Firm Heterogeneity and the Impact of Payroll Taxes*

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July 2022

Abstract

We study the impact of a large payroll tax cut for older workers in Hungary. Motivated by the predictions of a standard equilibrium job search model, we examine the heterogeneous impact of the policy. Employment increases most at low-productivity firms, which tend to hire from unemployment, while the effects are more muted for high-productivity firms. At the same time, wages only increase at high-productivity firms. These results point to important heterogeneity in the incidence of payroll tax subsidies across firms and highlight that payroll taxes have a significant impact on the composition of jobs in the labor market.

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1 Introduction

Payroll taxes and employer social security contributions account for just under 40% of the tax wedge in developed countries (OECD, 2022*a*) and there is a longstanding interest in understanding the impact of these polices on employment and wages. The standard approach in public finance suggests that the market-level elasticities of labor supply and demand determine the employment and wage impacts and the incidence of payroll taxes (see e.g. Gruber, 1997; Rothstein, 2010). This approach typically assumes that firms passively accept market-level wages and so the incidence of the payroll tax will be homogeneous across firms and workers. Nevertheless, recent empirical work shows that firms play an important role in wage determination. Wage premium differences across firms are large and important for understanding key labor market phenomena such as rising wage inequality (Card, Heining and Kline, 2013; Song et al., 2019). In this paper, we explore the impact of payroll taxes on employment and wages in the presence of firm heterogeneity and frictions.

To illustrate the important role firms play in shaping the impact of tax policies, we start by studying the effect of such polices in a standard search and matching model with heterogeneous firms (Postel-Vinay and Robin, 2002; Mortensen and Pissarides, 2003; Lise, Meghir and Robin, 2016; Moscarini and Postel-Vinay, 2018; Bagger and Lentz, 2019). In the model lower-quality, less productive firms tend to hire from unemployment and earn large rents on their workers. At the same time, higher-quality, more productive firms are larger as they do not only hire from unemployment, but they can also poach workers from lower-quality firms. Poached workers can get a larger share of the surplus or rents as they can use their previous job as an outside option in wage negotiations.

The model highlights that a payroll tax subsidy increases overall employment and wages just like in the standard framework. Importantly, however, the impact of the policy is heterogeneous across firms. Lower-quality firms, which tend to hire from unemployment benefit disproportionately from the policy as they can exploit that unemployed workers have limited bargaining power. At the same time, higher-quality firms poach more of their workers from other firms which means that workers are able to claim more of the surplus from the subsidy. Thus, the model suggests that employment effects are concentrated among lower-quality firms, while wage effects are concentrated among higher-quality firms.

We examine the empirical relevance of these predictions by studying the impact of an age-specific payroll tax cut in Hungary. In 2013 the monthly social security contribution decreased by HUF 14,500 per month[1] for all over-55 private sector employees. This led to a

[1]The average monthly gross wage was HUF 230,000 in Hungary in 2013, so the subsidy is about 6.3% of the average wage in 2013. A subsidy of equivalent size in the USA context would be $3500 per year based on the average salary in 2022.

5.3% decrease in the labor cost for an average over-55 private sector employee.

Using rich administrative data, we estimate the impact of the policy in a difference-in-differences framework, comparing men above the age cutoff to men just below.[2] We find a large increase in men's (and women's) employment in response to the policy. In response to the 5.3% decrease in labor costs of men, employment of the subsidized workers increased by 1.6%, implying a labor demand elasticity of -0.3 (s.e. 0.03). This increase in employment mainly came from non-employment and inactivity. The change in self-employment or public sector employment was limited, reflecting that these workers were ineligible for the payroll tax cut.

In line with the prediction of our illustrative model, we also find substantial heterogeneity across firm types. For a variety of measures of firm quality, the employment-increasing effect of the policy comes from lower-quality firms and lower-quality jobs, while the employment of older workers in higher-quality firms is unchanged. The differential response to the policy by firm type cannot be explained by the lower relative value of the tax subsidy at high-quality firms. Even if the labor cost decline is somewhat larger at low-quality firms, it is still non-negligible at high-quality firms (6% at low-quality firms vs. 4.5% at high-quality firms). The implied employment elasticity with respect to labor cost is -0.53 (s.e. 0.05) at low-quality firms and 0.01 (s.e. 0.06) at high-quality firms.

We present several additional pieces of evidence to highlight that our results reflect firm heterogeneity and not other factors. First, we examine the effect of the policy throughout the entire wage distribution similarly to Cengiz, Dube, Lindner and Zipperer (2019). We find that employment increased mainly at the bottom of the wage distribution at low-quality firms, while we find no indication for substantial change in employment in the upper part of the wage distribution where the change in labor cost was limited.[3] This suggests that our estimates pick up the effect of the payroll tax cut. Furthermore, we show that heterogeneity in responses is present even if we restrict the sample to similar workers. Even among low paid workers in low paying occupations, or among workers in the same education group, we find different responses to the policy by firm type. This suggests that the differences in responses to the payroll tax cut we uncover reflect firm heterogeneity and not simply the fact that better workers tend to work at better firms.

We also study the impact of the policy on wages. The model predicts that wages should

[2]We focus on men in the main analysis as for women there was another policy change instituted in 2011 that made it easier to retire earlier than the normal retirement age. That reform could potentially affect women's employment decisions post policy change. Still, in Section 7 we show that the labor market responses are very similar for women and men, which suggests that the reform studied here has a similar impact on men and women.

[3]Note that the amount of the tax cut was fixed, which implies that at higher wages the change was smaller relative to total labor costs.

increase for incumbent workers at high-productivity firms. This is exactly what we find: there is a significant increase in wages at high-productivity firms, but we find no change in wages for workers at low-productivity firms among older workers. We estimate that the overall pass-through of the policy is small, out of $1 subsidy only 22 cents benefit workers, while 78 cents go to firms. The pass-through varies by firm type. At high-quality firms the pass-through rate is 52 cents out of $1 subsidy, while at lower-quality firms the pass-through rate is close to zero and statistically insignificant.

We present several robustness checks to underscore these results. First, we vary the control group definition to make sure that our main estimates are not muted or exaggerated by the variation of the age-window used in the estimations and by potential spillovers to the control group. The main conclusions are unaffected by the choice of the control group.

Second, the comparison of the firm-level relationship between hiring subsidized workers and non-subsidized workers before and after the reform suggests that firms that hired more subsidized worker after the reform did not cut their hiring from non-subsidized workers. Accordingly, the policy is likely to have improved overall employment and not just led to substitution of subsidized workers for non-subsidized ones.

Third, we show that the estimated employment and wage responses are unlikely to reflect the windfall effects found to be important in the context of tax subsides affecting young Swedish workers (see Saez, Schoefer and Seim, 2019). We find no evidence that firms that had more affected old workers before the tax cut grew more quickly than other firms.[4] Furthermore, the change in wages and the incidence differences across firm types are robust to controlling for differences in the size of windfall shocks firms experience.

Fourth, our results are unlikely to reflect wage rigidities that could potentially bind low-quality and high-quality firms differently. Union membership is very low in Hungary and industry-level agreements are rare and set only weak requirements. Furthermore, we find that the heterogeneity between high- and low-productivity firms is present even if we look at employment changes among similarly sized firms. Our estimates do not reflect the presence of a binding minimum wage either. The estimated change in employment is not concentrated at the minimum wage. Even among workers earning more than 150% of the minimum wage we find a significant increase in employment at low-productivity firms. This suggests that the employment change does not simply come from some low-quality jobs becoming viable

[4] Interestingly, for the young workers we find similar windfall responses as in Sweden (see Figure D5). This suggest that it is not the economic environment *per se* which explains the differences between Hungary and Sweden, but responses might be different for older and younger workers in general. A potential explanation for this discrepancy is that firms might face more institutional constraints to increase young workers' wages without increasing their older co-workers' wages as that would imply a wage cut once someone is aged out from the subsidy. On the contrary, it is easier to implement a pay raise once older workers age into the policy.

following the payroll tax cut.

These empirical findings together with our theoretical framework point to interesting (and as far as we know so far undocumented) heterogeneity in the incidence of tax subsidies. Older workers employed by productive firms are able to extract more of the surplus from the subsidy and so the incidence of the subsidy (partly) falls on them. At the same time, older workers who are employed by less productive firms are benefiting from the tax subsidy through increased hiring, while firms capture a larger share of the surplus for these workers.

Since the tax subsidy also affected workers under 25, we can compare our estimates for older workers to impacts among younger workers. We find that the payroll tax cut increased employment among younger workers but heterogeneity across firms was more muted. We interpret this discrepancy through the lens of the model: most young workers have low bargaining power as they come from unemployment or temporary contracts that cannot easily be used in wage negotiations. As a result, the share of hiring a worker with low bargaining position is much larger at the young labor markets, thus good and bad firms are affected more similarly by the hiring subsidy. Furthermore, labor market institutions could serve a more important constraint for young workers, given the large share of them at or slightly above the minimum wage.

Our paper relates to several strands of the literature. First, the paper is closely related to studies of age-based employment subsidies (Kramarz and Philippon, 2001; Boockmann, Zwick, Ammermüller and Maier, 2012; Huttunen, Pirttilä and Uusitalo, 2013; Egebark and Kaunitz, 2018; Saez, Schoefer and Seim, 2019; Svraka, 2019). Our main contribution to this literature is that we focus on heterogeneity across firm types and by this we offer an explanation for the inconsistencies found in the literature. Our heterogeneity results are not without antecedents in the literature, although our data and institutional setting make it possible to provide a more comprehensive overview on the differing impacts of payroll tax cuts by job and firm types. In line with our results, Albanese and Cockx (2019) find that a wage cost subsidy in Belgium targeting employees above age 58 increased employment at firms with high shares of low-wage workers, and at small firms.

Our study also relates to the literature on payroll tax incidence in general. Studies using payroll tax reforms to analyze incidence provide mixed evidence. Some studies find that the burden of the payroll tax is shifted on the workers (Gruber, 1997; Anderson and Meyer, 2000). However, some later studies find that the burden of the payroll tax is mostly borne by the employer (Kugler and Kugler, 2009; Saez, Matsaganis and Tsakloglou, 2012; Saez, Schoefer and Seim, 2019; Benzarti and Harju, 2021)[5]. Evaluating the incidence of business

[5]Bozio, Breda and Grenet (2019) reconcile these seemingly conflicting results by the tax-benefit-linkage explanation. In our case, tax-benefit linkage is not directly affected by the reform, and the payroll tax did

tax credits, Carbonnier, Malgouyres, Py and Urvoy (2022) find that the incidence of wage gains is on high-skill workers. Analyzing place-based payroll taxes, Ku, Schönberg and Schreiner (2020) emphasize that wage rigidity plays a key role in determining the wage and employment responses to a payroll tax change. Lobel (2021) points out using a Brazilian tax reform that the pass-through of payroll tax cut to earnings is augmented for unionized workers, while Stokke (2021) shows based on place-based payroll taxes that the pass-through is larger at high-productivity firms.

Improving the employment prospects of vulnerable groups is a major policy priority for many governments. A number of countries have implemented targeted employment subsidies, such as payroll tax cuts, to support the employment of specific groups with low employment rates. Nevertheless, to date there is no conclusive evidence on whether such policies are successful. Some studies find non-negligible positive effects on employment (Kramarz and Philippon, 2001; Egebark and Kaunitz, 2018; Saez, Schoefer and Seim, 2019), while others find little evidence for employment effects (Boockmann, Zwick, Ammermüller and Maier, 2012; Huttunen, Pirttilä and Uusitalo, 2013). Furthermore, most of the literature ignores heterogeneous impacts.

The remainder of this paper proceeds as follows. Section 2 introduces a search model with heterogenous firms. In Section 3, we provide background on the payroll tax reform we study and describe the Hungarian administrative data used for our empirical analysis. We present our employment results in Section 4 and wage results in Section 5. We discuss welfare effects in Section 6. In Section 7 we provide results for younger workers and women excluded from our main analyses. Section 8 concludes.

2 The Effect of Tax Subsidies in Search Models

We study the impact of payroll taxes through the lens of a standard search and matching model. We introduce a tax subsidy in a framework with random search, heterogeneous firms and sequential bargaining on wages (Postel-Vinay and Robin, 2002). We study how changing the tax subsidy affects employment, wages, and the composition of job types in equilibrium. Our goal in this section is to illustrate that tax policies can have heterogeneous impact across different firms and not to model the specific tax policy implemented in Hungary. As a result, we abstract away from the age-specific nature of the tax cut. We also abstract away from worker heterogeneity and assume that job search is exogenous. These latter two assumptions can be relaxed without altering the basic predictions of the model.

not affect workers' future benefit, which was calculated based on wages and not based on the payments into the system. Such a feature of targeted subsidies is a common feauture of payroll tax cuts.

2.1 Setup

Firms are heterogeneous and characterized by productivity $y \in [0, \infty]$, with cumulative distribution function $\Psi()$. Workers are homogeneous. Workers are either unemployed or employed. If unemployed, they receive leisure of value b and search for jobs with probability one. If employed, they receive wage w, search for a new job with probability $s \in [0, 1]$ and can separate from their job exogeneously with probability $\delta \in [0, 1]$.[6]

Firms advertise vacancies at an increasing and convex cost $\kappa()$. Job market tightness is the ratio between total vacancies (v) and total search effort by the unemployed (u) and employed ($(1-\delta)(1-u)$):

$$\theta = \frac{v}{u + s(1-\delta)(1-u)}. \tag{1}$$

A searching worker locates an open vacancy with probability $\phi(\theta)$, increasing in θ. The probability for an open vacancy to meet a worker who is searching for jobs is $\phi(\theta)/\theta$, decreasing in θ.

Wage setting is based on sequential auction a la Postel-Vinay and Robin (2002). When an employed worker contacts an open vacancy, the prospective poacher and the incumbent employer observe each other's match qualities with the worker, and engage in Bertrand competition over contracts. The worker chooses the contract that delivers the larger value. For simplicity, we also assume that all the bargaining power is at the firms and so they are able to extract all rents from the workers.[7] Note, that even if workers are assumed to have no bargaining power, competition between firms for workers can still result in a very high labor share of firm revenues – this feature of the model is also pointed out by Dey and Flinn (2005). Details of the wage setting are provided in Appendix Section A.4.

2.2 Bellman Equations

The value of unemployment is the following:

$$V_u = b + \beta V_u, \tag{2}$$

[6] We find that besides an increase in entry rate, some of the responses to payroll tax cuts come from a decrease in moving to unemployment. This could be explained within our framework by introducing advance notice layoffs or by introducing endogenous job separation by assuming that with δ probability there is a negative effect on productivity (instead of exogenous separation of the job match). Since our goal is to illustrate some key mechanisms and not match all patterns in the data, we abstract away from advance notice layoffs here.

[7] It is straightforward to introduce some bargaining power of the worker in the model. Nevertheless, empirical studies find usually that bargaining power is quite small and so abstracting away from that will not alter the conclusions made below.

where β is the discount factor. Notice that the probability of finding a job does not show up in the above equation, which comes from the assumption that firms have all the bargaining power. Even if the unemployed get a job offer, it will not make them better off. This will not be the case for employed workers as job offers will make them better off by the competition they induce between firms.

The maximum value the firm is willing to promise to deliver to the worker is:

$$V(y,\tau) = y + \tau + \delta\beta V_u + (1-\delta)\beta V(y,\tau), \tag{3}$$

where τ is the lump-sum employment subsidy. Here, the possibility of the worker being poached by another firm is implicitly included in the $V(y,\tau)$ formula. Note also that if no outside offers arrive then the continuation value of the worker is $V(y,\tau)$. If the worker is poached then she is poached at value $V(y,\tau)$. Either way, the continuation value of the worker who survives the exogenous separation is $V(y,\tau)$, which is the maximum value the firm can deliver (Moscarini and Postel-Vinay, 2018).

Firms need to post vacancies to find workers. The value of posting vacancies will be the following:

$$\begin{aligned} V_v(y,\tau) = \max_{\nu} \Bigg\{ &-\kappa(\nu) + \beta\nu\frac{\phi(\theta)}{\theta}\Bigg(P(u)\Big[V(y,\tau)-V_u\Big] + \\ &+ (1-P(u))\int_0^y \Big[V(y,\tau)-V(y',\tau)\Big]d\Gamma(y')\Bigg)\Bigg\} \end{aligned} \tag{4}$$

where $-\kappa(\nu)$ is the cost of posting ν vacancies, which leads to $\nu\phi(\theta)/\theta$ chance to be matched to an applicant. In the value function above $P(u) = u/(u+(1-\delta)s(1-u))$ reflects the probability that a randomly drawn applicant is unemployed, which leads to the $V(y,\tau) - V_u$ profits, given that firms can extract all the surplus from the match. The chance that a randomly drawn applicant is employed is $1 - P(u)$ and the benefit of this from the firm perspective depends on the previous employer of the applicant. If the applicant works at a more productive firm, then the firm cannot attract that worker and so there is no benefit from being matched to that applicant. That is why the integral goes only to y in the above formula. Nevertheless, if the firm meets with an applicant employed at a firm with lower productivity y', then the firm can poach that worker and acquire the difference between the new surplus ($V(y,\tau)$) and the surplus at the previous firm ($V(y',\tau)$). The chance that the firm meets with an employed worker at firm y' depends on the vacancy distribution function $\Gamma(y) = \int_0^y \nu(y',\tau)d\Psi(y')/(\int_0^1 \nu(y',\tau)d\Psi(y'))$, where $\nu(y,\tau)$ is the optimal choice of vacancy of a firm y at tax subsidy level τ.

Plugging in $V(y,\tau)$ (equation (3)) and V_u (equation (2)) into equation (4), leads to:

$$V_v(y,\tau) = \max_{\nu} \Bigg\{ \underbrace{-\kappa(\nu)}_{\text{Cost of vacancy}} + \underbrace{\nu\frac{\phi(\theta)}{\theta}P(u)}_{\text{Probability meet unemp.}} \times \underbrace{\beta\left[\frac{y+\tau}{1-\beta+\delta\beta} - \frac{1-\delta\beta}{1-\beta+\delta\beta}\frac{b}{1-\beta}\right]}_{\text{Benefit meeting with an unemployed}} + \\ + \underbrace{\nu\frac{\phi(\theta)}{\theta}(1-P(u))}_{\text{Probability meet employed}} \times \underbrace{\beta\int_0^y \frac{y-y'}{1-\beta+\delta\beta}\Gamma(y')}_{\text{Benefit meeting with an employed}} \Bigg\}. \tag{5}$$

This equation highlights the key trade-offs firms face when they decide about posting a vacancy. The first part reflects the cost of posting. The second part reflects the (expected) benefit of meeting an applicant who is unemployed, while the third part reflects the (expected) benefit of meeting with an applicant who is employed. The equation also highlights the key channels through which payroll taxes affect vacancy posting and employment. In particular, the tax subsidies only appear in the second part of this equation, which reflects the benefits of hiring from unemployment. At the same time, the tax subsidy has no impact on the third part of the value of vacancy posting, hiring from employment, as all firms receive the tax subsidy and the competition for workers will shift the surplus from the firms to the worker. Note that this shift in incidence of the policy will take place even if firms have all the bargaining power.

The equation, therefore, highlights that the tax subsidy increases the benefit of hiring from unemployment, while it has no effect on hiring from employment. It is worth highlighting that such stark differences between hiring from employment and unemployment is a consequence of the fact that all bargaining power is at the firms in our simple model. In practice, the unemployed might have some bargaining power and so they can acquire some of the surplus from forming an employment relationship. Nevertheless, as long as workers do not have all the bargaining power, we will find that the tax subsidy benefits the firms if they hire from unemployment and the workers if they are poached from another firm.[8]

[8]In our model, following the search and matching literature, we assumed that unemployment benefit, b is unaffected by the previous wage (see e.g. Postel-Vinay and Robin, 2002; Bagger and Lentz, 2019). In practice, unemployment benefit might reflect previous wages. Still, as long as the replacement rate of the unemployment benefit is less than 100%, firms hiring from unemployment will be able to capture some of the tax subsidy. In Hungary, the replacement rate of the unemployment benefit is around 50% and so the pass-through of earlier wages to unemployment benefit will only be partial. Since our primary goal is illustration, we assume in our model that unemployment benefit is fixed.

2.3 Equilibrium

Equilibrium is where firms optimally post vacancies up to the point where the marginal value of posting a vacancy equals its cost – they maximize equation (5). Furthermore, market tightness, θ, and the distribution of vacancies, $\Gamma(y)$, are consistent with firms' vacancy posting decisions. Finally, the steady state equilibrium unemployment rate is:

$$u = \frac{\delta}{\delta + \phi(\theta)}. \tag{6}$$

In equilibrium more productive firms post more vacancies and as a result will employ more workers. This is because more productive firms earn more rent from hiring from unemployment and also they are more likely to fill their vacancies when they meet with an employed applicant. Formally, this is a simple consequence of the value function shown in equation (5), where both the (expected) benefits of meeting with an unemployed (second part) and with an employed applicant (third part) are strictly increasing in productivity (y).

We derive the formula for the equilibrium wage in Appendix A.4. Assuming constant relative risk aversion (CRRA) utility function with rate of relative risk aversion ζ ($\zeta \geq 0$ and $\zeta \neq 1$), we can derive the wage at firm y' of an individual arriving from firm y, following Postel-Vinay and Robin (2002):

$$\ln \xi(y+\tau, y'+\tau) = \frac{1}{1-\zeta} \ln \left[(y+\tau)^{1-\zeta} - \frac{(1-\zeta)\phi(\theta)}{\frac{1-\beta}{\beta} + \delta} \int_{y+\tau}^{y'+\tau} \bar{\Gamma}(x,\tau) x^{-\zeta} dx \right]. \tag{7}$$

The wage of workers whose wage is the first salary after unemployment is:

$$\ln \xi(b, y+\tau) = \ln \xi_u(y+\tau) = \frac{1}{1-\zeta} \ln \left[b^{1-\zeta} - \frac{(1-\zeta)\phi(\theta)}{\frac{1-\beta}{\beta} + \delta} \int_{b}^{y+\tau} \bar{\Gamma}(x,\tau) x^{-\zeta} dx \right]. \tag{8}$$

In equations (32) and (33), the first term in the brackets captures the maximum wage the type-y firm could pay ($y + \tau$) or the unemployment benefit (b). The second terms (the terms with the integral) capture the option value of working at the more productive firm. Intuitively, workers accept lower wages in exchange for the increased chances of higher wages in the future (Postel-Vinay and Robin, 2002). This option value increases with the relative productivity of the new employer, the job finding rate ($\phi(\theta)$) and the discount factor (β), and decreases with the rate of relative risk aversion and the job loss rate (δ).

2.4 Effects of the Employment Subsidy

We now study the effect of changing the tax subsidy. We describe what happens to the steady state equilibrium when we raise the subsidy amount. Here we focus on the intuition and leave further details and proofs to Appendix A. We also present a simple quantitative exercise where we simulate the effect of a tax subsidy that is 6% of the average wage in the economy – a similar size tax cut that was instituted in Hungary – to highlight the effect of the policy on employment and wages.

Since the tax subsidy increases the value of posting vacancies (see equation (5)), firms will post more vacancies, which leads to tighter labor markets (θ) and lower equilibrium unemployment rate u. Furthermore, the employment and wage impacts of the tax subsidy vary across firm types. As we discussed before, firms get the surplus from the tax subsidy if they hire from unemployment, but competition between firms imply that the tax subsidy will benefit the workers if they are poached or if they received an offer from another firm. Since low productivity firms tend to hire from unemployment, they will benefit disproportionately more from the tax subsidy. This is because for them the value of posting a vacancy shown in equation (5) is dominated by the part coming from the "benefits of meeting with an unemployed worker".

Nevertheless, the lower equilibrium unemployment rate implies that it is going to be less likely that a low productivity firm will meet an unemployed individual as $P(u)$ falls. This dampens the vacancy posting of low productivity firms though this effect will be small in practice whenever unemployment does not change radically. We underscore this intuition by quantifying the impact of a tax subsidy that is 6% of the average wage in the economy. We apply parameter values and functional form assumptions usually applied in the literature (we provide more details in Appendix A). The results are summarized in Panel (a) of Figure 1, which shows the percent change in employment at firms below and above median productivity. In line with the intuition described above, we find that employment increases by 3.7% at the low productivity firms, and just by 0.8% at the high productivity ones.

We also quantify the effect on wages in panel (b) of Figure 1. We expect that wages will increase less at low productivity firms, which mainly employ workers who come from unemployment or workers without any outside offer.[9] At the same time, most workers at high productivity firms are poached from another firm or have received an offer from another firm, but decided to stay. These workers can use the tax subsidy as the threat point when bargaining, which drives up their wages. In line with this intuition, we find that wages increase by around 0.8% at low productivity firms, which is close to a zero pass-through

[9]If they had an outside offer, that offer would have likely come from a more productive firm and so they would have been poached by that firm.

rate, while the wages increase by around 2.9% at high productivity firms. This latter reflects a 48% pass-through rate on average: workers at high productivity firms get around 48% of the tax subsidy.

To sum up, the model predicts an interesting heterogeneity in the incidence of the tax subsidy across firm types. The tax subsidy benefits low productivity firms and workers who are employed at high productivity firms. As a result, the tax subsidy affects the composition of jobs in the economy (as more low quality jobs are created) and benefits workers at better firms disproportionately. Next, we turn to testing this prediction by studying the impact of a large payroll tax cut implemented in Hungary.

3 Background and Data

3.1 Background and the Payroll Tax Cut

We study the impact of a large age-specific payroll tax cut instituted in Hungary in 2013. Before 2013, employers paid 28.5% social security contributions. In 2013, the government decreased social security contributions of employers by around 14,500 Hungarian Forints (HUF) per month for every employee older than 55. The gross average monthly salary was around 230,000 HUF, which corresponded to a net average of 151,000 HUF, so the payroll tax cut was around 6.3% of the average salary.[10] The cut applied to both new and ongoing private sector jobs. Workers in the public sector and the self-employed were not eligible for the cut.

Besides workers aged over 55, workers under the age of 25 were also eligible for the tax cut. We estimate the impact of the policy on them in Appendix D and briefly discuss the main findings in Section 7.2. Furthermore, workers in elementary occupations received the tax subsidy independently of their age.[11] In our primary analysis we include workers in elementary occupations, but our results are robust to the exclusion of those workers.

