Firm Heterogeneity and the Impact of Payroll Taxes*

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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 lower-productivity firms, which tend to hire from unemployment, while the effects are more muted for higher-productivity firms. At the same time, wages only increase at higher-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. Furthermore, from the perspective of the standard theory, there is little point in discussing whether tax policies affect the composition of jobs, and contribute to the creation of "low wage, bad jobs" or "high wage, good jobs" (Katz and Summers, 1989).

Nevertheless, a growing number of empirical studies highlight that firms play an active role in wage determination and significant wage premium differences are present across employers (see e.g. the early studies of Slichter, 1950; Lester, 1967; the debate on industry wage premium by Katz, 1986; Krueger and Summers, 1988; and the evidence on firm wage premium by Abowd, Kramarz and Margolis, 1999; Card, Heining and Kline, 2013 – for a review see Card, Cardoso, Heining and Kline, 2018). In the presence of such variation in rent across firms, the evaluation of tax policies, trade polices, and industrial policies should take into account their effect on the composition of jobs (Katz and Summers, 1989, more cites...). The incidence of these polices can also vary between different firm types, which influence the welfare implications of these policies. In this paper, we study whether impact of payroll taxes varies across firms and whether they change the composition of jobs in the economy.

To illustrate the important role firms could play in shaping the impact of tax policies, we first discuss 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).[1] 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 can grow larger as they do not only hire from unemployment, but they can also poach workers

[1]In this framework firm-level rents emerge from search frictions. Nevertheless, the rent could reflect information asymmetry (Shapiro and Stiglitz, 1984) or labor market power (Bhaskar, Manning and To, 2002; Manning, 2013; Berger, Herkenhoff and Mongey, 2022). We decided to apply a search framework to illustrate the importance of firm heterogeneity, since that framework has a long tradition to study the impact of taxes on allocation of jobs (see e.g. Pissarides, 1985; Smith, 1994; Boone and Bovenberg, 2002; Arseneau and Chugh, 2012; Golosov, Maziero and Menzio, 2013; Kreiner, Munch and Whitta-Jacobsen, 2015; Shephard, 2017; Bagger, Moen and Vejlin, 2021.)

from lower-quality firms. Nevertheless, 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 ($66) per month[2] for all over-55 private sector employees. This led to a 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.[3] We find a large increase in employment in response to the policy. In response to the 5.3% decrease in labor costs, employment of the subsidized workers increased by 1.6%, implying a labor demand elasticity of -0.30 (s.e. 0.03). This increase in employment mainly came from non-employment and inactivity. The change in self-employment and 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 higher-quality firms. Even if the relative decline in labor cost is somewhat larger at lower-quality firms, it is still non-negligible at higher-quality firms (6% at lower-quality firms vs. 4.5% at higher-quality firms). The implied employment elasticity with respect to labor cost is -0.53 (s.e. 0.05) at lower-quality firms and 0.01 (s.e. 0.06) at higher-quality firms.

[2]The average monthly net wage (wage net of employer payroll tax) was HUF 230,700 ($1,045) in Hungary in 2013, so the subsidy is about 6.3% of the average wage in 2013. A subsidy of equivalent size in the U.S. context would be $3500 per year based on the average salary in 2022.

[3]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. Therefore, we have a clearer empirical design by focusing on men. Nevertheless, we also analyze the change in labor market outcomes for women separately and show that the labor market consequences of the tax policy was very similar for them.

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 lower-quality firms, while we find no indication for substantial change in employment in the upper part of the wage distribution where the relative change in labor cost was limited.[4] 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 and among less-educated workers, 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. We estimate that the overall pass-through of the policy is small, out of \$1 of subsidy only 22 cents (s.e. 9 cents) benefit workers, while 78 cents (s.e. 9 cents) go to firms. The model predicts that wages should increase for incumbent workers at higher-productivity firms. This is exactly what we find: there is a significant increase in wages at higher-productivity firms, but we find no change in wages at lower-productivity firms. At higher-quality firms the pass-through rate is 60 cents (s.e. 13 cents) on the dollar, 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 estimation 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 workers after the reform did not cut their hiring of 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 (Saez, Schoefer and Seim, 2019). We find no evidence that firms that had more affected older workers before the tax cut grew more quickly than other firms.[5]

[4]Note that the the tax cut was lump sum, which implies that at higher wages the change was smaller relative to total labor costs.

[5]As we explain later, our set up also allows to study the impact of the tax policy on the young. Interestingly, for younger workers we find similar windfall responses as in Sweden. This suggest that it is not the

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 lower-quality and higher-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 higher- and lower-quality 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 lower-productivity firms. This suggests that the employment change does not simply come from some lower-quality jobs becoming viable 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. 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. ADD THE WELFARE CONSEQUENCES OF THAT ARGUMENT.

Since parallel to the tax subsidy on older workers, a tax subsidy affecting workers under 25 is also introduced, we can compare our estimated responses for older workers to impacts among younger workers. We find that the payroll tax cut mainly increased employment of younger workers. We also find similar heterogeneity as for the old though the differences between high and low productivity firms are smaller. We interpret the more muted heterogeneity through the lens of our illustrative model: most younger workers have low bargaining power as they come from unemployment or temporary contracts that cannot easily be used in wage negotiations. As a result, most young workers have a weak bargaining position with limited outside option, thus higher- and lower-productivity firms are affected similarly by the hiring subsidy.[6] Furthermore, labor market institutions could serve a more important constraint for younger workers, given the large share of them at or slightly above the minimum wage.

