in employment, noting that the SWB does not create an incentive to work overtime, and men are the category where most over time is observed; Decoster et al. (2010) show that the SWB pushes new people to enter the labour market and a moderate aggregate increase in hours worked both for female and male, single or in couple; Dagsvik et al. (2011) find that the SWB is more efficient in terms of cost per additional full-time equivalent position created than tax credits based on earnings and an aggregate positive impact at the intensive margin; de Mahieu (2021) simulates three different scenarios where the amount of SWB is increased and/or the phase-out rate is reduced. He shows that the simulated reforms slightly increase labour supply when measured in terms of participation and finds a negative impact at the intensive margin of the expansion of the SWB.

couples or single women with children, particularly in the context of work-benefit reforms in the UK and the US, which are among the most widely studied cases. In their review, Bargain and Peichl (2016) find that total elasticities are highest among married women and single mothers, with mean values of 0.43 and 0.59, respectively. However, the distribution of these estimates is highly variable. They also observe that, for these groups, most of the responses occur at the participation margin rather than the intensive margin. Evidence on single men and women is scarce, but available data suggest their elasticities are small. Similarly, small elasticities are found for men in couples. While our study also finds a stronger impact for women than for men, it identifies a more pronounced effect of labour supply at the intensive margin, both with respect to the ex-ante analyses for Belgium and the international literature. However, we observe this stronger effect only among young individuals who had a part-time contract before the reform. This difference may result from other studies assessing a broader population, compared to the specific group we observe reacting to the policy.

Finally, a study conducted independently in parallel with our analysis by Dewatripont and López Novella (2024) also examines the introduction of the SWB in 2000. By conducting a DiD analysis on a cross-section of individuals with low-wage rates, who are therefore eligible for the SWB, and another group with wages just above the eligibility threshold, they find a substantial increase in the probability of remaining in employment in the post-reform years. We complement this study by examining the intensive margin reactions of individuals already in salaried employment, using quarterly data. Dewatripont and López Novella (2024) also look at the year-on-year growth rate in time worked. However, this measure provides limited insight into the intensive margin responses. Specifically, since the SWB represents an increase in the wage rate, we do not expect to see a different growth rate in the time worked between the treated and control groups, except perhaps immediately following the reform's implementation. More precisely, we expect that treated individuals will choose a different (higher, if the substitution effect dominates) amount of time worked compared to the pre-reform period, starting from the moment they are treated, or after a few quarters if they take time to adjust to the reform.[3]

[3]As a robustness check, the authors conduct a DiD analysis using a panel of individuals selected in 1997Q2. However, based on our summary statistics in Appendix 4.D, which indicate that individuals' wages increase significantly over time, it is unlikely that many individuals with low wages in 1997Q2 would still have low wages—and therefore qualify for

This paper is organised as follows: Section 4.2 describes the SWB. In Section 4.3, we present a theoretical framework that shows how the policy is likely to impact the employees' behaviours. In Section 4.4, we present the data. Section 4.5 outlines the empirical strategy, Section 4.6 presents some summary statistics, Section 4.7 discusses the empirical results, and in Section 4.8, we provide the concluding remarks.

4.2 The policy

In Belgium, prior to 2000, all private sector employees contributed 13.07% of their gross salary towards SSC. On 20 December 1999, a law[4] introduced a reduction in SSC contributions for low-wage salaried employees, referred to as the SWB, effective from January 2000. As a result, individuals in salaried employment—excluding public servants—with a sufficiently low-wage rate began paying a rate of SSC of less than 13.07%.

The law introducing the SWB was not preceded by a specific public debate. We searched the newspaper archives of *Le Soir*—one of the major French newspapers of Belgium—from 1998 to December 1999, but found no references to this measure. This suggests that salaried employees did not anticipate the introduction of the policy. The introduction of the SWB has been rushed, as the exact rules for its proper computation were published only on 17 January 2000 by royal decree. This decree was motivated by the need to provide employers with clarity regarding the application of this reduction, as well as to give the National Social Security Office the possibility to establish its instructions and develop its computer programs for monitoring and calculating the reduction. Consequently, it is likely that a substantial number of employees did not receive the SWB during at least the first few months of 2000.

Interestingly, the SWB rules were amended as early as April 7, 2000. The maximum reduction in the rate of SSC was increased, and the pool of eligible individuals was expanded to include some with a higher wage rate. This change effectively doubled the yearly total cost of the in-work benefit, raising it to a total of approximately 160 million euros in nominal terms, which represented

the SWB—in the post-reform period.

[4] *Loi visant à octroyer une réduction des cotisations personnelles de sécurité sociale aux travailleurs salariés ayant un bas salaire* (Law aimed at granting a reduction in personal social security contributions for salaried workers with low wages).

about 0.07% of the GDP in 1999. Therefore, in this section, we will describe the rules applicable from 2000Q2, while the rules for 2000Q1 are reported only in Appendix 4.A.

Eligibility for the SWB is determined based on full-time equivalent earnings (FTEe), which represent the amount an individual would have earned if they had worked the legal maximum number of days in a month at their respective daily wage rate. In Belgium, the legal maximum number of days in a week is 5, which corresponds to approximately 22 days in a month, namely all Mondays to Fridays. This maximum number can differ for a limited number of sectors/professions. The Belgian National Social Security Office (NSSO) classifies a job as full-time if an individual is normally required to work this legal maximum amount. Note that an individual with a full-time contract may work fewer days than this legal maximum if they started or ended the job mid-month or if they experienced unpaid leave, or similar interruptions that are not covered by the salary. Therefore, to compute their FTEe, the NSSO multiplies the monthly gross labour earnings by the ratio of the number of days effectively worked by the individual to the legal maximum amount. Similarly, the NSSO classifies a job as part-time if the normal amount of time worked, as specified in the contract between the employee and the firm, is less than the maximum legal amount.[5] An individual with a part-time contract can also work less than the amount specified in the contract if they are sick or on unpaid leave, for example. Moreover, for an individual with a part-time contract, the employer must declare not the days worked in a month, but the number of hours worked in a month. Knowing that each working day lasts 8 hours (40 hours per week) until the end of 1998, 7.8 hours (39 hours per week) until the end of 2002, and 7.6 hours (38 hours per week) from 2003 onwards, the ONSS easily converts the hours worked into the number of working days. Then, as for individuals with a full-time contract, to compute the FTEe, the gross earnings of these individuals are multiplied by the ratio of this number of days to the legal maximum number of days.

Notice that for some jobs, the legal maximum number of working days in a week is 6 days instead of 5. These individuals do not work more hours than those with a 5-day-a-week contract, but instead work fewer hours per day. However, their legal maximum number of days in a month will be higher, at around 26 days. The difference does not affect the SWB that individuals are

[5]The difference can be minimal, even just one hour. This explains why there is a rather dispersed distribution in the time worked by individuals with a part-time contract.

entitled to. Indeed, assume that one of these individuals works 13 days in a month and their normal legal maximum is 26 days; they will still be considered as working 50% of the time over the month. Moreover, this difference between the 6-day and 5-day-a-week contracts only affects the reported number of days worked for individuals with a full-time contract. For part-time contracts, since the employer declares the hours worked and not the days, no difference will arise.

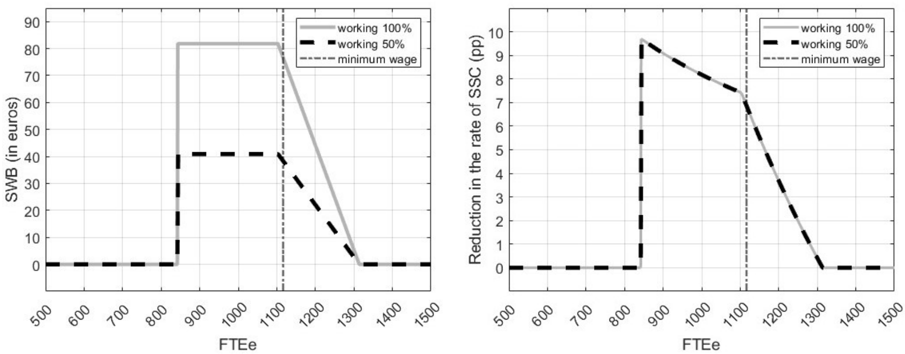
The SWB is calculated separately for each job an individual may hold in a month. Consequently, when we refer to the FTEe and working time, we are discussing the values associated with a single job within a month. At the end of this section, we will address the implications for individuals—approximately 10% of our sample—who hold multiple jobs within a month.

Figure 4.1, left panel, illustrates the monthly SWB in real terms that each individual receives as function of the FTEe from 2000Q2. We adjust all monetary values for inflation using the consumer price index provided by the Belgian National Statistical Office, with the value for 2000Q2 serving as the baseline. Specifically, we show the amount received by an individual working 50% of the time alongside that received by an individual working 100% of the time. The individual working 50% of the time receives half the nominal amount compared to the second individual. However, as shown in the right panel of the same figure, they experience the same reduction in percentage points in the rate of SSC. Indeed, if an individual works half the time for the same wage rate, they will also pay half of the nominal amount of SSC. Consequently, to achieve the same reduction in the rate of SSC, they must receive half of the nominal amount of the SWB.

Examining the figure in more detail, we can observe that below a lower threshold of €842, the SWB is zero. It remains constant between €842 and €1103, leading to a reduction in the rate of SSC by 9.7 and 7.4 pp. Between €1103 and €1313, the SWB gradually decreases (i.e. there is a phase-out region), returning to zero when FTEe reaches the upper threshold of €1313. Consequently, the reduction in the rate of SSC decreases from 9.7 pp to zero. Please refer to Table 4.A.2 for the formulas used to compute the SWB.

Figure 4.1 also displays the monthly legal minimum wage (represented by the black dash-dotted line) that individuals are entitled to receive by law if the work the maximum legal amount. Since all payments an individual receives over the year are included when computing the monthly minimum wage[6], the FTEe may fall below this minimum wage for some months of the year. However, overall, the SWB targets individuals earning approximately the minimum wage. Speculatively, this might explain why, in April 2000, the pool of eligible individuals was expanded to include some with higher FTEe, compared to the rules applied in 2000Q1. The presence of the monthly legal minimum wage also explains why the policymaker did not assign the SWB to individuals with FTEe below €842. This is likely the result of the assumption that no individuals have such a low level of FTEe.

Figure 4.1. SWB from 2000Q2



Notes: The left panel displays the SWB in euros received as a function of FTEe for an individual working the legal maximum amount of time per month in a job, as well as for an individual working half of this legal maximum. The right panel illustrates the resulting reduction in the rate of SSC in pp as a function of FTEe. The black dash-dotted line represents the monthly legal minimum wage.

The results presented in Figure 4.1 refer to white-collar (intellectual) workers. Blue-collar (manual) workers, in contrast, receive a level of SWB that is 1.08 times greater than white-collar workers. This higher value arises because blue-collar workers pay SSC on 108% of their gross remuneration, whereas white-collar workers pay SSC on 100% of theirs. This difference exists because blue-collar workers personally contribute to financing their

[6]In Belgium, there is no legal minimum hourly wage. However, there is a minimum monthly level of gross earnings that should be paid on average over a year to an individual working full-time. To compute this average, monetary payments made during the year, such as the yearly bonuses and the 13th-month salary, if available, are included. This amount is proportionally rescaled for part-time workers.

legal annual holidays (about 20 days a year), rather than their employers. As a result, since they pay higher SSC than white-collar workers, they also benefit from a higher SWB, allowing them to achieve the same reduction in the SSC rate as white-collar workers at the same wage rate. Moreover, since white-collar workers receive a salary from their employer for the legal annual holidays, these holidays are considered as days worked, and the salary for these days is included in the computation of the FTEe. Symmetrically, since blue-collar workers do not receive a salary from their employer for the legal annual holidays, they are not counted as working days for these individuals. This ensures that the FTEe of blue-collar workers remains the same in the month(s) when they take holidays, as both gross earnings and days worked will be lower. Instead, for both blue and white-collar workers, a salary is received from their employer for public holidays (10 days a year), which are then counted as working days.

Although the SWB has a legal incidence on employees and comes at no cost to the employers, it is not automatically assigned to the employees. Employers are required to report the monthly remuneration and the number of workdays (or hours for part-time workers) to the Belgian social security institution. The National Office of Social Security provides employers with a software programme to calculate the amount of the SWB based on the reported remuneration and workdays. One may wonder whether this reduction in SSC is correctly applied and whether employers might exploit this reduction by offering lower wages. Fortunately, these concerns are alleviated by the fact that most employers in Belgium do not compute the SSC themselves; they outsource this task to service providers known as *secrétariat social*. These providers apply the relevant legislation to calculate the SSC amount that both employees and employers need to pay. Therefore, there is anecdotal evidence that the reform, while not automatic, has been widely implemented. We can further verify this claim using our data from 2003Q1. For that quarter, we can compute the SWB individuals should have received based on their FTEe and compare it with the SWB they actually received. We can only perform this check for 2003Q1, as it is the first quarter for which we had access to data regarding the SWB received by employees, and we find a satisfactory match between the two statistics: For individuals for whom the SWB is reported in the data, we estimate an average monthly SWB of €21.1, compared to an observed value of €21.8. For individuals not receiving the SWB in the data, we predict an average monthly SWB of

€0.7—more details are provided in Appendix 4.B.

The reduction in the rate of SSC results in an increase in taxable income. Consequently, not all the savings in SSC provided by the SWB are actually retained by employees. In Section 4.3, we explore in greater depth the role of personal income tax in determining the change in the rate of SSC granted by the SWB.

During the period covered by our study, from 1997Q1 to 2002Q4, no significant reforms took place in Belgium's tax and benefit system.[7] Although the SWB was introduced as part of a broader package of reforms to individual taxation, which was passed on 10 August 2001[8], the other changes did not take effect until 2003 and 2004.[9]

We said that the SWB is calculated separately for each job an individual may hold within the same month. It should now be clear that if an individual has two jobs in a given month with the same wage rate, the amount of time worked in each job does not matter. For both jobs, they will receive the same reduction in the SSC rate, which is based on the wage rate. However, if an individual has two different low-wage rates, they will experience a different reduction in the rate of SSC for each job.

4.3 Theoretical predictions

In this section, we analyse the 2000 reform using a standard static neoclassical labour supply model. Given that few individuals may have received the SWB in

[7]For a thorough review of all reforms during this timeframe, please refer to Decoster et al. (2015) and the *Mémento fiscal* reports published by the Belgian Research and Documentation Service of the Ministry of Finance.

[8]*Loi portant réforme de l'impôt des personnes physiques* (Law on the reform of personal income tax).

[9]The legal maximum number of hours worked per week was reduced on two occasions: from 40 to 39 hours on 1 January 1999, and from 39 to 38 hours on 1 January 2003. Notably, the latter change was voluntary between July 2002 and January 2003. While the second reform falls outside our period of analysis, the first did not directly affect our observations. Indeed, for individuals with full-time contracts, the number of working days did not change due to these reforms. Similarly, for individuals with part-time contracts, there was no change in the way their hours were counted or any necessity to adjust their working time. Only the few individuals who were working just one hour less than the maximum legal limit are now classified as full-time employees. Moreover, in Section 4.7, we do not observe any divergent trends between our treatment and control groups from 1997/1998 to 1999 that would suggest a differential impact of this reform.

2000Q1, we discuss the introduction of the SWB based on the rules established from 2002Q2 onwards, assuming that the SWB was zero prior to 2000Q2. However, the same conclusion would still apply if we were to assume that the SWB set from 2000Q2 onwards simply reinforced the SWB introduced in 2000Q1. We also present the case for a white-collar worker; however, the same argument holds for blue-collar workers.

Each individual has a time endowment of $L_{\max}$. Since the SWB is calculated on a monthly basis, we assume that one period corresponds to a month, measuring the time endowment in days over this period. Let L_{law}, where $L_{\text{law}} < L_{\max}$, represent the legal maximum number of days an individual is permitted to work in a month. In Belgium, L_{law} is typically 22 days per month. Next, we define h, $h \leq L_{\text{law}}$, as the number of days worked by the individual.

Since our sample primarily consists of individuals with FTEe approximately the legal minimum wage, it is likely that labour income constitutes their main source of earnings. We denote the daily wage, gross of employees' SSC, by w, and the nominal rate of employees' SSC by τ. The rate τ applies to every additional euro earned by the individual. Labour earnings, net of employee SSC are subject to personal income taxation; in Belgium, the primary tax on taxable income is the federal tax rate. Let T' represent the marginal federal tax rate faced by an individual with a given taxable income.[10] Finally, R is the consumption the individual could achieve by setting $h = 0$, while remaining on the virtual linearised budget. Therefore, the individual's consumption is given by $C = (1-T')\cdot(1-\tau)\cdot w\cdot h+R$. The individual chooses the optimal number of days worked to maximise their utility function $\mathrm{U}(C, L) = \mathrm{U}(C, L_{\max}-h)$, which is strictly increasing at a decreasing rate in both C and leisure, subject to the time constraint $h \leq L_{\text{law}}$ and the budget constraint. Finally, FTEe $= w \cdot L_{\text{law}}$.