Figure 2 depicts the average effective payroll tax rate paid by employers by employee age before and after the payroll tax subsidy was implemented. It shows the discontinuity at age 55 after the policy (in black) compared to the constant rate of 28.5% before (in gray). After

[10] The exact rules were the following. Social security contribution paid by the employers was decreased from 28.5% to 14%, but the total amount that could be received was capped at 14,500 HUF. As the minimum wage in 2013 was 98,000 HUF, almost everybody hit the cap. For the few workers who earned exactly the minimum wage at 98,000 HUF in 2013, the tax cut was HUF 14,250. In 2014, the minimum wage was raised to 101,500 HUF.

[11] Long-term unemployed re-entering the labor market, people returning to work after child-care leave, or young workers entering the labor market could received the tax benefit for 2 years independently of their age. The prevalence of these other beneficiary groups is close to zero for those aged 52-57.

the policy the average tax rate is lower than 28.5% (rate without subsidy) at all ages due to the fact that workers in elementary occupations could get the tax subsidy independently of age. Furthermore, there is a drop from 26% to 20% or by about 6 percentage points from age 54 to 55. It is worth highlighting that such a drop in tax rate does not create a discontinuity in hiring incentives at age 55. From the firm's perspective, hiring someone at one day short of age 55 is almost the same as hiring someone at exactly age 55 as the difference is simply the one day in which higher taxes need to be paid, while once age 55 is reached, the same amount of tax subsidy is received. That is why we apply a difference-in-differences empirical strategy described in detail in Section 4, instead of a regression discontinuity strategy.

The reform only affected the social security contributions paid by the employers, while the part paid by the employees was unaffected. Employees before and after the reform paid a 16% flat tax rate and an employee social security contributions of 18.5%.[12] Furthermore, the reform did not affect the link between social security contributions and future benefits (such as pensions) as those are calculated based on gross wages and not based on contribution to the social security funds.

The tax cut was first publicly discussed in the Parliament on July 2, 2012, shortly after it was announced. The legislation was passed on October 15, 2012 and the tax change was effective from January 1, 2013. Due to the relatively short period of time between the announcement and enactment of the reform, anticipatory effects appearing before the implementation of the tax cut are likely to be negligible and we find no evidence of such effects in our empirical analysis.

In the main analysis, we study the impact of the reform among older men between 2010 and 2015. Throughout this period there were no other major labor market policy changes that affected older men. We focus on men to make sure that results are not driven by early retirement policy changes for women instituted in 2011.[13] Nevertheless, we find very similar results for women in Appendix C, summarized in Section 7.1 suggesting that our results are not gender specific.

Around this period the overall employment rate in Hungary was 64%, slightly below the OECD average (66%). The employment rate of older people (age 55-64) was only 46%, substantially below the OECD average (58%). The unemployment rate steadily fell between 2012 and 2015, which reflected a substitution of welfare programs with a public work scheme (Cseres-Gergely and Molnár, 2015). At the same time, employment in the private sector was

[12]The tax wedge on labor is quite high in Hungary. The social security contributions and the income tax together implies that the average tax wedge was close to 50% in this period, which is much higher than the OECD average of 35.5% (OECD, 2022*a*).

[13]A new pension policy for women was introduced in 2011, which granted an early retirement option for women with 40 years of work credits, regardless of age.

relatively stable: the prime-age population share employed in the private sector increased slightly from 38% to 39% between 2012 and 2015. Still, to make sure our results are not driven by the improvement of labor market conditions, we show robustness to restricting the sample to local labor markets where the share of employment in the private sector among the prime-age population was stable throughout the whole period.

Since our primary focus is to study the heterogeneous impact of the policy, it is worth discussing whether different type of firms face different labor market institutions. In Hungary, it is relatively easy to hire or dismiss workers (Tonin et al., 2009). Wage bargaining takes place mostly at the individual level. The rare collective wage bargaining is based on firm-level agreements and the coverage of these policies is low. Unionization rate is around 10% in this period, which one of the lowest in the OECD (Central Statistical Office, 2016; Borbély and Neumann, 2019; OECD, 2022*b*). The weak labor market institutions and the lack of any size-specific regulations imply that firms with different size or productivity do face similar institutional constraints in setting wages and employment.

3.2 Data

We use linked employer-employee administrative data from Hungary covering years 2010–2015 on a random 50% sample of the population. The data is drawn from the population registry and then labor market information is merged to every individual based on their social security contributions. Since we observe the total population (and not just those who have a job) our data fit well to the purpose of studying changes in employment in response to the policy.

We also observe in the data the employer of the worker (or self-employment). For employers with double book keeping we can merge the balance sheet information from the Hungarian tax authority.[14]. We restrict our main analysis to men because there was a change in retirement rules affecting only women throughout the period studied here. Still, we document women's employment responses in Appendix C, summarized in Section 7.1.

An individual is defined to be a private sector employee if the pension authority records employment on the 15th of a month at a private sector firm, excluding employment under the public works scheme.[15] We include part time workers, but adjust the employment indicator

[14]The monthly labor force status and wage indicators originate from the Hungarian Social Security Administration. The demographic indicators originate from the National Health Insurance Fund Administration of Hungary. The firm-specific indicators originate from the National Tax and Customs Administration of Hungary.

[15]To ensure that the same sample is used in the employment heterogeneity analyses, we consider only those firms as private sector employers for which the firm quality indicators are not missing. Also, to avoid that a few large companies drive our results, we exclude firms which have more than 10,000 workers. These are very large and unique firms in the Hungarian context, we only have seven such firms in the whole economy.

by working hours (e.g. working 20 hours per week is considered as 0.5 employment). We observe gross wage, which includes all income that enters the pension benefit calculations. For the wage regressions we calculate (monthly) wages as of May of each year.

We generate firm-specific indicators that we use in the heterogeneity analyses. Our baseline indicator of firm quality is the value added-based total factor productivity (TFP).[16]. As another indicator of firm quality, we perform an Abowd, Kramarz, Margolis (AKM) style decomposition of wages (Abowd, Kramarz and Margolis, 1999) and calculate firm wage premia.[17] and we also use firm-level average wage (discounted and averaged over 2010-2015) as a quality indicator. Finally, we also classify firms as foreign-owned if foreign ownership is above 50%. In the Hungarian context foreign ownership is a strong predictor of firm productivity and quality.

In our main empirical analysis, we restrict the sample to men, use workers aged between 52-57 (with workers aged 52-54 serving as the control group and workers aged 55-57 comprising the treatment group). We do not study the employment change of workers older than age 58 as those workers could retire before the reform, but not after the policy change due to the elimination of some early retirement options.[18] For the workers in our sample, the retirement age was 65 (and 64 for some older cohorts). We find no evidence that the cohorts with slightly older normal retirement age behave differently at age 52-57 so our main estimates are not driven by anticipation effects stemming from extending the retirement age.

Table 1 provides summary statistics on our data. The top panel suggests that the treatment and the control age groups are remarkably similar in terms of employment, wages and share of white collar jobs. The middle panel summarizes the distribution of treatment and control workers across higher and lower quality firms. For each measure (except for foreign ownership), we divide firms into above-median and below-median groups, taking the median based on all private sector workers, irrespective of their age. The share of workers at higher quality firms is very similar in the treatment and control groups. Finally, in the bottom panel we examine the industry composition of treatment and workers. Again, we find very

[16] We use the *prodest* Stata code of Rovigatti and Mollisi (2020), apply the estimation procedure of Wooldridge (2009) and take the firm specific average of the TFP indicator over 2010-2015.

[17] To estimate the firm wage premia, we use all sample years of the linked employer-employee administrative data data. We regress wages on individual and firm fixed effects, controlling for year fixed effects, age squared and age cubed and firm size.

[18] The earliest age to retire was age 58 until 2011, but that possibility was abolished then. To retire at age 58, someone needed to have a long term employment relationship and at least 37 years of employment history. Note that all workers aged between 52 and 57 between 2012 and 2015 (our main estimation sample) could only retire at the normal retirement age, and so workers in our sample were not affected directly or indirectly through anticipation effects in our main sample. Nevertheless, workers who turned 58 before 2012 will have lower employment rate and higher retirement rate than workers who turned 58 after 2012. By restricting the treatment ages to 55-57 in our analysis we make sure our employment changes are not affected by the change in retirement rules instituted one year before our policy change.

small differences suggesting that the treatment and the control groups are similar.

4 Effect on Employment

4.1 Descriptive Evidence

Figure 3 shows the share of men working at private sector companies by age before and after the payroll tax subsidy was introduced in 2013. Panel (a) shows raw employment rates by age before (year 2012, in black) and after (years 2013-2015, in gray) the policy. The figure highlights that employment rates in the private sector gradually decline with age from 41% to 32%. Furthermore, employment rates were similar in 2012 and 2013-2015 for workers younger than 55, which highlights that private sector employment was relatively stable in this period.[19] Finally, there is a clear divergence for workers 55 and older who are affected by the tax cut.

Panel (b) shows the change in employment at private sector companies for men at each age—the difference between the 2012 (black line) and the 2013-2015 employment rate (grey line) shown in Panel (a). In the spirit of our difference-in-differences strategy, we took out the average employment change between 2012 and 2013-2015 for the workers between age 41 and 54. The figure highlights that the employment change was significantly higher above the age 55 cutoff: a 55-year-old worker was 1 percentage point more likely to be employed shortly after the policy was introduced.

4.2 Main Results

To study the impact of the payroll tax subsidy in a difference-in-differences framework, we focus on workers aged 55-57 as our treated group and workers aged 52-54 as our control group. As we discussed above, the labor market characteristics and the employment composition across firm types and industries is quite comparable in the treatment and the control groups. We also explore below the sensitivity of the estimates to changing this treatment/control definition.

To study the impact of the tax cut on employment, we estimate the following equation

$$Emp_{it} = \theta_t + \sum_{k=52}^{k=57} \alpha_k \mathbb{I}[age_{it} = k] + \beta \mathbb{I}[t \geq t_{reform}] \cdot \mathbb{I}[age_{it} \geq 55] + \varepsilon_{it}, \tag{9}$$

[19]The average private sector employment rate between age 41 and 54 in 2013-2015 is 38.4, while it is 38.3 in 2012. Therefore, the employment rate in untreated population is almost the same pre and post policy.

where Emp_{it} is measure private sector employment of individual i in month t, θ_t are monthly time effects, $\mathbb{I}[age_{it} = k]$ are age effects, $\mathbb{I}[age_{it} \geq 55]$ is a dummy for the eligibility cut-off, which is age 55 in our context, and $\mathbb{I}[t \geq t_{reform}]$ is the post reform dummy, where t_{reform} is January 2013. In the baseline specification the t index runs from January 2012 to December 2015 and we restrict the sample to individuals who are between 52 and 57 years old. As a result, the control group includes those who are between 52 and 54 years old and the treatment group includes those who are between ages 55 and 57. We cluster the standard errors at the age×period level.

Our coefficient of interest is the β term which captures the differential change in private sector employment between the periods before and after the tax cut for treated workers relative to control workers. Panel A of Table 2 reports the baseline estimates on β – the difference-in-differences estimate on the impact of the tax cut on employment. We measure private sector employment (Emp_{it}) by including part time jobs (e.g. working 20 hours per week is considered as 0.5 employment.)[20]. Column (1) shows that private sector employment increased by 0.53 percentage point from the baseline 0.33 employment rate or by 1.59 percent as a result of the payroll tax cut. In Table 2, we also calculate the implied labor demand elasticity. The effective tax cut was 6.6 percentage points (5.27% decrease in labor costs), which implies that the increase in employment corresponds to an employment elasticity of -0.30. Appendix Table B1 shows that these results are virtually identical if instead of adjusting for fractional employment (e.g. working 20 hours per week is considered as 0.5 employment), we use a binary employment indicator.

Our elasticity estimate for overall employment is close to what others found in literature. For instance, (Laun, 2017) finds an employment elasticity of -0.22 for older worker in Sweden, while (Huttunen, Pirttilä and Uusitalo, 2013) finds an elasticity of -0.1 in Finland. For younger workers, (Saez, Schoefer and Seim, 2019) find an employment elasticity of -0.23, while (Egebark and Kaunitz, 2018) estimates -0.3 in response to the young worker tax cut instituted during the Great Recession.

Motivated by the prediction of the standard search and matching framework presented in Section 2, we also investigate whether the responses to the policy differs by firm type. Columns (2) and (3) of Table 2 summarize the key results. We use regression equation (9) with an outcome variable of being employed by a firm with below (Column 2) or above (Column 3) median total factor productivity. The results show that virtually all the employment increase comes from less productive firms, while the employment change is close to zero at more productive ones.

[20] The share of part time jobs was very low in this period. Among men, around 90% of all private sector jobs is a full-time job.

Table 2 also highlights that differences in employment responses cannot be fully explained by the differential impact of the policy on the change in labor cost. Since the amount of tax subsidy was the same for every worker, the proportional change in labor cost is a bit lower at more productive firms, which tend to pay more to their workers. Indeed, we calculate that the labor cost decreases more at low TFP firms than at high TFP firm (6.02% vs. 4.45%). Still, the change in labor cost was considerable even at high TFP firms, with an almost 4.5 percent decline in labor cost. As a result, the employment elasticity with respect to cost of labor is precisely estimated for the high TFP firms as well. The estimated elasticity is -0.53 at low productivity firms and 0.01 at high productivity ones, and the difference in responses to the tax cut between the two firm types are both statistically and economically significant.

4.3 Robustness and Credibility Checks

Parallel trends. The standard identifying assumption in difference-in-differences regressions is that employment in the treatment and the control would have evolved similarly in absence of the policy change. While this assumption cannot be tested directly, we can study whether the assumption holds pre-policy. To do that we estimate the evolution of differences between treatment and control over time using the following regression:

$$Emp_{it} = \theta_t + \sum_{k=52}^{k=57} \alpha_k \mathbb{I}[age_{it} = k] + \sum_{\substack{T=2010 \\ T\neq 2012}}^{T=2015} \beta_T \mathbb{I}[Year_t = T] \cdot \mathbb{I}[age_{it} \geq 55] + \varepsilon_{it}, \tag{10}$$

where the variable definitions are the same as for equation (9). In this regression β_T shows the difference between treatment and control firms and we report those in Figure 4. The red squares show the change in employment at high TFP firms, where we use employment at above median TFP firms as a dependent variable. The blue diamonds show the estimates at low TFP firms. The figure highlights that prior to the introduction of the policy, the employment rates of treated and control workers evolved similarly both at high and low TFP firms, suggesting that the control workers are likely good counterfactual for the treatment workers. At low TFP firms employment among treatment workers increased relative to the control group exactly when the reform was introduced in 2013. The impact on employment was around 0.5-0.6 percentage point over years 2013-2015 at low productivity firms. At the same time, employment at high productivity firms stayed similar among control and treatment workers.

SUTVA assumption and changing the treatment and control definitions. Another key assumption in difference-in-differences style regressions is that the treatment does not affect the control group—the so called stable unit value treatment assumption. In our case, this does not necessarily hold as those close to the age threshold will be aged in soon to the treatment, which could affect their labor market possibilities. Nevertheless, differential treatment effects still hold even if the differences disappear as we goes closer to the age 55 cut-off.[21] This spillover effect of the treatment to the control group should be less important as we go further away from the age 55 cut-off. Indeed, Panel (b) of Figure 3 shows that relative to the average employment rate between age 40 and 54, we estimate a slightly larger treatment effect, than relative to the average employment rate of those closer to the age cut-off. In Figure 5 we further explore the robustness of our employment results to alternative definitions of the treatment and control age groups. Panel (a) shows the estimates for overall employment, while Panel (b) at low and high TFP firms separately. The first few estimates from the left keep the benchmark treatment definition (aged 55-57), but use control groups father away from the age 55 cut-off, defining as control group those who are between 52 and 53 years old or 52 years old. Both the overall employment effect and the estimated difference between the high and low TFP firms are similar in these specifications. Next we explore the changes in treatment effect, while keeping constant the benchmark control definition. We show estimates when the treatment covers only those between between 56 and 57 or age 57. The estimated effects are virtually identical in all these specifications suggesting that our estimates are not sensitive to changing the age window in the estimation.

Effects throughout the wage distribution. We estimate the change in employment throughout the entire distribution of wages a la (Cengiz, Dube, Lindner and Zipperer, 2019). Since the payroll tax cut was lump sum, we expect that employment should be mainly affected at the bottom of the wage distribution, while the employment effect should be close to zero at the upper part of the wage distribution, where the lump sum subsidy only introduce a small (relative) change in labor cost. Panel (a) of Figure 6 shows the change in employment at all firms. The estimates shows that the largest employment effects arise for workers earning between 90% and 150% of the minimum wage, but that there are also substantial effects for workers between 150% and 300% of the minimum wage. At the same time, in line with the lump sum nature of the tax cut, we do not find any change in employment above 300% of the minimum wage. Panel (b) of Figure 6 shows the employment changes separately for low and high productivity firms. The figure demonstrates that most employment changes occurred at firms with low TFP (blue diamonds). At the same time, the changes in employment at

[21] That is why we do not apply a regression discontinuity approach here.

high TFP firms (red squares) are very small and close to zero throughout the entire wage distribution. This latter partly reflects that there are fewer low wage jobs at high TFP firms (see Appendix Figure B1 on the density of jobs at each wage category). Nevertheless, even if we consider the wage category between 150% and 300% of the minimum wage, where there is a high density of jobs at both low TFP and high TFP firms we find a clear differences in the employment changes: while the change in employment is substantial and statistically significant at low TFP firms, the change in employment is close to zero at high TFP firms.

Placebo groups unaffected by the tax cut. As we mentioned in Section 3, the reform only affected private sector employees, while self-employed and workers in public sectors were unaffected by the tax cut. Employment in these groups therefore should not be affected by the policy change. Furthermore, it is also possible that changes in private sector employment simply reflect switching from the public sector or from self-employment. Table 3 explores the source of employment increase of private sector employment by estimating our main regression equation (9) with mutually exclusive outcome variables: being employed, being self-employed, working in the public sector, or being inactive/unemployed. Since these outcome variables are collectively exhaustive, the increase in one outcome must reflect a decline in other ones. Table 3 shows that employment changes among self-employed is slightly negative and an order of magnitude smaller then the employment changes we found for private sector employees. As a result, the switch from self-employment to private sector employment can explain at most 10% of the total impact. Furthermore, the slight negative impact on self-employed was fully offset by the slight positive impact on public sector jobs. As a result, the increase in the share of private sector employees mainly comes from a decline in unemployment and inactivity. Appendix Figure B2 corroborates these findings by replicating the descriptive evidence on changes in private sector jobs (Panel (b) of Figure 3) for public sector job (Panel (a)) and for self-employed (Panel (b)). The change in employment in these two placebo groups is very small, suggesting that the increase in private sector employees in the treated age groups reflect that impact of the tax cut and not something else.

Effect by various firm quality measures. So far we have focused on the heterogeneous effect of the policy along one dimension of firm quality: firms' total factor productivity. Nevertheless, there are other potential ways to measure firm quality. For instance, our theoretical framework presented in Section 2 suggests that the heterogeneous incidence should emerge if we consider high paying firms. In panel (a) of Figure 7, we replicate the heterogeneity analysis in the employment effects with other firm quality measures (for short-run effects see Appendix Table B2).

Foreign owned firms are the most productive firms that are usually well integrated into the world economy. Those firms are offering the highest paying, highest quality jobs in the Hungarian context. The estimated employment change at those firms is close to zero and statistically insignificant. At the same time, domestic firms, which are usually less efficient, responded to the policy by creating many new jobs. A similar pattern can be observed when we measure firm-quality using average wages or AKM firm effects. Low-paying firms create many new jobs, changing the composition of jobs in the economy.

To make sure that the results are not driven by the endogenous response of total factor productivity and other quality measures to the reform, we replicate the heterogeneous effects using both pre-reform and post-reform years to define the firm quality indicators. Our results are almost the same using the different definitions (Appendix Table B3).

4.4 Worker Type vs. Firm Type Heterogeneity

So far we have focused on the heterogeneous responses to the policy by firm type. Nevertheless, the differential responses by firm type might simply reflect that different type of workers sort to different type of firms. For instance, high skilled workers might have more bargaining power and they also tend to work at higher TFP firms. To explore the empirical relevance of this interpretation of our main findings, we estimate the employment effects and firm heterogeneity for workers with similar skills.

In Table 4 we replicate the main analysis for various skill groups. Panel A shows the estimates when we examine change in employment at jobs earning less than 1.5 of the minimum wage and for jobs earning above that. This is a similar exercise as in Figure 6 where we studied the employment effects throughout the entire frequency distribution. The workers below 1.5 times the minimum wage are predominantly low skilled ones and we see that their employment also increases slightly at high TFP firms. When we focus on higher skilled workers with wages at least 1.5 times above the minimum wage, we still see a clear heterogeneity in the data. Almost all the employment changes come from low TFP firms, while high TFP firms do not hire even if they employ many workers in that wage category. These results suggest that the heterogeneous employment effect by firm quality is not driven by the different earnings composition of jobs by firm quality.

Panel B shows the main estimates by worker heterogeneity when we proxy workers' skill with occupation. We calculate the change in employment separately for low paid and high paid occupations. Low paid occupations are those that pay below the median on average and high paid are those paying above that. The table shows that the employment increased at both low paid and high paid occupations by similar percentage points (0.28 and 0.24

percentage point respectively). Furthermore, there is a clear heterogeneity within both low paid and high paid occupations: virtually all the employment change comes from low TFP firms. Columns (5) and (6) also highlights that the employment elasticity is similar in low paid and high paid occupations. At low TFP firms it is close to -0.50, while at high TFP firms it is close to zero within both occupation groups.

Finally, in Panel C we study worker heterogeneity by education. Since we do not observe education directly, we again rely on occupation information in our data. First, we use the Hungarian Labor Force Survey[22] that has detailed information on education and occupation for the same individuals for a large sample of workers. We calculate the mode of the education level for each four-digit occupation. Then we assess the employment change by the mode education-level in each occupation.

The table shows that the employment increase mainly comes from the lowest skilled workers with primary or lower-secondary education. There is also a slight increase in employment for workers with tertiary education and no change in upper-secondary jobs. When we look at employment changes within an education group, we find clear indication for firm heterogeneity in all cases. Employment at low TFP firms increased within every group and the elasticities vary between -0.22 and -0.69 (see Column 5). These elasticities are statistically significant in all cases at the 5% level. At the same time, there is no evidence for significant employment change at high TFP firms in any education group. The employment change is close to zero in all cases and the elasticities are statistically insignificant at the conventional levels. Overall, these findings highlight that the firm heterogeneity is present even if we focus on group of workers with the same skill level and so our main results reflect firm heterogeneity and not per se worker heterogeneity.

4.5 Effect on Worker's Transitions and Firm Dynamics

The estimated employment change can come from two sources: 1) workers who have been employed previously and now keep their job (incumbents); or 2) workers who were unemployed/inactive before (new entrants). Panel A of Table 5 decomposes our main employment effect to these two groups. We define incumbent workers as those how had a job in the previous 12 months (between $t-1$ and $t-13$), while new entrants had at least one month without job in that period. Then we estimate regression equation (9) using as outcome variable private sector employment being incumbent/new entrant.

Panel A of Table 5 summarizes the key findings. New entrants increase by around 0.15 percentage point, which is around 28% of the overall 0.53 percentage point increase reported

[22]The Hungarian Labor Force Survey (Hungarian LFS) is very similar to the Current Population Survey in the USA.

in Panel A of Table 2. This is nevertheless a quite substantial, 35% increase relative to the new entrants' baseline population share, which was around 4.3%. The incumbents increase by 0.38 percentage point, which is 72% of the overall 0.53 percentage point increase in employment. This is a 13% increase relative to the baseline new entrant share which is around 29%. So even if the increase in incumbents is more important empirically, the proportional increase is smaller for the incumbents than the new entrants.

These results highlight that the tax cut affected labor market transitions by both differentially higher labor market (re)entry and differentially lower exit among workers in the treated age group.[23]

Besides the labor market dynamics, we can also study the change in firm dynamics. Panel B Table 3 shows the decomposition of the total change in employment at newly entering firms (did not exist in the previous calendar years) and firms existed before. We find that almost all the employment creations come from firms that existed before, suggesting that no new firms were set up in response to the tax cut. Panel C corroborates these findings by showing that the employment mainly increased at firms that existed before 2012, while the change in employment at newly created firms is negligible.

4.6 Labor Market Institutions and the Minimum Wage

We discuss the potential role of labor market institution in explaining our main findings on firm heterogeneity. As we noted before in Section 3.1, unions are week in Hungary and central bargaining of wages is almost non-existent. As a result, larger firms do not usually face more organized workforce with more institutional protections. Still to make sure that our results are not simply driven by some sizable firms, we examine heterogeneity by firm size in Appendix Table B4. We divide firms into four size categories (1-25, 26-50, 51-100, and 101+ workers). We find that employment at low-productivity firms increases at every firm size category, while the employment effects among high-productivity firms (most of which are in the largest size category) there is no consistent employment effects.

We also discuss the potential impact of minimum wages on our results (Harasztosi and Lindner, 2019; Bíró, Prinz and Sándor, 2021). In the presence of binding minimum wages,

[23] Our theoretical model presented in Section 2 predicts an increase in re-entry rate as a consequence of more vacancy posting, while job destruction rate is kept exogenous and constant. Under these assumptions the change in incumbents could be explained by the presence of advance notice lay-off, which is quite common in Hungary for the elderly. When a worker is notified in advance, she might move to a new job without becoming unemployed, and so that transition will look like a job-to-job transition in data, even if it would be someone entering from unemployment from the model perspective. An alternative way to incorporate change in incumbents would be endogenizing job destruction in the model. While this latter approach might be important for calibrating the model to the data, we can still illustrate the key channels through which payroll tax cut affects employment and wages in search models without that feature.

the tax cut could make some jobs viable, which could explain why job creation takes place disproportionately at low productivity firms. That might play indeed some role, as we saw on Figure 6 that jobs were created around the minimum wage in response to the tax cut. Nevertheless, there is also a significant job creation substantially above the minimum wage at low TFP firms, which means that what we found does not simply reflect the interaction of minimum wages and tax cuts.