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 on increasing younger workers' wages without increasing their older co-workers' wages as that would imply a wage cut once someone is aged out from the subsidy. At the same time, it is easier to implement a pay raise once older workers age into the policy.

[6]In line with this prediction, we find more firm heterogeneity in responses among young workers who enter the labor market at quite young age and so had more experience when they received the tax subsidy.

Our paper relates to several strands of the literature. First, our study 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; Ku, Schönberg and Schreiner, 2020)[7]. Our results highlight that the shift of payroll taxes will depend on the type of firms or workers (young vs. old) studied. Evaluating the he incidence of business tax credits Carbonnier, Malgouyres, Py and Urvoy (2022) documents some heterogeneity in incidence by worker type, but firm heterogeneity is mainly ignored.

The paper is also 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). Studying the labor market consequences of such policies is particularly interesting given that they targets vulnerable groups with relatively low employment rate. Improving the employment and wage prospects of these workers is often a major policy priority for many governments. 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). Our main contribution to this literature is that we focus on heterogeneity across firm types and offer a potential 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 paper also relates to the literature studying the impact of tax policies in the presence of search frictions (see e.g. Pissarides, 1985; Smith, 1994) or imperfect competition (see e.g. Berger, Herkenhoff and Mongey, 2022). Most of the literature make some theoretical argument or apply some calibration to discuss the effect of tax policies on allocation of

[7]Bozio, Breda and Grenet (2019) reconcile these seemingly conflicting results by the tax-benefit-linkage explanation. In our case, tax-benefit linkages are not directly affected by the reform as the payroll tax did not affect workers' future benefits, which were 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.

labor and on composition of jobs. For instance, Breda, Haywood and Wang (2019) study the heterogenous impact of pay-roll taxes by calibrating search and matching model in the French context. Here we instead provide reduced form direct evidence on the heterogeneous impact of tax policies, which is a key prediction of these frameworks.

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.

2.1 Setup

Firms are heterogeneous and characterized by productivity $y \in [0, \infty]$, with cumulative distribution function $\Psi(\cdot)$. 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 $\delta \in [0, 1]$.[8]

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

[8]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.

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 as in 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. Similarly to Postel-Vinay and Robin (2002) and Moscarini and Postel-Vinay (2018), we also assume that all the bargaining power is at the firms and so they are able to extract all rents from the workers.[9] 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:

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

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

[9] 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.

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:

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

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's 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.[10]

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

[10]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.

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):

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

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

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

In equations (7) and (8), 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 quantify 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 lower-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 lower-productivity firm will meet an unemployed individual as $P(u)$ falls. This dampens the vacancy posting of lower-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.6). 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 lower-productivity firms, and just by 0.8% at the higher-productivity ones.

We also quantify the effect on wages in Panel (b) of Figure 1. Since in our main empirical exercise we study the change in wages for workers who had a job in the previous year, we study here the change in wages for these incumbent workers.[11] We expect that wages will increase less at lower-productivity firms, which mainly employ workers who came from unemployment and without any outside offer.[12] At the same time, most workers at higher-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 lower-productivity firms, which is close to a zero pass-through rate, while the wages increase by around 2.9% at higher-productivity firms. This latter reflects a 48% pass-through rate on average: workers at higher-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 lower-productivity firms and workers who are employed at higher-productivity firms. As a result, the tax subsidy affects the composition of jobs in the economy (as more lower-quality jobs are created) and benefits workers at better firms disproportionately. In the next section we turn to testing these predictions. Nevertheless, it is worth emphasizing that our illustrative model is only one way to generate the observed heterogeneity in data. In alternative models, firm-level rents generated by asymmetric information (see e.g Shapiro and Stiglitz, 1984) or by imperfect competition on the labor market (see e.g. Bhaskar, Manning and To, 2002; Manning, 2013;

[11] More precisely, we only study wage changes for workers who stayed at the firm or were poached from another firm. The model predicts that new entrant workers who were hired from unemployment experience a small decline in their wages with limited heterogeneity.

[12] 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.

Berger, Herkenhoff and Mongey, 2022) could also be potential sources of heterogeneity. Our aim is not testing these alternative models of the labor market, but to document the empirical relevance of the heterogeneous incidence of payroll taxes and its potential impact on the composition of jobs.

3 Background and Data

3.1 Background

We study the impact of a large age-specific payroll tax cut instituted in Hungary in 2013. Before 2013, employers paid 28.5% of wages in social security contributions. In 2013, the government decreased social security contributions of employers by around 14,500 Hungarian Forints (HUF, $66) per month for every employee older than 55. The average monthly salary net of employer payroll tax but before income tax and employee social security contributions was HUF 230,700 ($1,045) (Central Statistical Office, 2022) so the payroll tax cut was 6.3% of the average salary.[13] 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 discuss the impact of the policy on them in Section 7.2. Furthermore, workers in elementary occupations received the tax subsidy independently of their age.[14] In our primary analysis we include workers in elementary occupations, but our results are robust to the exclusion of those workers from the definition of private sector employment (see Appendix Table B4).