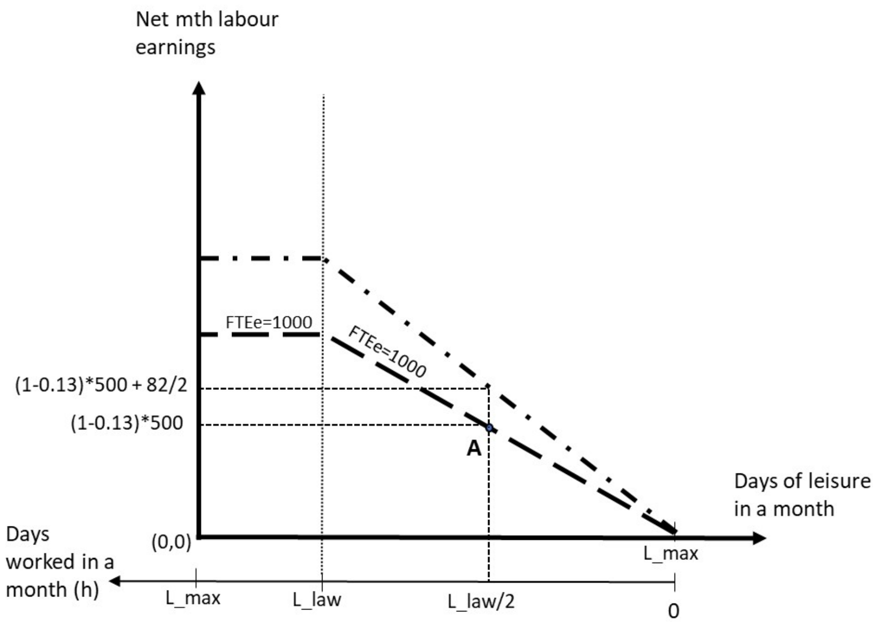
Assume, for simplicity, that $T' = 0$ and $R = 0$, which implies $C = (1 - \tau) \cdot w \cdot h$. In the pre-reform period, $\tau = 0.13$. Consider an individual who, in a given month after 2000Q1, has FTEe below €842 or above €1313. In this case, the individual is not eligible to receive the SWB. Thus, for these individuals, the budget set remains unaffected by the SWB. Instead, individuals with FTEe between €842 and €1060 receive a SWB $= 82 \cdot h/L_{\text{law}}$. If we set $\alpha = 82/L_{\text{law}}$, then SWB $= \alpha \cdot h$, and the consumption set for these individuals becomes $C = (1-0.13)\cdot w\cdot h+\alpha\cdot h = [(1-0.13)\cdot w+\alpha]\cdot h$, meaning that the SWB corresponds

[10] Since Belgium has a system of withholding tax, the net wage that employees receive each month is also determined by federal taxation.

to an increase in the wage rate, net of the employees' SSC. Similarly, let us assume that the post-reform wage rate gross of employees' SSC is such that $w' \cdot h_{\text{law}} = K' \in [1103, 1313]$. Then, these individuals receive a SWB equal to $[82 - 0.39 \cdot (K' - 1103)] \cdot h/h_{\text{law}}$. If we define $\beta = [82 - 0.39 \cdot (K' - 1103)]/h_{\text{law}} \geq 0$, which is independent of h, we can rewrite $C = [(1 - 0.13) \cdot w' + \beta] \cdot h$. Therefore, also for individuals with FTEe between €1103 and €1313, the SWB is equivalent to an increase in the wage rate net of employee's SSC.

For illustrative purposes, the dashed line of Figure 4.2 presents the budget constraint for an individual with FTEe = €1000 in the pre-reform period. Notably, individuals with the same wage rate but different numbers of working days are all positioned along this same line. If this individual maintains FTEe of €1000 in the post-reform period, their budget constraint becomes $C = [(1 - 0.13) \cdot w + \alpha] \cdot h$, where $\alpha = 82/L_{\text{law}}$. Thus, the budget constraint rotates clockwise, being represented by the dashed-dotted line.

Figure 4.2. Individual budget constraint before and after the reform



Notes: The dashed line represents the budget constraint in the pre-reform period for an individual with FTEe = €1000, who was working $h = \frac{L_{law}}{2}$ days, resulting in gross labour earnings of €500. After the reform, the individual, maintaining the same working time and FTE, obtains a SWB of $h \cdot \frac{82}{L_{law}} = \frac{82}{2}$.

For all individuals with $h < L_{\text{law}}$ and FTEe $\leq$ €1313 at the time of the reform announcement, the increase in the wage rate triggers both the substitution and income effects. There is some evidence that the substitution effect dominates the income effect when the wage rate is low (Cahuc et al., 2014). Given that the SWB targets individuals earning approximately the monthly minimum legal wage, this may also be the case in our framework. In that case, we would observe an increase in labour supply and, possibly, the working time of the eligible individuals due to the policy.

If an individual has $h = L_{\text{law}}$, and h cannot be higher than the legal maximum number of days, then we could not observe an increase in working time. Of course, this does not mean that we would observe a decrease: it could well be that the individual, since they cannot increase their working time, remains at the level $h = L_{\text{law}}$. In reality, in some cases, employees can work more than the legal maximum number of days. The main instrument to achieve this is voluntary supplementary hours of work (*heures supplémentaires volontaires*), which can amount to up to 100 hours per year. Thus, the employee can work a few hours or days above the legal limit. Since these extra hours must be remunerated at a higher rate than normal hours, this would lead to an increase in the FTEe. In turn, the increase in FTEe determines a decrease in SWB received, which could then reduce the attractiveness of increasing working time above L_{law}. Finally, employees can be asked to work more than the maximum amount in a month by their employers. In this case, however, the extra working time must be compensated with some compulsory days of rest so that, on average over the quarter, L_{law} is not surpassed.

Given that for individuals who already work the maximum amount L_{law} before the reform, the possibility to increase working time is limited, we will separate our analysis into individuals with a full-time contract just before the reform implementation and those with a part-time contract. However, remember that, despite individuals with a full-time contract being supposed to work L_{law}, they may still work less because they take periods not covered by remuneration, such as periods of sick leave. If employees have some control over these periods, we could imagine that they may respond to the policy by increasing the time worked at their expense.

Similarly, the SWB rules that apply to white and blue-collar workers are the same; however, they are subject to different rules concerning short-time work. Short-time work is an instrument that firms can use to place employees in temporary unemployment. In such cases, the employee would receive an allowance paid by the social security institutions equal to 60–70% of their gross remuneration instead of the salary from the employer. Specifically, blue-collar jobs are eligible for short-time work for reasons that do not apply to white-collar workers (e.g., adverse weather conditions, lack of work due to economic reasons). While there is some evidence that unionised workers may exert pressure to utilise short-time work to avoid lay-offs in the event of negative economic shocks—see Biancardi et al. (2022) for Italy—we do not know if a single employee may influence the amount of short-time work they receive or whether this is solely determined by the firm. If the former is true, blue-collar workers might have more opportunities to respond to the policy than their white-collar counterparts. This consideration motivates our decision to examine the heterogeneity in agents' responses to the introduction of the SWB along this dimension in Sub-section 4.7.2.

Notice that, instead of examining the impact of the reform in the post-reform period separately for individuals with part-time and full-time contracts just before the reform, we could divide our sample directly by considering the time worked by each individual pre-reform. In this paper, we use the first approach to determine working time pre-reform, as we believe it is more robust to temporary fluctuations in time worked, such as those caused by adverse weather conditions. However, in Section 4.7.2, we assess whether the results are robust to the second approach.

So far, we have set $T' = 0$. However, in most cases, individuals' labour income is subject to a positive marginal tax rate. In Belgium, the main component of personal income taxation is the federal tax rate. Since the SWB reduces the SSC, it indirectly raises taxable income. As a result, the reduction in the marginal tax rate due to the SWB is partially offset by federal taxes. For example, consider individuals benefiting from the maximum SSC reduction of approximately 9.7 pp provided by the SWB. If their marginal federal tax rate is 40%, their actual reduction in the overall marginal tax rate would be 5.82 pp, decreasing from 47.8% to 41.98%. The federal tax rate thus reduces the effective impact of the SWB. This effect is smaller if, for the same wage rate, the individual works fewer days and has a lower taxable income. For instance, if the same individual were subject to a marginal federal tax rate of 20%, their reduction in the overall marginal tax rate would have been of 7.76 pp. This illustrates how the reform's effects can vary for individuals with the same wage rate, depending on the amount of time worked before the reform; this is an additional reason to analyse separately the impact of the reform on individuals with a part-time or full-time contract pre-reform.

Under the strong assumption that for low-wage employees the only source of income is labour income, we can estimate the marginal federal tax rate for single individuals in the sample used in our analysis. However, we are unable to precisely determine the marginal tax rate for households with more than one member, as we lack data on the number of dependent children, and the marginal tax rate varies depending on this factor. Our findings in Appendix 4.C indicate that, for single individuals whose main job was part-time at the end of 1999Q4, the marginal federal tax rate ranged from 20% (for the treated individuals) to 25% (for the control individuals) on average. For single individuals with a full-time job, the marginal tax rate ranged from 31% (for the treated individuals) to 37% (for the control individuals) on average.

One caveat we have yet to discuss is that the increase in taxable income could push an individual into a higher federal tax bracket, for example, from 25% to 30%. In this case, the individual would not experience a reduction in the marginal tax rate due to the SWB but rather an increase. However, given that the maximum SWB amount an individual could receive in a year is €984 (€82 $\cdot$ 12), and the difference between the various tax brackets ranges from a minimum of €2,000 (but is often much higher, as shown in Table 4.C.1), it is unlikely that many individuals will change tax brackets because of the SWB.

Finally, the standard static neoclassical labour supply model assumes perfectly elastic labour demand at a given wage rate. This does not mean an individual can work any number of days for the same employer, but that they can secure a job with more days at the same wage rate with another employer if desired. In reality, however, labour demand is imperfectly elastic, and certain combinations of days worked may not be available. Therefore, when estimating the reform's effect, some robustness checks will include control variables that likely capture restrictions in labour demand, such as sector type and employer location.

4.4 The data

In Belgium, the Crossroads Bank for Social Security (CBSS) collects individuals' administrative data from Belgian social security institutions. The data cover a wide range of demographic characteristics and labour market outcomes.

From the CBSS datasets, we selected a random sample of approximately

100,000 individuals from the population of those who: I) Were between 21 and 60 years old at the end of 1999. These limits ensure that our sample consists of individuals likely to be in salaried employment, which is the focus of our analysis. Indeed, since we track these individuals over time, the 21-year-old limit ensures that they are at least 18 years old during the first quarter of observations and are, therefore, likely to have completed their secondary education, which typically ends at 18 in Belgium. Similarly, we exclude individuals approaching retirement; II) Remained consistently in salaried employment across all quarters of 1999, where an individual is considered to be in salaried employment in a quarter if they hold at least one active job at the end of that quarter; III) Were blue-collar or white-collar workers at the end of 1999Q4, excluding public servants, as they are ineligible for the SWB. This restriction is applied because we do not examine whether the reform influences individuals' decisions to exit public service employment; IV) Had at least one quarter in 1999 with a job with monthly FTEe below €1680 (in real terms, with 2000Q2 as baseline). This sample was selected to examine the responses of individuals in salaried employment to the policy. Therefore, it includes individuals who were attached to the labour market and did not consistently have high wages, as the SWB targets the left tail of the FTEe distribution. The €1680 limit was based on our ex-ante calculation using publicly available BCSS statistics, indicating that this limit would have allowed us to reduce the number of individuals in our sample with earnings always significantly higher from the eligibility threshold—€1313—while still maintaining a comparable number of individuals below and above this threshold.

Figure 4.3 presents the histogram of FTEe in €25 bins for these 100,000 individuals for 1999Q4. For each individual, we represent the FTEe of their main active job in the quarter, which is the job active at the end of the quarter or, if multiple jobs are active at the end of the quarter, the one with the highest FTEe. Alongside this, it shows the reduction in the SSC rate granted by the SWB as a function of FTEe. It is evident that most individuals in our sample have relatively high FTEe compared to the required level needed to receive the SWB. For this reason, as presented in detail in Section 4.5, we exclude from our baseline analysis individuals with FTEe above €1,413 in 1999Q4 to ensure that our treatment and control group have comparable FTEe. This results in the exclusion of approximately 73,000 individuals from our sample. Similarly, we exclude individuals with FTEe below €842 in 1999Q4, as such extremely low values are likely due to reporting errors. This restriction removes approximately 750 individuals from our sample.

For each individual in the sample, we collected demographic data and labour market outcomes spanning three years before and three years after the reform's implementation, specifically from 1997Q1 to 2003Q1. The demographic data are available only on an annual basis, recorded as of December 31 each year. Since our analysis is conducted quarterly, we assume that the value reported at the end of the year applies to all quarters of the same year. Among the demographic data, we have gender, age, nationality—this variable is grouped into four categories: Belgian national, EU-15, EU-28, and other countries—district (*arrondissement*) of residence—we aggregate the districts into provinces, of which there are 11 in Belgium, if the Brussels region is considered a province—household size—which identifies the total number of individuals in the household the individual belongs to—and household type—we group this variable in 6 categories, namely i) individuals who are living on their own, ii) married without children or unmarried cohabitant without children, iii) married with children or unmarried cohabitant with children, iv) head of a single-parent family, v) individuals aged over than 18 living with the parent(s) and vi) other person living under the same roof or resident in a collective household. We use these variables as controls—i.e., changes in family composition may influence the individual's labour supply decision—or for partitioning the sample to identify potential heterogeneous effects. Note that the CBSS data does not include information on educational attainment.

Labour market outcomes are recorded quarterly, either describing the situation on the last day of the quarter or presenting a total for the entire quarter. We have detailed information on how many jobs individuals had in a given quarter, how much the individual was working and earning in each of these jobs over the quarter, whether each job was under a part-time or full-time contract, and whether it was classified as a blue-collar or white-collar occupation. From the information on how much the individual has earned and worked in a job in a quarter, the BCSS computes the FTEe of the individual for that job.[11] Additionally, we know the job's industry type (one-digit NACE, which is the statistical classification of economic activities in the European Community), the size of the employer (employers can have

[11]we can manually do the same using the information on working time and earnings, and we find the same value for this statistic.

fewer than 5 employees, [5,9], [10,19], [20,49], [50,99], [100,199], [200,499], [500,999], >1000 employees), and the district (*arrondissement*) where the employer is located. We aggregate the districts into provinces. We also know the SWB received in each job by eligible individuals, but only for 2003Q1, which is the first quarter BCSS provides data on this statistic.

For full-time contracts, employers report the days worked by an individual, while for part-time contracts, they report hours worked. As individuals may transit between part-time and full-time contracts or hold both types within a quarter, we standardise the unit of measurement by converting hours into days: hours worked are divided by 8 for 1997Q1–1998Q4, by 7.8 for 1999Q1–2002Q4, and by 7.6 from 2003Q1 onwards. As described in Section 4.2, some individuals may work more than the normal number of days specified in their contract within a given quarter. For example, in the sample used in the empirical analysis, approximately 1.5% of observations with a full-time contract in 1999Q4 worked more than 66 ($22 \cdot 3$) days in that quarter, despite 66 days being the upper normal limit. Of these, 90% did not work more than 70 days. Almost no individuals with a part-time contract worked more than 66 days in 1999Q4. However, there exists another, larger group of individuals—about 9% of those with a full-time contract in 1999Q4—who worked approximately 78 ($26 \cdot 3$) days. These are individuals who, by contract, divide their weekly working hours across six days rather than five. Consequently, while they do not actually work more hours than those with a five-day-a-week contract, they appear to work more days in 1999Q4. This issue becomes particularly problematic if individuals transition from a six-day to a five-day regime (or vice versa) over time. Such changes could result in a misleading decrease (or increase) in working time, even when no actual change has occurred. Therefore, to allow for individuals who genuinely work beyond their usual contractual hours while simultaneously mitigating distortions caused by these regime changes, we winsorise the total number of days worked in a full-time job within a quarter to 68, 69, or 70 days (depending on the quarter). This cap corresponds to the normal number of days worked in a quarter—64, 65, or 66—plus four days, so as to account for about the 90% of the individuals who actually work some extra days in a quarter than the normal maximum amount. As robustness checks, instead of winsorising the working time, we rescale the working time for individuals with a 6-days-a-week contract to make it equivalent to 5 days a week. Indeed, for each individual, with some approximation, we know how much they work compared to the normal maximum amount for their contract. For example, if we know that an individual works 39 days but only 50% of the maximum amount, we can deduce that their maximum amount is 78 days. Then, if for individuals with a 5-days-a-week contract the maximum is 66 days in that quarter, we can assume that this individual is working 33 days. Unfortunately, we know only the percentage of work of an individual with respect to the maximum in brackets of 10%, making this conversion relatively imprecise. Finally, as an alternative robustness check, we remove the winsorisation altogether.