We also demonstrated in the previous Section 4.5 that firm dynamics and new firms entering after 2012 are not the major source of job creation (see Table 3) and around 78% of the jobs come from incumbent workers. This again suggests that the extra jobs are unlikely to simply reflect jobs that were not viable before. Finally, as we will show later, we also find that the tax cut passed through to workers at high TFP firms, so it is not per se that high TFP firms do not change their behavior, they rather respond differently to the policy.

4.7 The Role of Economic Environment

As we discussed in Section 3.1, the Hungarian labor market was booming in this period. To understand the importance of the local labor market, we study the impact of the policy by local labor market conditions in Appendix Table B5. The country consists of 197 districts. We first divide districts by unemployment rate in 2012 and study the impact separately in the two regions. The effect of the tax subsidy in employment is somewhat larger in regions with above median unemployment rate, where the average unemployment rate was around 18.3%, than at below median-level regions, where the unemployment rate was around 8.6% (0.65 percentage points vs. 0.55 percentage points.). Nevertheless, the heterogeniety is very similar across firms, as almost all the employment change comes from low TFP firms.

In addition to that, we also divide districts by the change in private sector employment rate. In stable labor markets the change in private sector employment is less then 2 percentage points (in absolute value), while in improving labor markets the change is more than 2 percentage points. The change in employment and the heterogeneity pattern is very similar in the booming and stable environments.[24]. Overall, these findings suggests that the particular economic condition unlikely to play a major role explaining our main findings.

Finally, the employment effects and the heterogeneity patterns are similar in districts with below and above median share of individuals aged 55-57 (who form the treatment ages in our analysis).

[24]Unfortunately, we have not enough districts with substantial decline in labor market conditions and so we cannot study the impact of the tax cut in recessionary environment

4.8 Substitution

A common concern about targeted tax cuts is that firms may substitute subsidized workers for non-subsidized ones. This substitution could bias our main estimates, if they lead to substantial change in employment in the control group. Nevertheless, as we discussed in Section 4.1, there is no indication of any significant change in employment in the data. The lack of large employment responses in the control group is not surprising given that only a low share of the workers are subsidized and so the substitution effect on untreated workers should be limited.[25]

A different concern from the policy maker perspective could be that firms who hire more subsidized workers might decide to hire fewer prime age or other non-subsidized workers. We directly test the empirical relevance of this concern by studying the firm-level relationship between hiring subsidized and non-subsidized workers before and after the policy change in Appendix Figure B3. The figure shows the nonparametric (binscattered) relationship between (two-year) change in employment of subsidized workers and that of non-subsidezed ones (relative to the employment in the baseline). We calculate the pre-policy relationship by studying the change between 2010 and 2012 (black dots and line) and the post-policy relationship between 2012 and 2014 (blue stars and line). We also calculate the no substitution counterfactual (red squares and line): how much the pre-policy relationship changes if firms would increase their hiring from the non-subsidized workers as we estimated in the benchmark analysis (Table 2), but do not decrease hiring from non-subsidized workers. This no substitution counterfactual is closely aligned with the post reform relationship, indicating that substitution from non-subsidized workers is limited in our context.

5 Effect on Wages

5.1 Main Results

Motivated by the illustrative model predictions presented in Section 2, we study the impact on wages in this section.

First we study the impact of wages on new entrants by estimating the following regression equation:

[25] This argument is similar to the one made in Appendix Section B in Cengiz, Dube, Lindner and Zipperer (2019). Given that the share of subsidized workers in the aggregate production function is small, realistic values of labor-labor substitution puts an upper bound on the size of employment changes of the untreated population.

$$\ln w_{it} = \sum_{k=52}^{k=57} \alpha^k \mathbb{I}[age_{it} = k] + \theta \mathbb{I}[Year_t \geq t_{reform}] + \beta \mathbb{I}[Year_t \geq t_{reform}] \cdot \mathbb{I}[age_{it} \geq 55] + \varepsilon_{it}. \tag{11}$$

where w_{it} is the gross wage of individual i in May at year t. Notice that for wages we use yearly data and so the time period reflects years in this section. We study the change in wages at the yearly level as this is the level of observation.[26] In our case, t_{reform} is 2013.

A key limitation of the regression equation above is that it considers the same proportional wage changes across the entire wage distribution. Nevertheless, given the lump-sum nature of the tax subsidy, we expect that the proportional increase in wages will be quite small for high wage earners and could be much larger for low wage earners. To take this into account, we assess the impact of the policy by considering the "treatment intensity" of the tax cut. In particular, we calculate the size of the payroll tax cut relative to the wages in the previous year, formally $S_{it-1} = 14,500/w_{it-1}$, where 14,500 HUF is the subsidy amount. This variable goes from 14.5% for low wage earners and goes to zero for very high wage earners, and reflect the percent change in wages that would occur if all the subsidy were passed through to the worker. Then we run the following regression:

$$\begin{aligned} \ln w_{it} = &\sum_{k=52}^{k=57} (\alpha_0^k + \alpha_1^k S_{it-1}) \mathbb{I}[age_{it} = k] + (\theta_0 + \theta_1 S_{it-1}) \mathbb{I}[Year_t \geq t_{reform}] + \\ &+ (\beta_0 + \beta_1 S_{it-1}) \mathbb{I}[Year_t \geq t_{reform}] \cdot \mathbb{I}[age_{it} \geq 55] + \varepsilon_{it}. \end{aligned} \tag{12}$$

where we basically interact each term in regression equation (11) with the gap measure, S_{it-1}. To calculate S_{it-1}, we need to rely on the previous year's wage and so we can only run this regression for workers who worked in the previous year (incumbent workers). Looking through the lens of our model, this is exactly what we should do. The model predicts that the incidence of the policy would be heterogeneous among incumbent workers (who have a job offer and stayed or have been poached) and not among the new entrants (who come from unemployment and have low bargaining power).

Furthermore, to make sure that our gap measure S_{it-1} is not contaminated by the policy itself we only use one post policy year 2013 and one pre-policy year 2012 in the benchmark regression. Later we perform a robustness check where we define the gap based on wages two

[26] We only see yearly income for employment relationships spanning throughout the entire year. This is a common feature of administrative social security data (see e.g. Germany).

years before, formally $S_{it-2} == 14,500/w_{it-2}$, and then we use data from 2014 and 2012. In the benchmark specification we also focus on full-time, full-month workers, to minimize measurement error in wages, and present robustness checks which include part-time workers.

The results of the wage regressions are reported in Table 6. Column (1) estimates wage effects for new entrants using equation (11). The change in the wages of new entrants is economically small and statistically insignificant. This is in line with the prediction of the model that suggests that the effect of the policy on new entrants should be limited.

Table 6 also shows the estimates for the incumbent workers for whom we can calculate the treatment intensity. Column (2) suggests that the average impact of the tax cut on wages among incumbent workers is positive. The coefficient showing the treatment effect post policy in relation to the subsidy rate ($\beta_1 S_{it-1}$) is 0.22 (s.e. 0.09). This implies that a $1 increase in the subsidy would result in a 22 cent increase in wages on average, or that average pass-through is 22% with firms capturing 78% of the subsidy amount on average.

We now look into the heterogeneity in this treatment effect. We estimate the following equation, using the notation of equation (12):

$$\begin{aligned}\ln w_{it} = &\sum_{k=52}^{k=57} (\alpha_0^k + \alpha_1^k S_{it-1} + \alpha_2^k Q_{j(i,t-1)} + \alpha_3^k S_{it-1} Q_{j(i,t-1)})\mathbb{I}[age_{it} = k] + \\ &+ (\theta_0 + \theta_1 S_{it-1} + \theta_2 Q_{j(i,t-1)} + \theta_3 S_{it-1} Q_{j(i,t-1)})\mathbb{I}[Year_t \geq t_{reform}] + \\ &+ (\beta_0 + \beta_1 S_{it-1} + \beta_2 Q_{j(i,t-1)} + \beta_3 S_{it-1} Q_{j(i,t-1)})\mathbb{I}[Year_t \geq t_{reform}] \cdot \mathbb{I}[age_{it} \geq 55] + \varepsilon_{it},\end{aligned} \tag{13}$$

where we interact all coefficients in equation (12) with $Q_{j(i,t-1)}$, the individual i's firm quality in the previous year. We use the firm quality from the previous year to make sure that our estimates are not simply driven by transitioning to higher quality firms, but in Appendix Table B6 we show that the estimated treatment effects are even larger with applying current firm quality.

Column (3) of Table 6 shows the main estimates on treatment effect heterogeneity. The estimates show that the wage effects are driven by high-productivity firms. In high-quality firms, the pass-through rate is 53% (the sum of β_1 and β_3, which is 59% - 6%) and statistically significant. At the same time, the pass through rate is close to zero and statistically insignificant at low quality firms. This is consistent with our model which predicts that high-quality firms that compete for workers with other firms need to share the gains from the tax cut with their workers. On the other hand, lower-quality firms which employ predominantly workers without strong outside option can keep the subsidy amount.

5.2 Robustness and Credibility Checks

Parallel trends. Similarly to the employment estimates, the key identifying assumption in such a difference-in-difference style regression is that wages at high productivity firms in the treated ages would have evolved similarly to that in the control ages in the absence of the payroll tax cut. While this assumption cannot be tested directly, we can test whether the assumption holds in the pre-policy years. We estimate the same regression equation as for the main analysis, but we shift the time window to the pre-reform years and assume pre-reform (hypothetical) treatment years. Figure 8 shows the estimated pass-through when we estimate regression equation (13) using years 2011-2012 (assuming $t_{eligible} = 2012$) and 2010-2011 ($t_{eligible} = 2011$). We report the estimated pass-through at low productivity firms (β_1 from equation (13)) and high productivity firms ($\beta_1 + \beta_3$ from equation (13)). In both pre-reform placebo analysis, we find no indication for any wage change at high or low productivity firms. The effects are therefore specific to the actual treatment year.

SUTVA assumptions and changing the treatment and control definitions. Similarly to the employment estimates we also study the sensitivity of our estimates to changing the treatment and control groups to alleviate the concerns related to spillovers to the control group and the potential violation of the SUTVA assumption. Figure 9 shows the pass-through estimates for all firms (panel a) and by firm quality (panel b). The estimated patterns remain very similar if we define the control group farther away from the age 55 cut-off by using control group as workers who are 52 and 53 years old or 52 years old. We also explore how the estimates change if we define narrower treatment age groups. We show estimates when the treatment covers only those between between 56 and 57 or age 57. The estimated effects are virtually identical in all these specifications suggesting that our estimates are not sensitive to changing the age window in the estimation.

Wage change estimates at various subsidy rate categories. So far we assumed linear relationship between treatment intensity, S_{it-1} and wage changes. We also study the non-parametric relationship by estimating the change in wages for subsidy rate categories separately. In particular, we estimate regression equation (13) but replace the treatment intensity variable with a set of dummy variables showing different levels of subsidy rate. Figure 10 shows the main estimates separately for low (blue diamond) and high (red square) productivity firms. In the figure, subsidy rate increases from the left to the right and so the average past wages, w_{it-1} fall as we go to the right. The figure demonstrates that at low subsidy levels the wage changes are small for both high and low productivity firms. As we increase the subsidy rate, we do not see any wage gain for low productivity firms. To the contrary, for high productivity firms there is a gradual increase in wage changes as we move to lower wages and higher subsidy rate as we would expect if the wage changes were driven by the tax cut. The non-parametric relationship between the subsidy rate and wage changes, therefore, corroborates that the estimated wage changes at high TFP firms are driven by the tax cut and not something else.

Robustness to include part-time workers Since in our data we do not perfectly observe hours worked, so far we have focused on full-time workers whose wage information is more precisely estimated. Column (4) of Table 6 shows the estimated change in wages when we include part time workers in the sample. The estimated pass-through at high productivity firms declines with including part-time workers (from 52% to 32%) and it remains significant both economically and statistically.

Robustness to two-years change. So far we have focused on one-year changes post policy. We made this restriction because we wanted to make sure that policy change itself does not affect treatment intensity S_{it-1} through the change in previous year wages. Next we redefine the treatment as $S_{it-2} = 14500/w_{it-2}$ and then study two years changes post policy. Column (4) of Table 6 shows the estimates when apply two-year changes. The estimated pass-through is similar to the one-year change. In panel (b) of Figure 8 we also report two-year wage changes. It suggests that between 2010-2012, the wages of control and treated workers evolved fairly similarly with the divergence happening only when the tax cut was introduced in 2013.

Effect by various firm quality measures. Similarly to the employment estimates, we replicate the heterogeneity analysis in the wage effects using other indicators of firm quality. We report the results in panel (b) of Figure 7. The workers at foreign firms experienced a substantial wage increase, which was equivalent almost with a full-pass through, workers at domestic firms did not experience any wage increases. A similar pattern can be observed when we measure firm-quality using average wages or AKM firm effects. High paying firms pass through the policy to wages, whereas the pass-through of the policy to wages is around zero at low paying firms.

Overall, we see that a similar pattern of incidence emerges for a wide class of firm quality measures. Therefore, the heterogeneity in incidence that we uncovered here is a basic feature of the labor market, and not just tied to one specific quality measure. Our estimates also imply that the quality of jobs will change in response to the tax cut, as lower quality firms will create more jobs than high-quality ones.

5.3 Rent Sharing and Windfall Effects

Recent empirical evidence found that firms that received larger rent or windfall as a result of a tax cut for younger workers, grew more rapidly in the context of Sweden (Saez, Schoefer and Seim, 2019). We study the presence of such windfall effects in the context of the tax cut for the old in Hungary. The main results are summarized in Appendix Figure B4. We compare firms that have a high share of subsidized workers aged 55 and above with firms that have a medium share. Similarly to Saez, Schoefer and Seim (2019) we find a mean reversion in the subsidy rate (ratio of the windfall revenues to the total payroll). Looking at firm size, wages and sales revenue after the reform, those trend similarly for firms with high and medium shares of subsidized workers, and so we find no clear indication that windfall effects are important for that population. Interestingly, when we apply the same exercise for evaluating the impact on younger workers in Hungary, we find remarkably similar findings as in Saez, Schoefer and Seim (2019).[27] This suggests that lack of windfall effect for the older workers unlikely to reflect per say the different economic environment, but the differential impact of the tax cut on young and old workers.

Another important finding in Saez, Schoefer and Seim (2019) is that firms shared equally the rents coming from the tax cut between young treated and untreated workers. Such rent sharing would work against finding any wage effects in our empirical design that compares the wage change between treated and untreated workers. Still, as we demonstrated above, we find clear indication of wage changes between treated and untreated workers for high productivity firms.

Nevertheless, we directly assess the implication of rent sharing in Column (6) of Table 6. We calculate the firm level rent as in Saez, Schoefer and Seim (2019) by taking the ratio of all the tax cuts instituted in 2013 (including those affecting the young and elementary occupations) and the pre-reform total wage bill. Then we include that windfall measure in equation (13) and interact it with the age categories, the post reform dummy, and the post reform times treatment age dummy, and the interaction with treatment intensity variable, S_{it-1} (including all those other variables that are interacted with treatment intensity in equation (12)). The results show that including the windfall effects in the regression does not change the estimated pass-through at high and low productivity firms. If anything the estimated pass-through effects are slightly larger at high productivity firms (59% instead of 52% in the benchmark estimate) and still close to zero at low productivity firms once we take

[27] Appendix Figure D5 implement the same windfall analysis for the young workers. Similarly to Saez, Schoefer and Seim (2019), we find no pre-trends between high windfall and medium windfall firms for the young, and increase in revenue and employment at high windfall firms (relative to medium windfall) post policy.

into account the windfall effects. Appendix Table B7 also shows that the windfall effects do not change the pass-through estimates when other firm quality measures applied.

The treated post windfall coefficient suggests that firms hit by larger windfall increase more the treated population wages than the untreated ones, though the coefficient is only borderline significant. Furthermore, those effects are less important at lower wages where the labor cost cuts are more relevant. Nevertheless, the magnitude of these effects are small given that the average windfall rate was 2.7% in our sample. Overall, these findings suggests, that our results are unlikely to be altered by the rent sharing documented in Saez, Schoefer and Seim (2019).

6 Welfare Analysis

Our results suggest that the payroll tax cut targeted at workers above 55 increased the employment of the targeted workers at lower-quality firms and increased their wages at higher-quality firms. In this section we evaluate the policy's welfare impact, taking into account its costs and fiscal externalities.

We follow the method proposed by Hendren and Sprung-Keyser (2020) to calculate the Marginal Value of Public Funds (MVPF) for the age-dependent payroll tax subsidies. We apply the following formula:

$$\text{MVPF} = \frac{\text{WTP}}{\text{Net Cost}}, \tag{14}$$

where the Willingness to Pay (WTP) is the sum of individuals' willingness to pay for the policy out of their own income and the net cost is the net impact of the policy on the government budget.

The WTP consists of three parts. First, the part of the subsidy that is received by workers enters workers' WTP with a positive sign. To calculate this, we first calculate the per capita average amount of the subsidy (using the employment rate and average effective subsidy rates). Then, based on the estimated pass-through in Table 6, we determine the fraction of the subsidy that goes to the workers. Second, workers who gain employment as a results of the tax subsidy lose their unemployment benefits which enters their WTP with a negative sign. Here, we rely on the estimated treatment effects on employment (Table 6) and the average unemployment benefit as observed in our data. Third, workers who gain employment are paid wages by their employers which enters their WTP with a positive sign – to calculate this part of the WTP, we estimate the employment effect by wage categories. The net cost is the sum of the subsidy minus the benefits a non-employed person receives minus the taxes paid after the additional wage due to increasing employment.

We calculate the MVPF two different ways. First, we assume the policy maker only cares about worker's welfare and social marginal utility on the employers is zero. In that case, we do not incorporate producer surplus to WTP. Second, we assume that social marginal utility is the same on workers and employers and so we incorporate producer surplus to WTP.

We present the calculations in Table 8. When the policy maker only cares about the worker welfare, the overall MVPF is 0.27. Such a low MVPF reflects the fact that our estimates imply that most of the tax cut benefited the employers. Nevertheless, the MVPF is much larger at high productivity firms (0.45) than at low productivity ones, where it is close to zero. The difference is mainly due to the higher pass-through rate of the subsidy to workers at high quality firms.

Once we include producer surplus in the MVPF calculation then the relation is the opposite: payroll tax cuts targeting high-productivity firms have lower MVPF (0.99) than payroll tax cuts targeting low-productivity firms (1.52). This is because when the allocation of rents between employers and employees does not matter, the employment creation will dominate the welfare calculations. Since employment creation mainly takes place at low productivity firms, the MVPF will be larger for subsidizing those type of firms.

We can also compare our MVPF estimates to those estimated in the literature. Paradisi (2021) calculates that MVPF is 1.02 for the young worker tax cut analyzed by Saez, Schoefer and Seim (2019). Our comparable MVPF calculation that equally weights producers and consumers is 1.22, which implies that the tax cut in Hungary had larger value.

7 Effect on Women and Young Workers

7.1 Women

We exclude women from the main analysis to make sure that our results are not driven by a pension policy, introduced in 2011 for women.[28] Nevertheless, we estimate the same difference-in-differences model for women as for men. In Appendix Section C we provide further details, here we just describe the main findings.

Overall, we find very similar results for women. Appendix Tables C1 and C2 show that the payroll tax cut is estimated to increase private sector employment of women by 0.51 percentage point (vs. 0.53 percentage point for men) in the 55-57 age group compared to the 52-54 age group over the years 2013-2015. The heterogeneous employment patterns by firm quality are also similar among men and women: employment increases more at low

[28]The so-called "Women 40" policy grants an early retirement option for women with 40 years of work credits, regardless of age. Because we do not observe the full employment history of older women, we cannot rule out their eligibility to the early retirement option.

TFP firms, however the difference by firm type is smaller for women. The firm heterogeneity patterns are also present if we look at job categories based on wage and education level separately, suggesting that the heterogeneity is driven by firms. Appendix Figure C1 shows the wage effects for women by firm quality at different levels of effective subsidy rate. Similarly to men, wages increase only at high TFP firms and only at high subsidy rates, though the increase is smaller for women.

7.2 Young Workers

Besides the payroll tax cut for the older workers, a similar tax cut was also introduced for workers under age 25 in 2013. The tax cut lead to 6.6% reduction in the labor cost. We apply the same difference-in-differences model as for the older population to examine the impact of the policy on these workers. We describe here the basic results and provide further details in Appendix Section D.

The overall impact of the tax cut on employment was larger for younger workers than for older workers (see Appendix Table D2). The estimated employment elasticity with respect to the cost of labor is -0.77. We find similar heterogeneity in the employment responses for the young though the heterogeneity is more sensitive to the firm quality measure applied. When we use the TFP measure as firm quality the employment elasticity at low TFP firms is close to one, and around half of that at high TFP firms. When we measure quality by AKM firm effects or foreign/domestic ownership we find more striking heterogeneity: most of the employment responses emerge at low quality firms, while responses at high quality firms is close to zero as we see for the older workers (see Appendix Figure D3).

When we study the evolution of employment changes, we find a gradual increase in employment over time (Figure D4). This is similar to what (Saez, Schoefer and Seim, 2021) documented for Sweden. Turning to wages, we find no indication for significant wage differences between treated and untreated young workers (see Appendix Figure D6) or significantly higher wage growth at firms that were highly exposed to the tax cut as they employed many young worker before the tax policy (see Appendix Figure D5). Therefore, we find no evidence for the rent sharing in the context of young workers in Hungary. At the same time, we find that firms that are more exposed to the tax cut grow more.

Overall these findings are in broadly in line with the labor market responses documented by Saez, Schoefer and Seim (2019) in response to young worker tax cut in the Swedish context. Nevertheless, our finding that employment creation is predominantly made by lower quality firms is a so far undocumented feature of the policy. Furthermore, while there are many similarities in the responses of the young and older workers in Hungary, there are also notable discrepancies. For instance, we find no indication for wage changes for the young, while we estimate clear wage differences for the old. This difference could be driven by some wage rigidities that constraint firms' pass-through differently for young and old workers. For instance, passing through the tax cut to young workers could mean a wage increase for a 22-24 years old and then a wage cut once they reach age 25. At the same time, passing through the tax cut would simply mean that once age 55 is reached a pay raise is implemented. This later might be more feasible than the former because workers dislike pay cuts (Bewley, 1998; Kaur, 2019).

Another difference is that the heterogeneity in employment seems to be less strong for the young than for the old, at least for certain quality measures. Through the lens of our illustrative model, this could be explained by the fact that there are many more new entrants at the young worker's labor market than at the labor market of the old. Workers who are entering the labor market, or workers in probationary period, have no credible outside option and so firms can hire them and extract all the rents. If the share of these type of workers is large in a labor market, there will be smaller differences in the hiring incentives of the low and high productivity firms. In line with that interpretation, we show that the employment differences between high and low TFP firms are less pronounced among workers who enter the labor market at later ages and so have shorter work history (if any) by age 22-24. At the same time, more experienced workers entering the labor market at younger ages (age 18-19) seem to be affected by the tax cut just like the old: employment increases only at low TFP firms (Table D2).

8 Conclusion

This paper provides theoretical and empirical evidence for the heterogeneous impact of payroll tax subsidies on employment and wages by firm types. Based on an equilibrium search model we show that the effect of a payroll tax subsidy is positive on employment but this effect decreases with firm productivity. On the other hand, the positive effect on wages increases with firm productivity.

We exploit the introduction of age-dependent payroll tax reductions in Hungary and using rich administrative data, we provide empirical evidence that supports the predictions of our model. We estimate positive employment effects and small positive wage effects among older workers. However, there are substantial heterogeneities across firm types. The positive effect of the payroll tax cut on employment is much at lower-quality firms, while the wage effect is stronger at higher-quality firms.

Overall, our results highlight that at lower-quality firms, the incidence of payroll tax cuts mainly falls on firms, while at higher-quality firms, the incidence mainly falls on workers.

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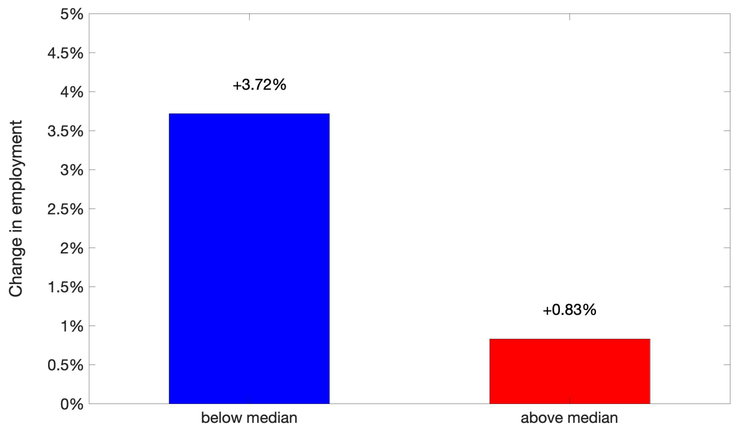
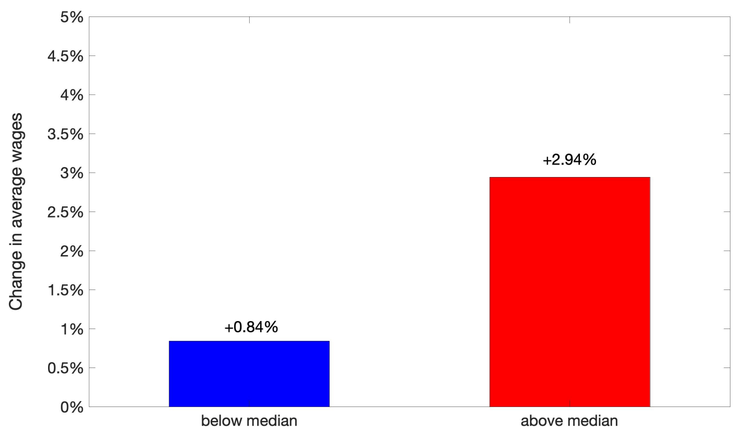
Figures and Tables

Figure 1: Simulation Results: Employment and Wage

(a) Employment

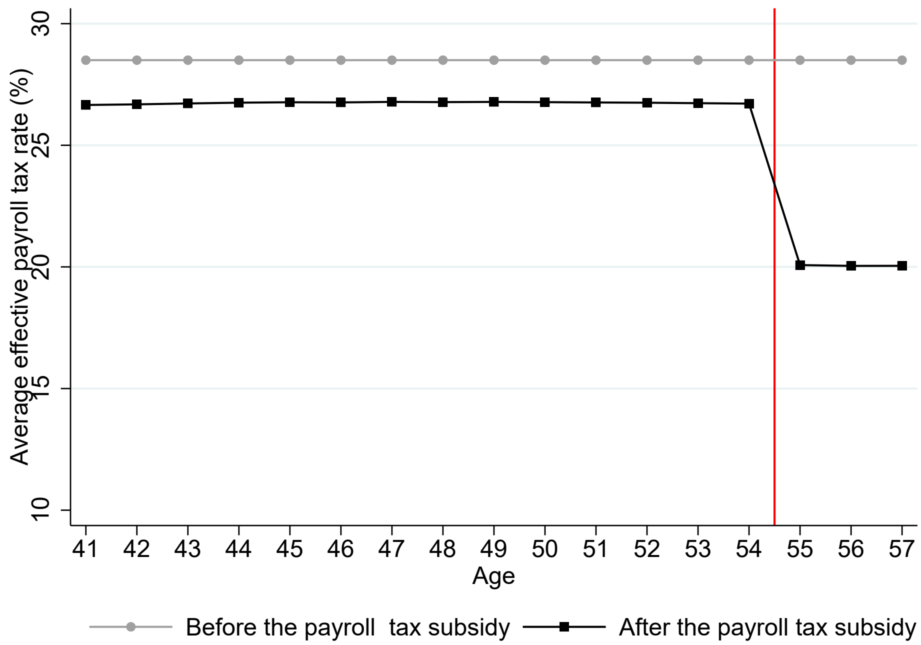


(b) Wage of Incumbent Workers



Note: The figure shows the effect of a tax subsidy that is 6% of the average wage in the economy. Panel (a) shows the impact of the subsidy on employment in percentage change by productivity category of the employer (below or above median productivity). Panel (b) shows the impact of the subsidy on the wage of incumbent workers by productivity category of the current employer (below or above median productivity). The group of incumbent workers consists of workers who earn the first wage since unemployment but were already working the previous period and workers who already had a wage bargaining.