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 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 26.3% to 20% or by about 6.3 percentage points from age 54 to 55. It is worth highlighting that such a drop in the tax rate

[13]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 HUF 14,500. As the minimum wage in 2013 was HUF 98,000 ($444), almost everybody hit the cap. For the few workers who earned exactly the minimum wage at HUF 98,000 in 2013, the tax cut was HUF 14,250. In 2014, the minimum wage was raised to HUF 101,500 ($460).

[14]Long-term unemployed re-entering the labor market, people returning to work after child-care leave, or younger 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.

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 for 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 employers, while the part paid by the employees was unaffected. Employees before and after the reform paid a 16% flat-rate tax and employee social security contributions of 18.5%.[15] Furthermore, the reform did not affect the link between social security contributions and future benefits (such as pensions) as those are calculated based on net wages and not based on contributions 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.[16] Nevertheless, we find very similar results for women 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 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

[15]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*).

[16]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.

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 types of firms face different labor market institutions. In Hungary, it is relatively easy to hire or dismiss workers (Tonin, 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. The unionization rate was around 10% in this period, 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. Since our sample is drawn from the whole population (and not just those who have a job) our data can be used to study 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-entry bookkeeping we can merge balance sheet information from the Hungarian tax authority.[17]. We restrict our main analysis to men because there was a change in retirement rules affecting women throughout the period studied here. Still, we document women's employment responses 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 firms with double book keeping.[18] We include part-time workers, but adjust the employment indicator by working hours (e.g. working 20 hours per week is considered as 0.5 employment). Our main outcome in the wage regression is the full-time adjusted (monthly) net wage as of May of each year wage, which is the wage net of employer payroll tax but before income tax and employee social security contributions are deducted. Our net wage measure (often abbreviated to "wage") includes

[17]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.

[18]We focuse in firms with double book keeping as most quality measure (e.g. TFP) is only available for them. In addition to that we exclude from the benchmark analysis firms which have more than 10,000 workers to avoid that some outliers drive the results. These are very large and unique firms in the Hungarian context, we only have seven such firms in the whole economy. Appendix Table B2 shows that our results are robust to the inclusion of the largest firms and single-entry bookkeeping firms – the estimated employment effects and heterogeneities by firm quality are stronger under the extended definition.

base payment, bonuses and overtime pay.

Appendix Table B1 provides a comparison of employment statistics based on the administrative data we use with official statistics which are based on the Hungarian Labor Force Survey. These statistics are very similar, indicating the reliability of the employment indicators we define based on the administrative data.

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).[19]. 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.[20] 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. Foreign firms also tend to be more export oriented and more profitable.

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. We restrict our sample to the non-retired population to ensure that the estimated employment effects are not driven by the aging-out of already retired individuals from our sample.[21] 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 treat-

[19]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.

[20]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.

[21]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. Also, Appendix Table B4 shows that the estimated employment effects are stronger if we include the old-age pensioners in the sample but the heterogeneity patterns we report in Section 4.2 remain.

ment 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 control workers. Again, we find very 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.[22] 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 subtracted 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 treatment 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 across the two groups.

[22]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.

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

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

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. 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 of 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.)[23]. Column (1) shows that private sector employment increased by 0.53 percentage points from a baseline of 33% 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 B3 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 have found in the literature. For instance, Laun (2017) finds an employment elasticity of -0.22 for older worker in Sweden, while Huttunen, Pirttilä and Uusitalo (2013) find an elasticity of -0.1 in Finland. For younger workers, Saez, Schoefer and Seim (2019) find an employment elasticity of -0.23 in Sweden, while Egebark and Kaunitz (2018) estimate an elasticity of -0.3 in response to the young worker tax cut instituted during the Great Recession in Sweden.

Motivated by the prediction of the standard search and matching framework presented in Section 2, we also investigate whether responses to the policy differ by firm type. Columns (2) and (3) of Table 2 summarize the key results. We use regression equation (9) with an

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

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.

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 slightly lower at more productive firms, which tend to pay more to their workers. Indeed, we calculate that the labor cost decreases more at lower-TFP firms than at higher-TFP firms (6.02% vs. 4.45%). Still, the change in labor cost was considerable even at higher-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 higher-TFP firms as well. The estimated elasticity is -0.53 at lower-productivity firms and 0.01 at higher-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 control groups 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 the β_T coefficients show the difference between treatment and control firms in year T and we report those in Figure 4. The red squares show the change in employment at higher-TFP firms, where we use employment at above median TFP firms as a dependent variable. The blue diamonds show the estimates at lower-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 higher- and lower-TFP firms, suggesting that the control workers are likely a good counterfactual for the treatment workers. At lower-TFP firms employment among treatment workers increased relative to the control group exactly when the reform was intro-

duced in 2013. The impact on employment was around 0.5-0.6 percentage point over years 2013-2015 at lower-productivity firms. At the same time, employment at higher-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 treatment value assumption. In our case, this does not necessarily hold as those close to the age threshold age into the treatment, which could affect their labor market possibilities. Nevertheless, differential treatment effects still hold even if the differences disappear as we go closer to the age 55 cut-off.[24] 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) shows the estimates for employment at low and higher-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 first those who are between 52 and 53 years old and then only 52-years-old individuals. Both the overall employment effect and the estimated difference between the higher- and lower-TFP firms are similar in these specifications. Next we show estimates when the treatment group is narrowed, while keeping constant the benchmark control definition. We show estimates first when the treatment group covers only those between between 56 and 57 and then when it covers only 57-year-old individuals. 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, similarly to the approach of 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 introduces 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

[24]This is why we do not apply a regression discontinuity approach here.