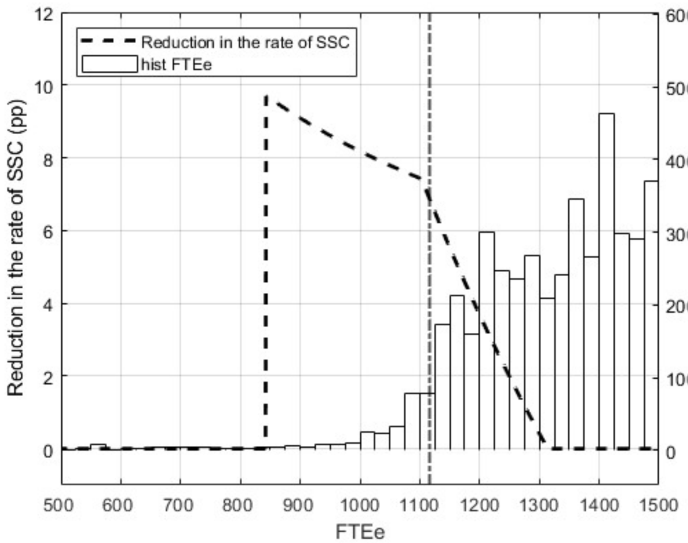
Note that working time and earnings for each job held in a quarter are reported as totals for the entire quarter, making it impossible to determine their distribution across the quarter. Thus, we assume individuals have the same FTEe for all months within a quarter. This may differ from actual FTEe if working time and earnings are unevenly distributed across the quarter. As a result, the predicted reduction in the rate of SSC (and, consequently, the SWB) may differ from the actual reduction that the employee receives. However, as shown in Appendix 4.B, the difference between the predicted and observed SWB is limited, implying that the average FTEe we can compute from the data are a good proxy for the real FTEe of the individuals over the quarter.

4.5 Empirical strategy

To estimate the causal impact of receiving the SWB on the quarterly number of days worked, we employ the DiD approach. To implement this method, we select, from our sample of individuals who satisfy conditions I) to IV) specified in Section 4.4, a group of individuals likely to have FTEe below €1313 in the post-reform period—thus qualifying for the SWB—and a group likely to have FTEe above €1313—thus not receiving the SWB. We then compare the evolution of the total number of days worked between these two groups from 1997Q1 to 2002Q4. All quarters between 1997Q1 and 1999Q4 are in the control period, while the treatment period is from 2000Q1 to 2002Q4. Recall that in 2000Q1, the SWB amount was lower, and fewer individuals may have received it compared to 2000Q2 onward. Therefore, in some specifications, we will include 2000Q1 as part of the control periods.

We use the FTEe from the individual's main job at the end of the quarter to predict whether they are likely to have high or low FTEe in the post-reform period. Specifically, we classify individuals as treated by the reform if their

Figure 4.3. Histogram of FTEe and reduction in the rate of SSC



Notes: The bars represent the histogram of FTEe - bins of €25 - for individuals in salaried employment in 1999Q4 who are part of our random sample of about 100,000 individuals, with the number of individuals indicated on the right vertical axis. The dashed line is the reduction in the rate of SSC in pp as a function of FTEe from 2000Q2. The black vertical dash-dotted line indicates the minimum wage level.

FTEe were between €842 and €1263 in 1999Q4. We do not consider individuals with FTEe between €1263 and €1313 in 1999Q4 as treated because, as shown in Figure 4.D.1, they likely have FTEe above €1313 post-reform, making them ineligible for the SWB.

For the same reason, not all individuals with FTEe between €842 and €1263 in 1999Q4 will necessarily continue to have low FTEe in every post-reform quarter. As shown in Figure 4.D.2, the proportion of treated individuals whose FTEe are above €1313 in a given post-reform quarter increase over time. On average, this proportion is around 22% in the post-reform period. Therefore, our strategy captures only the intent-to-treat effects (ITT) of the reform.

Conversely, individuals with FTEe between €1363 and €1413 in 1999Q4 are assigned to the control group. Individuals with FTEe slightly above €1313 are excluded for two reasons. Firstly one could argue that if treated individuals increase their working time at the expense of the control individuals, those most affected are likely to be individuals whose FTEe are slightly above those of the treated individuals. Secondly we exclude these individuals to reduce the risk of including individuals in the control group whose FTEe are likely to fall below €1313 in the post-reform period—Figure 4.D.3 shows that around 23% of the individuals with FTEe between €1313 and €1363 in 1999Q4 who remain in salaried employment in any given post-reform quarter have FTEe below €1313. Instead, for our control group of individuals with FTEe between €1363 and €1413, Figure 4.D.4 shows that, on average, only around 9% of these individuals who remain in salaried employment in any given post-reform quarter have FTEe below €1313.

In Section 4.3, we showed that, for individuals with full-time contracts in 1999Q4, the possibility of increasing working time is limited. Consequently, we divided both the treatment and control groups into two partitions based on whether their main job in 1999Q4 was part-time or full-time. The summary statistics in Section 4.6 demonstrate that these two groups exhibit different behaviour during the pre-reform period. This results in 8,090 treated and 2,111 control individuals whose main job was part-time in 1999Q4, and 6,966 treated and 3,853 control individuals with a full-time job. Not surprisingly, there are more individuals with sufficiently low FTE to be included in the treatment group and holding a part-time contract than those with a full-time contract, suggesting a positive relationship between working time and wage rates.

Let us define individuals as i and quarters as t, where t spans from 1997Q1 to 2002Q4. $Y_{i,t}$ represents the total number of days worked by individual i in quarter t across all jobs held by the individual during that quarter. In some specifications, as a robustness check, $Y_{i,t}$ represents the total labour earnings gross of employees' SSC across all jobs of individual i in quarter t. Indeed, if an individual works more or less time for a relatively similar wage rate due to the policy, we would expect to see a corresponding change in gross labour earnings. We classify $Y_{i,t}$ as not reported if the individual is not in salaried employment during quarter t. This implies that for some individuals, there may be missing observations for one or more quarters $t \in$ [1997Q1, 1998Q4] or $t \in$ [2000Q1, 2002Q4]—remember that all individuals in our sample are employed in 1999.

We define the individual unobserved fixed effects by a_i, and T_t represents time dummies, one for each quarter. These time dummies capture the temporal evolution of the outcome in the control group. Let us define D_i as a dummy variable that takes the value 1 if an individual is in the treatment group and the value 0 if the individual is in the control group. Finally, in some robustness checks in Section 4.7.1 we include a set of time-varying covariates $X_{i,t}$ to control for observed changes in the composition of the treatment and control group. To avoid introducing a bias in the estimation, the covariates must remain unaffected by the treatment. We begin by evaluating whether the results of our estimation change when dummy variables for family type and a discrete variable for the number of household members are included. Alternatively, we assess whether the results change when dummies for industry type and whether the individual is a blue-collar or white-collar worker are added. The results of both regressions need to be interpreted with caution, as we cannot exclude that the increase in the wage rate thanks to the policy may lead to changes in family composition, or that individuals might choose to work in a different sector and/or contract type to achieve the desired change in working time.

We then estimate the following event study model[12]:

$$Y_{i,t} = \alpha_0 + \alpha_t \cdot T_t + a_i + \beta_t \cdot T_t \cdot D_i + \phi \cdot X_{i,t} + u_{i,t}. \tag{4.1}$$

This equation's primary parameters of interest are the coefficients β_t, which denote the interaction between the dummy variable identifying whether an individual is in the treatment group and the time dummies. Specifically, the estimated β_t values indicate the effect of being part of the treatment group on the quarterly number of days worked for each quarter before and after the reform, with 1999Q4 serving as the reference quarter. If the coefficients for β_t are not jointly different from zero for the quarters preceding the policy implementation—indicating that, prior to the reform, the treatment and control groups were following the same trend in the number of days worked—then we can confidently interpret the coefficients for the quarters following the reform implementation as the average causal impact of the reform on the treatment group—strictly speaking, this is an ITT given that some observations are non-compliant in the post-reform period.

While the event study model is intuitive for assessing the presence of pre-trends, we also present the results of the standard DiD model. Indeed, the DiD enables us to quickly obtain a single estimate of the average effect of the reform in the post-reform period. In this model, instead of the interaction between the time dummies and D_i, we incorporate the dummy $W_{i,t}$, which takes the value 1 for individuals in the treatment group during the post-treatment period —

[12]For a survey of the DiD literature, including event study regressions, please refer, among others, to Roth et al. (2023), and de Chaisemartin and d'Haultfoeuille (2024).

specifically, from 2000Q1 onward — and zero otherwise. Given that it is likely few eligible individuals were receiving the SWB in the first months of 2000, in some specifications, we assume that 2000Q1 is also included in the pre-reform period.

$$Y_{i,t} = \alpha_0 + \alpha_t \cdot T_t + a_i + \beta \cdot W_{i,t} + \phi \cdot X_{i,t} + u_{i,t}. \tag{4.2}$$

To interpret the results of our estimation strategy, we assume that the reform does not incentivise individuals to exit salaried employment. If this were the case, any observed increase in working time among individuals who remain in salaried employment could be attributed to the reform encouraging those with the lowest working time to leave salaried employment. However, while an income effect may trigger a reduction in working time at the intensive margin if it is stronger than the substitution effect, there is no clear reason to expect an increase in the wage-rate to drive treated individuals out of salaried employment. Some might still argue that the policy could discourage work in households where one member receives the SWB, leading another member to leave salaried employment due to the increased household income. To address this concern, one may want to repeat the analysis, focusing exclusively on single individuals in 1999Q4. Unfortunately, our sample size is too small to allow for a separate analysis of the approximately 10% of the observations in our sample that pertain to single individuals.

However, any change in working time may reflect a shift at the intensive margin[13] and/or the reform increasing the retention rate in salaried employment. For instance, assume the reform encourages some treated agents to remain in salaried employment—this scenario is supported by the study of Dewatripont and López Novella (2024), who find an increase in the retention rate in salaried employment for individuals receiving the SWB. If these individuals work, on average, more days than those who would have stayed without the reform, this would positively affect the working time of the treated group. On the other hand, if the reform retains employees with lower working time than those who would have remained in salaried employment without it, this would negatively affect the working time of the treated

[13]Notice that for the reform to have an impact at the intensive margin, we would not necessarily need treated individuals to have an absolute increase in their days worked. The reform could still have a positive effect on the working time of treated individuals at the intensive margin if, for example, it motivates them to maintain their level of timed worked, which would have otherwise decreased without the reform.

individuals.

Let us now assume that we find the reform increases the working time of treated individuals post-reform—In Section 4.7, it will become clear why we discuss this scenario in detail. If the reform were to increase the retention rate of individuals who work a low amount of time, then the intensive response would need to be strongly positive to offset this negative effect. However, a particularly strong positive response would contradict findings in the literature on ex-ante studies of SWB, which report only small positive effects at the intensive margin. It would also contradict evidence from studies on other in-work benefits, which also show a small impact on those already employed—in this latter case, a stronger reaction could be due to the fact that SWB does not have the disincentive effect on labour supply that is typically associated with in-work benefits based on gross labour earnings. For these reasons, our interpretation is that if the overall effect of the reform on the working time of treated individuals is positive, neither of the two effects is negative, even though we cannot disentangle the contributions of each.

One final challenge to interpreting the results arises from individuals who leave salaried employment in a post-reform quarter but subsequently re-enter salaried employment in a later quarter. It is possible that these individuals re-enter salaried employment because they anticipate, or realise upon receiving a job offer, that by accepting a job they will receive a higher net wage as a result of the SWB. Similarly to before, if some individuals re-enter salaried employment thanks to the exemption and have relatively high working time compared to those already in salaried employment, an increase in working time will be observed in the treated group in comparison with the control group. Conversely, the opposite holds true if these individuals who re-enter have relatively low working time. To evaluate the extent to which a relatively small group of individuals re-entering salaried employment might affect our estimation results, we also estimate the model under the assumption that, once an individual exits salaried employment during the post-reform period, their observations are excluded from all subsequent quarters.

4.6 Summary statistics

Before presenting the results of our empirical approach, we first provide some summary statistics on the composition of the treatment and control groups in 1999Q4. This is followed by graphs showing the distribution of days worked in 1999Q4, as well as the average quarter-to-quarter change in the number of days worked and FTEe.

In Table 4.1, the second and third columns present summary statistics on the composition of the treatment and control groups in 1999Q4 for individuals whose main job at the end of 1999Q4 was a part-time job. Although our empirical approach does not require the treatment and control groups to be balanced across these dimensions, it is reassuring that they are balanced for the majority of them. There are only three notable differences: the treatment group consists of younger individuals who work fewer days per quarter, and it has a higher proportion of employees living with their parent(s). These differences likely reflect the fact that wage rates are lower at the beginning of the career, and that young individuals are more likely to still live with their parents. In Sub-section 4.7.2, to assess whether differences in age may impact the results, we partition our treatment and control groups into three age groups: individuals aged between [21,35] years, between (35,50], and between (50,60] at the end of 1999Q4. We define a relatively wide interval to categorise young people because the relatively small number of observations does not allow for finer partitions. Interestingly, about 5% of individuals in both the treatment and control group held more than one job at the end of 1999Q4. While categorised as part-time in the baseline analysis, we will exclude this 5% of multiple job-holders in a robustness check, as they may not accurately represent individuals working less than the maximum amount of time in a quarter. Additionally, around 10% of individuals held two or more jobs during 1999Q4, with 4% having more than one job at some point in the quarter but only one job by the end. This last percentage reflects the number of individuals who changed jobs during 1999Q4.

For full-time contract holders in 1999Q4, more substantial differences are observed between the treatment and control groups. Notably, the treatment group has a higher proportion of female workers, indicating that there are more females with full-time jobs and low FTEe than males. Similarly, there are less blue-collar workers in the treatment group. These differences justify our robustness checks, where we partition our sample into males and females, as well as blue-collar and white-collar workers. As expected, full-time individuals are less likely to hold multiple jobs than their part-time counterparts. However, the proportion of full-time workers who likely changed jobs during 1999Q4 is similar to that of individuals with a part-time contract.

Since the total number of days worked in a quarter is our outcome of

Table 4.1. Descriptive statistics for the treatment and control group for 1999Q4

	Part-time in 1999Q4		Full-time in 1999Q4	
	Treatment	Control	Treatment	Control
Females (%)	84.44%	83.66 %	64.94%	48.79%
Belgian national (%)	88.07%	93.13%	88.73%	89.88%
Average age	35.4	37.2	33.2	33.7
Singles (%)	10.79%	8.86%	13.62%	13.57%
Single parent (%)	7.64%	8.05%	4.52%	3.92 %
Couple without children (%)	17.92%	16.77%	20.97%	20.37%
Couple with children (%)	46.81%	54.71%	31.57%	37.40%
Living with the parent(s) (%)	13.36%	8.81%	23.73%	20.53%
Avg number of family members	3.08	3.13	2.90	2.93
Blue collars	61.50%	60.35%	58.70 %	63.51%
Avg days worked in 1999Q4	33.59	37.61	58.51	60.60
Median days worked in 1999Q4	33.33	35.26	64	65
Multiple jobs in 1999Q4	9.65%	8.67%	4.89%	5.92%
Multiple jobs end of 1999Q4	5.96%	5.21%	1.46%	1.71%
Changing job	3.94%	3.43%	3.63%	4.12%
N individuals	8,090	2,111	6,966	3,853

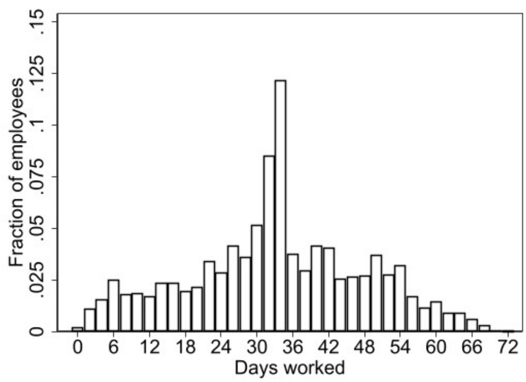
Notes: This table presents summary statistics for 1999Q4 for individuals whose main job at the end of 1999Q4 was part-time or full-time. The control group includes individuals with FTEe between €842 and €1263, while the treatment group includes those with FTEe between €1363 and €1413. The proportion of individuals with multiple jobs in 1999Q4 refers to those with more than one job during the quarter, while the proportion of individuals with multiple jobs at the end of 1999Q4 refers to those with multiple active jobs at the quarter's end. The proportion of individuals who changed jobs is calculated as those with multiple jobs during 1999Q4 but only one job at the end of the quarter. We winsorise the days worked for full-time jobs to 70 days for 1999Q4. See Section 4.4 for the definitions of the other variables.

interest, Figure 4.4 shows the distribution of days worked in 1999Q4 for treated agents. As expected, the dispersion in working days is relatively small for full-time workers but significantly higher for those with part-time contracts. Indeed, most full-time employees work close to the maximum legal limit, approximately 66 days, in 1999Q4. About 9% of full-time employees work for 70 days. This results from our winsorisation process, where we assume that all full-time jobs are capped at this number in 1999Q4. Despite the winsorisation, some individuals may hold multiple jobs during the quarter, resulting in a total sum of days worked exceeding 70 days. However, this occurs in only 0.7% of treated employees with part-time contracts in 1999Q4 and 1.25% of treated full-time employees in 1999Q4, which is why we mostly do not have observations where the total working time in 1999Q4 exceeds 70 days. Similar conclusions apply to individuals in the control group as shown in Figure 4.E.1. However, control individuals holding a part-time job as their main job at the end of 1999Q4 exhibit a distribution of working days shifted to the right compared to the treatment group. This reflects their higher average and median number of days worked reported in Table 4.1.