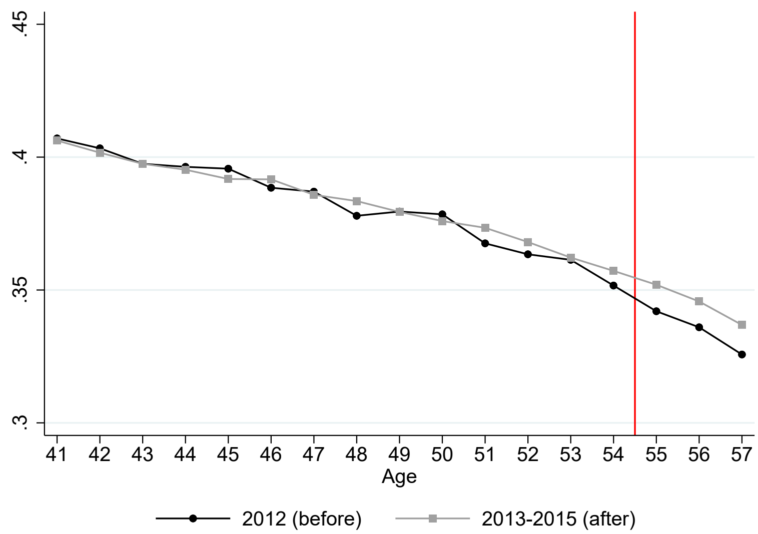
Figure 2: Average Payroll Tax Rate by Age



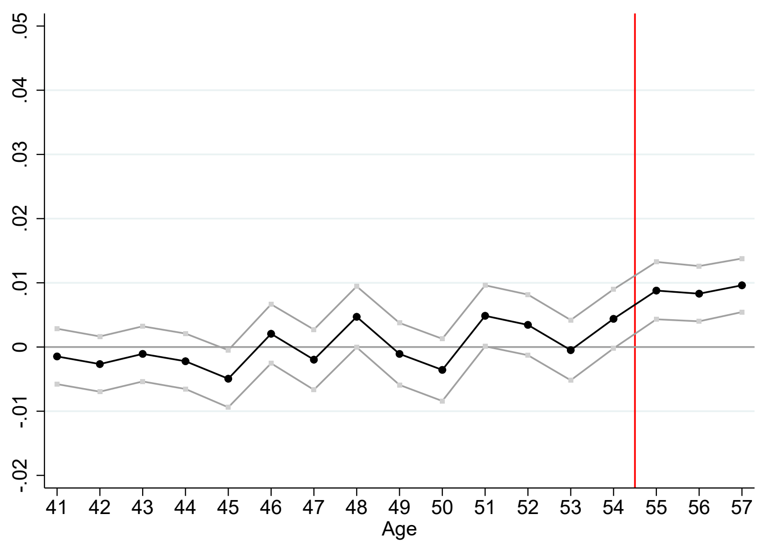
Note: This figure shows the average payroll tax rate by worker age for male workers. Before the implementation of the payroll tax subsidy, the payroll tax rate was a flat 28.5%. Between 2013-2015 (after the implementation of the subsidy), the payroll tax rate was 28.5% minus the subsidy. Using the observed gross wages in years 2013-2015 and the prevalence of beneficiaries, we calculate the effective payroll tax rate. We consider the following beneficiary groups: ages at or above 55; career starters (who had a work experience of less than 180 days); long-term unemployed (who were registered unemployed for at least 6 months the previous 9 months); people returning to work after a child-care leave and people working in elementary occupations.The age-specific subsidy and the subsidy of elementary occupations was 14.5% but capped at 14,500 HUF per month. The subsidy of career starters, long-term unemployed and people returning to work after a child-care leave was 28.5% but capped at 28,500 HUF per month for two years and 14.5% for a third year (then capped at 14,500 HUF).

Figure 3: Employment in Private Sector Companies by Age

(a) Employment Rate

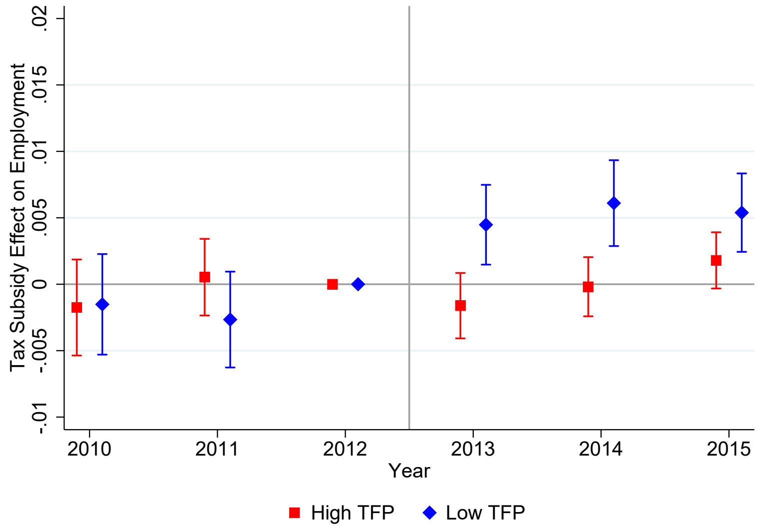


(b) Change in Employment Rate



Note: The sample is restricted to men. Panel (a) shows the employment rate in private sector companies by age separately for year 2012 (before the implementation of the payroll tax subsidy) and for years 2013-2015 (after the implementation of the payroll tax subsidy). Panel (b) shows the differences between years 2013-2015 and 2012, adjusted to mean zero over ages 41-54, with the 95% confidence interval (standard errors clustered on the individual-level). The vertical red line shows the age threshold where the tax subsidy became effective from 2013.

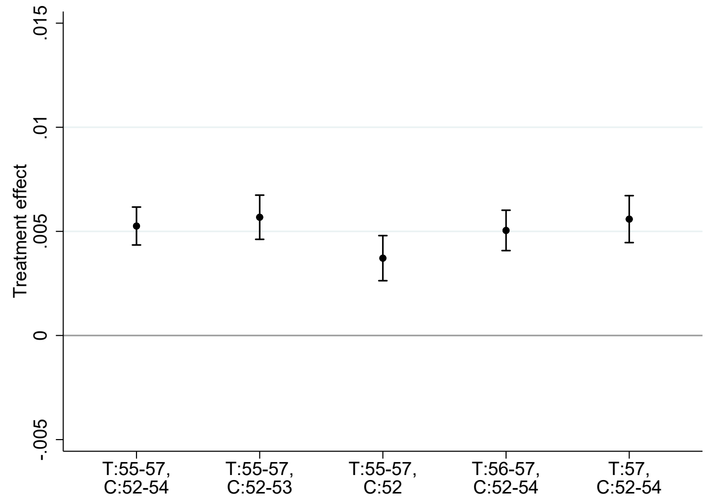
Figure 4: Employment in Private Sector Companies: Heterogeneity by TFP



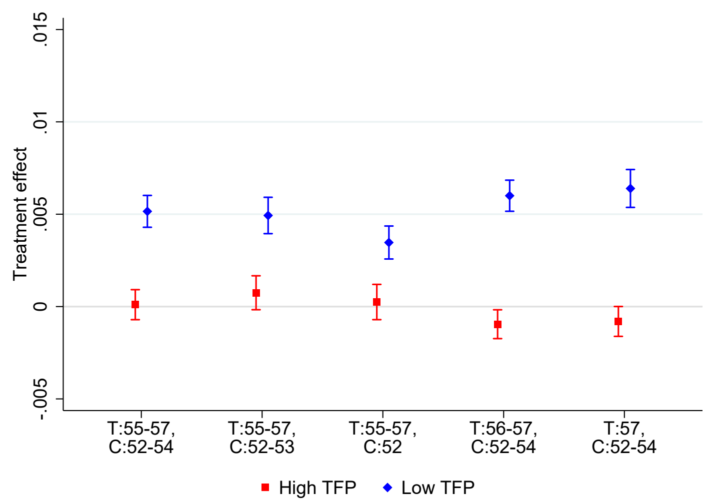
Note: The figure shows the change in employment for the treated age group (affected by the payroll tax subsidy) relative to the control age group (similar age group, but unaffected by the tax subsidy) before and after the reform. In particular, we plot the β_T from equation (10). The treated individuals are aged 55 to 57, while the control individuals are aged 52 to 54. The sample is restricted to men. The 95% confidence intervals are reported, where the standard errors are clustered at the age $\times$ period level.

Figure 5: Employment in Private Sector Companies: Alternative Control and Treatment Ages

(a) All



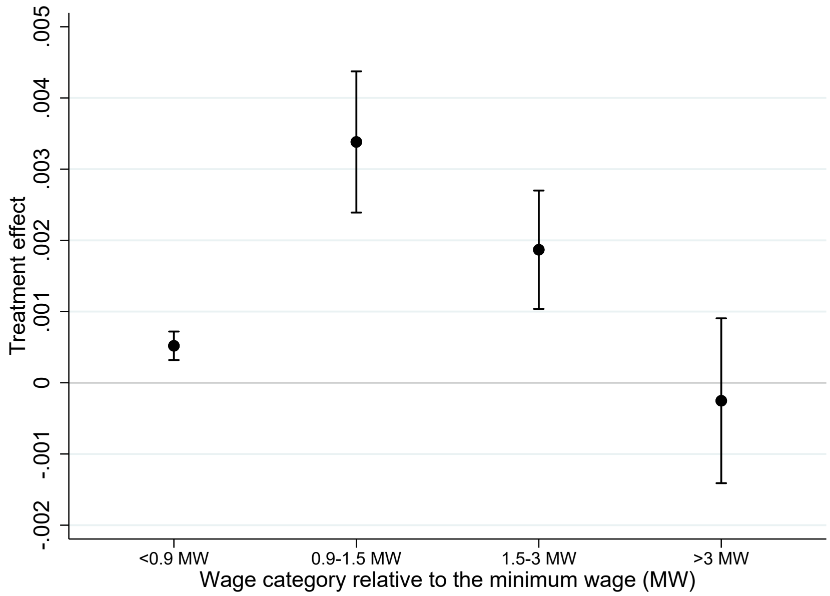
(b) By TFP



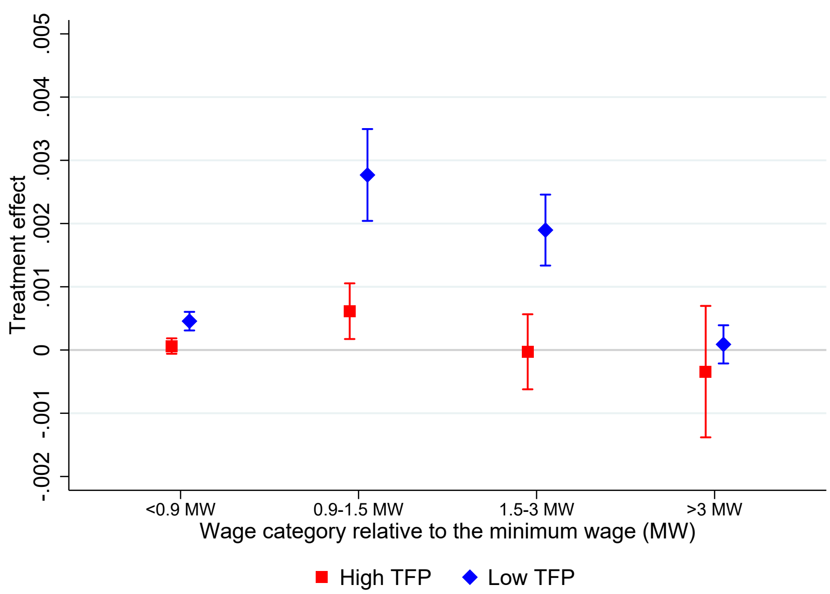
Note: The figures show estimated employment effects with the corresponding 95% confidence intervals, based on equation (9), where the standard errors are clustered at the age $\times$ period level. These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period (after the introduction of the payroll tax subsidy in 2013). "T" denotes treatment group ages, "C" denotes control group ages. The sample is restricted to men.

Figure 6: Private Sector Employment Change Throughout the Entire Frequency Distribution of Wages

(a) All



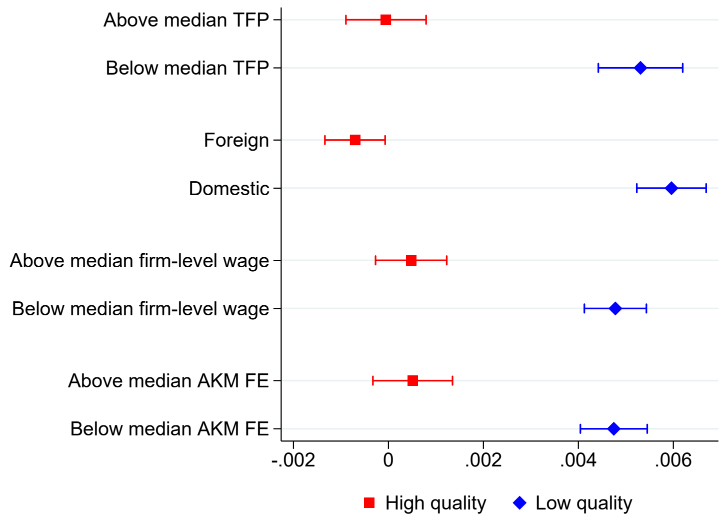
(b) By TFP



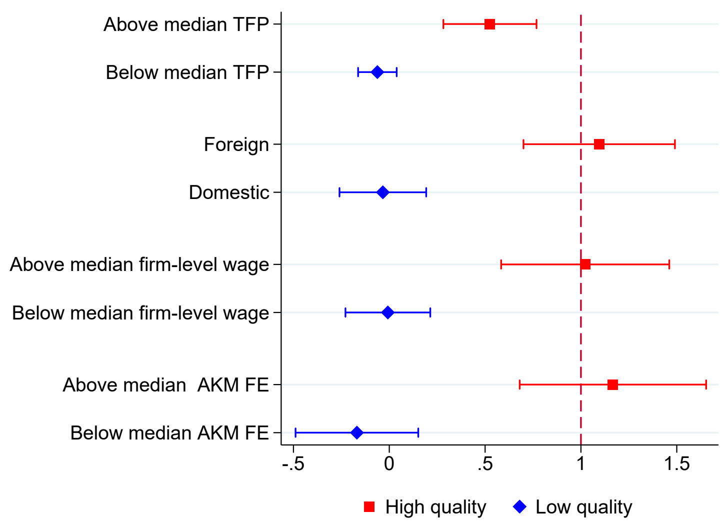
Note: The figure shows the estimated employment effects with the corresponding 95% confidence intervals, based on equation (9), with clustering at the age × period level. These are difference-in-differences estimates that compare the change in employment in a given wage category (and at a given type of firm) between year 2012 and the 2013-2015 period (after the introduction of the payroll tax subsidy in 2013). The sample is restricted to men. The treated group comprises ages 55-57 and the control group comprises ages 52-54.

Figure 7: Effect on Employment and Wages for Various Firm Quality

(a) Employment



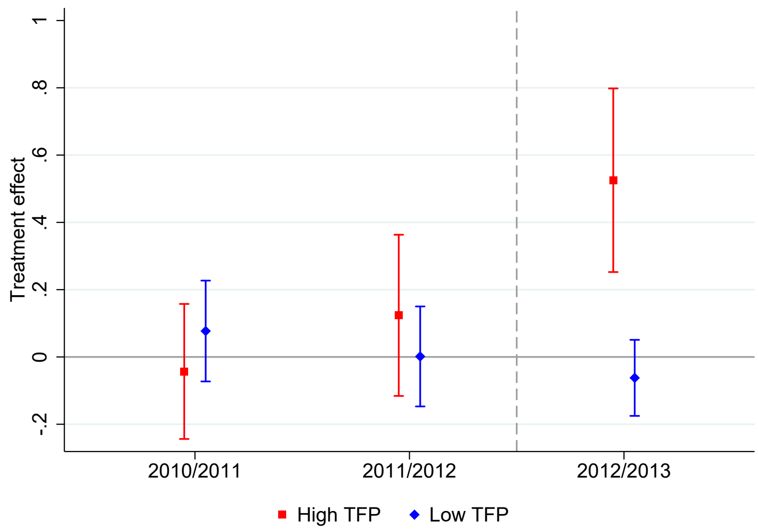
(b) Wage



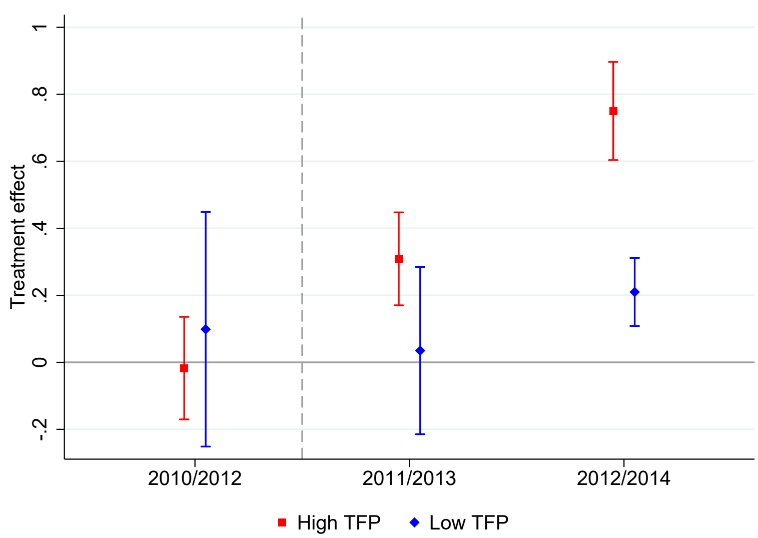
Note: Panel (a) shows treatment effect estimates on private sector employment from the model in equation (9) with 95% confidence interval. These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. In each regression, the outcome is the binary indicator of private sector employment at a firm with the given characteristic. In each regression, we control for age and quarterly date effects. Panel (b) shows the treatment effect on log wage with 95% confidence interval, using the results reported in Table 7. Heterogeneity is with respect to a binary indicator showing whether the firm is above median with respect to firm-level variables for firm quality ("high quality"), except for foreign ownership, in which case a binary indicator of foreign ownership being above 50% is used. In both panels, the sample is restricted to men. The treated group comprises ages 55-57 and the control group comprises ages 52-54.

Figure 8: Private Sector Wage Effects by TFP Over Time

(a) One-Year Wage Effects



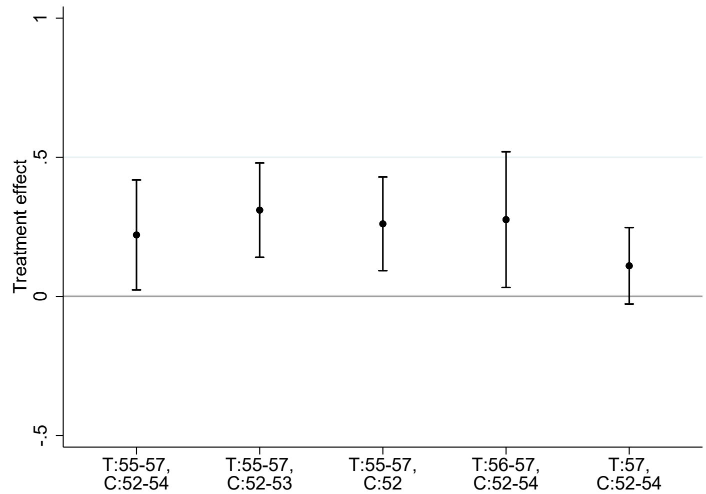
(b) Two-Year Wage Effects



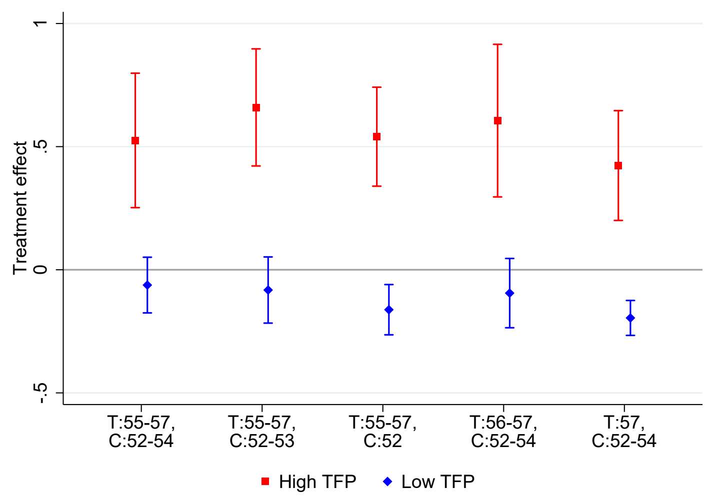
Note: The figures show treatment effects on log(wage) in private sector companies with the corresponding 95% confidence intervals, based on equation (12). Each estimation is based on the two-year sample indicated on the x-axis. In Panel (a), the wage effects are estimated over two consecutive years. In Panel (b), the wage effects are estimated over a year and two years after. The treatment years of 2011 and 2012 are pre-reform hypothetical treatment years. The sample is restricted to men. The treated group comprises ages 55-57 and the control group comprises ages 52-54.

Figure 9: Private Sector Wage Effects by TFP: Alternative Control and Treatment Ages

(a) All

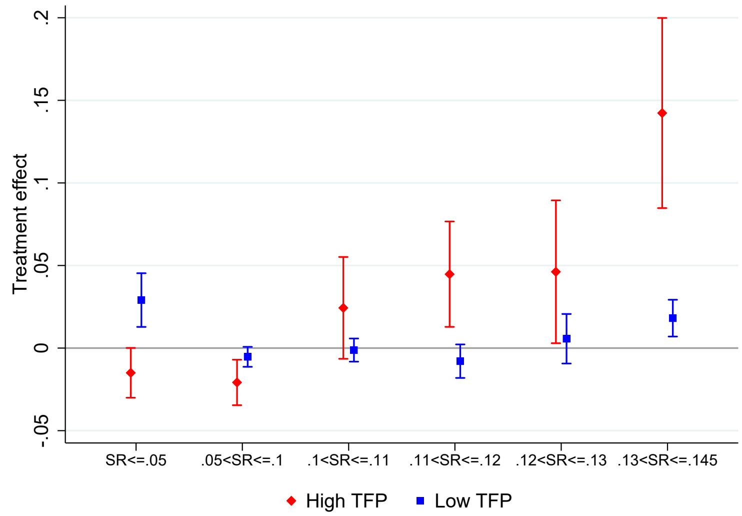


(b) By TFP



Note: The figure shows predicted treatment effects on log(wage) in private sector companies with the corresponding 95% confidence intervals, based on equations (12) and (13), using the parameter estimates for β_1 and β_3. "C" denotes control group ages while treatment ages are denoted by "T". The standard errors are clustered at the age $\times$ period level. The sample is restricted to men.

Figure 10: Private Sector Wage Effects by TFP and Subsidy Rate Categories



Note: The figure shows predicted treatment effects on log(wage) in private sector companies with the corresponding 95% confidence intervals at different categories of the subsidy rate (SR). A modified version of equation (12) is estimated, in which the linear S_{it-1} in the last interaction term is replaced with categories of S_{it-1} listed on the x-axis of the figure. The standard errors are clustered at the age $\times$ period level. The sample is restricted to men. The treated group comprises ages 55-57 and the control group comprises ages 52-54.

Table 1: Summary Statistics of Treated and Control Group in 2012

	(1) Age 52-54 (Control)	(2) Age 55-57 (Treated)
Panel A: Labor Market Characteristics		
Private sector employment	0.34	0.32
Monthly private sector wage (HUF)	218,529	217,000
White collar job (private sector workers)	0.31	0.31
Panel B: Firm quality Composition		
Above median TFP	0.49	0.48
Above median firm FE	0.49	0.48
Above median firm level average wage	0.51	0.51
Foreign ownership	0.23	0.22
Panel C: Industry Composition		
Agriculture	0.08	0.08
Manufacturing	0.35	0.36
Construction	0.10	0.10
Wholesale and retail trade	0.11	0.10
Accommodation and food service	0.02	0.02
Transportation and storage	0.12	0.10
Administrative and support	0.05	0.06
Total number of individuals	123,154	141,875

Note: The table shows summary statistics for treated and and control group in 2012. The treated group comprises ages 55-57 and the control group comprises ages 52-54. Panel A shows the labor market characteristics: the share of workers in the private sector; the monthly (full-time equivalent) wage in the private sector; and the share of private sector workers in white collar jobs. Panel B shows the share of workers at firms with above median firm quality. We calculate the median quality (measured by TFP, AKM firm effects, average wage) based on all prime age workers. In Panel B we also report the share of workers at foreign firms, which is another proxy of firm quality in the Hungarian context. Further details on how we calculated the each quality measure data are provided in Section 3. Panel C shows the share of workers in various industries.