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 higher-productivity firms. The figure demonstrates that most employment changes occurred at firms with lower-TFP (blue diamonds). At the same time, the changes in employment at higher-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 higher-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 lower-TFP and higher-TFP firms we find a clear differences in the employment changes: while the change in employment is substantial and statistically significant at lower-TFP firms, the change in employment is close to zero at higher-TFP firms.

Placebo groups unaffected by the tax cut. As we mentioned in Section 3, the reform only affected private sector employees, while the self-employed and workers in the public sector 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 the private sector employment increase by estimating our main regression equation (9) with mutually exclusive outcome variables: being employed in the private sector (including employment at single-entry bookkeeping firms and at firms with more than 10,000 workers, thus using a broader private sector employment definition than the definition used throughout the rest of the paper), 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 the subsidy had a positive effect on private sector employment at private sector firms – due to the inclusion of the smallest (single-entry bookkeping) firms, the estimated effect is stronger than the baseline results (see Appendix Table B2 for a comparison of the definitions). Table 3 also shows that there is a slight reduction in the likelihood of being self-employed but it is much smaller than 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 15% of the total increase in private sector employment (considering all categories of private sector firms). Furthermore, the slight negative impact on self-employment was fully offset by the slight increase in 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 the self-employed (Panel (b)). The change in employment in these two placebo groups is very small, suggesting that the increase in private sector employment in the treated age groups reflect the 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 B5).

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 B6).

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 types 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 the 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 wage 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 higher-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 lower-TFP firms, while higher-TFP firms do not hire more 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 that pay above the median on average. The table shows that employment increased by a similar amount at both low paid (0.28 percentage points) and high paid (0.24 percentage points) occupations. Furthermore, there is clear heterogeneity within both low paid and high paid occupations: virtually all the employment change comes from lower-TFP firms. Columns (5) and (6) also highlight that the employment elasticity is similar in low paid and high paid occupations. At lower-TFP firms it is close to -0.50, while at higher-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[25] 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 modal 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 lower-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 higher-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

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

we focus on group of workers with the same skill level and so our main results reflect firm heterogeneity and not worker heterogeneity.

4.5 Effect on Worker Transitions and Firm Dynamics

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

Panel A of Table 5 summarizes the key findings. Employment for new entrants increases by around 0.15 percentage point, which is around 28% of the overall 0.53 percentage point increase reported in Panel A of Table 2. This is nevertheless a quite substantial, 35% increase relative to baseline population share (4.3%) of new entrants. Employment for incumbents increases 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 share (29%) of incumbents.

These results highlight that the tax cut affected labor market transitions by inducing both higher labor market (re)entry rates and lower exit rates among workers in the treated age group.[26]

Besides the labor market dynamics, we can also study the change in firm dynamics. Panel B of Table 5 shows the decomposition of the total change in employment into newly entering firms (did not exist in the previous calendar years) and firms that existed before. We find that almost all the employment creation comes 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 employment mainly increased at firms that existed before 2012, while the change in employment at newly created firms is negligible.

[26] Our theoretical model presented in Section 2 predicts an increase in re-entry rate as a consequence of more vacancy posting, while the job destruction rate is kept exogenous and constant. Under these assumptions the change the employment of 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 the data, even if it would be someone entering from unemployment from the model's perspective. An alternative way to incorporate change in the employment of 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 cuts affect employment and wages in search models without that feature.

4.6 Labor Market Institutions and the Minimum Wage

As we noted before in Section 3.1, unions are weak in Hungary and central bargaining of wages is almost non-existent. As a result, larger firms do not usually face more organized workforces with more institutional protections. Still to make sure that our results are not simply driven by large firms, we examine heterogeneity by firm size in Appendix Table B7. We divide firms into two size categories, using the definitions of OECD (2022*c*): micro and small firms (1 to 49 employees) and medium-sized and large firms (50 or more employees). More refined categorization is hindered by the fact that the vast majority of the smallest (micro) firms have below median TFP and the vast majority of the large firms have above median TFP. We find that employment at lower-productivity firms increases in both firm size categories, while among higher-productivity firms there is no consistent employment effect in either firm size category.

We also discuss the potential impact of minimum wages on our results (Harasztosi and Lindner, 2019; Bíró, Prinz and Sándor, 2022). In the presence of binding minimum wages, the tax cut could make some jobs viable, which could explain why job creation takes place disproportionately at lower-productivity firms. That might play some role: as we saw on Figure 6, some 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 lower-TFP firms, which means that what we found does not simply reflect the interaction of the minimum wage with the tax cut.

We also showed in Section 4.5 that firm dynamics and new firms entering after 2012 are not the major source of job creation (see Table 5) 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 a higher share of the tax cut was passed through to workers at higher-TFP firms, suggesting that higher-TFP firms respond differently to the policy rather than not changing their behavior at all.

4.7 The Role of the 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 B8. The country consists of 197 districts. We first divide districts by unemployment rate in 2012 and study the impact separately in districts with above- and below-median unemployment rates in Panel A. The effect of the tax subsidy on 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 average 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 lower-TFP firms.