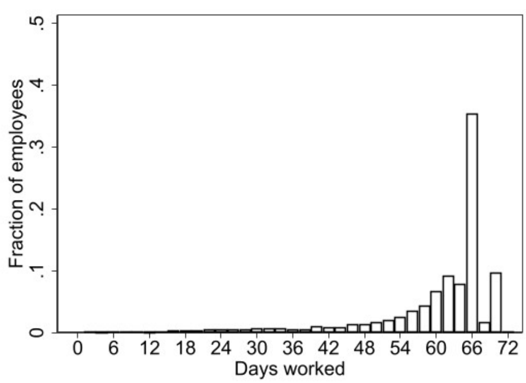
In Figure 4.E.2, we show that dividing the treated part-time and full-time workers into white-collar and blue-collar groups uncovers further heterogeneity in the distribution of working days by contract type. Only around 20% of blue-collar, full-time employee workers exactly the maximum number of days in a quarter. In contrast, nearly all white-collar workers (approximately 90%) work the maximum amount. First, these findings reflect that annual legal holidays are not counted as working days for blue-collar workers, and second, that blue-collar workers are eligible for short-time work, whereas white-collar workers are not. Unfortunately, we cannot disentangle the two causes. They also support our heterogeneity analysis, based on whether the job is white or blue-collar, in Sub-section 4.7.2. One should remember that, apart from short-time work, employees may experience periods not covered by remuneration for other reasons, such as periods of sick leave, maternity/paternity leave, or unpaid time off. This explains why not all white-collar employees work the maximum amount. Instead, the distribution of working time for both part-time blue-collar and white-collar workers is relatively similar, suggesting that contractual hours are similarly dispersed for both groups.

One might wonder whether the distribution of days worked, as shown in Figures 4.4 and 4.E.1, is influenced not only by individuals having different contractual days or taking varying periods of not covered by remuneration during the quarter, but also by individuals who take on or drop a second job, change jobs mid-quarter, or start working partway through the quarter. To eliminate the impact of these latter factors, we repeated the analysis for a subset of treated individuals who held a single job throughout 1999Q3 and 1999Q4 and had the same employer at the end of both 1999Q2, 1999Q3 and 1999Q4. Figure 4.E.3 shows that this subset of treated individuals exhibits about the same distribution of days worked as our treatment group, indicating that there is indeed a genuine dispersion in contractual time and/or periods not covered by remuneration among employees. Consequently, this also confirms that full-time employees may work fewer days than the maximum amount due to periods not covered by remuneration.

Figure 4.4. Days worked in 1999Q4 for the treated employees



(a) Part-time jobs



(b) Full-time jobs

Notes: The figure shows the histogram of the quarterly number of days worked in 1999Q4, with bins of 2 days, for treated individuals with part-time or full-time contracts at the end of 1999Q4 - 8,090 and 6,966 individuals respectively. We winsorise the days worked for full-time jobs to 70 days for 1999Q4.

From quarter to quarter, we may expect an individual to change their working time if they switch contract types, such as moving from a part-time to a full-time contract, or vice versa. A change in working time may also occur if an individual experiences a reduction or increase in the number of jobs they have simultaneously. Moreover, working time may change if an individual moves from one part-time contract to another that requires a different amount of working time. Additionally, both for part-time and full-time contracts the number of days not covered by remuneration may fluctuate, even when the contract remains the same. Finally, all agents may see an increase in working time if, in a given quarter, they work beyond the days normally specified in their contract. If significant variations in individual working time occur between quarters during the pre-reform period, this may indicate that individuals could also have the opportunity to adjust their working time in the post-reform period. Therefore, we examine the changes in working time pre-reform, specifically between 1999Q3 and 1999Q4, separately for individuals with a part-time and a full-time contract at the end of 1999Q4. Notice that for both quarters, for individuals with a five-day-a-week contract, there are 66 working days. Therefore, we winsorise the number of days worked for individuals with a full-time contract in both quarters to 70.

Figure 4.5, left panel, illustrates the change in working time between 1999Q3 and 1999Q4 for our 8,090 treated individuals with a part-time job at the end of 1999Q4. We observe that many of these individuals experience variations in their working time from one quarter to the next, with the average change equal to 2.5 days and the absolute average change equal to 6.1 days. We observe that the change in days worked is more pronounced for the subset of individuals who changed employers between 1999Q3 and 1999Q4 (4.92% of the 8,090 individuals in the sample). The average (absolute average) change for this group is 1.6 (12.3) days. Similarly, the change is greater for individuals who transitioned from a full-time contract in 1999Q3 to a part-time contract in 1999Q4 (2.36% of the sample), with an average (absolute average) change of -7.7 (15.9) days. However, changes in the days worked are still observed for individuals who stayed with the same employer and maintained a part-time contract in both 1999Q3 and 1999Q4 (93.65% of the sample). In this case, the average (absolute average) change is 2.7 (5.6) days. This suggests that fluctuations in working time can also occur when an individual does not change employer or contract type, possibly due to variations in the duration of the part-time contracts, the number of non-remunerated days or the number of legal holidays.[14]

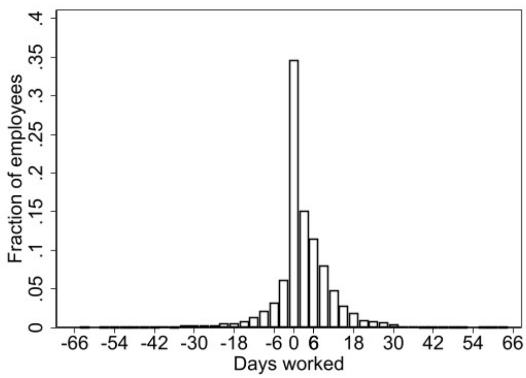
Figure 4.5, right panel, illustrates the change in working time between 1999Q3 and 1999Q4 for our 6,966 treated individuals who held a full-time job at the end of 1999Q4. Many of these individuals experienced significant variations in their working time from one quarter to the next, with the average (absolute average) change amounting to 4.3 (9.0) days. The variation in days worked is more pronounced for those who changed employers between 1999Q3 and 1999Q4 (4.45% of the 6,966 individuals in the sample). For this group, the average (absolute average) change is 7.0 (14.3) days. Similarly, individuals who transitioned from a part-time contract in 1999Q3 to a full-time contract in 1999Q4 (2.76% of the sample) experienced a larger variation, with an average (absolute average) change of 18.7 (20.5) days. However, changes in the number of days worked are also present among individuals who remained with the same employer and have a full-time contract in both 1999Q3 and 1999Q4 (93.80% of the sample). For this group, the average (absolute average) change is 3.9 (8.6) days, indicating that non-remunerated days or days of legal holidays can fluctuate considerably from one quarter to another.

One might question whether also FTEe change considerably from quarter to quarter. Indeed, our empirical approach assumes that FTEe in 1999Q4 are a strong predictor of FTEe in the post-reform period. However, this assumption breaks down if there is significant individual-level variation in FTEe. To address this concern, in Figure 4.6 for treated individuals with a part-time (left panel) and full-time job (right panel) at the end of 1999Q4, we examine the change in FTEe between 1999Q3 and 1999Q4. Encouragingly, FTEe are notably more stable than working time, with few individuals showing deviations exceeding ±€50.

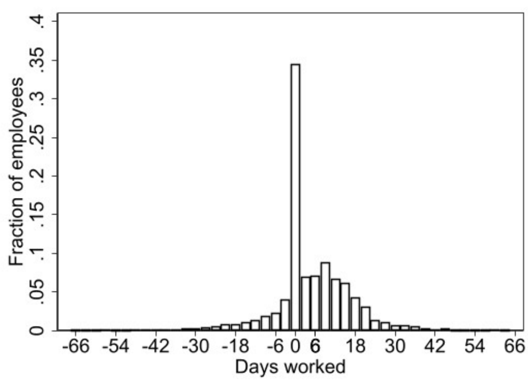
It remains unclear whether the variation in days worked is related to the change in FTEe. Indeed, the underlying assumption of neoclassical labour supply theory is that individuals can work more (or less) time for the same wage rate. However, if this assumption does not hold true, specifically if there is a positive (or negative) correlation between changes in time worked and

[14]The fact that the distribution of legal holidays for blue-collar workers may vary throughout the year further justifies the heterogeneity check when we divide part-time workers into white/full-collar workers. However, as shown in Table 4.1, the proportion of blue-collar workers is balanced between the treatment and control groups, suggesting that, even if the distribution of legal holidays varies throughout the year, this should apply equally to both the treatment and control groups.

Figure 4.5. Change days worked between 1999Q3 and 1999Q4 - Treated employees



(a) Part-time jobs

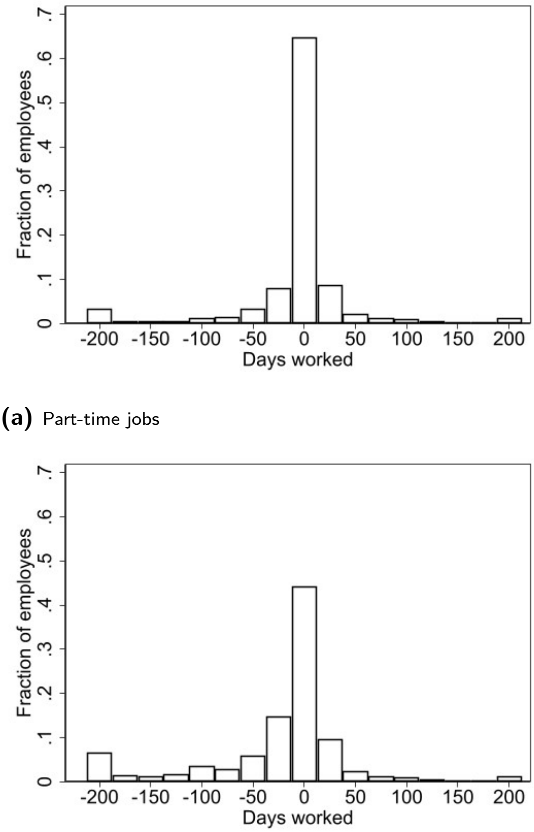


(b) Full-time jobs

Notes: The figure shows the histogram of the change in days worked between 1999Q3 and 1999Q4, with bins of 3 days, for those treated individuals with a part-time (8,090) and full-time (6,966) job at the end of 1999Q4.

FTEe, this could influence the impact that the SWB can have. For instance, consider a scenario in which an individual, due to the income effect, desires to reduce their working time. However, if this decision requires them to accept a reduction in their wage rate, the individual may ultimately choose not to reduce their working time at all. In our data, the correlation between changes in days worked and FTEe between 1999Q3 and 1999Q4 for individuals who

Figure 4.6. Change FTEe between 1999Q3 and 1999Q4 - treated employees



(a) Part-time jobs

(b) Full-time jobs

Notes: The figure shows the histogram of the change in FTEe between 1999Q3 and 1999Q4, with bins of €25, between 1999Q3 and 1999Q4 for those treated individuals with a part-time (8,090) and full-time (6,966) job at the end of 1999Q4. To plot this graph, we winsorise the change in FTEe to ±200.

have a part-time job as their main job at the end of 1999Q4 is -0.093 for the treatment group and -0.068 for the control group. For individuals who have a full-time job as their main job at the end of 1999Q4 the correlation is -0.015 for the treatment group and -0.086 for the control group. The values are relatively small, hence mitigating the aforementioned risk.

4.7 Empirical results

Figure 4.7 shows the estimated β_t values from equation (4.1), calculated using a within-regression estimator for individuals whose main job at the end of 1999Q4 was a part-time contract. We observe that pre-reform, the treatment and control groups exhibited similar trends in the number of days worked. Formally, we do not reject the null hypothesis that the β_t coefficients are jointly equal to zero in the pre-reform period.[15] Post-reform, we see an increase in the days worked in the treatment group compared to the control group. Consistent with our suspicion that few individuals actually received the SWB at the beginning of 2000, no increase in days worked is observed in 2000Q1 or 2000Q2. Moreover, the initial increase in the value of the coefficients following the implementation of the reform could correspond to the observation that treated individuals may need time to respond to the policy. At the same time, as time progresses, the proportion of individuals in the treatment group whose FTEe exceed €1313—and who therefore no longer benefit from the exemption—also increases. This could explain why, after a few quarters, the coefficients β_t stabilise.

Table 4.2. DiD estimates on the quarterly number of days worked - main job is part-time at the end of 1999Q4

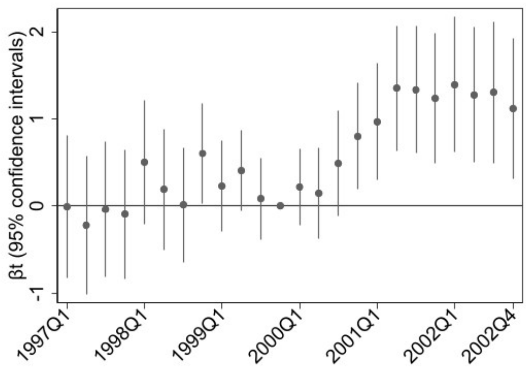
β	0.860***
(se)	(0.261)
P-value	0.001
[95% ci]	[0.350 1.373]
R-sqr	0.015
N obs	213,629
N individuals	10,201

Notes: This table shows the result of estimating equation (4.2) using a within-regression estimator. The post treatment period starts with 2000Q2 and and no control variables $X_{i,t}$ are included in this regression. sd are clustered at the individual level. ***,**,** denote statistical significance at the 1%, 5% and 10%, respectively.

Table 4.2 reports the results of estimating equation (4.2), assuming 2000Q1 is within the pre-reform period. The reform is found to have increased days worked by 0.86 days, statistically significant at the 1% level. The estimated

[15]We find that the F-statistic for the Wald test is 1.32, and the significance level of the test is 20.48%.

Figure 4.7. Event study estimates on the quarterly number of days worked - main job is part-time at the end of 1999Q4



Notes: This figure illustrates the coefficients β_t estimated from equation (4.2) using a within-regression estimator. Standard errors are clustered at the individual level. The treatment and control groups consist solely of individuals whose main job was part-time at the end of 1999Q4. The reference quarter is 1999Q4. No control variables $X_{i,t}$ are included in this regression.

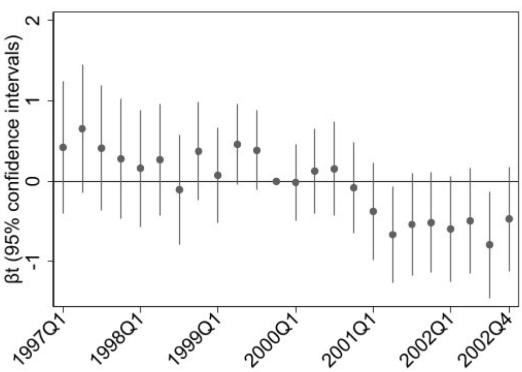
effect is substantial. For the treatment group during the post-treatment period, had the reform not occurred, the quarterly average number of days worked is approximately 35.4. An increase of 0.86 days thus represents a significant rise of about 2.43% in the number of days worked. The average reduction in the rate of SSC for the treatment group during the post-reform period (excluding 2000Q1) is approximately 3.39 pp, which correspond to a a net wage increase of approximately 3.90.%[16] These results suggest that the elasticity of days worked with respect to the net wage rate is 0.62. One might be concerned that the proportion of individuals in the control group earning below €1,313 in the post-reform period could bias our estimates. However, we find that the control group experiences, on average, only a 0.29 pp reduction in the rate of SSC after the reform, alleviating this concern.

For individuals whose main job at the end of 1999Q4 was a full-time contract, Figure 4.8 shows that for two quarters post-reform, β_t is negative and statistically significant. While this may suggest that, for individuals

[16]This assumes that the individuals remain in the same marginal tax brackets after they receive the SWB. Further discussion on the role of the federal tax rate is provided in section 4.3.

working close to the maximum legal number of days, the income effect may prevail over the substitution effect, the robustness of this conclusion is cast into doubt by two main observations. First, we should note that the coefficients β_t appear to decrease over time, starting from the pre-reform period. Therefore, the treatment and control groups may have been following different trends pre-reform, which are not detected due to the low power of our regression. Second, in the next subsections on robustness checks and heterogeneity analysis, we will show that for individuals with a full-time contract in 1999Q4, the parallel trend assumption is violated in some key specifications.

Figure 4.8. Event study estimates on the quarterly number of days worked - main job is full-time at the end of 1999Q4



Notes: Same as Figure 4.7.