Table 2: Employment Effects of the Tax Cut

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Change in the Probability of Employment			
— $After \times Treated$	0.0053***	0.0053***	-0.0001
	[0.0005]	[0.0005]	[0.0004]
Panel B: Percent Change in Employment			
—Without subsidy	0.330	0.167	0.163
—With subsidy	0.335	0.172	0.163
—Percent change in employment	1.59%	3.18%	-0.03%
Panel C: Percent Change in Labor cost $(1 + \tau_{ss})$			
—Without subsidy	1.27	1.26	1.28
—With subsidy	1.20	1.18	1.22
—Percent change in labor cost	-5.27%	-6.02%	-4.45%
Panel D: Implied elasticity (Panel B/Panel C)			
— Elasticity	-0.30	-0.53	0.01
	[0.03]	[0.05]	[0.06]

Note: The table assesses the change in employment at all firms (Column 1), at firms with below median TFP (Column 2), and at firms with above median TFP (Column 3). Panel A reports the β coefficient of equation (9). That coefficient reflects the difference-in-differences estimate on the change in employment between year 2012 and the 2013-2015 period in the treatment relative to the control. The treated group comprises ages 55-57 and the control group comprises ages 52-54. Panel B calculates the percent change in employment using the difference-in-differences estimates from Panel A. The first row shows the employment rate in the treated and control age group in the pre-reform year. The second row adds to that baseline the estimated change in treatment in Panel A. Panel C calculates the percent change in labor cost using a difference-in-differences estimation for tax rates, which estimation is analogous to the estimation for employment rate. The first row show the average labor cost in the control group between 2013-2015. The second row calculates the average labor cost for the treated group taking into account all the tax cut. Panel D calculates the employment elasticity with respect to the wage change by taking the ratio of the percent change of employment (Panel B) and labor cost (Panel C). Cluster robust standard errors reported in brackets, clustering at the age $\times$ period level, *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Effect of the Payroll Tax Cut by Employment Categories

	(1) Employment
Employment at private sector companies	0.0053***
	[0.0005]
	⟨0.3459⟩
Public sector	0.0016***
	[0.0003]
	⟨0.0622⟩
Self-employed	-0.0014***
	[0.0003]
	⟨0.0971⟩
Inactive/unemployed	-0.0057***
	[0.0007]
	⟨0.4980⟩

Note: Cluster robust standard errors in brackets, clustering at the age $\times$ period level, *** p<0.01, ** p<0.05, * p<0.1. Mean outcome in May 2012 in angle brackets. The table shows estimates from the model in equation (9). These are difference-in-differences estimates that compare the change in the employment in the given category between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. The sample is restricted to men. Total number of observations: 9,003,984. The treated group comprises ages 55-57 and the control group comprises ages 52-54.

Table 4: Employment Effects of the Tax Cut by Various Subgroups

	(1)	(2)	(3)	(4)	(5)	(6)
		Employment			Elasticity	
	All firms	Low TFP	High TFP	All firms	Low TFP	High TFP
Panel A: By Wage						
Jobs paying at most 1.5×minimum wage	0.0039***	0.0032***	0.0007***	0.0062***	0.0046***	0.0016***
	[0.0005]	[0.0004]	[0.0002]	[0.0005]	[0.0004]	[0.0003]
	{35%}	{27%}	{8%}			
	⟨0.1239⟩	⟨0.0922⟩	⟨0.0316⟩			
Jobs paying above 1.5×minimum wage	0.0016***	0.0020***	-0.0004	-0.17	-0.55	0.07
	[0.0005]	[0.0003]	[0.0005]	[0.05]	[0.08]	[0.09]
	{65%}	{24%}	{40%}			
	⟨0.2221⟩	⟨0.0748⟩	⟨0.1473⟩			
Panel B: By Occupation						
Low paid occupation	0.0028***	0.0030***	-0.0001	-0.29	-0.55	0.03
	[0.0004]	[0.0003]	[0.0002]	[0.04]	[0.05]	[0.05]
	{51%}	{28%}	{24%}			
	⟨0.1716⟩	⟨0.0956⟩	⟨0.0761⟩			
High paid occupations	0.0024***	0.0023***	0.0001	-0.25	-0.47	-0.02
	[0.0006]	[0.0003]	[0.0005]	[0.06]	[0.06]	[0.11]
	{49%}	{19%}	{30%}			
	⟨0.1743⟩	⟨0.0716⟩	⟨0.1028⟩			
Panel C: By Education						
Primary and lower-secondary education jobs	0.0038***	0.0037***	-0.0001	-0.29	-0.54	0.02
	[0.0005]	[0.0004]	[0.0003]	[0.04]	[0.06]	[0.05]
	{70%}	{37%}	{33%}			
	⟨0.2354⟩	⟨0.1140⟩	⟨0.1214⟩			
Upper-secondary education jobs	-0.0000	0.0004**	-0.0004	0.00	-0.22	0.34
	[0.0003]	[0.0002]	[0.0003]	[0.10]	[0.11]	[0.26]
	{16%}	{8%}	{8%}			
	⟨0.0547⟩	⟨0.0256⟩	⟨0.0291⟩			
Tertiary education jobs	0.0013***	0.0011***	0.0001	-0.54	-0.69	-0.15
	[0.0003]	[0.0002]	[0.0003]	[0.12]	[0.13]	[0.44]
	{14%}	{7%}	{7%}			
	⟨0.0528⟩	⟨0.0258⟩	⟨0.0270⟩			

Note: The share of workers aged 52-57 in year 2012 by job and TFP categories are reported in curly brackets. Mean outcome in May 2012 in angle brackets. The table shows estimates from the model in equation (9). These are difference-in-differences estimates that compare the change in employment in a given occupation category and at a given firm type between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. The sample is restricted to men. Total number of observations: 9,003,984. The treated group comprises ages 55-57 and the control group comprises ages 52-54. In each regression, we control for age and quarterly date effects. High (low) paid occupations are those four-digit occupations where the average wage was above (below) its median in 2012. The education categories of jobs are defined by imputing the modal education level of employees of the same four-digit occupation code in the 2013 Labor Force Survey of the Central Statistical Office of Hungary. Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Employment in Private Sector Companies: New Entrants and Incumbents

	(1)	(2)	(3)
		Employment	
	All firms	Low TFP	High TFP
Panel A: By Employment History			
New entrants	0.0015***	0.0014***	0.0001
	[0.0002]	[0.0002]	[0.0001]
	⟨0.0425⟩	⟨0.0267⟩	⟨0.0159⟩
Incumbents	0.0038***	0.0039***	-0.0001
	[0.0005]	[0.0004]	[0.0004]
	⟨0.2873⟩	⟨0.1409⟩	⟨0.1464⟩
Panel B: By Firm Age			
New firms	0.0001	0.0002*	-0.0001***
	[0.0001]	[0.0001]	[0.00004]
	⟨0.0054⟩	⟨0.0045⟩	⟨0.0008⟩
Old firms	0.0052***	0.0051***	0.0001
	[0.0005]	[0.0005]	[0.0004]
	⟨0.3247⟩	⟨0.1625⟩	⟨0.1622⟩
Panel C: By Firm Establishment Date			
Firms established after 2012	-0.0001	0.0002*	-0.0003***
	[0.0001]	[0.0001]	[0.0001]
	⟨0.0000⟩	⟨0.0000⟩	⟨0.0000⟩
Firms existed in 2012	0.0053***	0.0051***	0.0002
	[0.0004]	[0.0004]	[0.0004]
	⟨0.3301⟩	⟨0.1670⟩	⟨0.1631⟩

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Mean outcome in May 2012 in angle brackets. In panel A, the outcome is either employment conditional on less than 12 months employment the past 12 months (new entrants) or employment conditional on 12 months employment the past 12 months (incumbents). In panel B, the outcome is either employment at a firm that existed and had at least one worker the previous calendar year (old firms) or employment at a firm that did not exist or had zero worker the previous calendar year (new firms). In panel C, the outcome is either employment at a firm that existed in 2012 or employment at a firm that was established after 2012. These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. The sample is restricted to men. Total number of observations: 9,003,984. The treated group comprises ages 55-57 and the control group comprises ages 52-54. In each regression, we control for age and quarterly date effects.

Table 6: Private Sector Wage Effects

Outcome variable: log(wage)	(1) New entrants Baseline	(2) Incumbents Baseline	(3) Incumbents Baseline	(4) Incumbents With part-time	(5) Incumbents Two-year effects	(6) Incumbents Extended
Treat × After	0.022	-0.019*	0.008	0.022***	-0.026***	0.011
	[0.018]	[0.010]	[0.005]	[0.007]	[0.006]	[0.013]
Treat × After × Subsidy rate		0.221**	-0.062	-0.174**	0.210***	-0.097
		[0.090]	[0.053]	[0.068]	[0.046]	[0.181]
High TFP × Treat × After			-0.044***	-0.036***	-0.038***	-0.051**
			[0.012]	[0.008]	[0.017]	[0.018]
High TFP × Treat × After × Subsidy rate			0.587***	0.484***	0.540***	0.687***
			[0.122]	[0.064]	[0.076]	[0.201]
Windfall × Treat × After						0.561*
						[0.309]
Windfall × Treat × After × Subsidy rate						-6.324**
						[2.601]
Pass-through rate						
Pass-through rate, all firms		0.221**				
		[0.090]				
Pass-through rate, low TFP			-0.062	-0.174**	0.210***	-0.097
			[0.053]	[0.068]	[0.046]	[0.181]
Pass-through rate, high TFP			0.525***	0.310***	0.750***	0.590***
			[0.124]	[0.095]	[0.067]	[0.092]
Observations	13,429	97,789	97,789	112,713	82,910	97,789

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Outcome variable is log(wage). The sample is restricted to men. Except for column (4), the sample is restricted to full-time workers. Except for the first column, the sample is restricted to individuals with continuous employment the preceding 12 months (incumbents). In the first column, the sample is restricted to individuals without continuous employment the preceding 12 months (new entrants). The results are from a three-way difference-in-differences model with age groups (55-57 as treated group, 52-54 as control group), time (2012 is baseline year, 2013 is after the intervention) and the continuous variable of subsidy rate relative to last year's wage, as written in equations (12) and (13). For the subsidy rate the minimum of lagged subsidy rate in the treatment ages in 2013 is subtracted from the variable. Subsidy rate is simplified subsidy rate that takes into account only the age- and occupation-based subsidy. The table reports heterogeneity results, where everything is interacted with a binary indicator showing whether the firm has above median TFP. In column (5), the wage effects are estimated over 2012 and 2014. In column (6), we also interact the treatment, age, year and subsidy rate indicators with the firm specific continuous indicator of one-year lag of the windfall, calculated as the ratio of age- and occupation specific payroll tax subsidies payable after the reform and the total payroll.

Table 7: Private Sector Wage Effects: Heterogeneity by Firm Quality Indicators

Outcome variable: log(wage)	(1) TFP	(2) Foreign ownership	(3) Mean firm wage	(4) AKM FE
Treat × After	0.008	0.003	-0.004	0.016
	[0.005]	[0.012]	[0.013]	[0.020]
Treat × After × Subsidy rate	-0.062	-0.034	-0.007	-0.170
	[0.053]	[0.115]	[0.113]	[0.163]
High quality × Treat × After	-0.044***	-0.067***	-0.060***	-0.081***
	[0.012]	[0.014]	[0.001]	[0.015]
High quality × Treat × After × Subsidy rate	0.587***	1.129***	1.030***	1.336***
	[0.122]	[0.193]	[0.165]	[0.192]
Pass-through rate, low TFP	-0.062	-0.034	-0.007	-0.170
	[0.053]	[0.115]	[0.113]	[0.163]
Pass-through rate, high TFP	0.525***	1.095***	1.022***	1.167***
	[0.124]	[0.202]	[0.224]	[0.248]
Observations	97,789			

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1. Outcome variable is log(wage). The sample is restricted to men. Baseline result is from a three-way difference-in-differences model with age groups (55-57 as treated group, 52-54 as control group), time (2012 is baseline year, 2013 is after the intervention) and the continuous variable of subsidy rate relative to last year's wage, as written in equation (12). For the subsidy rate the minimum of lagged subsidy rate in the treatment ages in 2013 is subtracted from the variable. Subsidy rate is simplified subsidy rate that takes into account only the age- and occupation-based subsidy. The other columns report heterogeneity results, where everything is interacted with a binary indicator showing whether the firm is above median with respect to firm-level variables for firm quality ("high quality"), except for foreign ownership, in which case a binary indicator of foreign ownership being above 50% is used.

Table 8: Marginal Value of Public Funds

	(1) All firms	(2) Low TFP	(3) High TFP
(1) Direct cost	5116	2402	2774
(2) Subsidy going to workers	974	-128	1255
(3) Benefit receipt of non-employed who become employed	328	328	-6
(4) Additional net wages of non-employed who become employed	510	473	-10
(5) Additional tax revenue	438	401	-9
(1)-(3)-(5) Net cost	4349	1673	2789
(2)+(4)-(3) Willingness to pay (WTP), workers only	1155	17	1251
(1)+(4)-(3) Willingness to pay (WTP), workers and firms	5297	2547	2770
MVPF, workers only	0.27	0.01	0.45
MVPF, workers and firms	1.22	1.52	0.99

Note: Per worker average monthly amounts in HUF for workers aged 55 and above. The direct cost (row 1) is the subsidy amount times the employment rate of the subsidized group. The subsidy going to workers (row 2) is based on the wage effect results reported in Table 6. The benefit receipt of non-employed who become employed (row 3) is based on the estimated employment effect of the reform and the average unemployment benefit amount. The additional net wages of non-employed who become employed (row 4) is based on estimated employment effects by wage categories. The additional tax revenue (row 5) is the total estimated income tax and social security contributions paid after workers hired due to the reform.

Appendix

A The Effect of Tax Subsidies in Search Models

A.1 Setup

Firms are heterogeneous, characterized by productivity $y \in [0, \infty]$, with cumulative distribution function $\Psi()$. A job offer is a draw of a firm productivity from the vacancy distribution $\Gamma()$ with probability distribution function $\gamma()$. For simplicity, we assume that the output of an y-productivity firm is also y.

Workers are homogeneous. Workers are either unemployed or employed. If unemployed, they receive leisure of value b and search for jobs with probability one. If employed, they receive wage w, search for a new job with probability $s \in [0, 1]$ and can separate from their job exogeneously with probability $\delta \in [0, 1]$.

Firms can advertise vacancies at the increasing and convex cost $\kappa()$. Job market tightness is the ratio between total vacancies (v) and total search effort by the unemployed (u) and employed ($(1-\delta)(1-u)$):

$$\theta = \frac{v}{u + s(1-\delta)(1-u)}. \tag{15}$$

The probability for a searching worker of locating an open vacancy is $\phi(\theta)$, increasing in θ. The probability for an open vacancy of meeting a worker who is searching for jobs is $\phi(\theta)/\theta$, decreasing in θ.

Wage setting is as in the sequential auction model of Postel-Vinay and Robin (2002). When an employed worker contacts an open vacancy, the prospective poacher and the incumbent employer observe each other's match qualities with the worker, and engage in Bertrand competition over contracts. The worker chooses the contract that delivers the larger value. For simplicity, we also assume that all the bargaining power is at the firms and so they are able to extract all rents from the workers.[29]

A.2 Bellman Equations

The value of unemployment, using that firms extract all the rents from unemployed workers, making them indifferent between working and remaining unemployed:

$$V_u = b + \beta V_u, \tag{16}$$

where β is the discount factor. Thus,

$$V_u = \frac{b}{1-\beta}. \tag{17}$$

[29]It is straightforward to introduce some bargaining power of the worker in the model. Nevertheless, empirical studies find usually that bargaining power is quite small and so we do not miss a lot by abstracting away from that.

The maximum value the firm is willing to promise to deliver to the worker is:

$$V(y,\tau) = y + \tau + \delta\beta V_u + (1-\delta)\beta V(y,\tau), \tag{18}$$

where τ is the employment subsidy. Here, the possibility of the worker being poached by another firm is implicitly included in the $V(y,\tau)$ formula. Note also that if no outside offers arrive then the continuation value of the worker is $V(y,\tau)$. If the worker is poached then she is poached at value $V(y,\tau)$. Either way, the continuation value of the worker who survives the exogenous separation is $V(y,\tau)$, which is the maximum value the firm can deliver (Moscarini and Postel-Vinay, 2018).
After rearrangement:

$$(1-\beta+\delta\beta)V(y,\tau) = y + \tau + \frac{\delta\beta b}{1-\beta}. \tag{19}$$

The value of posting vacancies is, using that workers have no bargaining power:

$$V_v(y,\tau) = \max_{\nu}\Bigg\{ -\kappa(\nu) + \beta\nu\frac{\phi(\theta)}{\theta}P(u)\Big[V(y,\tau) - V_u\Big] + \\ + \beta\nu\frac{\phi(\theta)}{\theta}(1-P(u))\int_0^y \Big[V(y,\tau) - V(y',\tau)\Big]d\Gamma(y')\Bigg\}. \tag{20}$$

Using equation (19), equation (4) can be rewritten:

$$V_v(y,\tau,\nu) = \max_{\nu}\Bigg\{ -\kappa(\nu) + \beta\nu\frac{\phi(\theta)}{\theta}P(u)\Bigg[\frac{y+\tau+\frac{\delta\beta b}{1-\beta}}{1-\beta+\delta\beta} - V_u\Bigg] + \\ + \beta\nu\frac{\phi(\theta)}{\theta}(1-P(u))\int_0^y \Bigg[\frac{y-y'}{1-\beta+\delta\beta}\Bigg]d\Gamma(y')\Bigg\}, \tag{21}$$

where the probability that a randomly drawn job applicant is unemployed is:

$$P(u) = \frac{u}{u+(1-\delta)s(1-u)}. \tag{22}$$

As in Bagger and Lentz (2019), the sampling distribution from the vacancy pool is the recruitment intensity weighted firm-type distribution:

$$\Gamma(y) = \frac{\int_0^y \nu(y',\tau)d\Psi(y')}{\int_0^1 \nu(y',\tau)d\Psi(y')}. \tag{23}$$

The total amount of vacancies is $v = \int_0^1 \nu(y',\tau)d\Psi(y')$.

A.3 Equilibrium

The cumulative distribution of employment is $L()$, with:

$$L(y) = (1-\delta)\Big[1 - s\phi(\theta)(1-\Gamma(y))\Big]L(y) + \phi(\theta)\Gamma(y)u. \tag{24}$$

Employment at firms with productivity y is:

$$l(y) = (1-\delta)\left[\Big[1 - s\phi(\theta)(1-\Gamma(y))\Big]l(y) + s\phi(\theta)\gamma(y)\int_0^y l(y')dy'\right] + \phi(\theta)\gamma(y)u. \tag{25}$$

The steady state rate of unemployment is:

$$u = (1-\phi(\theta))u + \delta(1-u). \tag{26}$$

Thus,

$$u = \frac{\delta}{\delta + \phi(\theta)}. \tag{27}$$

Firms maximize their profit and so they post vacancies up to the point where the marginal value of a vacancy is zero.

$$\begin{aligned}\kappa'(\nu(y,\tau)) = \beta\frac{\phi(\theta)}{\theta}P(u)\left[\frac{y+\tau+\dfrac{\delta\beta b}{1-\beta}}{1-\beta+\delta\beta} - V_u\right] + \\ + \beta\frac{\phi(\theta)}{\theta}(1-P(u))\int_0^y\left[\frac{y-y'}{1-\beta+\delta\beta}\right]d\Gamma(y'). \end{aligned}\tag{28}$$

The equilibrium solution of Θ and $\Gamma(y)$ satisfies equations (1), (22), (23), (24), (27) and (28).

A.4 Wage

This section is based on Postel-Vinay and Robin (2002).

Contracts can be renegotiated by mutual consent. If a worker of a firm with productivity y receives an outside offer from a firm with productivity y' then three events can occur:

1. *Worker is poached:* The poaching firm wins the competition over the incumbent firm if $y' > y$ and the wage increases.

2. *Wage renegotiation:* If the worker meets an outside firm that would be willing to offer greater value than the worker's current contract but cannot offer more than the worker's current firm, the contract is renegotiated and the worker stays. After the introduction of the employment subsidy, wage renegotiation happens if $q(w,y) < y' + \tau < y + \tau$, where $q(w,y)$ is the threshold productivity, defined by $\xi(q(w,y),y) = w$, where $\xi()$ is the wage function. I.e., $q(w,y)$ is the lowest productivity y' such that the Bertrand competition between firm y and firm y' raises the wage above w. Importantly, the

introduction of the employment subsidy increases the probability of wage renegotiation at the incumbent firm.

3. *No change:* If neither of the above two conditions is met, the worker stays at the current firm and the wage remains unchanged.

Competition between firms implies that workers are moving in the direction of extracting the full value of the employment subsidy, using the full surplus extraction at the less productive firm as the outside option.

$U(x)$ is the instantaneous utility flow from income x. The value of employment at firm of type y and paid wage w is $V_e(w, y+\tau)$. The type-y firm optimally offers to the unemployed worker the wage $\xi_u(y+\tau)$ that exactly compensates this worker for his opportunity cost of employment:

$$V_e(\xi_u(y+\tau), y+\tau) = V_u. \tag{29}$$

A worker moves to a potentially better match with a firm type-y' if it offers at least the wage $\xi(y, y')$ defined by:

$$V_e(\xi(y, y'), y') = V_e(y, y). \tag{30}$$

Lower offers are outbid by the type-y incumbent firm.

The Bellman equation for the value of employment is the following:

$$\begin{aligned}
&\Big(\delta + \frac{1-\beta}{\beta} + \phi(\theta)\bar{\Gamma}(q(w, y+\tau), \tau)\Big)V_e(w, y+\tau) = \\
&= U(w) + \phi(\theta)\Big[\Gamma(y+\tau, \tau) - \Gamma(q(w, y+\tau), \tau)\Big]E_\Gamma\{V_e(X, X)|q(w, y+\tau) \leq X \leq y+\tau\}+ \\
&+ \phi(\theta)\bar{\Gamma}(y+\tau, \tau)V_e(y+\tau, y+\tau) + \delta V_u,
\end{aligned} \tag{31}$$

where $q(w, y+\tau)$ is the threshold productivity, defined by $\xi(q(w, y), y) = w$. The second term in the right hand side of equation (31) captures the employment value after a wage increase at the current firm, whereas the third term captures the value of employment at a higher productivity firm (after being poached).

Assuming CRRA utility function with ζ rate of relative risk aversion ($\zeta \geq 0$ and $\zeta \neq 1$), we can derive the expression of wages, following Postel-Vinay and Robin (2002):

$$\ln \xi(y+\tau, y'+\tau) = \frac{1}{1-\zeta}\ln\Big[(y+\tau)^{1-\zeta} - \frac{(1-\zeta)\phi(\theta)}{\frac{1-\beta}{\beta}+\delta}\int_{y+\tau}^{y'+\tau}\bar{\Gamma}(x, \tau)x^{-\zeta}dx\Big]. \tag{32}$$

The wage of workers whose wage is the first salary after unemployment is:

$$\ln \xi(b, y+\tau) = \ln \xi_u(y+\tau) = \frac{1}{1-\zeta}\ln\Big[b^{1-\zeta} - \frac{(1-\zeta)\phi(\theta)}{\frac{1-\beta}{\beta}+\delta}\int_{b}^{y+\tau}\bar{\Gamma}(x, \tau)x^{-\zeta}dx\Big]. \tag{33}$$

Note, that the direct effect of the tax subsidy τ on $\xi(b, y+\tau)$ is zero. The negative terms in the above two equations capture the option value of employment: workers accept lower wages to work at more productive firms because workers trade a lower wage now for increased chances of higher wages. tomorrow (Postel-Vinay and Robin, 2002).