In addition, we also divide districts by the change in private sector employment rate in Panel B. 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.[27] Overall, these findings suggest that local economic conditions are unlikely to play a major role in explaining our main findings.

Finally, Panel C shows that 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).

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 among individuals in the control group. 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.[28]

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 substi-

[27] We do not have enough districts with substantial decline in labor market conditions and so we cannot study the impact of the tax cut in a recessionary environment.

[28] 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.

tution counterfactual (red squares and line): how much the pre-policy relationship would change if firms increased their hiring of 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 of the tax cut on wages in this section. First we study the impact on the wages of new entrants by estimating the following regression equation:

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

where w_{it} is the net 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.[29] 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 wage in the previous year, formally $S_{it-1} = 14,500/w_{it-1}$, where HUF 14,500 is the subsidy amount. This variable goes from 14.5% for low wage earners to zero for very high wage earners, and reflects the percent change in wages that would occur if all the subsidy were passed through to the worker. Then we run the following regression:

[29] 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).

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

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). This is consistent with our model which predicts that the incidence of the policy would be heterogeneous among incumbent workers (who have a job offer and stay or are poached) but not among 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 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):

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

where we interact all coefficients in equation (12) with $Q_{j(i,t)}$, the individual i's firm quality. To check that our estimates are not simply driven by transitioning to higher quality firms, in Appendix Table B9 we show that the estimated treatment effects are robust to using the firm quality in the previous year.

Column (3) of Table 6 shows the main estimates on treatment effect heterogeneity. The estimates show that the wage effects are driven by higher-productivity firms. In higher-quality firms, the pass-through rate is 60% (the sum of β_1 and β_3, which is 68% plus -8%) and statistically significant. At the same time, the pass through rate is close to zero and statistically insignificant at lower-quality firms. This is consistent with our model which predicts that higher-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 options can keep the subsidy amount. This heterogeneity holds both for workers who remain at the same firm and who transition to another firm (columns (4) and (5)), although the pass-through rate of the subsidy is higher (albeit statistically insignificant due to the small sample size) for those who change employer, which is in line with the predictions of the theoretical model.

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 higher-productivity firms in the treated ages would have evolved similarly to those 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 $t_{reform} = 2012$) and 2010-2011 ($t_{reform} = 2011$). We report the estimated pass-through at lower-productivity firms (β_1 from equation (13)) and higher-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 lower-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 workers who are 52 and 53 years old or 52-year-olds only as the control group. We also explore how the estimates change if we define narrower treatment age groups. We show estimates when the treatment includes only those between 56 and 57 and when it includes only 57-year-olds. The estimated effects are similar 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 the 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 higher- and lower-productivity firms. As we increase the subsidy rate, we do not see any wage gain for lower-productivity firms. For higher-productivity firms there is a gradual increase in wage changes as we move to lower wages and higher subsidy rates 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 higher-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 (6) of Table 6 shows the estimated change in wages when we include part time workers in the sample. The estimated pass-through at higher-productivity firms declines when including part-time workers (from 60% to 41%) but it

remains both economically and statistically significant.

Robustness to two-year change. So far we have focused on one-year changes post policy. We made this restriction because we wanted to make sure that the policy change itself does not affect treatment intensity S_{it-1} through changes in the previous year's wage. As a robustness check, we redefine the treatment as $S_{it-2} = 14500/w_{it-2}$ and study two-year changes. Column (6) of Table 6 shows the estimates when we examine two-year changes. The estimated pass-through is somewhat higher (78% versus 60% at higher-productivity firms). 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 and in Table 7. Workers at foreign, higher-wage and higher-wage-premium firms experienced substantial wage increases, equivalent to almost full pass through. At the same time, workers domestic, lower-wage and lower-wage-premium firms did not experience any wage increases.

Overall, we see that a similar pattern of incidence emerges for a wide class of firm quality measures. This suggests that the heterogeneity in incidence that we uncover is not tied to one specific quality measure and is a basic feature of the labor market. 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 higher-quality ones.

Effect by education categories. We estimate wage effects by education categories and report the results in Appendix Table B10. Education is defined by the mode of the education level for each four-digit occupation (see Section 4.4 for details). The table shows that for all three education categories the pass-through rate of the subsidy is bigger at higher-TFP firms. Also, the pass-through rate is higher and its heterogeneity is stronger at higher education category jobs (in line with Hall and Krueger, 2012).

Effect by firm size. We also examine the heterogeneity of wage effects by two firm size categories, using the same categorization as for the employment effects. The results reported in Appendix Table B11 indicate that qualitatively the pattern of the wage effects is similar both at micro and small firms (size 1-49) and at medium-sized and large firms (size 50+), although the pass-through rate at higher-quality firms is higher at medium-sized and large

firms (65%) than at micro and small firms (45%).

5.3 Rent Sharing and Windfall Effects

Recent empirical work shows that firms that received larger rents or windfalls 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 older workers 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 of such workers. Similarly to Saez, Schoefer and Seim (2019) we find mean reversion in the subsidy rate (ratio of the windfall revenues to the total payroll). Firm size, wages and sales revenue after the reform 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 this population. Interestingly, when examine the impact of a tax cut on younger workers in Hungary in Section 7.2, we find remarkably similar findings as in Saez, Schoefer and Seim (2019).[30] This suggests that the lack of windfall effects for older workers is unlikely to reflect the different economic environment, and that the tax cut impacts younger and older workers differently.