Our estimated effect for individuals with part-time jobs in 1999Q4 is higher than the results from discrete-choice labour supply models for Belgium, which report an elasticity at the intensive margin between 0 and 0.1 (Bargain et al., 2014; de Mahieu, 2021). Similarly, in their review of Western Europe and the US, Bargain and Peichl (2016) find that own-wage labour elasticity at the intensive margin is relatively small. However, neither piece of evidence accounts for whether an individual's main contract was full-time or part-time. As a result, they do not specifically present results for individuals who are likely to experience a more significant change in incentives due to a change in their wage rate. Moreover, we find that the SWB has a more positive impact on the time worked by people already in employment compared to other in-work benefits (Immervoll and Pearson, 2009; OECD, 2011; Laun, 2019; Brewer and Hoynes, 2019; Kleven, 2024). An explanation for this more positive outcome is the eligibility criterion based on FTE rather than earnings, which allows individuals to increase their working time without losing the in-work benefit.

As discussed in Section 4.5, our empirical approach cannot determine whether the observed increase in time worked results from greater working hours at the intensive margin or from individuals with relatively high working hours remaining in salaried employment due to the reform. This suggests that our estimate, when considering the intensive margin elasticity, may be upward biased. However, regardless of the effect (or combination of effects) that contributes to the observed outcomes, since the increase in participation also determines an increase in the time worked, we can conclude that the policy has increased the time worked among the treated group.

As addressed in the previous section, we cannot rule out the possibility that when an individual leaves salaried employment in the post-reform period, they may later return to salaried work if they anticipate that accepting a job will lead to a higher net wage due to the SWB. In fact, one of the main reasons for introducing in-work benefits is to make salaried employment more financially attractive than unemployment or inactivity. Therefore, we repeat our baseline analysis but assume that once an individual exits salaried employment in the post-reform period, their observations are excluded for all subsequent quarters. Notice that, among our treated (control) individuals with a part-time contract at the end of 1999Q4 there are 1,401 (303) individuals who, after exiting salaried employment in one of the post-reform quarters, re-enter salaried employment. When re-estimating Equation (4.1)—see left panel of Figure 4.F.1—we find that the estimated effect remains consistent with our baseline results, although the increase in days worked post-reform is less pronounced. When estimating Equation (4.2), the ITT is 0.53, statistically significant at the 5% level (vs. 0.86). This suggests that part of the observed increase in working time in the baseline may be attributed to the reform encouraging some individuals, who were no longer working, to re-enter salaried employment and join with relatively high working hours. However, this only accounts for part of the originally estimated effect and could still be considered, in itself, a positive outcome of the reform. If we repeat the same procedure for individuals with a full-time contract at the end of 1999Q4—respectively 1,065 and 519 individuals from the treatment and control groups re-enter salaried employment after having exited for one or more quarters post-reform—we do not observe any change in the estimated values for β_t, which remains non-significant post-reform. See the right panel of Figure 4.F.1.

4.7.1 Robustness checks

In this section, we assess whether our findings for individuals with a part-time contract as their main contract at the end of 1999Q4 are robust when we apply different assumptions to define the treatment and control groups, include control variables in $X_{i,t}$ in the estimated model, or exclude individuals holding more than one job at the end of 1999Q4. We then examine the impact of the reform when the outcome variable is total gross labour earnings, rather than days worked. Additionally, we assess the robustness of the results when we change the method used to identify individuals who worked less than the maximum legal limit in 1999Q4. Specifically, instead of looking at whether the contract of the main job at the end of the quarter was part-time or full-time, we examine the actual time worked in that job during 1999Q4—remember that Figure 4.4 has shown that some individuals with part-time contracts work close to the maximum legal amount in a quarter, while others with full-time contracts work relatively few days during a quarter. For individuals with a full-time contract at the end of 1999Q4, the results of all these robustness checks are reported in Appendix 4.G. At the end of this section, we also assess the robustness of our results to our winsorisation at 68, 69, or 70 days (depending on the quarter) for full-time jobs. We first test the robustness of our results by rescaling the number of days worked for individuals with a full-time 6-days-a-week contract, rather than applying a winsorisation. Finally, we assess the robustness of our results when we remove any winsorisation altogether. The results of all these robustness checks are compared to the estimation results from the previous section, which are referred to as the baseline results.

We begin by examining whether the effect of the reform remains robust to changes in the limit on FTEe used to define the treatment and control groups. Specifically, we expand the treatment group to include all agents with FTEe between €842 and €1313 in 1999Q4, rather than between €842 and €1263. This adjustment brings into the treatment group individuals who are less likely to benefit from a significant reduction in the rate of SSC during the post-reform period. The size of the treatment group increases to 10,476 individuals (up from 8,090). In Figure 4.D.1, we show that the average number of individuals with a part-time contract and FTEe between €1263 and €1313, who remain employed in a post-reform quarter and have FTEe above €1313 in that quarter, is 48% (an increase from 22% in the baseline). Consequently, the average number of treated individuals with FTEe between €842 and €1313 in 1999Q4, who remain employed in a post-reform quarter and have FTEe above €1313 in that quarter, is 28%. The results from estimating Equation (4.1) remain similar – see Figure 4.G.1. However, the positive effect appears to attenuate more quickly than in the baseline as time passes, consistent with the fact that we include individuals in the treatment group who are increasingly likely not to receive the SWB. We also estimate Equation (4.2), treating 2000Q1 as part of the control period, and find that the number of days worked increases by 0.83 (statistically significant at the 1% level) days (compared to 0.86 in the baseline).

Second, we increase the FTEe thresholds for inclusion in the control group. Specifically, we now include only individuals with FTEe between €1413 and €1463, instead of those with FTEe between €1363 and €1413. This adjustment results, when looking at the agents with a part-time contract at the end of 1999Q4, in a control group of 2,847 individuals (compared to 2,111 in the baseline). The rationale for this change is that, while it might lower the likelihood of these individuals following the same trend in days worked as the treated group during the pre-reform period, it also decreases the number of individuals in the control group with FTEe below €1313 post-reform. As shown in Figure 4.D.5, the average proportion of individuals in this control group who remain employed in a post-reform quarter and have FTEe below €1313 decreases to 7.17%, compared to 9.20% in the baseline. The estimation results of Equation (4.1) in Figure 4.G.2 continue to indicate an increase in working time post-reform for treated individuals. However, some discrepancies in the evolution of working time between the treatment and control groups arise before the reform, particularly in the first pre-reform year. This highlights the importance, for the validity of our analysis, of having individuals in the control group with FTEe similar to those in the treatment group.

Thirdly, we further examine the extent to which "imperfect compliance"—i.e., treated (control) individuals with FTEe above (below) €1313 in the post-reform period—might influence our estimation results. We adopt an alternative specification by defining the treatment and control groups based on the FTEe over two quarters, specifically 1999Q3 and 1999Q4. Individuals are classified as treated if they have FTEe between €842 and €1263 in both quarters, and as part of the control group if their FTEe fall between €1363 and €1413 in both quarters. For individuals with a part-time contract at the end of 1999Q4, the treatment group comprises 7,298 individuals instead of 8,090, while the control group includes 1,253 individuals instead of 2,111. This approach allows us to exclude individuals with more volatile FTEe and to reduce the proportion of treated (control) individuals with high (low) FTEe in the post-reform period. The average share of individuals in the treatment group with high FTEe in the post-reform period decreases to 20.05% from 22.09%. Moreover, the average share of individuals in the control group with low FTEe in the post-reform period decreases to 6.20% from 9.20%. The treatment effect is robust with our baseline results when estimating Equation (4.1)—see Figure 4.G.3—and when estimating Equation (4.2). Specifically, the ITT is 0.70 (statistically significant at the 5% level).

Fourth, we demonstrate how the results change when control variables are included in our baseline regressions. We start by adding a set of controls to account for changes in family composition: a set of dummies identifying family status (i.e., single, single parents, individuals living with their parent(s), married/cohabiting individual with children, and married/cohabiting individual without children) and the number of household members—remember that these variables are recorded on an annual basis, meaning they retain the same value across all quarters within each year. This addition is based on the assumption that changes in family structure could directly influence an individual's labour supply decisions. In our sample of treated individuals, approximately 36% of them had a different family status at the end of one year between 1997 and 2002 (excluding 1999) compared to the end of 1999. Similarly, about 49% were in households with a different number of members than at the end of 1999. Interestingly, Figure 4.G.4 shows that the estimated effect remains almost identical—the ITT from estimating equation (4.2) is equal to 0.92 (statistically significant at the 1% level). These results suggest that changes in the composition of our treatment and control groups across these dimensions are relatively minor.

Similarly, one could include a set of dummy variables for the employer's sector, the province where the employer is located, and the contract type (blue-collar or white-collar) among the regressors of our baseline model. Different sectors, labour market conditions where the employer is located, and contract types may impose specific constraints on agents' ability to respond to the policy. Approximately 38% of our treated individuals worked in a different sector between 1997Q1 and 2002Q4 compared to the sector in which they worked in 1999Q4. Similarly, 29% had an employer in a different province than in 1999Q4. Additionally, 17% had a different type of contract – either blue-collar or white-collar – in one of these quarters compared to 1999Q4. We find that the treatment effect remains very similar to our baseline results when estimating Equation (4.1)—see Figure 4.G.5. Similarly, when estimating Equation (4.2), the ITT is 0.91 (statistically significant at the 1% level). As for the family controls, these results indicate that changes in the composition of our treatment and control groups along these dimensions are relatively minor.

Fifth, we assess whether our regression results for individuals whose main job was part-time at the end of 1999Q4 are robust to the exclusion of approximately 5% of individuals who held more than one active job at that time—treatment group reduces to 7,608 individuals and the control to 2,004 individuals. Not surprisingly, given the small number of individuals with multiple jobs, the treatment effect remains consistent with our baseline results – see Figure 4.G.6. The ITT values from estimating Equation (4.2) are 0.90 (statistically significant at the 1% level).

Sixth, one may use, as an outcome variable, not the total number of days worked by individual i in quarter t across all their jobs, but rather the total gross labour earnings by the individual in quarter t across all their jobs. When estimating equation (4.1) – see Figure 4.G.7—we observe some divergence in the evolution of quarterly gross labour earnings between the treatment and control groups during the pre-reform period. However, this divergence is relatively small when compared to the post-reform increase in the treatment group relative to the control group. When estimating equation (4.2), assuming that 2000Q1 is within the pre-reform period, we find that the reform has increased quarterly gross earnings by €47 (statistically significant at the 5% level). For the treatment group during the post-treatment period had the reform not occurred, the quarterly average gross earnings would be approximately €2190. Thus, our estimate implies that the reform has increased quarterly gross earnings by about 2.1%, which is approximately the increase observed in working days. This result is reassuring, as any increase in working time must also be accompanied by a roughly equivalent increase in gross earnings, assuming that the wage rate an individual can receive remains relatively stable across periods.

Seventh, instead of dividing the sample based on whether an individual's main contract in 1999Q4 was part-time or full-time, we split the sample according to the number of days worked in their main contract at the end of

1999Q4. First, we tested the alternative sample selection where we included in both our treatment and control groups only individuals who worked 40 days or fewer in the main job at the end of 1999Q4. This led us to have a treatment group of 6,568 individuals (compared to 8,090 at baseline) and a control group of 1,603 (compared to 2,111 at baseline). This procedure resulted in the exclusion from the treatment (control) group of our baseline analysis of 2,303 (801) individuals who had a part-time contract in 1999Q4 but worked more than 40 days in 1999Q4, and the inclusion of 784 (293) individuals who had a full-time contract in 1999Q4 but worked fewer than 40 days. We also tested the sample selection where we included only individuals who worked 50 days or fewer in the main job by the end of 1999Q4.[17] Both for individuals working fewer than 40 and 50 days in 1999Q4, we observe an increase in the number of days worked post-reform. This aligns with our baseline results from estimating Equation (4.1), as shown in Figure 4.G.8. The ITT values from estimating Equation (4.2) are 0.80 and 0.62 (both statistically significant at the 5% level), respectively. These results highlight that the baseline results for individuals with a part-time contract were not driven by the proportion of individuals with a part-time contract working a high number of hours. They also suggest that full-time individuals who work only a few days in a quarter are likely to respond similarly to the policy as part-time individuals. However, the more individuals with relatively high working days in 1999Q4 we included in our treatment and control groups, the smaller the effect of the reform is—i.e. the ITT decreases when we include individuals who work between 40 and 50 days in our analysis. This suggests that individuals working a high number of days may not react to the policy.

Finally, we assess the robustness of our results to our winsorisation at 68, 69, or 70 days (depending on the quarter) for full-time jobs. We first rescale the number of days worked for individuals with a full-time 6-days-a-week contract, rather than applying a winsorisation. While we present results for both individuals with part-time and full-time jobs in 1999Q4, the former

[17]This led us to have a treatment group of 8,222 individuals (compared to 8,090 at baseline) and a control group of 2,203 (compared to 2,111 at baseline). Moreover, it resulted in the exclusion from the baseline treatment (control) group of 1,116 (392) individuals who had a part-time contract in 1999Q4 but worked more than 50 days in 1999Q4, and the inclusion of 1,248 (484) individuals who had a full-time contract in 1999Q4 but worked fewer than 40 days.

group has little chance of being impacted by these changes.[18] When estimating (4.1), Figure 4.G.10 shows that the effects are mostly identical to our baseline. When we remove any winsorisation, but do not rescale the working time of individuals with a 6-days-a-week contract, we find that for full-time employees in 1999Q4, the estimated β_t coefficients are slightly more negative post-reform than in the baseline—see Figure 4.G.11. This is likely because, among the treatment group of full-time employees, there are more individuals with 6-days-a-week contracts than in the control group (9.32% vs. 6.96%). Indeed, assume that individuals with 6-days-a-week contracts in both the treatment and control groups are not affected by the reform. Then, if they have the same propensity to switch to a 5-days-a-week job or exit salaried employment post-reform, we would still observe a greater decrease in working days in the treatment group than in the control.

4.7.2 Heterogeneity analysis

In this section, we divide our sample based on dimensions that are unlikely to change over time at the individual level but may offer insights into the groups most affected by the reform. Firstly, as discussed in Section 4.3, blue-collar workers may have more flexibility in responding to the exemption, as their contracts allow for short-time work. Moreover, blue-collar and white-collar workers may react differently to the reform due to different preferences regarding the amount of time worked or distinct constraints in labour demand.

We first classify individuals with part-time contracts in 1999Q4 into employees with a blue-collar or white-collar contract. Following the same procedure used to obtain the baseline results, we then divide them into treatment and control groups. Among individuals with part-time blue-collar (white-collar) jobs in 1999Q4, 5,249 (3,358) are in the treatment group and 1,294 (847) are in the control group. Despite the potential differences between blue-collar and white-collar workers, the estimation of Equation (4.1) shows that both groups respond similarly to the policy—see the left panel of Figure 4.H.1 for blue-collar and the right panel for white-collar workers. For white-collar workers, the effect appears to be less precisely estimated, likely due to the smaller number of observations. The observation that the effect is not definitely more pronounced for blue-collar workers compared to

[18] In principle, the results of this group can also be impacted, as individuals with a part-time contract at the end of 1999Q4 could have full-time contracts in other quarters.

white-collar workers suggests that, even if blue-collar workers can be placed on short-time work by their employers, they may have little control over these days. We then repeat the same analysis for individuals with full-time contracts at the end of 1999Q4. Among those with full-time blue-collar (white-collar) jobs in 1999Q4, 4,089 (2,877) are in the treatment group, and 2,447 (1,406) are in the control group. Notably, in Figure 4.H.2, we observe that for full-time employees—both blue-collar and white-collar—the treated and control groups show significantly different pre-reform trends in the time worked. Specifically, pre-reform, the estimated β_t are positive and statistically significant for individuals with a full-time blue-collar job in 1999Q4, and negative and statistically significant for individuals with a full-time white-collar job. Thus, the seemingly parallel trend pre-reform in Figure 4.8 was likely the result of these two opposite trends cancelling out. This suggests that individuals with full-time contracts and FTEe low enough to be included in the treatment group may represent a specific segment of the employed population, whose employment prospects differ substantially from those with FTEe high enough to be included in the control group. As a result, our analysis may not fully capture the impact of the reform on the subset of the population working near the maximum amount.

Then, we partition our sample based on gender and estimate Equation (4.1) separately for individuals with part-time contracts in 1999Q4, distinguishing between male and female employees. Female employees constitute the majority of observations in our sample of part-time workers. Specifically, we observe 6,831 females and 1,259 males in the treatment group, and 1,776 females and 345 males in the control group. Unsurprisingly, being the majority of the observations, the females drive the results in Figure 4.7, which mirror those for the entire population—see Figure 4.H.3, left panel. For male employees, we do not observe an effect post-reform, as shown in the right panel of Figure 4.H.3, even though the number of observations is relatively small to obtain precisely estimated results. However, this finding is consistent with the literature on in-work benefits, which often reports stronger effects for females than for males. For female employees with a full-time contract in 1999Q4 (4,524 treated and 1,880 control observations), there is no observed change in working time following the reform. However, the coefficients β_t seem to be in a decreasing trend pre-reform, which prevents definitive conclusions from being drawn for this subset of the population—see Figure 4.H.4, left panel. In contrast, for males with full-time contracts (2,442 treated and 1,973 control observations), the treatment and control groups follow the same trend pre-reform. Post-reform, the treated group appears to show a reduction in working time compared to the control group, although this difference is not statistically significant—see right panel of Figure 4.H.4. It could be tempting to interpret this result as evidence that, for full-time males, the income effect may outweigh the substitution effect, leading to a decrease in working time—which would align with the findings of the ex-ante simulations for Belgium (Orsini, 2006; de Mahieu, 2021), showing a reduction in working time among individuals working more than the legal limit, who are predominantly men. However, we need to verify whether the observed result may also be caused by opposing pre-trends between full-time blue-collar and full-time white-collar employees that cancel each other out. It turns out that this is indeed the case. Figure 4.H.5 shows that the seemingly parallel trend pre-reform in the right panel of Figure 4.H.4 is due to different, opposing pre-trends for male full-time blue-collar (1,678 treated and 1,517 control observations) and male full-time white-collar employees (764 treated and 456 control observations).