The derivation of equations (32) and (33) is based on Postel-Vinay and Robin (2002). We start from equation (31). Plugging in $w = y+\tau$ into equation (31), using that $q(y, y) = y$ gives

$$V_e(y+\tau, y+\tau) = \frac{U(y+\tau) + \delta V_u}{\frac{1-\beta}{\beta} + \delta}. \tag{34}$$

We plug this expression in to equation (31) and use that by definition, $V_e(w, y+\tau) = V_e(q(w, y+\tau), q(w, y+\tau))$:

$$\begin{aligned}
&\Big(\delta + \frac{1-\beta}{\beta}\Big)V_e(w, y+\tau) = U(w) + \delta V_u - \\
&- \phi(\theta)\bar{\Gamma}(q(w, y+\tau), \tau)V_e(q(w, y+\tau), q(w, y+\tau)) + \phi(\theta)\bar{\Gamma}(y+\tau, \tau)V_e(y+\tau, y+\tau) + \\
&+ \phi(\theta)\int_{q(w,y+\tau)}^{y+\tau} \frac{U(x) + \delta V_u}{\frac{1-\beta}{\beta} + \delta} d\Gamma(x) = U(w) + \delta V_u - \\
&- \phi(\theta)\bar{\Gamma}(q(w, y+\tau), \tau)V_e(q(w, y+\tau), q(w, y+\tau)) + \phi(\theta)\bar{\Gamma}(y+\tau, \tau)V_e(y+\tau, y+\tau) + \\
&+ \phi(\theta)[V_e(x, x)\Gamma(x)]_{q(w,y+\tau)}^{y+\tau} - \phi(\theta)\int_{q(w,y+\tau)}^{y+\tau} \frac{U'(x)}{\frac{1-\beta}{\beta} + \delta}\Gamma(x)dx = U(w) + \delta V_u - \\
&- \phi(\theta)V_e(q(w, y+\tau), q(w, y+\tau)) + \phi(\theta)V_e(y+\tau, y+\tau) - \phi(\theta)\int_{q(w,y+\tau)}^{y+\tau} \frac{U'(x)}{\frac{1-\beta}{\beta} + \delta}\Gamma(x)dx = \\
&= U(w) + \delta V_u - \phi(\theta)V_e(q(w, y+\tau), q(w, y+\tau)) + \phi(\theta)V_e(y+\tau, y+\tau) - \\
&- \phi(\theta)[V(x, x)]_{q(w,y+\tau)}^{y+\tau} + \phi(\theta)\int_{q(w,y+\tau)}^{y+\tau} \frac{U'(x)}{\frac{1-\beta}{\beta} + \delta}\bar{\Gamma}(x)dx = \\
&= U(w) + \delta V_u + \phi(\theta)\int_{q(w,y+\tau)}^{y+\tau} \frac{U'(x)}{\frac{1-\beta}{\beta} + \delta}\bar{\Gamma}(x)dx.
\end{aligned} \tag{35}$$

It follows that

$$\begin{aligned}
&\Big(\delta + \frac{1-\beta}{\beta}\Big)V_e(q(w, y+\tau), q(w, y+\tau)) = U(q(w, y+\tau)) + \delta V_u = \\
&= U(w) + \delta V_u + \phi(\theta)\int_{q(w,y+\tau)}^{y+\tau} \frac{U'(x)}{\frac{1-\beta}{\beta} + \delta}\bar{\Gamma}(x)dx,
\end{aligned} \tag{36}$$

and after rearrangement,

$$U(w) = U(q(w, y+\tau)) - \phi(\theta)\int_{q(w,y+\tau)}^{y+\tau} \frac{U'(x)}{\frac{1-\beta}{\beta} + \delta}\bar{\Gamma}(x)dx. \tag{37}$$

Next, we use that $q(\xi(y+\tau, y'+\tau), y'+\tau) = y+\tau$ and $q(\xi(b, y+\tau), y+\tau) = b$, giving the following results:

$$U(\xi(y+\tau, y'+\tau)) = U(y+\tau) - \phi(\theta)\int_{y+\tau}^{y'+\tau} \frac{U'(x)}{\frac{1-\beta}{\beta}+\delta}\bar{\Gamma}(x)dx, \tag{38}$$

$$U(\xi(b, y+\tau)) = U(b) - \phi(\theta)\int_{b}^{y+\tau} \frac{U'(x)}{\frac{1-\beta}{\beta}+\delta}\bar{\Gamma}(x)dx. \tag{39}$$

Using the utility function $U(x) = x^{1-\zeta}$ immediately gives equations (32) and (33).

The equilibrium within-firm distribution of wages has two components, the employer effect (y) and a random effect (q) that characterizes the most recent wage mobility. We denote with $G(q|y,\tau)$ the cumulative distribution function of the conditional distribution of bargaining position within the pool of workers within type-y firms.

$$G(w|y,\tau) = \tilde{G}(q|y,\tau) = \frac{(1+\Upsilon\bar{\Gamma}(y,\tau))^2}{(1+\Upsilon\bar{\Gamma}(q,\tau))^2} \tag{40}$$

for all $q \in [0, y]$, where $\Upsilon = \phi(\theta)s/\delta$ and $\bar{\Gamma} = 1 - \Gamma$.

Equation (40) is derived through the following steps (based on Postel-Vinay and Robin, 2002) – to simplify notation, we omit the subsidy parameter here. Let $\tilde{L}(y)$ denote the fraction of workers at firms with productivity less than y. Hence, $\tilde{L}(y)(1-u)$ is the number of workers at firms with productivity less than y. The density of workers at type y firms is denoted by $\tilde{l}(y)$.

In equilibrium,

$$u\phi(\theta)\Gamma(y) = \left[\delta + \phi(\theta)s(1-\Gamma(y))\right]\tilde{L}(y)(1-u). \tag{41}$$

After rearrangement and plugging in the equilibrium value of y,

$$\tilde{L}(y) = \frac{\Gamma(y)}{1 + \dfrac{\phi(\theta)s}{\delta}\bar{\Gamma}(y)}. \tag{42}$$

Differentiation of this term with respect to y gives:

$$\tilde{l}(y) = \frac{1 + \dfrac{\phi(\theta)s}{\delta}}{(1 + \dfrac{\phi(\theta)s}{\delta}\bar{\Gamma}(y))^2}\gamma(y). \tag{43}$$

There are $G(w|y)\tilde{l}(y)(1-u)$ workers employed at firms of type y, and paid at most w. Workers leave this category either because they are laid off or because they receive an offer from a firm with productivity $y \geq q(w,y)$ that grants them a wage increase or induces them to leave their current firm. On the inflow side, workers enter this category either from a firm less productive than $q(w,y)$, or they come from unemployment. The steady-state equality of inflow and outflow is:

$$\big[\delta + \phi(\theta)s\bar{\Gamma}(q(w,p))\big]G(w|y)\tilde{l}(y)(1-u) = \phi(\theta)u\gamma(y) + \phi(\theta)s\tilde{L}(q(w,p))(1-u)\gamma(y). \quad (44)$$

Using $\phi(\theta)u = \delta(1-u)$, plugging in equations (42) and (43) to equation (44) and using the notation $\Upsilon = \phi(\theta)s/\delta$ gives equation (40).

A.5 Effects of the Employment Subsidy

As in Bagger and Lentz (2019), hiring intensity increases in firm productivity y because both the output and the acceptance rate increase with y in the right hand side of equation (28). Using that $\kappa()$ is increasing in ν leads us to Result 1.

Result 1 *Hiring intensity is increasing in firm productivity:* $\dfrac{\partial \nu(y,\tau)}{\partial y} > 0.$

Our next result follows directly from equation (28), using that $\kappa()$ is increasing and convex in the amount of vacancies.

Result 2 *The direct effect of the employment subsidy on vacancy posting is positive.*

Result 2 implies that due to its effect on vacancy posting, the employment subsidy has a positive effect on job market tightness (θ), which in turn, decreases the equilibrium unemployment rate u.

It follows directly from equation (27) that the impact of the tax subsidy on the rate of unemployment is negative, using that θ increases in τ (due to increasing vacancy posting) and $\phi(\theta)$ increases in θ. Equation (22) can be rewritten as:

$$P(u) = \frac{\delta}{\delta + (1-\delta)s\phi(\theta)}. \quad (45)$$

Using that $\phi(\theta)$ increases in θ, which in turn increases in τ, Result 3 immediately follows.

Result 3 *The equilibrium value of unemployment and $P(u)$ decrease in τ.*

Looking at firms' optimality condition – equation (28), the direct effect of the subsidy (τ) on the right hand side of the equation is the same for all firms. Based on the convexity of the vacancy cost function $\kappa()$ and using that $\nu(y,\tau)$ increases in y, it follows that the increase in vacancies ($\nu(y,\tau)$) is smaller at higher values of y and approaches zero as $y \to \infty$.

Based on equation (25) and using that $\Gamma(0) = 0$, and $\int_0^0 l(y')y' = 0$:

$$l(0) = \frac{\phi(\theta)\gamma(0)u}{1-(1-\delta)[1-s\phi(\theta)(1-\gamma(0))]} = \frac{\phi(\theta)\gamma(0)u}{\delta + (1-\delta)s\phi(\theta)(1-\gamma(0))}. \quad (46)$$

Using Result 2, the direct effect of the tax subsidy (i.e., considering only the effect on posted vacancies and not the effect on job market tightness and unemployment) on $l(0)$ is positive.

Also based on equation (25) and using that $\lim_{y\to\infty}\Gamma(y) = 1$, and $\int_0^\infty l(y')y' = u$, it follows that

$$\lim_{y\to\infty} l(y) = \lim_{y\to\infty} \frac{(1-\delta)s\phi(\theta)\gamma(y)(1-u) + \phi(\theta)\gamma(y)u}{\delta}. \tag{47}$$

This leads us to Result 4.

Result 4 *The direct effect of employment subsidy on the value of vacancy decreases with firm productivity and approaches zero at the highest productivity levels. The direct effect of employment subsidy on employment is positive at the lowest productivity firm and approaches zero at the highest productivity levels.*

Using equations (46) and (47) and plugging in equation (27) for u gives the ratio of employment at the lowest and highest quality firm:

$$\lim_{y\to\infty} \frac{l(0)}{l(y)} = \lim_{y\to\infty} \frac{\gamma(0)}{\gamma(y)} \cdot \frac{\delta}{\delta + (1-\delta)s\phi(\theta)(1-\gamma(0))} \cdot \frac{\delta}{\delta + (1-\delta)s\phi(\theta)}. \tag{48}$$

The direct effect of the tax subsidy on the first ratio on the right hand side is positive due to the vacancy distribution shifting towards less productive firms (Result 4). If the equilibrium effects of the subsidy on $P(u)$ are small then the positive effect on $\lim_{y\to\infty} \dfrac{\gamma(0)}{\gamma(y)}$ remains (based on equation (28)).

Note that the last ratio in equation (48) equals $P(u)$ (see equation (45)) and the term before differs only by the $(1-\gamma(0))$ term.

It follows that if the equilibrium effects of the subsidy on $P(u)$ are small then the impact of the subsidy on $\lim_{y\to\infty} \dfrac{l(0)}{l(y)}$ is positive.

Result 5 *In equilibrium, the effect of the tax subsidy on the ratio of employment at the lowest and highest quality firm is positive if the equilibrium effects on the probability of a randomly drawn job applicant being unemployed are small.*

In section A.6 we present simulation results for the equilibrium employment (and wage) effects of the tax subsidy.

Turning to the impact of the subsidy on wages, equation (32) leads to Result 6.

Result 6 *The direct effect of the subsidy on wages if positive for workers whose wage is not the first wage after unemployment.*

The wages at the lowest productivity firm are determined by equation (33), because once an employer receives an alternative offer she is the poached by the competing (more productive) firm. As the subsidy has no direct effect on wages under equation (33), the direct effect of the subsidy on wages is zero for workers at the lowest productivity firms.

At firms above the lowest productivity level a non-zero fraction of workers (determined by equation (40)) earn wage that is not the first wage after unemployment, on which the impact of the subsidy is positive (see Result 6).

Result 7 *The direct effect of the subsidy on wages is zero for workers at the lowest productivity firms. The direct effect of the subsidy on wages is positive at firms above the lowest productivity level.*

The indirect effect of the tax subsidy on wages cannot be derived analytically. First, its positive effect on $\phi(\theta)$ increases the negative wage implications of the option value in equations (32) and (33). On the other hand, we know from from Result 4 that the subsidy shifts the distribution of vacancies towards less productive firms, thus $\bar{\Gamma}$ decreases as a consequence of the tax subsidy but this decreasing effect varies with firm productivity.

Note also that the direct effect of the employment subsidy on the wages of new entrants is zero, as it follows from equation (33). Intuitively, young workers enter the labor market as non-employed, thus, essentially, poaching and wage renegotiation are not relevant for them. This means that new entrants cannot use current wages as outside option to achieve full surplus extraction – instead, they accept any offer (as the reservation threshold of firm productivity is zero), and can start bargaining over wages once employed. Also, the firm heterogeneity in the employment effects of the subsidy is smaller if all workers are new entrants since then low and high productivity firms hire from unemployment to the same extent, thus low productivity firms no longer benefit disproportionately more from the tax subsidy.

A.6 Simulations

The functional forms used in the simulations are the following.

The cost function, based on Bagger and Lentz (2019) is:

$$\kappa(v(y,\tau)) = \frac{v(y,\tau)^{(1+1/c_v)}}{1+1/c_v},$$

where $c_v > 0$ determines curvature. The job-finding rate is similar to Moscarini and Postel-Vinay (2018): $\phi(\theta) = A\theta^{\alpha}$.

The parameters used in the simulations are the following:

- The subsidy is 6% of the average wage without subsidy tax: $\tau = \bar{w}_0 \times 0.06$.
- y has $Pareto(\lambda, lb)$ distribution, where λ is the scaling parameter and lb is a drift that shifts the original Pareto distribution, such that the lower bound is equal to lb. During the simulations $\lambda = 1.25$ and $lb = 1000$.
- $\zeta = 0.95$, which is the exponent in the CRRA utility function, implying close to log-utility. The simulation results are robust to different ζ values. It has primarily an effect on the wage change.
- $A = 1/4$, to calibrate an unemployment rate of around 20%.
- $\alpha = 1/2$, similar to Moscarini and Postel-Vinay (2018).

- The employment-to employment transition rate (EE) is 0.041, which is in line with the empirical data for Hungary (12-month transition rate between employers among the continuously working old workers). The searching intensity (s) is a direct mapping of this parameter, see the derivations in Moscarini and Postel-Vinay (2018). To obtain s, we solve for:

$$\phi(\theta)(1-\delta)\delta s \int_0^1 \frac{1-s}{\delta+(1-\delta)s\phi(\theta)s} ds = EE^{\cdot}. \tag{49}$$

- $\beta = 0.95$, which matches the monthly value of $0.95^{1/12}$ by Moscarini and Postel-Vinay (2018).

- $b = lb = 1000$, thus the workers' outside option is the same as the output of the lowest productivity firm.

- $c_v = 0.006$, similarly to Bagger and Lentz (2019).

- Job destruction rate $\delta = 0.1$, corresponding to the 12-month separation rate observed in the data for Hungary.

Table A1 displays the simulated impact of the tax subsidy on unemployment, job market tightness and job finding rate. The rate of unemployment decreases by 1.8 percentage points form its baseline rate of 22.3%. At the same time, both job market tightness and job finding rate increase as a consequence of the subsidy.

Appendix Table A1: Steady-State Parameters

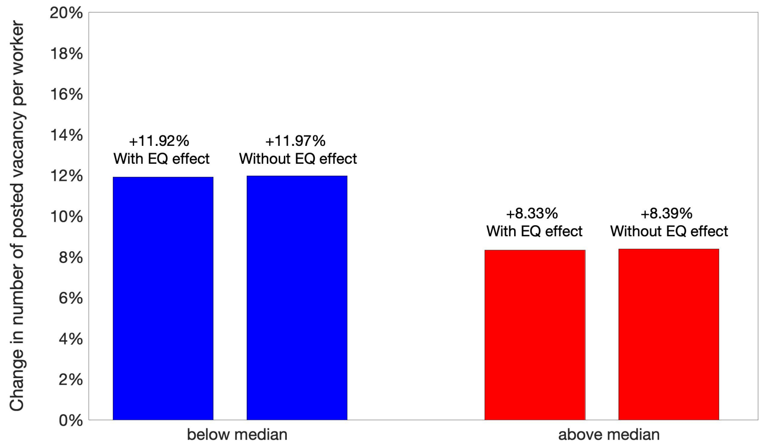
	(1)	(2)	(3)
Tax subsidy rate	0%	6%	Δ (15%)
Unemployment	0.223	0.206	-0.017
Job market tightness (θ)	1.935	2.380	0.445
Job finding rate	0.348	0.386	0.038

Note: The table shows the steady-state unemployment rate (defined by equation (27)), job market tightness (θ, defined by equation (15) and job finding rate ($\phi(\theta)$).

Figure A1 shows that the tax subsidy increases the vacancy posting activities of firms. In line with our theoretical considerations, the impact is bigger at less productive firms. At less productive firms, the vacancies posted increase by 12%, whereas at more productive firms only by 8.3%. These simulated impacts are slightly higher if we ignore the equilibrium effects in the model. Figure A2 shows that as a consequence of the increased vacancy posting activities, employment at less productive firms increases by 3.7%, while employment at more productive firms increase by 0.8%.

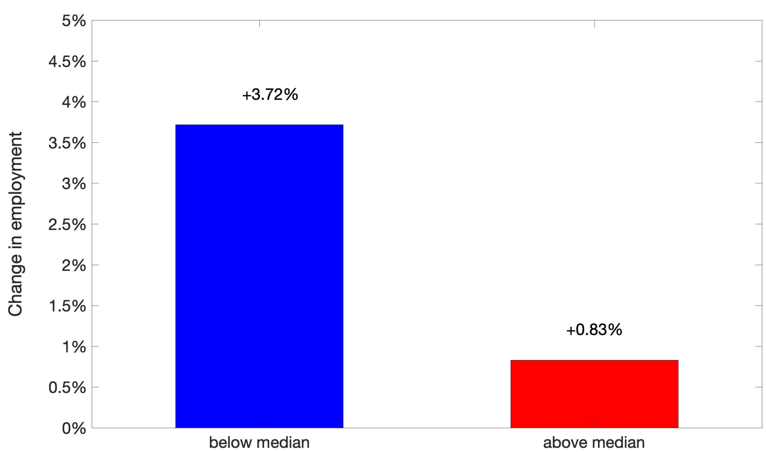
Turning to wages (Figure A3), the wage impact of the tax subsidy for workers who were not employed the previous period is essentially zero, while it is 2.3% for the rest of the workers ("incumbents"). Finally, among incumbent workers, the wage effect is small (0.8%) at less productive firms, whereas it is larger (3%) at more productive firms (Figure A4).

Appendix Figure A1: Simulation Results: Vacancies by Firm Productivity



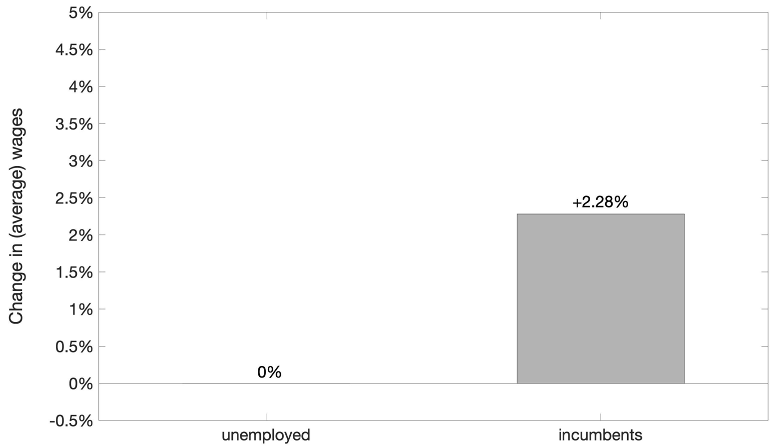
Note: The figure shows the impact of the subsidy on the number of posted vacancies per worker by productivity category of the employer (below or above median productivity). For each productivity category, the left bars show the impact with equilibrium (EQ) effects, the right bars show the impact without equilibrium effects.

Appendix Figure A2: Simulation Results: Employment by Firm Productivity



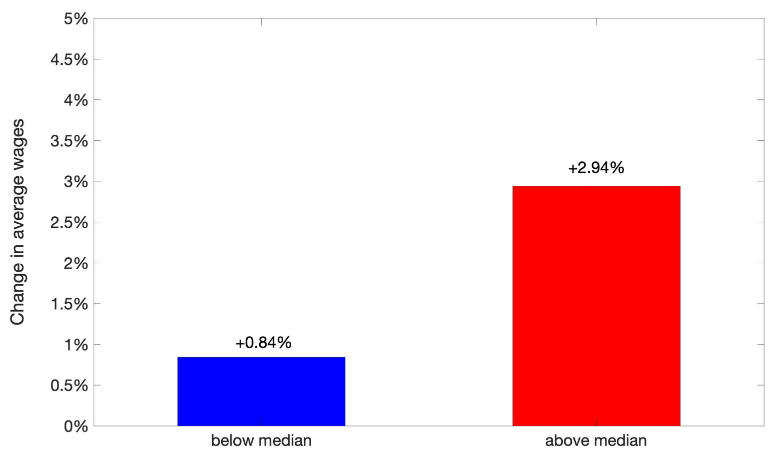
Note: The figure shows the impact of the subsidy on employment in percentage change by productivity category of the employer (below or above median productivity).

Appendix Figure A3: Simulation Results: Wage by Previous Labor Force Status



Note: The figure shows the impact of the subsidy on the wage of workers who were not employed ("unemployed") or were employed ("incumbents") the previous period. The latter group includes workers who earn the first wage since unemployment and also those who already had a wage bargaining.

Appendix Figure A4: Simulation Results: Wage of the Incumbent Workers by Firm Productivity

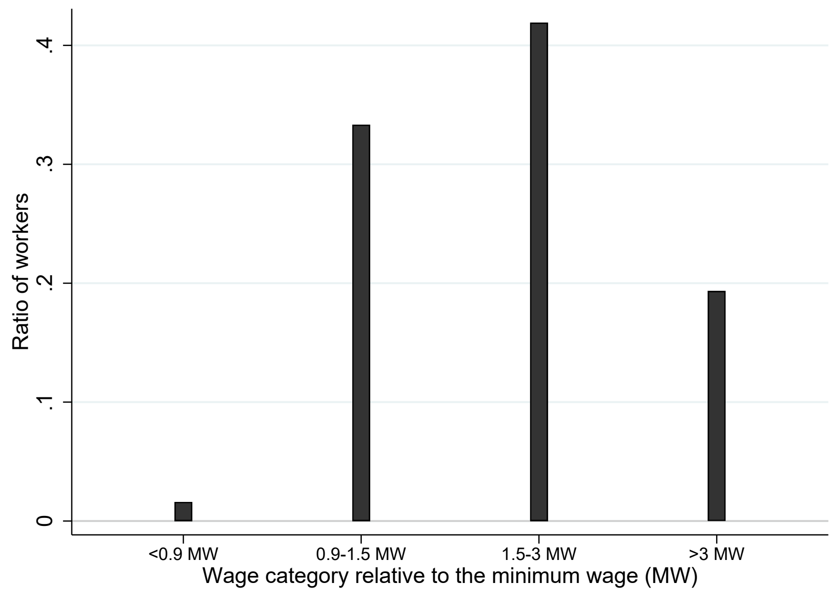


Note: The figure shows the impact of the subsidy on the wage of incumbent workers by productivity category of the current employer (below or above median productivity). The group of incumbent workers consists of workers who earn the first wage since unemployment but were already working the previous period and workers who already had a wage bargaining.

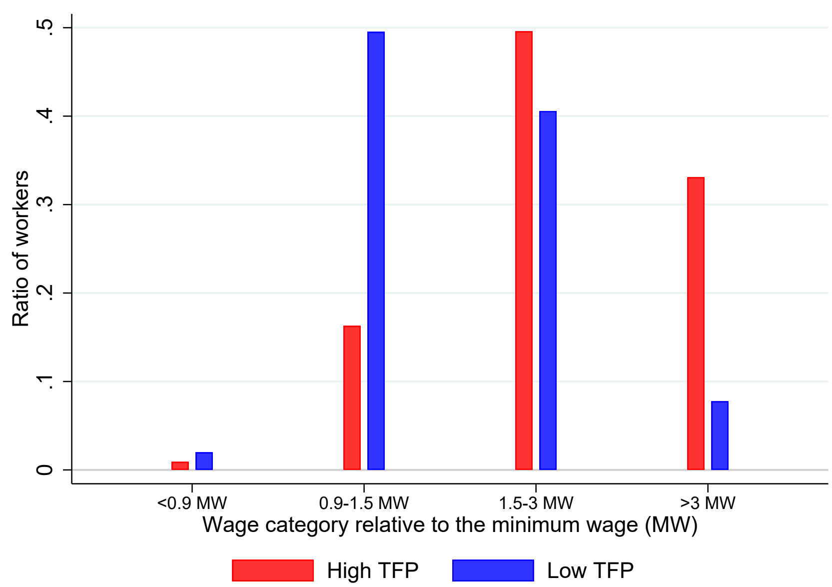
B Additional Figures and Tables

Appendix Figure B1: Distribution of Workers in Private Sector Companies by Wage Categories

(a) All



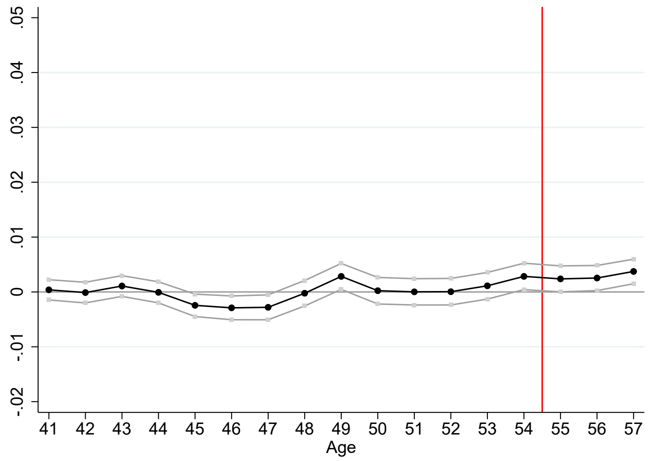
(b) By TFP



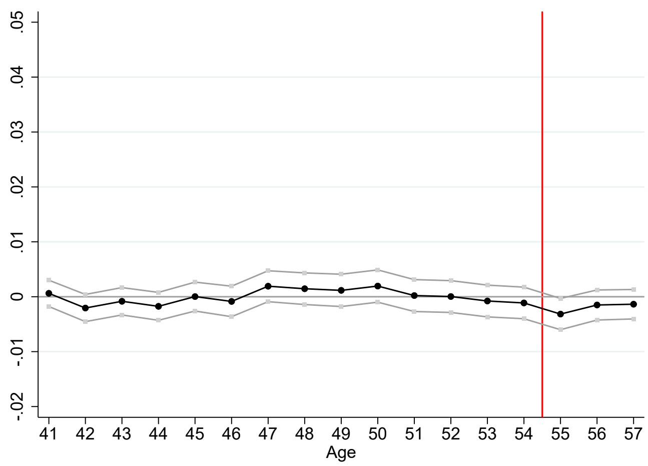
Note: The figure shows the distribution of male workers aged 52-57 by wage categories in 2012-2015 and by the TFP of the employer (in Panel (b)). High (low) TFP firms have above (below) median TFP.