Another important finding in Saez, Schoefer and Seim (2019) is that firms shared the rents coming from the tax cut equally 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 higher-productivity firms.

Nevertheless, we directly assess the implication of rent sharing in column (8) 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 younger workers and workers in elementary occupations) and the pre-reform total wage bill. We include this windfall measure in equation (13) and interact it with the age categories, the post reform dummy, and the post reform by treatment age dummy, and the interaction with the treatment intensity variable, S_{it-1} (including all 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 higher- and lower-productivity firms. If

[30] Appendix Figure D5 implements the same windfall analysis for younger workers. Similarly to Saez, Schoefer and Seim (2019), we find no pre-trends between high windfall and medium windfall firms among younger workers, but document an increase in revenues and employment at high windfall firms (relative to medium windfall firm) after the tax cut.

anything the estimated pass-through effects are slightly larger at higher-productivity firms (65% instead of 60% in the benchmark estimate) and still close to zero at lower-productivity firms once we take into account the windfall effects. Appendix Table B12 also shows that the windfall effects do not change the pass-through estimates when other firm quality measures are applied.

The treated post-reform windfall coefficient in column (8) of Table 6 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, these effects are less important at lower wages (higher subsidy rates) 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 subsidy. We apply the following formula:

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

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 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. Under the first approach, we assume the policy maker only cares about workers' welfare and the social marginal utility of employers is zero. In this version case, we do not incorporate the part of the tax cut that goes to employers into the WTP. In an alternative calculation, we assume that social marginal utility is the same on workers and employers and so we incorporate the part of the tax cut that goes to employers into the WTP.

We present the calculations in Table 8. When the policy maker only cares about workers' 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 employers. The MVPF is much larger at higher-productivity firms (0.51) than at lower-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 higher-quality firms.

Once we include the part of the tax cut going to employers into the WTP, the relationship between the MVPF and firm quality flips: payroll tax cuts targeting higher-productivity firms have lower MVPF (0.99) than payroll tax cuts targeting lower-productivity firms (1.52). This is because when the incidence of the tax cut between employers and employees does not matter, the employment creation effect will dominate the welfare calculations. Since employment creation mainly takes place at lower-productivity firms, the MVPF will be larger for subsidizing these 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 a slightly larger 1.22.

7 Effect on Women and Younger 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. The "Women 40" policy gives an early retirement option to women with 40 years of work credits, regardless of age. Because we do not observe the full employment history of older women, we cannot assess their eligibility to

the early retirement option. Nevertheless, in this section we estimate the same difference-in-differences model for women. Here we summarize the main results and in Appendix Section C we provide further details.

Overall, results for men and women are very similar. 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 lower-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 higher-TFP firms and only at high subsidy rates, though the increase is smaller for women.

7.2 Younger 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 summarize the the basic results here 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 of younger workers though the heterogeneity is more sensitive to the firm quality measure applied. When we use TFP as the measure of firm quality the employment elasticity at lower-TFP firms is close to one, and around half of that at higher-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 lower-quality firms, while responses at higher-quality firms are 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 younger 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 rent sharing in the context of younger 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 broadly in line with the labor market responses documented by Saez, Schoefer and Seim (2019) in response to the young worker tax cut in Sweden. Nevertheless, our finding that employment creation predominantly happens at lower-quality firms is a so far undocumented feature of the policy. Furthermore, while there are many similarities in the responses of younger and older workers in Hungary, there are also notable differences. For instance, we find no indication for wage changes for younger workers, while we estimate clear wage differences for older workers. This difference could be driven by wage rigidities that constrain firms' pass-through differently for younger and older workers. For instance, passing through the tax cut to younger 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. The latter might be more feasible than the former because workers dislike pay cuts (Bewley, 1998; Kaur, 2019).

Another difference is that the heterogeneity in employment effects seems to be less pronounced for younger workers than for older workers, 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 in the labor market for younger workers than in the labor market for older workers. 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 types of workers is large in a labor market, there will be smaller differences in the hiring incentives of lower- and higher-productivity firms. In line with that interpretation, we show that the employment differences between higher- and lower-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 older workers: employment increases only at lower-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 driven by 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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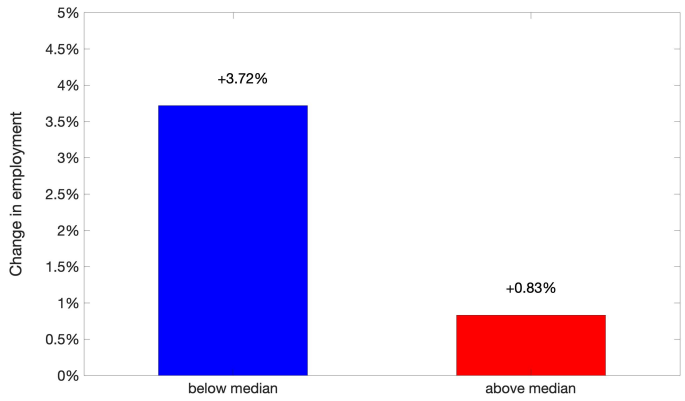
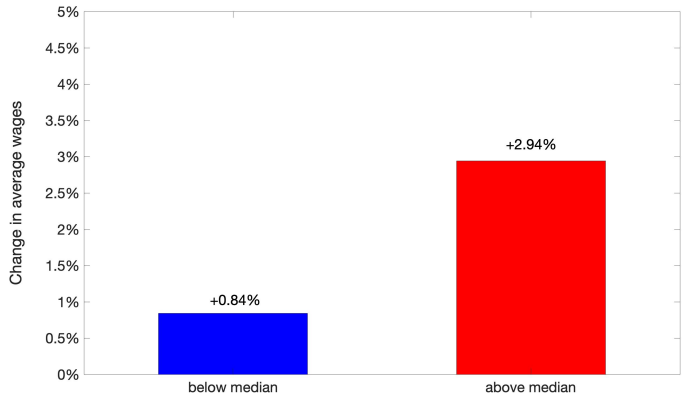
Figures and Tables