We then verify whether the effect we observe varies based on the age of the individuals. Specifically, we repeat our baseline analysis for individuals who have a part-time contract at the end of 1999Q4 and are between [21,35], (35,50], and (50,65] years old in 1999Q4. In the first group, the treatment (control) group comprises 4,303 (934) individuals; in the second group, it comprises 3,141 (980) individuals; and in the last group, it comprises 646 (197) individuals. Interestingly, we find a positive and strong reaction among the younger treated individuals – see Figure 4.H.6—whereas there is no change post-reform for the oldest groups – see Figures 4.H.7 and 4.H.8. One possible explanation for this different reaction is that while younger individuals in our sample work more than older individuals—the average working time in 1999Q4 is 36.9 days for individuals between [21, 35] and 32.7 days for individuals between (35, 50]—they may have different types of contracts compared to older individuals. Namely, they are more likely to have temporary contracts and interim contracts[19] (Quintelier, 2020). It is possible that these types of contracts make it easier to modify the amount of working time, for example, when the individual renews the contract. We also conduct the same partition by age for individuals with a full-time contract in 1999Q4. For individuals between

[19] An interim job is the situation where an employee is employed by a temporary work agency with the purpose of being assigned to a client company

[21,35], the treatment (control) group consists of 4,469 (2,411); for individuals between (35,50], the treatment (control) group consists of 2,088 (1,220); and for individuals between (50,65], the treatment (control) group consists of 409 (222). We do not observe any impact of the reform on any of these three groups—see the right panel of Figures 4.H.6, 4.H.7, and 4.H.8. This is also consistent with the reform having an impact on individuals with unstable work arrangements, while individuals with a full-time contract are unlikely to fall into this category, irrespective of their age.

4.8 Conclusion

Many countries have adopted in-work benefits to make paid work financially more attractive. However, when eligibility is based on earnings, this may create a part-time trap, where individuals have little incentive to work additional time to avoid losing the benefit or may even have an incentive to reduce their working time to become eligible. In this paper, we study the introduction of the SWB, an in-work benefit introduced in Belgium in 2000 for individuals in salaried employment with a low-wage rate. Thanks to the eligibility criteria based on the wage rate, this benefit does not diminish if an individual works more at the same wage rate. We aim to assess the effect of the reform on the amount of time worked by individuals in salaried employment.

To conduct our analysis, we utilise an individual panel dataset obtained from the Crossroads Bank for Social Security (CBSS), which collects a comprehensive range of administrative information from Belgian social security institutions, and we apply a difference-in-differences (DiD) approach. Specifically, we compare the evolution of working days between two groups of individuals always in salaried employment in 1999 selected based on their wage rate in 1999Q4. The treated groups is made of individuals who, in 1999Q4, had a low-wage rate and were, therefore, likely to receive the SWB in the post-reform period, conditional on remaining in salaried employment. The control group consists of individuals with a higher wage rate in 1999Q4, who were unlikely to benefit from the SWB in the post-reform period.

When looking at the control and treatment individuals whose main job was part-time at the end of 1999Q4, both groups show a similar evolution in working time during the pre-reform period. Therefore, we can interpret the observed relative change in working time between the control and treatment groups in the quarters following the implementation of the reform as the average causal impact of the reform on the treatment group. We find that the reform increases the quarterly number of days worked by 0.86 days, which corresponds to a substantial elasticity of days worked with respect to the net wage rate of approximately 0.62. This finding is confirmed in a wide array of robustness checks where we apply different assumptions to define the treatment and control groups, include control variables to account for observed changes in the composition of the treatment and control groups, examine the impact of the reform when the outcome variable is total gross labour earnings, change the method used to identify individuals who worked less than the maximum legal. Instead, we do not observe an increase in working time for individuals with full-time contracts at the end of 1999Q4. However, for this latter group, in some robustness checks, the parallel trends assumption did not hold.

Furthermore, when examining whether the effect of the policy varies based on certain demographic characteristics of the beneficiaries, we find that the policy's impact is evident only among young individuals with part-time contracts in 1999Q4, while no effect is seen for older individuals. Instead, we still observe no effect on young individuals with full-time contracts. Given that young individuals are more likely to hold temporary or interim contracts (Quintelier, 2020), it is possible that these types of contracts make it easier to modify the amount of working time, for example, when the individual renews the contract. Similarly, the impact of the reform is present among part-time female employees, but does not seem present among males. However, the estimates for males are particularly imprecise due to the low number of observations in this group.

While we cannot determine the exact extent to which the effect for individuals working relatively short periods of time is due to an increase in the number of days worked among those who would have stayed in salaried employment without the reform, or whether the reform reduced the exit rate from salaried employment, the results still suggest that in-work benefits, when eligibility is based on the wage rate, lead to a substantial positive labour market response from certain subgroups of the treated individuals.

This study does not provide an answer regarding one of the primary margins that in-work benefits are supposed to affect, namely whether the SWB increases the entry rate from inactivity/unemployment to salaried employment. While it would be possible to perform this analysis using the same procedures employed in this paper, one major challenge of this avenue of research is that past FTEe are a poorer predictor of future FTEe in the case of individuals in unemployment and inactivity. Therefore, dividing these individuals into treatment and control groups based on their past wages may yield less informative results concerning whether they receive the SWB in the post-reform period. Still, this question deserves further investigation.

Appendix

4.A SWB rules in 2000Q1 and from 2000Q2 to 2002Q4

In this Appendix, we present the formulas for computing the SWB in Table 4.A.1 for 2000Q1, and in Table 4.A.2 for the period from 2000Q2 until the end of 2002Q2, which is the last quarter included in our analysis. Between 2000Q1 and 2000Q2, the nominal values of the FTEe required for eligibility for the SWB were increased to account for inflation. However, the real values remained unchanged.

Table 4.A.1. SWB for 2000Q1

White-collar		
FTEe	SWB	Reduction in the rate of SSC (pp)
≤ 843	0	0 pp
(853,1054]	$\text{FTEw} \cdot 64$	From 7.5 to 6.1 pp
(1054,1215]	$\text{FTEw} \cdot (64 - 0.39 \cdot (\text{FTEe} - 1,054))$	From 6.1 to 0 pp
$> 1,215$	0	0 pp
Blue-collar		
≤ 843	0	0 pp
(853,1054]	$\text{FTEw} \cdot 64 \cdot 1.08$	From 7.5 to 0 pp
(1054,1215]	$\text{FTEw}(64 \cdot 1.08 - 0.39 \cdot 1.08(\text{FTEe} - 1,054))$	From 6.1 to 0 pp
$> 1,215$	0	0 pp

Notes: The tables show the SWB applicable in 2000Q1, with all monetary values expressed in real terms, using the CPI for 2000Q2 as the baseline. Full-Time Equivalent working time (FTEwt) refers to the proportion of time an individual works in a month compared to the maximum possible amount.

4.B Observed vs simulated SWB

In this section, from our sample of approximately 100,000 individuals meeting conditions I–IV, we select a sub-sample of those who held at least one job in 2000Q1. Then, for each job an individual may have in 2003Q1, we calculate

Table 4.A.2. SWB from 2000Q2 to 2002Q4

White-collar		
FTEe	SWB	Reduction in the rate of SSC (pp)
≤ 843	0	0 pp
(843,1103]	FTEw $\cdot$ 82	From 9.7 to 7.4 pp
(1103,1313]	FTEw $\cdot$ $(82 - 0.39 \cdot (\text{FTEe} - 1,103))$	From 7.4 to 0 pp
$> 1,313$	0	0 pp
Blue-collar		
≤ 843	0	0 pp
(843,1103]	FTEw $\cdot$ 82 $\cdot$ 1.08	From 9.7 to 7.4 pp
(1103,1313]	FTEw $\cdot$ $(82 \cdot 1.08 - 0.39 \cdot 1.08 \cdot (\text{FTEe} - 1,103))$	From 7.4 to 0 pp
$> 1,313$	0	0 pp

Notes: The tables show the SWB applicable from 2000Q2 until the end of 2002Q2. The same conventions as in Table 4.A.1 are applied.

the predicted SWB based on the individual's working time and labour earnings and compare it with the actual SWB the individual receives.

A discrepancy between the predicted and observed SWB could arise from three factors. First, some employers may not have requested the SWB for their employees—in Section 4.2, we explain why this is likely to be a minor concern. Second, while the SWB is calculated on a monthly basis, our data on working time and labour earnings are available at the quarterly level. Therefore, we assume that the working time and earnings associated with each job are evenly distributed across the quarter. Third, the monetary variables provided by the CBSS are grouped at the quarterly level in intervals of €25. As a result, we cannot determine the exact SWB and earnings received by an individual in a given quarter. For example, if an individual receives a SWB between [€0,€25] in a quarter, we assume they receive €4.17 per month.

Despite these data limitations, for the approximately 28,000 jobs with observed SWB greater than zero in 2003Q1, the average monthly SWB observed in the data is €21.8, while our prediction provides an average of €21.1. The absolute average difference between the simulated and observed SWB is €6.2. In contrast, for the 61,382 jobs where the observed SWB is zero, our average simulated monthly SWB is €0.7. These results show that, despite our data on earnings and working time being at a quarterly level, we can still approximate the observed SWB relatively well. Similarly, the small predicted average SWB for individuals who, according to the data, do not receive the SWB suggests that very few eligible individuals fail to receive the SWB post-reform because their employer did not apply for it.

4.C Federal income tax rate

In 1999, the taxable income, which for low-wage employees can be assumed to be primarily determined by labour income after deducting both the employee's and employer's SSC, was subject to the rates listed in Table 4.C.1. The monetary values are expressed in real terms, using 2000Q2 as the baseline.

The Belgian legislator has also established a threshold that is exempt from the federal tax rate. For a single person, this threshold was approximately €5275 in 1999. For example, an individual with a taxable income of €5274 would have been taxed at 0% rate on an additional euro. However, an individual with a taxable income of €5275 would have been taxed at 25% on an additional euro, as it would fall within the first bracket of table 4.C.1. This exempted amount was lower for members of couples and higher for those with dependent children.

Using our data, we can easily compute the marginal federal tax rate paid by single individuals in 1999, assuming that the only source of income for these individuals was labour income. We perform this computation for the four subsamples of individuals used in our empirical analysis. The first two subsamples include the treated (8,090 individuals) and control (2,111) individuals who worked a limited amount of time prior to the reform. Specifically, these are individuals who satisfy conditions I-V of Section 4.4 and, by the end of 1999Q4, had a part-time main job with FTEe between €842 and €1263 (treatment group) or between €1363 and €1413 (control group). For these individuals, the marginal average federal tax rate is 19.73% for the treated group and 25.61% for the control group.

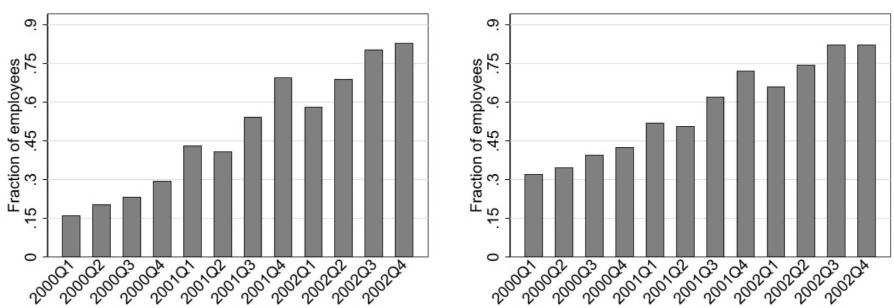
The last two subsamples consist of individuals who worked a high amount of time prior to the reform: namely, individuals who satisfy conditions I-V of Section 4.4 and, by the end of 1999Q4, had a full-time main job with FTEe between €842 and €1263 (treatment group, 6,966 individuals) or between €1363 and €1413 (control group, 3,853 individuals). For these individuals, the marginal average federal tax rate is 31.03% for the treated group and 37.14% for the control group.

Table 4.C.1. Marginal federal tax rate by taxable income

Taxable income		Tax rate
Lower limit	Upper limit	
0	6466	25%
6466	8596	30%
8596	12248	40%
12248	28149	45%
28149	42223	50%
42223	61953	52.5%
61953	$+\infty$	55%

4.D Treated (control) individuals with high (low) FTEe post-reform

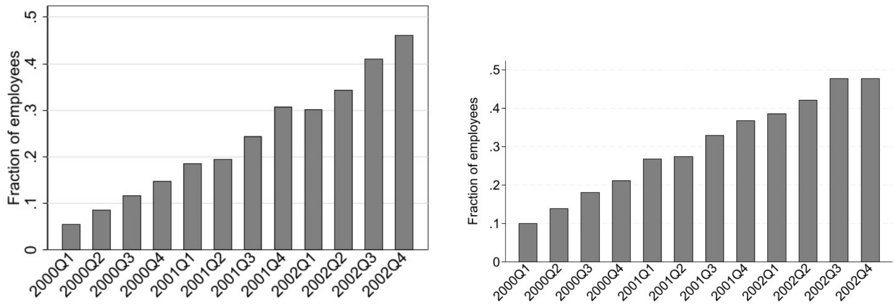
Figure 4.D.1. Proportion of treated individuals with FTEe between €1263 and €1313 that have FTEe above €1313 post-reform



(a) Part-time jobs **(b)** Full-time jobs

Notes: Take the individuals who meet conditions I-IV, with FTEe between €1263 and €1313 and a part-time or full-time work at the end of 1999Q4. We calculate the number of these individuals in salaried employment for each post-reform quarter. Then, for each post-reform quarter, we show the proportion of these individuals in salaried employment with FTEe above €1313.

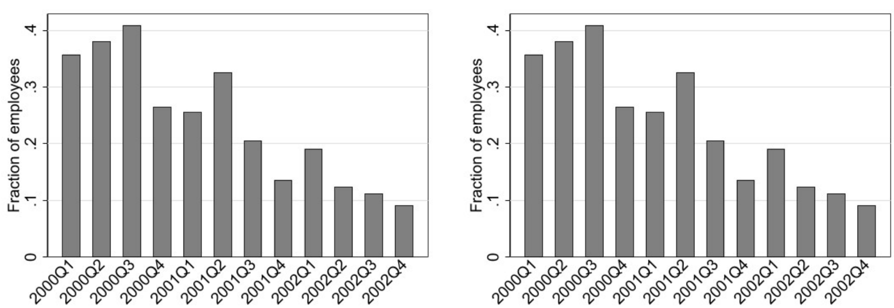
Figure 4.D.2. Proportion of treated individuals with FTEe between €842 and €1263 that have FTEe above €1313 post-reform



(a) Part-time jobs **(b)** Full-time jobs

Notes: Take the individuals who meet conditions I-IV, with FTEe between €842 and €1263 and a part-time or full-time work at the end of 1999Q4. We calculate the number of these individuals in salaried employment for each post-reform quarter. Then, for each post-reform quarter, we show the proportion of these individuals in salaried employment with FTEe above €1313.

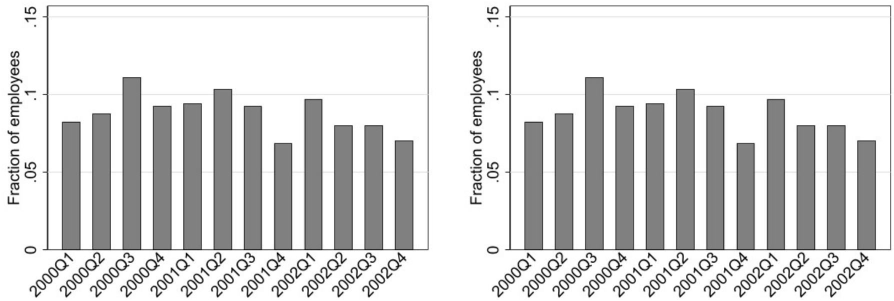
Figure 4.D.3. Proportion of individuals with FTEe between €1313 and €1363 that have FTEe below €1313 post-reform



(a) Part-time jobs **(b)** Full-time jobs

Notes: Take the individuals who meet conditions I-IV, with FTEe between €1313 and €1363 and a part-time or full-time work at the end of 1999Q4. We calculate the number of these individuals in salaried employment for each post-reform quarter. Then, for each post-reform quarter, we show the proportion of these individuals in salaried employment with FTEe below €1313.