Appendix Figure B2: Change in Employment Rate in Placebo Groups

(a) Public Sector Employment Rate

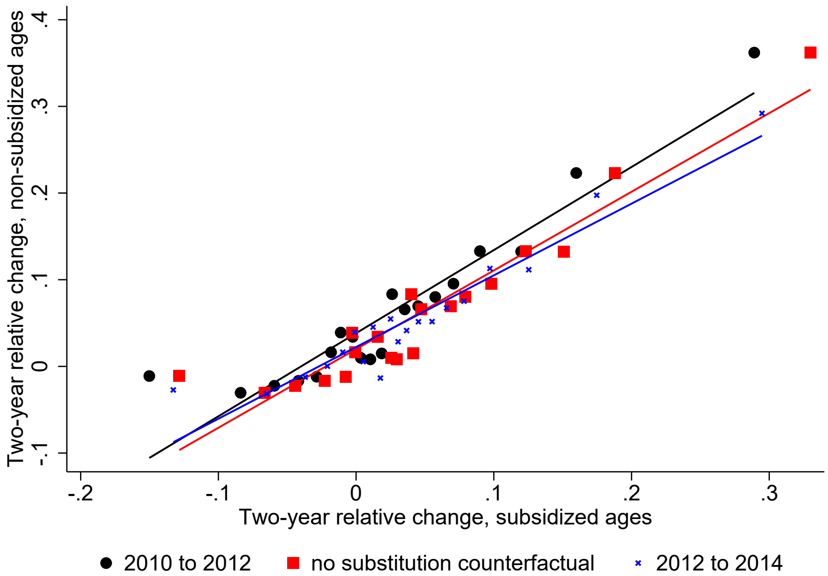


(b) Self-employment Rate



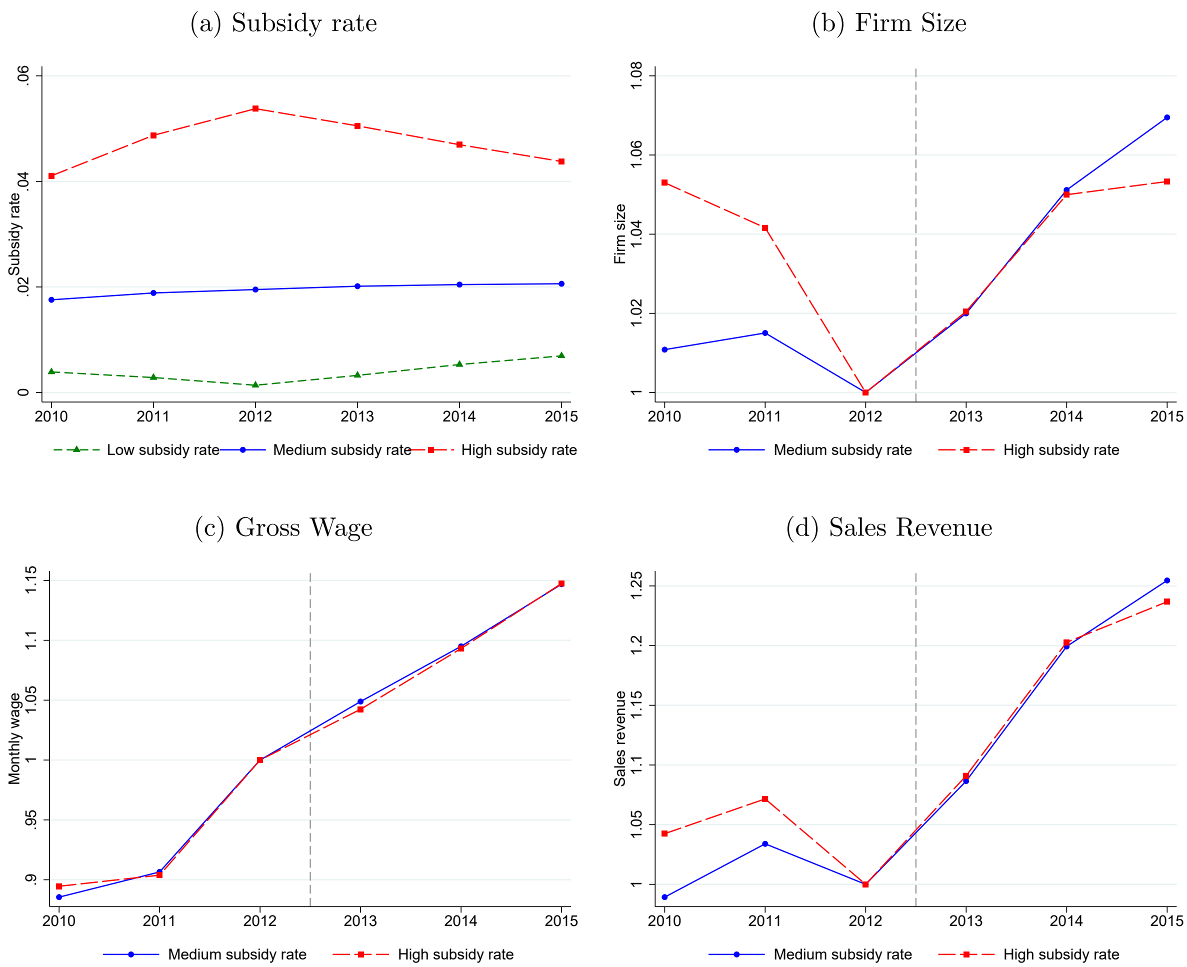
Note: Panel (a) refers to employment in the public sector where the payroll tax subsidy did not apply. Panel (b) refers to the self-employed who were not eligible for payroll tax subsidy. Both panels show the difference between years 2013-2015 and 2012, adjusted to mean zero over ages 41-54, with the 95% confidence interval (standard errors clustered on the individual-level). The vertical red line shows the age threshold where the tax subsidy became effective from 2013.

Appendix Figure B3: Firm-Level Relative Growth in Employment by Age Group



Note: On the x-axis, we indicate the two-year change from year t to year $t+2$ in the number of workers aged up to 24 or at least 55 (subsidized ages) relative to the observed firm size in year t. On the y-axis, we indicate the same two-year relative change in the number of workers aged 25-54 (non-subsidized ages). We exclude firms with less than 10 registered workers (5 workers in our sample, on average). After this restriction, we also exclude those firms that are not in the sample throughout years 2010-2014. We report binscatter plot of the observations with a linear fitted regression line. The black dots and line refer to relative change from 2010 to 2012 (i.e., before the introduction of the tax subsidy). The blue dots and line refer to relative change from 2012 to 2014 (with the tax subsidy being introduced in 2013). The red dots and line correspond to a counterfactual scenario under which the 2010-2012 relative change in employment rate in the subsidized age groups is increased by 9.4%, which is the estimated average rate of increase, while the 2010-2012 change in employment rate in the non-subsidized ages is left at its observed value.

Appendix Figure B4: Evidence for Windfall Effects



Note: We replicate the basic results of Saez, Schoefer and Seim (2019). Using the observations of year 2012, we calculate the subsidy payable after people aged 55 and above after the reform relative to the total payroll ("subsidy rate"). Then, we calculate the quartiles of the subsidy rate, excluding firms with zero subsidy rate, and group firms into four categories. The "low subsidy rate" firms have either zero subsidy rate or belong to the bottom quartile. The "middle subsidy rate" firms have subsidy rate in the middle two quartiles. The "top subsidy rate" firms have subsidy rate in the top quartile. We then compare the outcomes – subsidy rate, firm size, average gross wage and sales revenue – relative to 2012 of the so generated groups (focusing on the middle and high subsidy rate groups). Firms are categorized in 2012 (last pre-reform year).

Appendix Table B1: Elasticity of Employment, Binary Indicator of Employment

	(1) All firms	(2) Low TFP	(3) High TFP
Employment effect			
—Coeff.	0.0054***	0.0053***	0.0001
—SE	[0.0005]	[0.0005]	[0.0004]
Employment rate			
—Without subsidy	0.342	0.176	0.176
—With subsidy	0.347	0.182	0.182
—Percent change in employment	1.58%	3.00%	0.05%
Labor cost $(1 + \tau_{ss})$			
—Without subsidy	1.27	1.26	1.28
—With subsidy	1.20	1.18	1.22
—Percent change in labor cost	-5.27%	-6.02%	-4.45%
Implied elasticity	-0.30	-0.50	-0.01
	[0.03]	[0.05]	[0.06]

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1. The first row shows estimates from the model in equation (9) with the difference that the outcome is a binary indicator of private sector employment, instead of the employment indicator weighted by working hours. These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. The sample is restricted to men. The treated group comprises ages 55-57 and the control group comprises ages 52-54. The elasticity estimations are based on the difference-in-differences estimation results. Low (high) TFP firm have below (above) median TFP.

Appendix Table B2: Heterogeneity by Firm Characteristics, Short-Run Estimates (2012-2013): Impact on Employment in Private Sector Companies

	(1) Baseline	(2) TFP	(3) Foreign ownership	(4) Mean firm wage	(5) AKM FE
Treatment effect	0.0029***				
	[0.0005]				
Low quality, treatment eff.		0.0045***	0.0035***	0.0032***	0.0036***
		[0.0004]	[0.0004]	[0.0003]	[0.0004]
High quality, treatment eff.		-0.0016***	-0.0006	-0.0003	-0.0007
		[0.0005]	[0.0003]	[0.0005]	[0.0006]

Note: Cluster robust standard errors in brackets, clustering at the age $\times$ period level, *** p<0.01, ** p<0.05, * p<0.1. The table shows estimates from the model in equation (9), restricting the sample to 2012-2013. These are difference-in-differences estimates that compare the change in employment between year 2012 and 2013. The sample is restricted to men. The treated group comprises ages 55-57 and the control group comprises ages 52-54. In each regression, the outcome is the binary indicator of private sector employment at a firm with the given characteristic. In each regression, we control for age and quarterly date effects.

Appendix Table B3: Robustness Check: Firm Quality Indicators Defined Pre-Reform

	(1)	(2)	(3)	(4)
Outcome variable: employment				
Firm quality uses post-reform years	TFP	Foreign ownership	Mean firm wage	AKM FE
Low quality	0.0053***	0.0060***	0.0048***	0.0047***
	[0.0005]	[0.0004]	[0.0003]	[0.0004]
High quality	-0.0001	-0.0007**	0.0005	0.0005
	[0.0004]	[0.0003]	[0.0004]	[0.0004]
Firm quality uses pre-reform years only	TFP	Foreign ownership	Mean firm wage	AKM FE
Low quality	0.0059***	0.0062***	0.0040***	0.0032***
	[0.0005]	[0.0004]	[0.0003]	[0.0004]
High quality	-0.0006	-0.0008**	0.0013***	0.0010**
	[0.0005]	[0.0003]	[0.0004]	[0.0004]
Outcome variable: log(wage)				
Firm quality uses post-reform years	TFP	Foreign ownership	Mean firm wage	AKM FE
Low quality	-0.0008	-0.0023	-0.0006	-0.0077*
	[0.0021]	[0.0044]	[0.0032]	[0.0040]
High quality	0.0389***	0.0908***	0.0855***	0.1004***
	[0.0064]	[0.0166]	[0.0182]	[0.0199]
Firm quality uses pre-reform years only	TFP	Foreign ownership	Mean firm wage	AKM FE
Low quality	-0.0038	-0.0019	-0.0022	-0.0091**
	[0.0031]	[0.0054]	[0.0030]	[0.0030]
High quality	0.0410***	0.0841***	0.0980***	0.0942***
	[0.0067]	[0.0083]	[0.0210]	[0.0155]

Note: Cluster robust standard errors in brackets, clustering at the age $\times$ period level, *** p<0.01, ** p<0.05, * p<0.1. The sample is restricted to men aged 52-57 (55-57 as treated group, 52-54 as control group) Outcome variable is private sector employment in the top two panels and log(wage) in the bottom two panels. The employment effects are based on equation (9). The wage effects are based on equation (13). In the panels denoted "firm quality uses post-reform years", the firm quality indicators are generated based on 2010-2015, except for the AKM firm fixed effect, which uses all sample years of the linked employer-employee administrative data. In the panels denoted "firm quality uses pre-reform years only", the TFP, mean firm wage and foreign ownership indicators are defined in 2012 and then extrapolated to the other sample years. The AKM firm fixed effect is defined on years 2003-2012.

Appendix Table B4: Heterogeneity by Firm Size and TFP: Impact on Employment in Private Sector Companies

	(1) Low TFP	(2) High TFP
Employment at firms with 1-25 workers	0.0006**	0.0004**
	[0.0003]	[0.0002]
	{25%}	{3%}
	⟨0.0816⟩	⟨0.0107⟩
Employment at firms with 26-50 workers	0.0010***	-0.0005***
	[0.0002]	[0.0001]
	{8%}	{3%}
	⟨0.0258⟩	⟨0.0101⟩
Employment at firms with 51-100 workers	0.0013***	0.0001
	[0.0002]	[0.0001]
	{7%}	{5%}
	⟨0.0227⟩	⟨0.0151⟩
Employment at firms with 101+ workers	0.0023***	0.0000
	[0.0002]	[0.0004]
	{11%}	{39%}
	⟨0.0366⟩	⟨0.1271⟩

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1. Mean outcome in May 2012 in angle brackets. The table shows estimates from the model in equation (9). These are difference-in-differences estimates that compare the change in employment between year 2012 and 2013-2015. The sample is restricted to men. Total number of observations: 9,003,984. The treated group comprises ages 55-57 and the control group comprises ages 52-54. The share of workers aged 52-57 in year 2012 by firm size and TFP categories are reported in curly brackets. In each regression, the outcome is the binary indicator of private sector employment at a firm with the given characteristic. In each regression, we control for age and quarterly date effects.

Appendix Table B5: Heterogeneity by Local Labor Market Conditions and TFP: Impact on Employment in Private Sector Companies

	(1) Low TFP	(2) High TFP
Districts with below median unemployment rate in 2012	0.0055***	-0.0014**
	[0.0008]	[0.0007]
	⟨0.1807⟩	⟨.2040⟩
Observations	3,603,336	3,603,336
Districts with above median unemployment rate in 2012	0.0065***	-0.0003
	[0.0008]	[0.0008]
	⟨0.1706⟩	⟨0.1315⟩
Observations	3,938,028	3,938,028
Districts with stable labor market conditions	0.0050***	-0.0005
	[0.0005]	[0.0005]
	⟨0.1650⟩	⟨0.1585⟩
Observations	5,278,340	5,278,340
Districts with improving labor market conditions	0.0051***	0.0011
	[0.0007]	[0.0008]
	⟨0.1718⟩	⟨0.1601⟩
Observations	4,400,856	3,421,239
Districts with below median ratio of aged 55-57	0.0054***	-0.0007
	[0.0006]	[0.0006]
	⟨0.1538⟩	⟨0.1662⟩
Observations	4,287,445	4,287,445
Districts with above median ratio of aged 55-57	0.0050***	0.0009*
	[0.0007]	[0.0004]
	⟨0.1808⟩	⟨0.1583⟩
Observations	4,716,539	4,716,539

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1. Mean outcome in May 2012 in angle brackets. The table shows estimates from the model in equation (9). These are difference-in-differences estimates that compare the change in employment between year 2012 and 2013-2015. The sample is restricted to men. The treated group comprises ages 55-57 and the control group comprises ages 52-54. In each regression, the outcome is the binary indicator of private sector employment at a firm with the given characteristic. In each regression, we control for age and quarterly date effects. In each row, we restrict the sample to districts (1st level Local Administrative Units) that satisfy the given condition. Hungary is divided into 197 districts. In the top part of the table, the districts are categorized according to the unemployment in 2012, as observed in the T-STAR regional database of the Hungarian Central Statistical Office (the districts of Budapest are missing). In districts with below and above median unemployment rate in 2012, the mean of the unemployment rate in 2012 was 8.6% and 18.3%, respectively. In the middle part of the table, the districts are categorized by private sector employment rate change between 2012-2015. In districts with stable labor market conditions, the change private sector employment rate between 2012-2015 is between -2 and +2 percentage points, with a mean of 0.1 percentage point. In districts with improving labor market conditions, the change private sector employment rate between 2012-2015 is above +2 percentage points, with a mean of 3 percentage points. We exclude here the few districts with more than -2 percentage points decline in private sector employment rate. In the bottom part of the table, the districts are categorized according to the ratio of men aged 55-57 within the male population aged 18-57 in 2012. In districts with below and above median ratio of men aged 55-57, the mean of this ratio is 0.074 and 0.085, respectively.

Appendix Table B6: Robustness Check: Private Sector Wage Effects, Heterogeneity by Current Firm Quality

Outcome variable: log(wage)	(1) New entrants Baseline	(2) Incumbents Baseline	(3) Incumbents Baseline	(4) Incumbents With part-time	(5) Incumbents Two-year effects	(6) Incumbents Extended
Treat × After	0.022	-0.019*	0.008	-0.021**	0.021**	0.012
	[0.018]	[0.010]	[0.007]	[0.009]	[0.009]	[0.016]
Treat × After × Subsidy rate		0.221**	-0.077	0.149*	-0.191**	-0.129
		[0.090]	[0.070]	[0.080]	[0.085]	[0.215]
High TFP × Treat × After			-0.046***	-0.045***	-0.040***	-0.053**
			[0.013]	[0.014]	[0.006]	[0.021]
High TFP × Treat × After × Subsidy rate			0.678***	0.632***	0.600***	0.780***
			[0.137]	[0.164]	[0.038]	[0.242]
Windfall × Treat × After						0.545*
						[0.277]
Windfall × Treat × After × Subsidy rate						-5.971**
						[2.588]
Observations	13,429	97,789	97,789	112,713	82,910	97,789

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1. Outcome variable is log(wage). The sample is restricted to men. Except for column (4), the sample is restricted to full-time workers. Except for the first column, the sample is restricted to individuals with continuous employment the preceding 12 months (incumbents). In the first column, the sample is restricted to individuals without continuous employment the preceding 12 months (new entrants). The results are from a three-way difference-in-differences model with age groups (55-57 as treated group, 52-54 as control group), time (2012 is baseline year, 2013 is after the intervention) and the continuous variable of subsidy rate relative to last year's wage, as written in equations (12) and (13), with the difference that current firm quality ($Q_{j(it)}$) is used. For the subsidy rate the minimum of lagged subsidy rate in the treatment ages in 2013 is subtracted from the variable. Subsidy rate is simplified subsidy rate that takes into account only the age- and occupation-based subsidy. The table reports heterogeneity results, where everything is interacted with a binary indicator showing whether the firm has above median TFP. In column (5), the wage effects are estimated over 2012 and 2014. In column (6), we also interact the treatment, age, year and subsidy rate indicators with the firm specific continuous indicator of one-year lag of the windfall, calculated as the ratio of age- and occupation specific payroll tax subsidies payable after the reform and the total payroll.

Appendix Table B7: Impact of the Tax Subsidy on Wages in Private Sector Companies, Wage Model Extended with Windfall Indicator

Outcome variable: log(wage)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	TFP		Foreign ownership		Mean firm w		AKM FE	
	Baseline	Extended	Baseline	Extended	Baseline	Extended	Baseline	Extended
Treat × After	0.008	0.011	0.003	0.006	-0.0004	0.016	0.016	0.032
	[0.005]	[0.013]	[0.012]	[0.009]	[0.013]	[0.013]	[0.020]	[0.020]
Treat × After × Subsidy rate	-0.062	-0.097	-0.034	-0.107	-0.007	-0.131	-0.169	-0.322
	[0.053]	[0.181]	[0.115]	[0.136]	[0.113]	[0.143]	[0.163]	[0.196]
High quality × Treat × After	-0.044***	-0.051**	-0.067***	-0.070***	-0.060***	-0.070***	-0.081***	-0.092***
	[0.012]	[0.018]	[0.014]	[0.013]	[0.010]	[0.012]	[0.015]	[0.019]
High quality × Treat × After × Subsidy rate	0.587***	0.687***	1.129***	1.190***	1.030***	1.138***	1.336***	1.463***
	[0.122]	[0.201]	[0.193]	[0.209]	[0.165]	[0.205]	[0.192]	[0.254]
Windfall × Treat × After		0.561*		0.434		-0.194		-0.272
		[0.309]		[0.288]		[0.292]		[0.310]
Windfall × Treat × After × Subsidy rate		-6.324**		-4.332**		0.295		1.469
		[2.601]		[1.553]		[1.568]		[2.375]
Observations	97,789							

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1. Outcome variable is log(wage). The sample is restricted to men. The results are from a three-way difference-in-differences model with age groups (55-57 as treated group, 52-54 as control group), time (2012 is baseline year, 2013 is after the intervention) and the continuous variable of subsidy rate relative to last year's wage. For the subsidy rate the minimum of lagged subsidy rate in the treatment ages in 2013 is subtracted from the variable. Subsidy rate is simplified subsidy rate that takes into account only the age- and occupation-based subsidy. The table reports heterogeneity results, where everything is interacted with a binary indicator showing whether the firm is above median with respect to firm-level variables for firm quality ("high quality"), except for foreign ownership, in which case a binary indicator of foreign ownership being above 50% is used (equation (13)). In addition, we also interact the treatment, age, year and subsidy rate indicators with the firm specific continuous indicator of one-year lag of rent sharing, calculated as the ratio of age- and occupation specific payroll tax subsidies payable after the reform and the total payroll.

C Effect on Women

Women were eligible to the payroll tax-cut but they were also targeted by a pension policy introduced in 2011. The so-called "Women 40" policy grants an early retirement option for women with 40 years of work credits, regardless of age. Years spent on maternity benefits also count towards the work credits, with the restriction that a woman must have been employed for 32 years (or at least 25 years if she has 5 or more children). As we do not observe the full employment history of older women, we cannot rule out their eligibility to the early retirement option.

To ensure that our results are not driven by the pension policy, we exclude women from the main analysis. Nevertheless, we estimate the employment and wage effects of the payroll tax-cut among older women as well. We briefly summarize how the subsidy affected older women in Section 7.1 and we discuss the results in more detail below.

Employment effects. We estimate the same difference-in-differences model for women as for men, specified in equation (9). The control and treatment groups consist of women aged 52-54 and 55-57, respectively. We compare the change in their employment between 2012 and the 2013-2015 period, the year before and the years after the introduction of the payroll tax subsidy. Among older women private sector employment increased by 0.51 percentage points (2.16%) as a result of the tax cut (see Table C1).The overall employment effect was almost identical among older men (0.53 percentage points, 1.59%). Table C1 also shows the implied labor demand elasticity. The 5.35% decrease in labor costs and the resulting 2.16% increase in employment of women aged 55-57 over 2013-2015 imply a labor demand elasticity of -0.40. Overall, the employment effect and the implied labor demand elasticity are similar among older women and men, somewhat larger and more elastic among women.

Heterogeneity by firm quality. To investigate whether the employment effect for women differs by firm quality, we estimate the difference-in-differences model, specified in equation (9) with the outcome variable is either employment at a low TFP or at a high TFP firm. We apply exactly the same definition for high and low TFP firms as for me: low (high) TFP firms are defined as firms below (above) the worker weighted median total factor productivity calculate based on all prime age adults (including men and women). Table C2 shows that private sector employment of older women increases more at worse quality firms, the increase is 0.37 vs. 0.14 percentage points at low vs. high TFP firms. This translates to a -0.48 (s.e. 0.07) employment elasticity at low TFP firms and a -0.29 (s.e. 0.10) at high TFP ones. Therefore there is a clear and statistically significant difference in the employment responses at high and low quality firms albeit those differences are less stark for women than for men.

To see whether the heterogeneous employment effects are driven by firms or employee characteristics we estimate heterogeneity by firm productivity among similar workers. Table C2 shows that women, employment at low-productivity firms increased by 0.21 percentage points in low paying occupations while the change in employment at high productivity firms is close to zero (the same estimates are 0.30 and -0.01 for men). Similarly, employment effect is 0.22 percentage points in jobs where the modal education level is primary or lower-secondary according to the LFS (Hungarian Labour Force Survey) compared to only 0.08 percentage points at high productivity firms (0.37 vs. 0.01 for men). Overall, the findings highlight that the tax cut among women is smaller at high-productivity firms than at low

TFP firms, at most levels of pay and levels of education of the employee. This suggests that the heterogeneous employment patterns are driven by firm heterogeniety and not worker heterogeneity.

Wage effects by firm quality. We also estimate the wage effects of the tax-cut among older women. Figure C1 shows the wage effects from 2012 to 2013 for women by firm quality at different levels of effective subsidy rate. The patterns of wage effects are similar for women and men (see Figure 10 for men). Wages increase only at high TFP firms and lower wages with a higher corresponding effective subsidy rate increase more. However, the wage increase we see at high-productivity firms is somewhat smaller for women.

Appendix Table C1: Elasticity of Employment: Women

	(1) All firms	(2) Low TFP	(3) High TFP
Employment effect			
—Coeff.	0.0051***	0.0037***	0.0014***
—SE	[0.0007]	[0.0005]	[0.0005]
Employment rate			
—Without subsidy	0.236	0.130	0.106
—With subsidy	0.241	0.134	0.107
—Percent change in employment	2.16%	2.85%	1.32%
Labor cost $(1 + \tau_{ss})$			
—Without subsidy	1.26	1.25	1.27
—With subsidy	1.19	1.17	1.21
—Percent change in labor cost	-5.35%	-5.88%	-4.60%
Implied elasticity	-0.40	-0.48	-0.29
	[0.06]	[0.07]	[0.10]

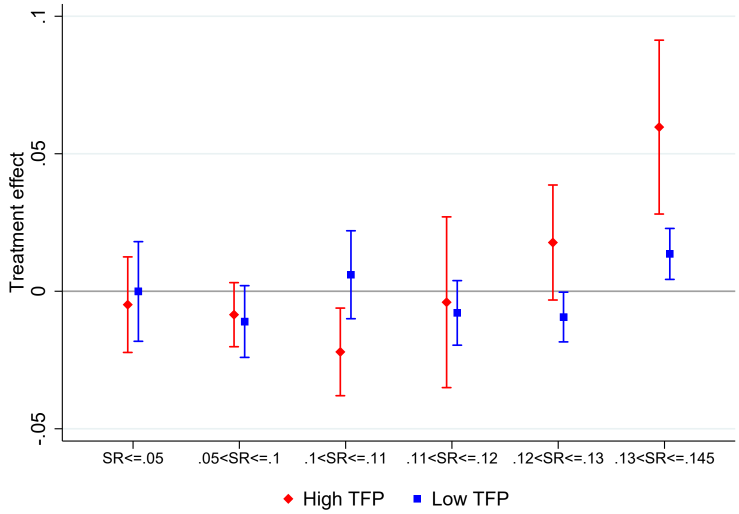
Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1. The first row shows estimates from the model in equation (9). These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. The sample is restricted to women. The treated group comprises ages 55-57 and the control group comprises ages 52-54. The elasticity estimations are based on the difference-in-differences estimation results. Low (high) TFP firm have below (above) median TFP.