Figure 1: Employment and Wages Changes by Firm Productivity in the Search Model

(a) Employment

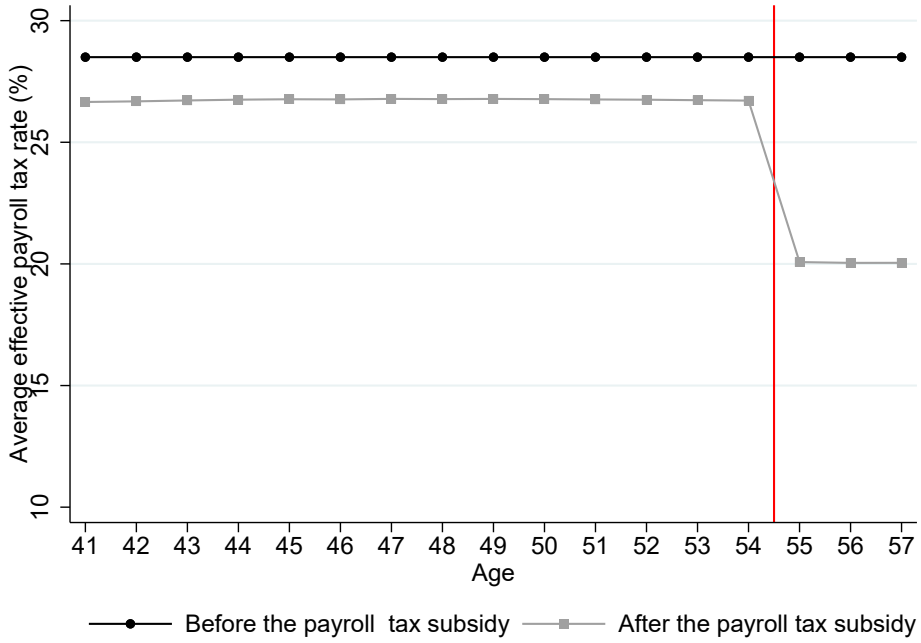


(b) Wages of Incumbent Workers



Note: Figure shows the effect of a tax subsidy that is 6% of the average wage in the economy. We solve numerically the model presented in Section 2 using the functional form assumptions and parameter values usually applied in the literature (see Appendix Section A.6). Panel (a) shows the impact of the subsidy on employment in percentage terms by productivity category of the employer (below- or above-median productivity). Panel (b) shows the impact of the subsidy on the average wages of incumbent workers by productivity category of the current employer (below- or above-median productivity). The incumbent workers are those workers who had a job the previous year – they were either employed at the same firm the previous year or were poached in the previous period from another firm.

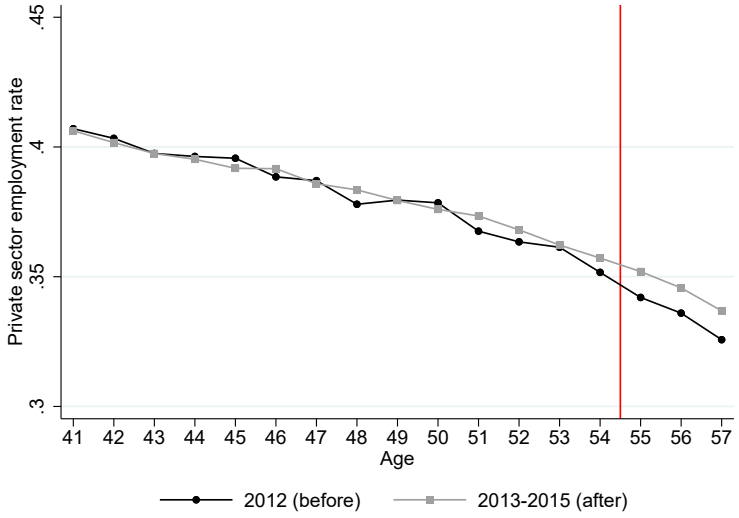
Figure 2: Employers' Social Security Contribution Rate by Workers' Age