Figure 4.D.4. Proportion of control individuals with FTEe between €1363 and €1413 that have FTEe below €1313 post-reform

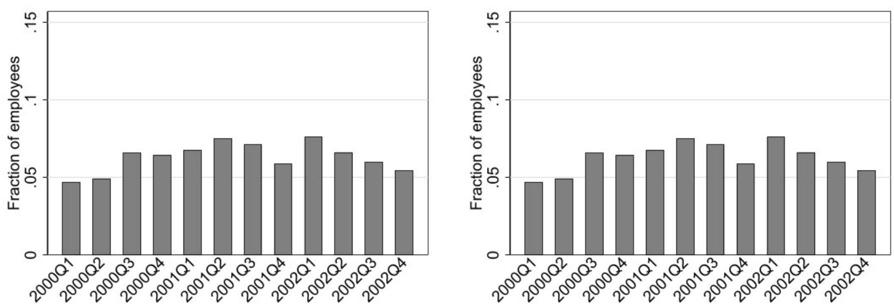


(a) Part-time jobs

(b) Full-time jobs

Notes: Take the individuals who meet conditions I-IV, with FTEe between €1363 and €1413 and a part-time or full-time work at the end of 1999Q4. We calculate the number of these individuals in salaried employment for each post-reform quarter. Then, for each post-reform quarter, we show the proportion of these individuals in salaried employment with FTEe below €1313.

Figure 4.D.5. Proportion of control individuals with FTEe between €1413 and €1463 that have FTEe below €1313 post-reform



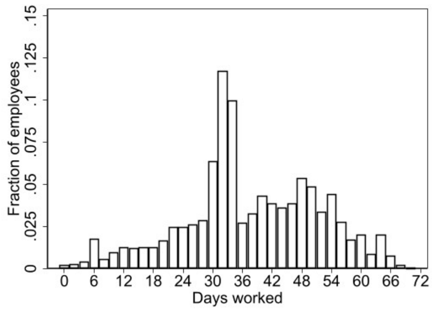
(a) Part-time jobs

(b) Full-time jobs

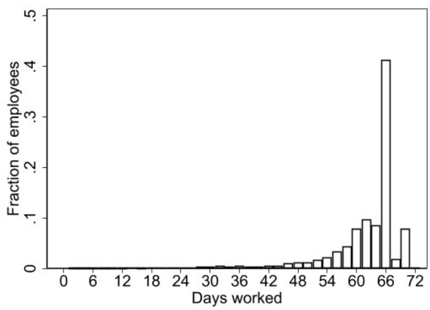
Notes: Take the individuals who meet conditions I-IV, with FTEe between €1413 and €1463 and a part-time or full-time work at the end of 1999Q4. We calculate the number of these individuals in salaried employment for each post-reform quarter. Then, for each post-reform quarter, we show the proportion of these individuals in salaried employment with FTEe below €1313.

4.E Additional summary statistic

Figure 4.E.1. Days worked in 1999Q4 for the control employees



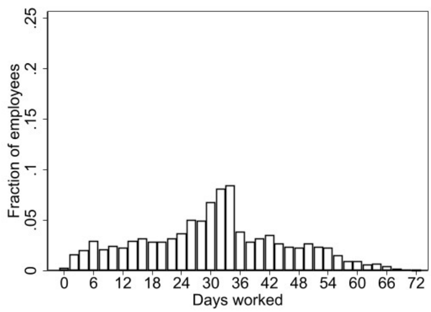
(a) Part-time jobs



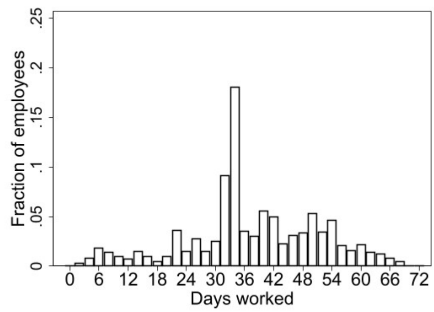
(b) Full-time jobs

Notes: The figure shows the histogram of the quarterly number of days worked in 1999Q4, with bins of 2 days, for control individuals with a part-time or full-time contract at the end of 1999Q4 - 2,111 and 3,843 individuals respectively. We winsorise the days worked for full-time jobs to 70 days for 1999Q4.

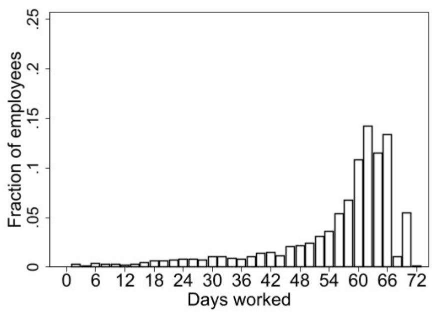
Figure 4.E.2. Days worked in 1999Q4 for the treated employees



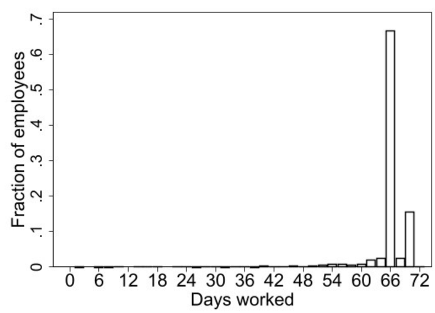
(a) Part-time blue-collar jobs



(b) Part-time white-collar jobs



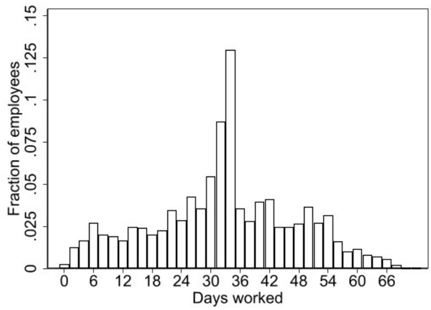
(c) Full-time blue-collar jobs



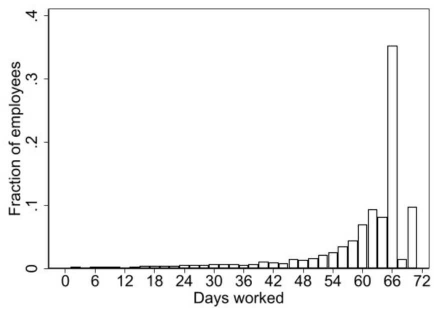
(d) Full-time white-collar jobs

Notes: The figure shows the histogram of the quarterly number of days worked in 1999Q4, with bins of 2 days, for treated individuals with part-time blue-collar (4,975 individuals), part-time white-collar (3,115), full-time blue-collar (4,089) or full-time white-collar (2,877) contract at the end of 1999Q4. We winsorise the days worked for full-time jobs to 70 days for 1999Q4.

Figure 4.E.3. Days worked in 1999Q4 for a subsample of the treated employees



(a) Part-time jobs

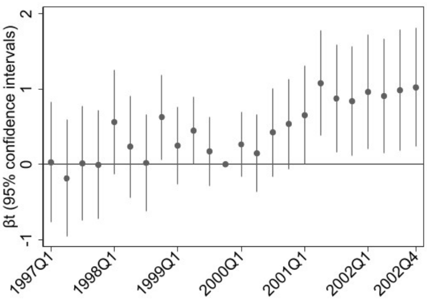


(b) Full-time jobs

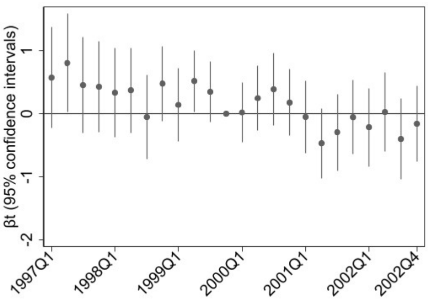
Notes: The figure shows the histogram of the quarterly number of days worked in 1999Q4, with bins of 2 days, for treated individuals with part-time or full-time contracts at the end of 1999Q4 who have a single job throughout 1999Q3 and 1999Q4 and had the same employer at the end of 1999Q2, 1999Q3 and 1999Q4 - 6,872 and 6,229 individuals respectively. We winsorise the days worked for full-time jobs to 70 days for 1999Q4.

4.F Additional empirical results

Figure 4.F.1. Estimated treatment effects on the quarterly number of days worked - exclude the re-entry of agents in salaried employment.



(a) Part-time

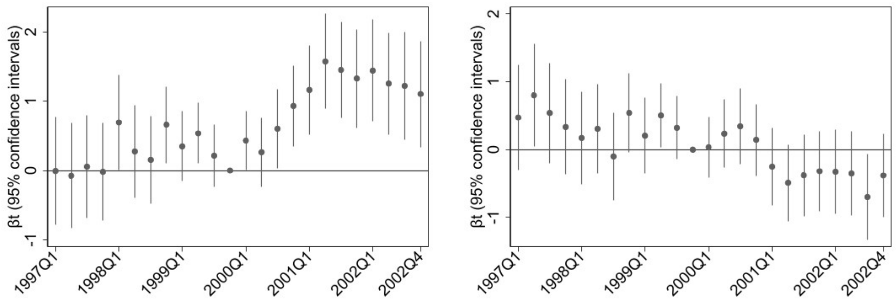


(b) Full-time

Notes: Same as Figure 4.7 - left panel - and Figure 4.8 - right panel. Compared to the baseline, we assume that once an individual exits salaried employment post-reform, they remain outside salaried employment. This change affects the observations of 1,401 (303) individuals in the treatment (control) group for those with a part-time contract in 1999Q4 and 1,065 (519) individuals in the treatment (control) group for those with a full-time contract in 1999Q4

4.G Robustness checks

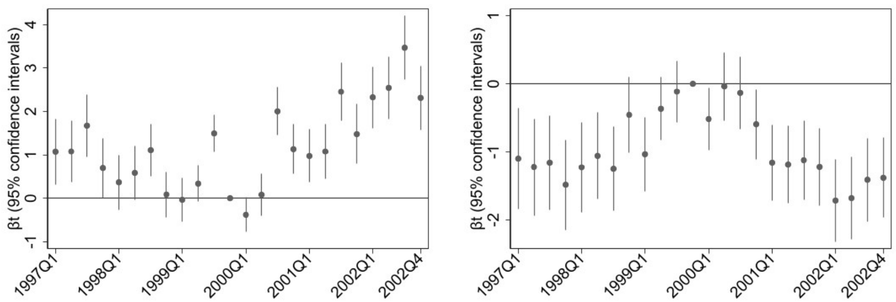
Figure 4.G.1. Estimated treatment effects on the quarterly number of days worked - we expand the treatment group



(a) Part-time jobs **(b)** Full-time jobs

Notes: Same as Figure 4.7 - left panel - and Figure 4.8 - right panel. Compared to the baseline, we include in the treatment group individuals with FTEe between €842 and €1313.

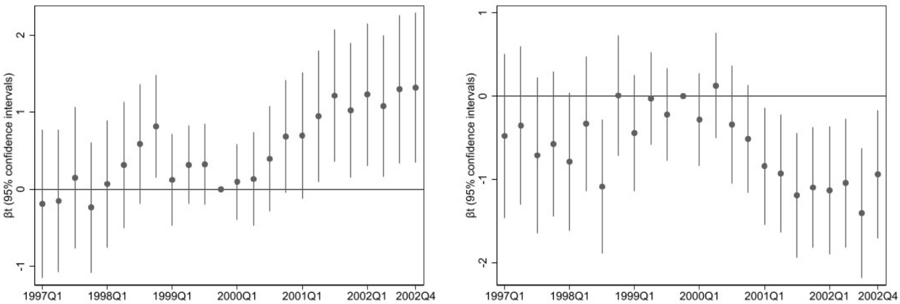
Figure 4.G.2. Estimated treatment effects on the quarterly number of days worked - we shift the control group



(a) Part-time jobs **(b)** Full-time jobs

Notes: Same as Figure 4.7 - left panel - and Figure 4.8 - right panel. Compared to the baseline, we include in the control group individuals with FTEe between €1413 and €1463.

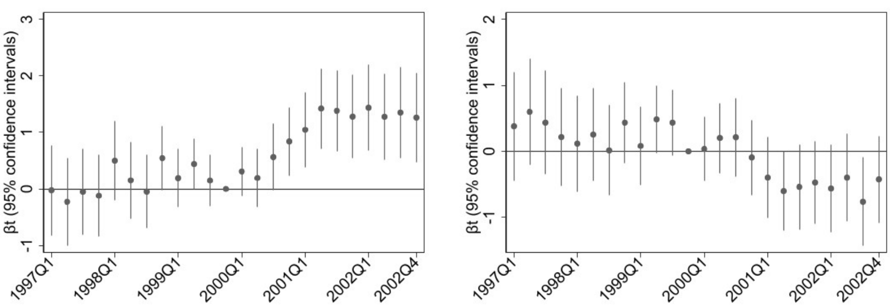
Figure 4.G.3. Estimated treatment effects on the quarterly number of days worked - we use the FTEe over 1999Q3 and 1999Q4 to define the treatment and control group



(a) Part-time jobs **(b)** Full-time jobs

Notes: Same as Figure 4.7 - left panel - and Figure 4.8 - right panel. Compared to the baseline, we include in the treatment group individuals with FTEe between €842 and €1263 at the end of 1999Q3 and 1999Q4, and in the control group individuals with FTEe between €1363 and €1414 at the end of 1999Q3 and 1999Q4.

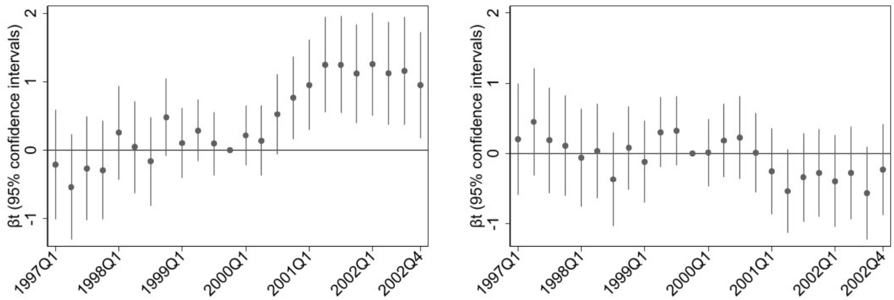
Figure 4.G.4. Estimated treatment effects on the quarterly number of days worked - we add controls on family composition



(a) Part-time jobs **(b)** Full-time jobs

Notes: Same as Figure 4.7 - left panel - and Figure 4.8 - right panel. Compared to the baseline, we add the family composition and number of household members among the regressors.

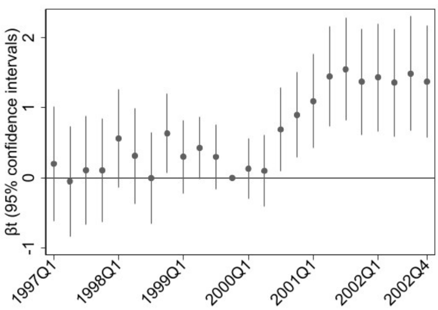
Figure 4.G.5. Estimated treatment effects on the quarterly number of days worked - we include controls on employers' and job characteristics.



(a) Part-time jobs **(b)** Full-time jobs

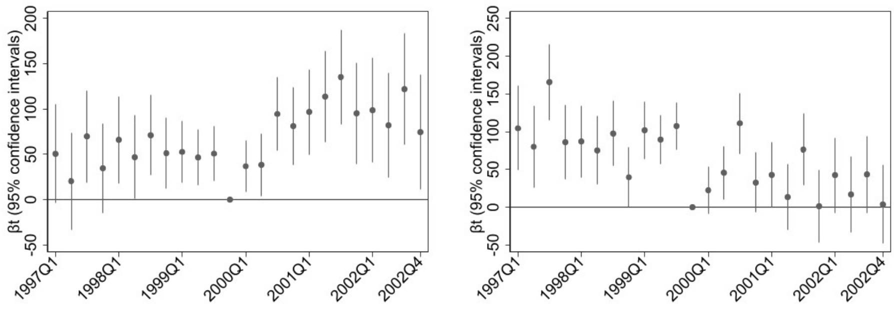
Notes: Same as Figure 4.7 - left panel - and Figure 4.8 - right panel. Compared to the baseline, we add a set of dummy variables for the employer's sector, the province where the employer is located, and the contract type (blue-collar or white-collar).

Figure 4.G.6. Estimated treatment effects on the quarterly number of days worked - we include only individuals with a single part-time active job at the end of 1999Q4



Notes: Same as Figure 4.7. Compared to the baseline, we look at individuals with only one part-time active job at the end of 1999Q4.

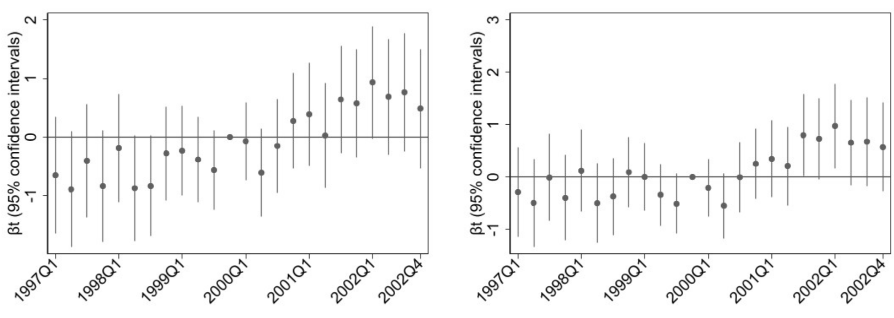
Figure 4.G.7. Estimated treatment effects on the quarterly gross earnings



(a) Part-time **(b)** Full-time

Notes: Same as Figure 4.7 - left panel - and Figure 4.8 - right panel. Compared to the baseline, we use as outcome the quarterly gross labour earnings an individual receives in a quarter over all jobs.