Appendix Table C2: Employment Effects of the Tax Cut by Various Subgroups for Men and Women

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment, men			Employment, women		
	All firms	Low TFP	High TFP	All firms	Low TFP	High TFP
All jobs	0.0053***	0.0053***	-0.0001	0.0051***	0.0037***	0.0014***
	[0.0005]	[0.0005]	[0.0004]	[0.0007]	[0.0005]	[0.0005]
Jobs paying at most 1.5×minimum wage	0.0039***	0.0032***	0.0007***	0.0062***	0.0046***	0.0016***
	[0.0005]	[0.0004]	[0.0002]	[0.0005]	[0.0004]	[0.0003]
	{35%}	{27%}	{8%}	{33%}	{20%}	{12%}
Jobs paying above 1.5×minimum wage	0.0016***	0.0019***	-0.0003	-0.0011**	-0.0007**	-0.0004
	[0.0005]	[0.0003]	[0.0005]	[0.0005]	[0.0003]	[0.0004]
	{65%}	{24%}	{40%}	{67%}	{36%}	{31%}
Low paid occupation	0.0028***	0.0030***	-0.0001	0.0021***	0.0021***	-0.0000
	[0.0004]	[0.0003]	[0.0002]	[0.0005]	[0.0003]	[0.0004]
	{51%}	{28%}	{24%}	{57%}	{36%}	{20%}
High paid occupations	0.0024***	0.0023***	0.0001	0.0030***	0.0015***	0.0014***
	[0.0006]	[0.0003]	[0.0005]	[0.0005]	[0.0004]	[0.0004]
	{49%}	{19%}	{30%}	{43%}	{21%}	{22%}
Primary and lower-secondary education jobs	0.0038***	0.0037***	-0.0001	0.0029***	0.0022***	0.0008**
	[0.0005]	[0.0004]	[0.0003]	[0.0005]	[0.0003]	[0.0003]
	{70%}	{37%}	{33%}	{52%}	{31%}	{21%}
Upper-secondary education jobs	-0.0000	0.0004**	-0.0004	0.0011***	0.0007**	0.0004*
	[0.0003]	[0.0002]	[0.0003]	[0.0003]	[0.0003]	[0.0003]
	{16%}	{8%}	{8%}	{38%}	{19%}	{16%}
Tertiary education jobs	0.0013***	0.0011***	0.0001	0.0013****	0.0010***	0.0003
	[0.0003]	[0.0002]	[0.0003]	[0.0002]	[0.0001]	[0.0002]
	{14%}	{7%}	{7%}	{12%}	{6%}	{6%}

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1. The table shows estimates from the model in equation (9). These are difference-in-differences estimates that compare the change in employment in a given occupation category and at a given firm type between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. The treated group comprises ages 55-57 and the control group comprises ages 52-54. In each regression, we control for age and quarterly date effects. The share of workers aged 52-57 in year 2012 by job and TFP categories are reported in curly brackets. High (low) paid occupations are those four-digit occupations where the average wage was above (below) its median in 2012. The education categories of jobs are defined by imputing the modal education level of employees of the same four-digit occupation code in the 2013 Labor Force Survey of the Central Statistical Office of Hungary.

Appendix Figure C1: Wage Results for Women: Private Sector Wage Effects by TFP and Subsidy Rate Categories



Note: The figure shows predicted treatment effects on log(wage) in private sector companies with the corresponding 95% confidence intervals at different categories of the subsidy rate (SR). A modified version of equation (13) is estimated, in which the linear S_{it-1} in the last interaction term is replaced with categories of S_{it-1} listed on the x-axis of the figure. The standard errors are clustered at the age $\times$ period level. The sample is restricted to women. The treated group comprises ages 55-57 and the control group comprises ages 52-54.

D Effect on Younger Workers

Parallel with the introduction of the payroll tax-cut for older workers, a similar tax cut was applied for under-25 workers. We briefly summarize the main results we find for younger workers under Section 7.2 and here we provide further details.

We estimate the impact of the payroll tax-cut in a difference-in-differences framework, comparing younger workers below the age 25 cutoff to workers just above (ages 22-24 vs. 25-27) during 2012-2015 (before and after the introduction of the subsidy in 2013). In 2015, the government introduced the Youth Guarantee Program recommended by the European Council, which targeted workers younger than age 25, however the take-up rate of the program was very small. In 2015 there were only a few thousand participants. The exclusion of the participants in the Youth Guarantee Program does not affect our results.

Baseline results - employment effects. We replicate the main estimations we did for the older age group. First, Figure D1 shows the effective average payroll tax rate for ages 20-40 before and after the implementation of the tax subsidy. We see a discontinuity at age 25 after the policy was implemented (in blue) compared to the constant rate of 28.5% before (in black). There is a jump from 17 % to 24% from age 24 to 25, which is a slightly larger average effective tax cut than for workers above 55 (a cut of 7 vs. 6 percentage points for the younger and older age groups respectively). The decrease in tax cut at younger ages reflect the gradual increase in wages and so the lower proportional subsidy rate. Furthermore, career starters received some extra subsidies and the share of those workers steadily declines by age.

Figure D2 depicts employment in private sector companies for men by age before and after the payroll tax subsidy was introduced in 2013. Panel (a) shows raw employment rates by age before (year 2012, in black) and after the policy (years 2013-2015, in gray). It shows that employment rates increase rapidly with age between ages 20 and 26, are roughly constant between ages 26 and 35 and then start slowly declining. Comparing the period before and after the policy, this figure suggests that employment rates were similar in 2012 and 2013-2015 for most age groups, but show clear divergence below 26.

Panel (b) shows estimates of the age-specific differences in employment at private sector companies for males before vs after the payroll tax subsidy was introduced. It suggests that for ages above 25 changes in employment rates were close to zero (somewhat below zero at age 35 and at ages 39-40) but age-specific employment levels strongly diverge between the pre- and the post-reform periods among the young below 25. A 24-year-old worker was close to 2 percentage points more likely to be employed shortly after the policy was introduced (years 2013-2015). The gap widens as age decreases. Overall, this figure suggests that the payroll tax cut had a positive employment effect among younger workers. This effect is larger than for older employees above 55 (2 vs. 1 percentage point).

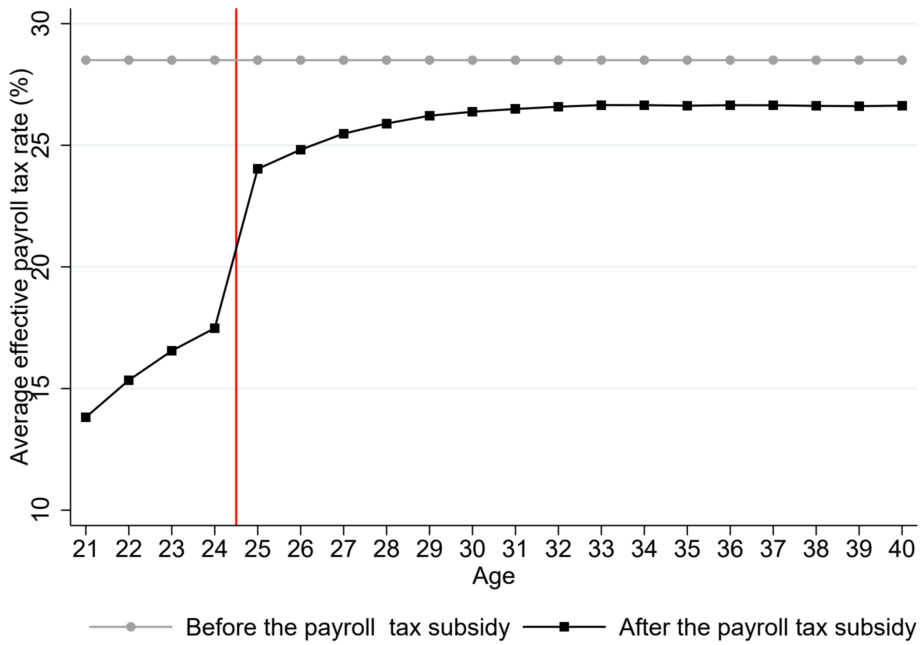
We estimate the same difference-in-differences regression for younger workers as for older workers (specified in equation (9)), where employees aged 22-24 are in the treatment group and the 25-27 age group acts as control group. Table D2 shows the baseline results both for old and young workers. Among younger workers private sector employment increased by 1.6 percentage points (5.1%) as a result of the payroll tax cut, compared to the 0.53 percentage points (1.6%) increase among the old. We show the elasticity of employment in Table D1. The 1.6 percentage points (5.1%) increase in employment and the 6.6% decrease in labor costs for the 22-24 age group over years 2013-2015 imply a labor demand elasticity of -0.77. Overall, the employment effect is larger and labor demand is more elastic for younger workers.

We also assess whether the treatment effect differs by firm characteristics among the younger workers. In Figure D3 (which is analogous to Panel a of Figure 7 for the older age group), we see that there is some variation in employment effects by firm quality, effects are larger at worse quality companies — firms with below-median TFP, domestic firms, and worse paying firms (based on the average wage). However, as opposed to older workers, employment effects of the tax-cut among the young are positive and significant for all firm types. For example, Table C1 shows that there is an increase in employment among older workers only at low TFP firms, whereas the impact on younger workers is positive both at low and high TFP companies (0.9 and 0.7 percentage points). The annual employment effects over 2012-2015 (Figure D4) are also similar at low and high TFP firms for the younger age group. Nevertheless, the heterogeneity by firm quality is more pronounced if we estimate the employment effect of the tax-cut for experienced young workers (working at least 6 months at ages 18-19), whereas the variation disappears among non-experienced workers (see Table D2).

Windfall effects. We also assess potential windfall effects at firms that already employed many young workers from the subsidized age group (below age 25) before the tax-cut was implemented, following the strategy of Saez, Schoefer and Seim (2019). We compare firms that have a high share of subsidized workers below age 25 with firms that have a medium share in 2012 (last pre-reform year). We did the same exercise for the older subsidized age group in Appendix Figure B4. Again, Panel (a) of Figure D5 indicates a mean reversion in the subsidy rate (ratio of the windfall revenues to the total payroll) and gross wages trend similarly for firms with high and medium shares of young subsidized workers based on Panel (c). However, we see some divergence in the evolution of firm size and sales revenue (Panel (b) and (d) of Figure D5); both of them grew faster at firms with a high subsidy rate, suggesting a small positive impact of a larger tax windfall on growth. Pre-reform time trends of the growth indicators were similar in the two groups of firms with medium vs. high subsidy rate.

Wage effects. We assess the impact on wages among younger workers in a similar fashion as for the older subsidized age group, specified in equation 12. Figure D6 shows the wage effects for young workers from 2012 to 2013 at different levels of the effective subsidy rate. We find no significant change in wages at either level of subsidy rate. Also, there is no heterogeneity by firm productivity, which might be due to the fact that a higher share of young workers are new entrants or have limited employment histories, among whom the theoretical model predicts little or even negative wage effects of the subsidy.

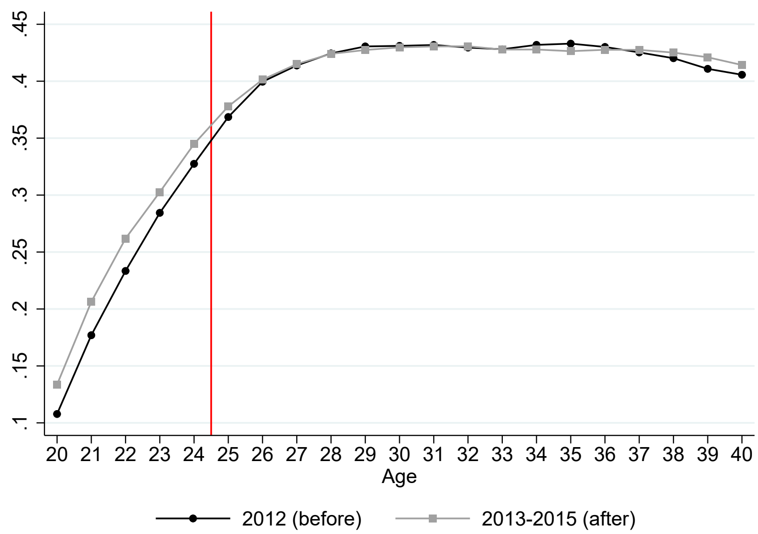
Appendix Figure D1: Average Payroll Tax Rate by Age (Age 20-40)



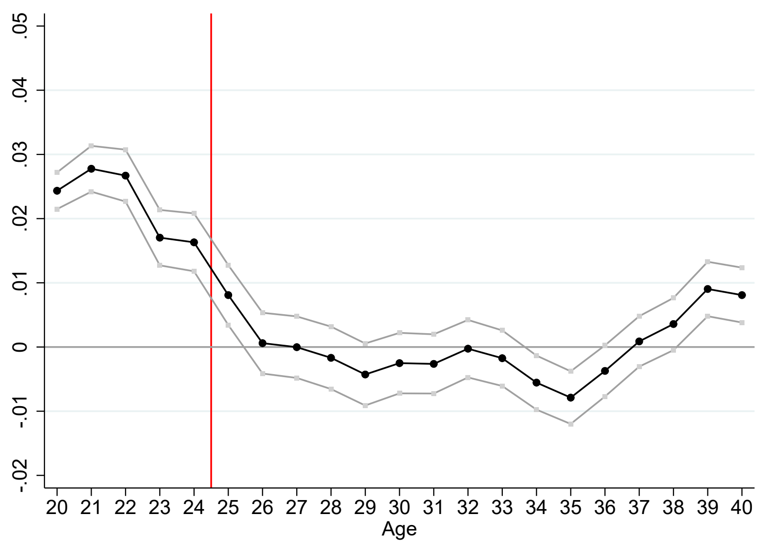
Note: This figure shows the average payroll tax rate by worker age for male workers. Before the implementation of the payroll tax subsidy, the payroll tax rate was a flat 28.5%. Between 2013-2015 (after the implementation of the subsidy), the payroll tax rate was 28.5% minus the subsidy. Using the observed gross wages in years 2013-2015 and the prevalence of beneficiaries, we calculate the effective payroll tax rate. We consider the following beneficiary groups: ages below 25; career starters (who had a work experience of less than 180 days); long-term unemployed (who were registered unemployed for at least 6 months the previous 9 months); people returning to work after a child-care leave and people working in elementary occupations.

Appendix Figure D2: Employment in Private Sector Companies by Age (Age 20-40)

(a) Employment Rate

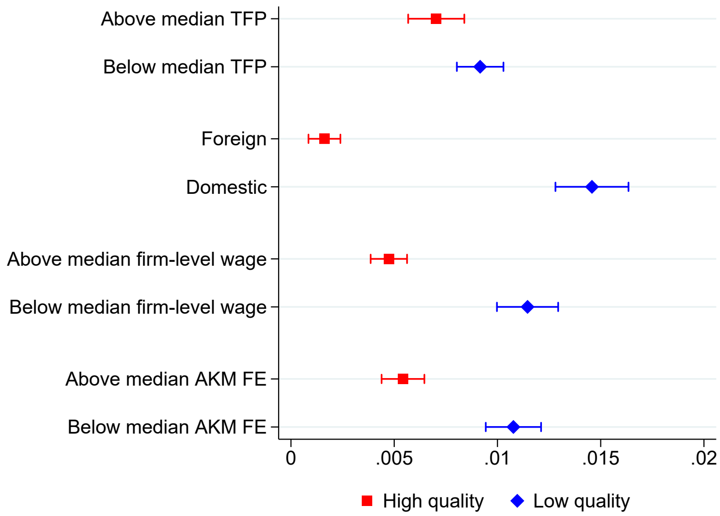


(b) Change in Employment Rate



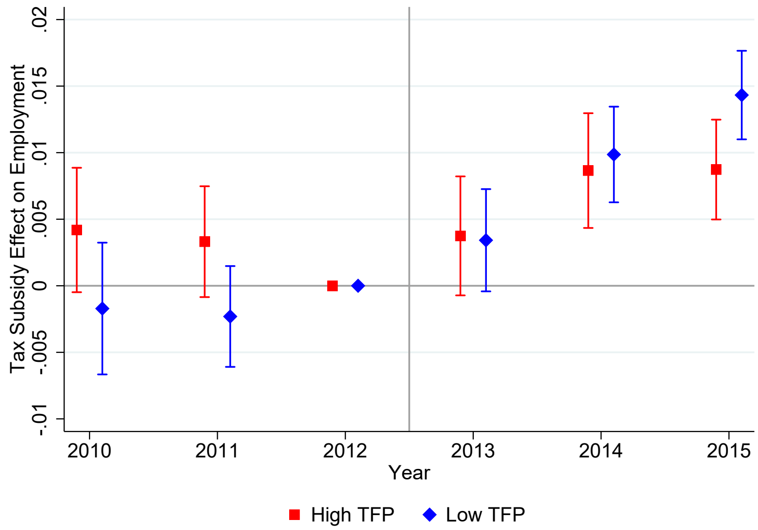
Note: The sample is restricted to men. Panel (a) shows the employment rate in private sector companies by age separately for year 2012 (before the implementation of the payroll tax subsidy) and for years 2013-2015 (after the implementation of the payroll tax subsidy). Panel (b) shows the differences between years 2013-2015 and 2012, adjusted to mean zero over ages 25-40, with the 95% confidence interval (standard errors clustered on the individual-level). The vertical red line shows the age threshold where the tax subsidy became effective from 2013.

Appendix Figure D3: Heterogeneity by Firm Characteristics: Impact on Employment in Private Sector Companies (Age 22-27)



Note: The figure shows treatment effect estimates from the model in equation (9) with 95% confidence interval. These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. The treated group comprises ages 22-24 and the control group comprises ages 25-27. In each regression, the outcome is the binary indicator of private sector employment at a firm with the given characteristic. In each regression, we control for age and quarterly date effects.

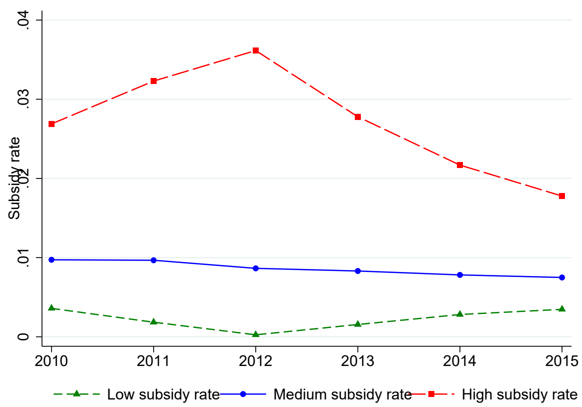
Appendix Figure D4: Employment Effects: Heterogeneity by TFP (Age 22-27)



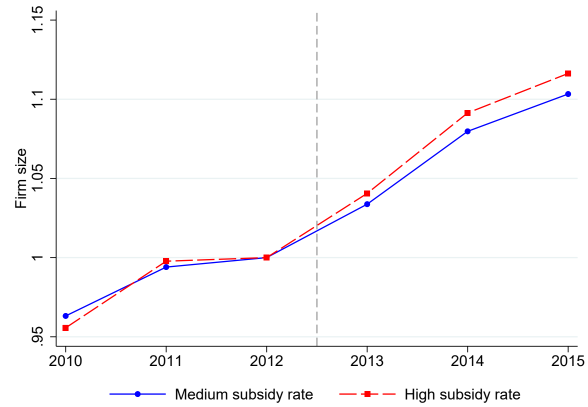
Note: The figures show the change in employment for treated age groups (affected by the payroll tax subsidy) relative to the control age groups (similar age group, but unaffected by the tax subsidy) before and after the reform. In particular, we plot the β_T from equation (10). The treated individuals are aged between 22 and 24, while the control individuals are aged 25 to 27. The 95% confidence intervals are reported, where the standard errors are clustered at the age $\times$ period level.

Appendix Figure D5: Evidence for Windfall Effects, Considering Subsidies Paid After Young Workers

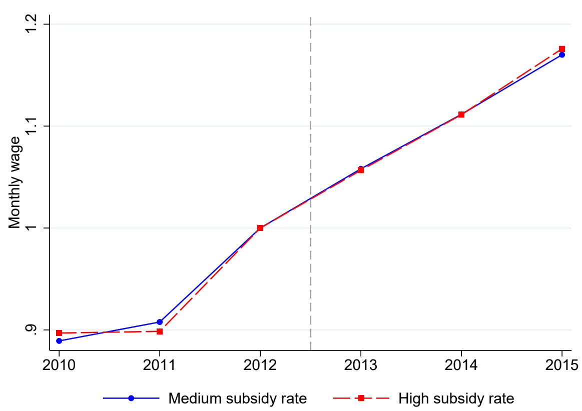
(a) Subsidy rate



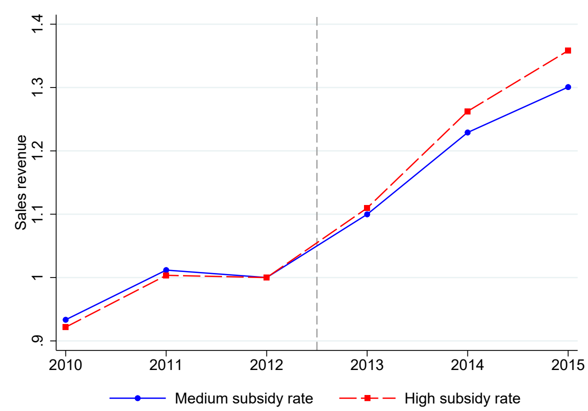
(b) Firm Size



(c) Gross Wage

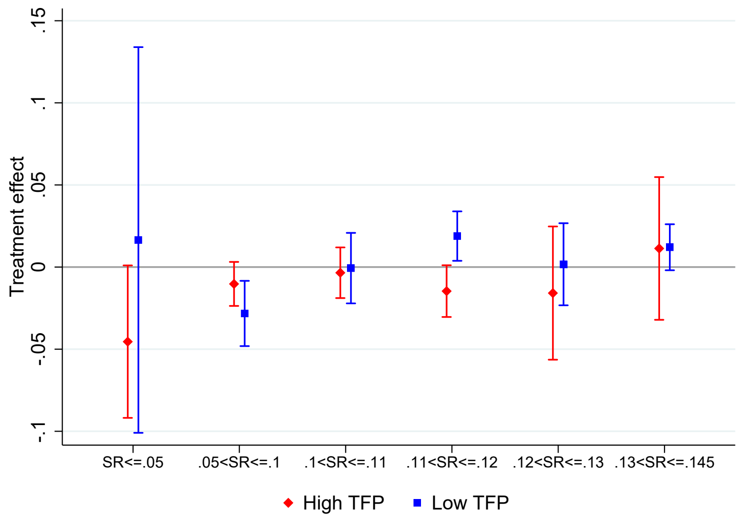


(d) Sales Revenue



Note: We replicate the basic results of Saez, Schoefer and Seim (2019). Using the observations of year 2012 (last pre-reform year), we calculate the subsidy payable after the reform to workers aged up to 24 relative to the total payroll ("subsidy rate"). Then, we calculate the quartiles of the subsidy rate, excluding firms with zero subsidy rate, and group firms into four categories. The "low subsidy rate" firms have either zero subsidy rate or belong to the bottom quartile. The "middle subsidy rate" firms have subsidy rate in the middle two quartiles. The "top subsidy rate" firms have subsidy rate in the top quartile. We then compare the outcomes – subsidy rate, firm size, average gross wage and sales revenue – relative to 2012 of the so generated groups (focusing on the middle and high subsidy rate groups).

Appendix Figure D6: Private Sector Wage Effects for Younger Workers by TFP and Subsidy Rate Categories



Note: The figure shows predicted treatment effects on log(wage) in private sector companies with the corresponding 95% confidence intervals at different categories of the subsidy rate (SR). A modified version of equation (13) is estimated, in which the linear S_{it-1} in the last interaction term is replaced with categories of S_{it-1} listed on the x-axis of the figure. The standard errors are clustered at the age $\times$ period level. The sample is restricted to men. The treated group comprises ages 22-24 and the control group comprises ages 25-27.

Appendix Table D1: Elasticity of Employment (Age 22-27)

	(1) All firms	(2) Low TFP	(3) High TFP
Employment effect			
—Coeff.	0.0162***	0.0092***	0.0070***
—SE	[0.0011]	[0.0006]	[0.0007]
Employment rate			
—Without subsidy	0.317	0.142	0.175
—With subsidy	0.333	0.151	0.182
—Percent change in employment	5.11%	6.45%	4.02%
Labor cost $(1 + \tau_{ss})$			
—Without subsidy	1.25	1.23	1.26
—With subsidy	1.17	1.15	1.18
—Percent change in labor cost	-6.61%	-7.03%	-5.96%
Implied elasticity	-0.77	-0.92	-0.67
	[0.05]	[0.06]	[0.07]

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1. The first row shows estimates from the model in equation (9). These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. The treated group comprises ages 22-24 and the control group comprises ages 25-27. The elasticity estimations are based on the difference-in-differences estimation results. Low (high) TFP firm have below (above) median TFP.

Appendix Table D2: Impact on Employment by Experience

	(1)	(2)	(3)
		Employment	
	All Firms	Low TFP	High TFP
Young workers	0.0162***	0.0092***	0.0070***
	[0.0011]	[0.0006]	[0.0007]
	⟨0.3171⟩	⟨0.1421⟩	⟨0.1750⟩
Young workers, experienced	0.0110***	0.0164***	-0.0054***
	[0.0020]	[0.0011]	[0.0018]
	⟨0.4821⟩	⟨0.2311⟩	⟨0.2510⟩
Young workers, non-experienced	0.0221***	0.0111***	0.0111***
	[0.0012]	[0.0007]	[0.0007]
	⟨0.3002⟩	⟨0.1330⟩	⟨0.1672⟩

Note: Cluster robust standard errors in brackets, clustering at the age × period level, *** p<0.01, ** p<0.05, * p<0.1. Mean outcome in May 2012 in angle brackets. The table shows estimates from the model in equation (9). These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. The treated group comprises 22-24 (young) and the control group comprises 25-27 (young). The sample of young is further split by working at least 6 months at ages 18-19 ("experienced" versus "non-experienced"). In each regression, the outcome is the binary indicator of private sector employment at a firm with the given characteristic. In each regression, we control for age and quarterly date effects. Total number of observations, old: 9,003,984. Total number of observations, young: 8,611,542, experienced young: 707,259, non-experienced young: 8,004,351.