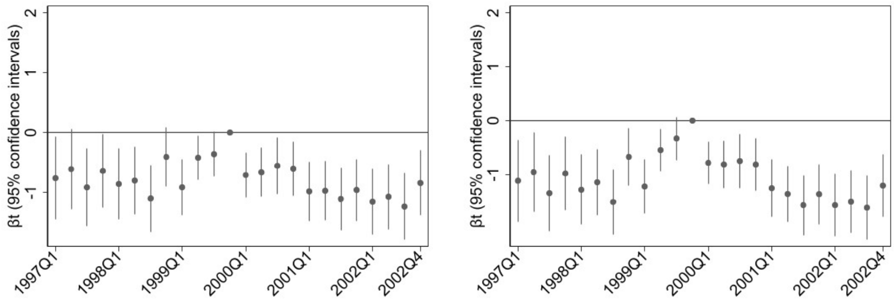
Figure 4.G.8. Estimated treatment effects on the quarterly number of days worked - main job has less than 40/50 days worked at the end of 1999Q4



(a) Less than 40 days **(b)** Less than 50 days

Notes: Same as Figure 4.7. Compared to the baseline, here we look only at individuals who work less than 40/50 days in their main job at the end of 1999Q4.

Figure 4.G.9. Estimated treatment effects on the quarterly number of days worked - main job has more than 40/50 days worked at the end of 1999Q4

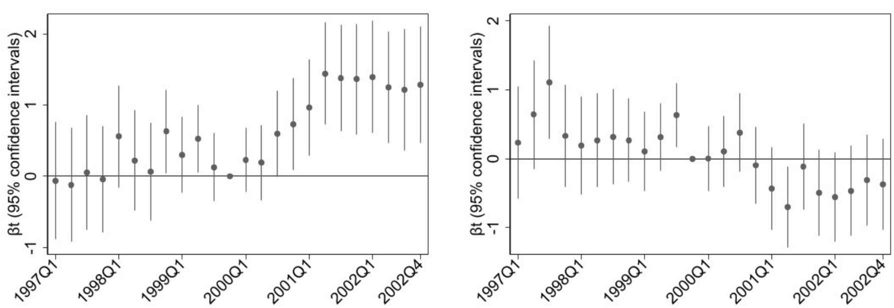


(a) More than 40 days

(b) More than 50 days

Notes: Same as Figure 4.7. Compared to the baseline, here we look only at individuals who work more than 40/50 days in their main job at the end of 1999Q4.

Figure 4.G.10. Estimated treatment effects on the quarterly number of days worked - we rescale the working time of full-time six-days-a-week contracts

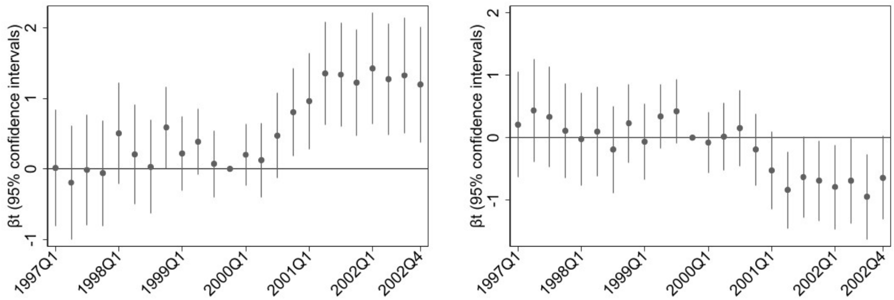


(a) Part-time

(b) Full-time

Notes: Same as Figure 4.7. Compared to the baseline, we rescale the working time of full-time six-days-a-week contracts.

Figure 4.G.11. Estimated treatment effects on the quarterly number of days worked - we do not winsorise the working time of full-time jobs

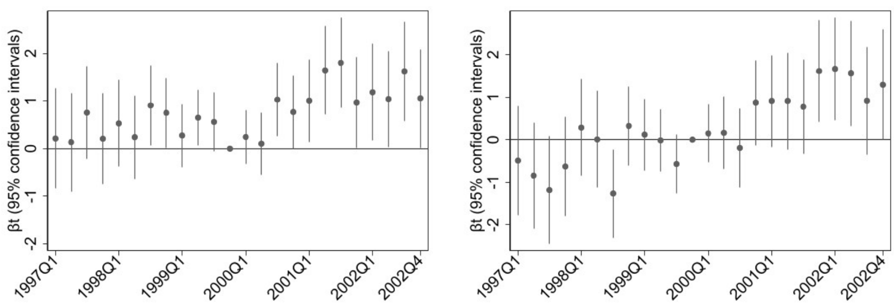


(a) Part-time **(b)** Full-time

Notes: Same as Figure 4.7. Compared to the baseline, we do not winsorise the working time of full-time jobs.

4.H Heterogeneity analysis

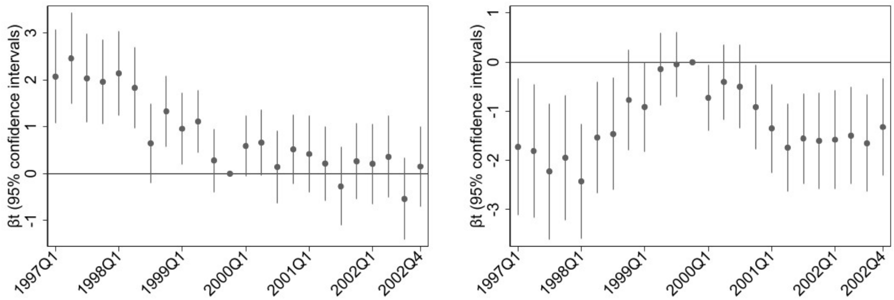
Figure 4.H.1. Proportion of treated individuals with FTEe between €1263 and €1313 with FTEe above €1313 post reform - part-time blue/white-collar



(a) Part-time blue-collar jobs **(b)** Part-time white-collar jobs

Notes: Same as Figure 4.7. Compared to the baseline, we include in the treatment and control groups only individuals whose main job was part-time blue-collar (right panel) or part-time white-collar (left-panel) in 1999Q4.

Figure 4.H.2. Proportion of treated individuals with FTEe between €1263 and €1313 with FTEe above €1313 post reform - full-time blue/white-collar

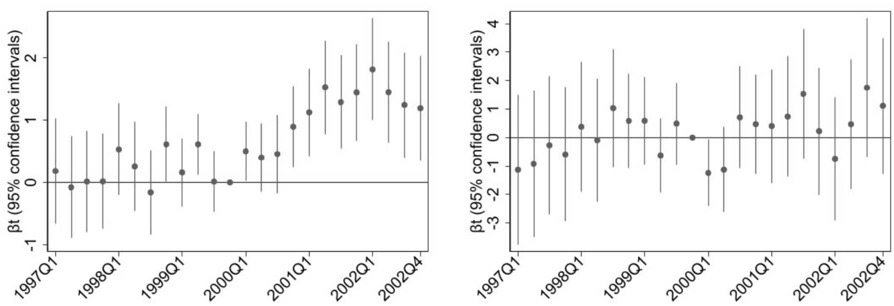


(a) Full-time blue-collar jobs

(b) Full-time white-collar jobs

Notes: Same as Figure 4.7. Compared to the baseline, we include in the treatment and control groups only individuals whose main job was full-time blue-collar (right panel) or full-time white-collar (left-panel) in 1999Q4.

Figure 4.H.3. Proportion of treated individuals with FTEe between €1263 and €1313 with FTEe above €1313 post reform - male and female employees having a part-time contract at the end of 1999Q4

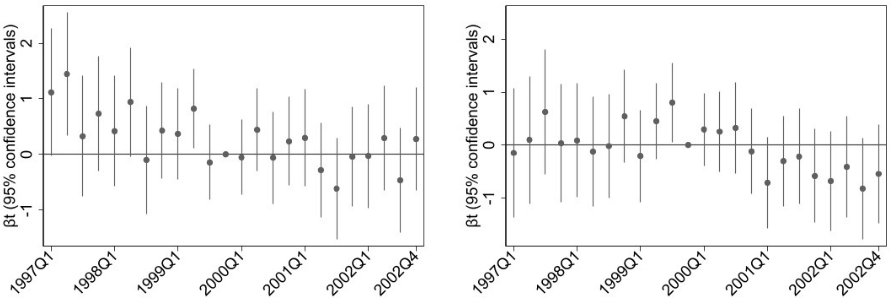


(a) Female employees with a part-time job

(b) Male employees with a part-time job

Notes: Same as Figure 4.7. Compared to the baseline, we include in the treatment and control groups only part-time female employees (right panel) or part-time male employees (left-panel) in 1999Q4.

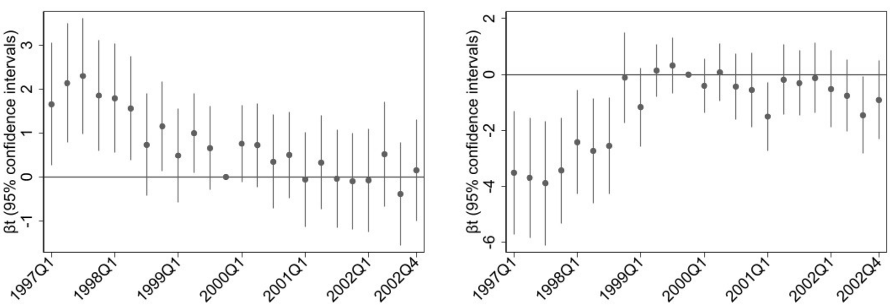
Figure 4.H.4. Proportion of treated individuals with FTEe between €1263 and €1313 with FTEe above €1313 post reform - male and female employees having a full-time contract at the end of 1999Q4



(a) Female employees with a full-time job **(b)** Male employees with a full-time job

Notes: Same as Figure 4.7. Compared to the baseline, we include in the treatment and control groups only full-time female employees (right panel) or full-time male employees (left-panel) in 1999Q4.

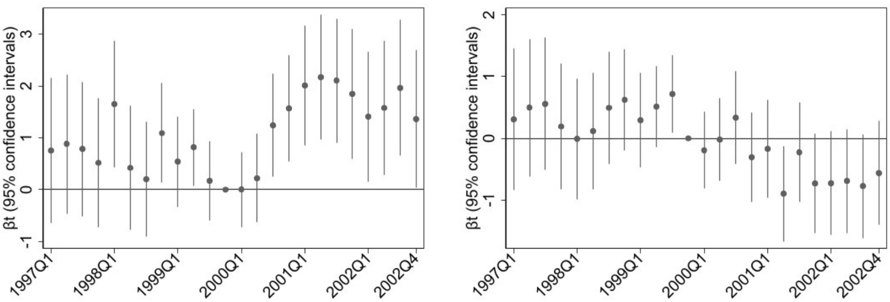
Figure 4.H.5. Proportion of treated individuals with FTEe between €1263 and €1313 with FTEe above €1313 post reform - male employees having a full-time blue-collar or white-collar contract at the end of 1999Q4



(a) Male employees with a full-time blue-collar job **(b)** Male employees with a full-time white-collar job

Notes: Same as Figure 4.7. Compared to the baseline, we include in the treatment and control groups only full-time blue-collar male employees (right panel) or full-time white-collar male employees (left-panel) in 1999Q4.

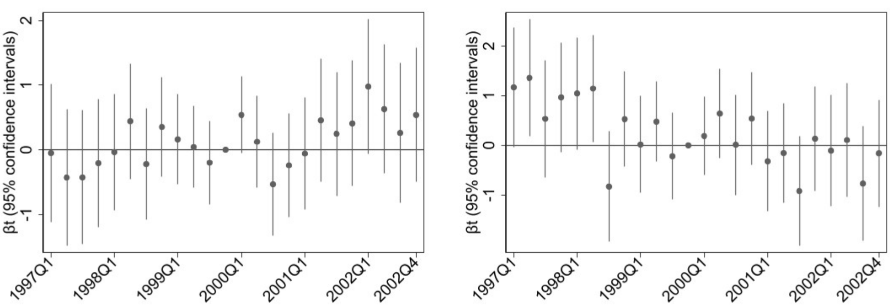
Figure 4.H.6. Proportion of treated individuals with FTEe between €1263 and €1313 with FTEe above €1313 post reform - Individuals who were between 21 and 35 years old at the end of 1999Q4



(a) Part-time jobs **(b)** Full-time jobs

Notes: Same as Figure 4.7 - left panel - and Figure 4.8 - right panel. Compared to the baseline, we include in the treatment and control groups only individuals who were between 21 and 35 years old at the end of 1999Q4.

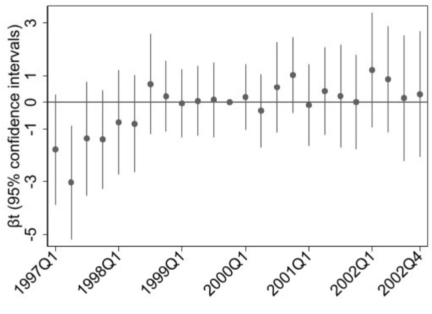
Figure 4.H.7. Proportion of treated individuals with FTEe between €1263 and €1313 with FTEe above €1313 post reform - Individuals who were between 36 and 50 years old at the end of 1999Q4



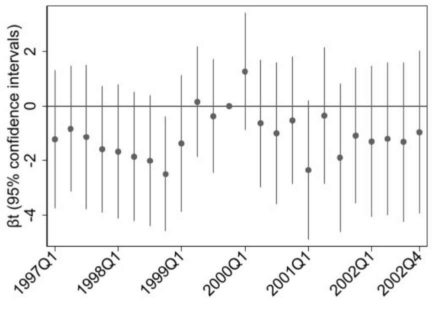
(a) Part-time jobs **(b)** Full-time jobs

Notes: Same as Figure 4.7 - left panel - and Figure 4.8 - right panel. Compared to the baseline, we include in the treatment and control groups only individuals who were between 36 and 50 years old at the end of 1999Q4.

Figure 4.H.8. Proportion of treated individuals with FTEe between €1263 and €1313 with FTEe above €1313 post reform - Individuals aged 51 or older at the end of 1999Q4



(a) Part-time jobs



(b) Full-time jobs

Notes: Same as Figure 4.7 - left panel - and Figure 4.8 - right panel. Compared to the baseline, we include in the treatment and control groups only individuals aged 51 or older at the end of 1999Q4.

5

General conclusion

This thesis evaluates two policies aimed at reducing social security contributions for either firms or salaried workers in Belgium, focusing on the effect of the reforms on their behaviour. Chapter 2 and Chapter 3 examine a permanent exemption for new employers from paying social security contributions on the gross wage of one employee. They demonstrate that some low-productivity firms started hiring thanks to the exemption; however, due to their low productivity, the majority of these firms hired only one employee. Moreover, these chapters reveal that firms that would have hired even in the absence of the reform do not respond to the reduction in the cost of hiring. This is because the cost of attracting one more jobseeker is relatively high, and the social security contributions offset only a small part of this cost. Chapter 4 evaluates a payroll tax reduction for low-wage salaried workers in Belgium. It shows that the policy resulted in an increase in the number of days worked for young individuals with part-time contracts.

The central message of this thesis is the importance of targeting policies at the agents most likely to respond to changes in financial incentives. While a relatively wide range of agents is eligible for both policies—namely, new employers and low-wage employees—we observe that only a subset of individuals within each group responds to the policies. Targeting the payroll tax reduction for low-wage salaried workers at young agents would be relatively simple. Restricting policies to certain age segments of the population is, indeed, not uncommon in tax and benefit systems. However, future research is needed to deepen our understanding of why this group responded more strongly than the others. Only by understanding the precise mechanism behind why some individuals are more responsive, we ensure that their reaction does not come at the expense of other dimensions that contribute to their overall welfare. Targeting the permanent exemption for new employers at the most responsive groups would be more challenging. We find that agents who would have hired their first employee without the policy do not alter their hiring behaviour. However, identifying these agents is far from straightforward because, as suggested by our models, this would require precisely estimating their productivity. Moreover, even if these agents do not modify their hiring behaviour, it was not ex ante clear that this would have been the case. This involves estimating the expected cost of attracting an additional job seeker and the expected benefit of having an additional employee, which can only be computed by modelling the reform or, potentially, with extremely detailed firm data.

To conclude, this thesis shows that by using both data along with microeconometrics methods and models to represent the behaviour of agents, we can provide valuable information on how firms and salaried workers respond to changes in the social security contributions which are levied from their wages. In turn, this evidence, together with the knowledge accumulated from past research, can help us develop policies that make the best use of public expenditures.

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