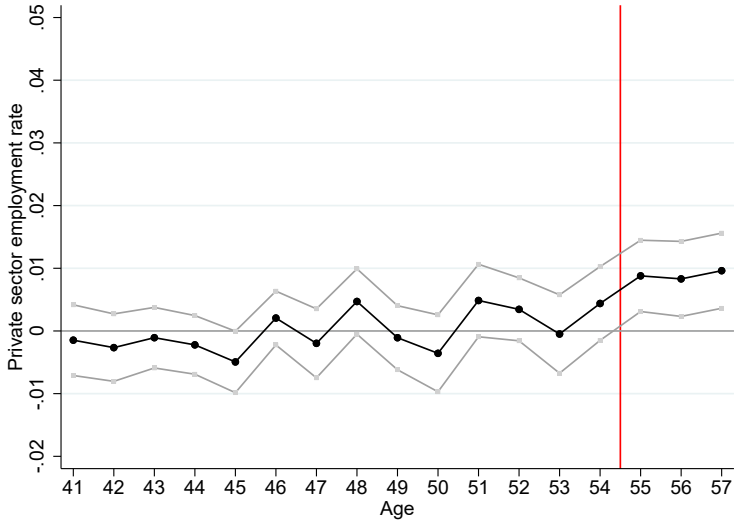
Note: Figure shows the average employer social security contribution rate by worker age for male workers. Before the implementation of the payroll tax subsidy, the payroll tax rate was a flat 28.5%. Between 2013-2015 (after the implementation of the subsidy), the payroll tax rate was 28.5% minus the subsidy. All individuals passed age 55 was eligible for a HUF 14,500 per month. Certain individuals were also eligible for the tax subsidy independently of their age (see Section 3.1 for the details)

Figure 3: Employment in Private Sector Companies by Age

(a) Private Sector Employment Rate

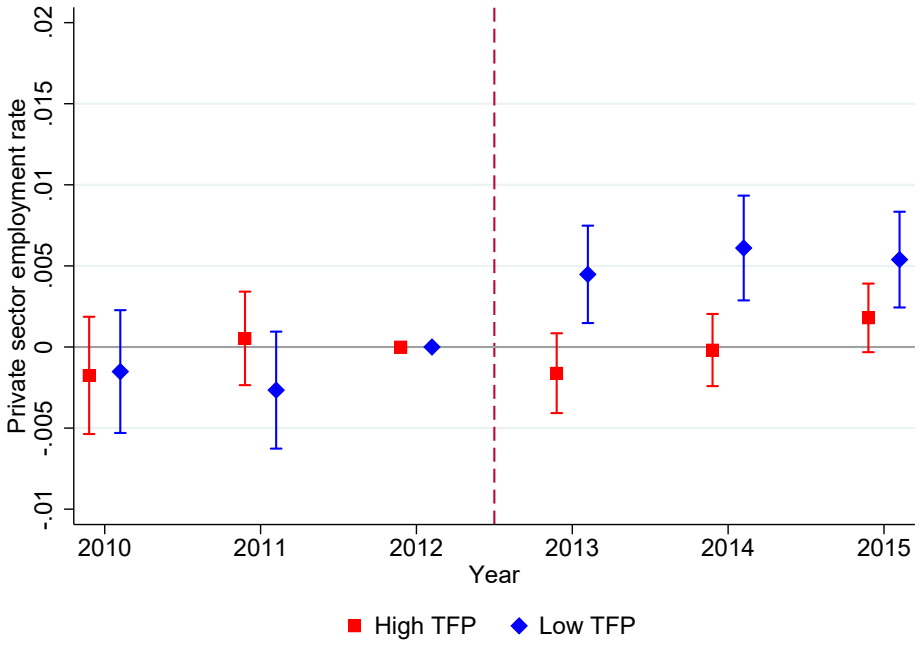


(b) Change in Private Sector Employment Rate



Note: Figure shows changes in private sector employment between the periods before and after the introduction of the age-specific payroll tax subsidy. Panel (a) shows the employment rate in private sector companies by age separately for year 2012 in black (before the implementation of the payroll tax subsidy) and for years 2013-2015 in grey (after the implementation of the payroll tax subsidy). Panel (b) shows the difference in employment rates between years 2013-2015 and 2012 relative to the average changes between age 41 and 54, with the 95% confidence interval (standard errors clustered at the age × period level). The vertical red line shows the age threshold where the tax subsidy became effective from 2013.

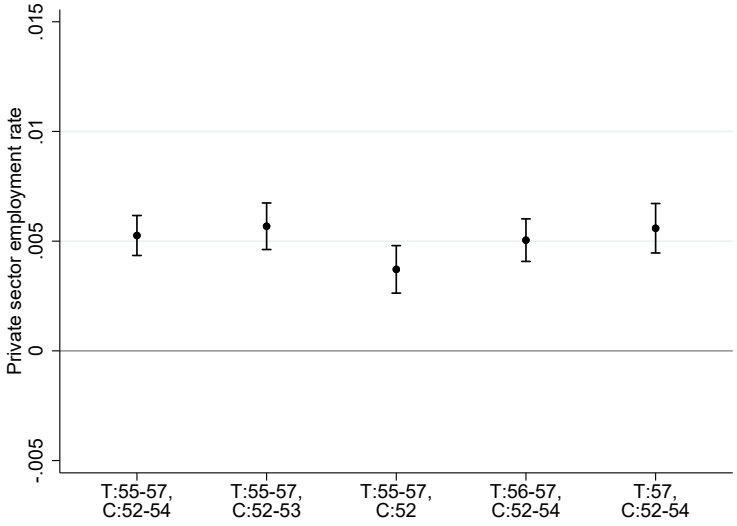
Figure 4: Employment in Private Sector Companies by Year: Heterogeneity by TFP



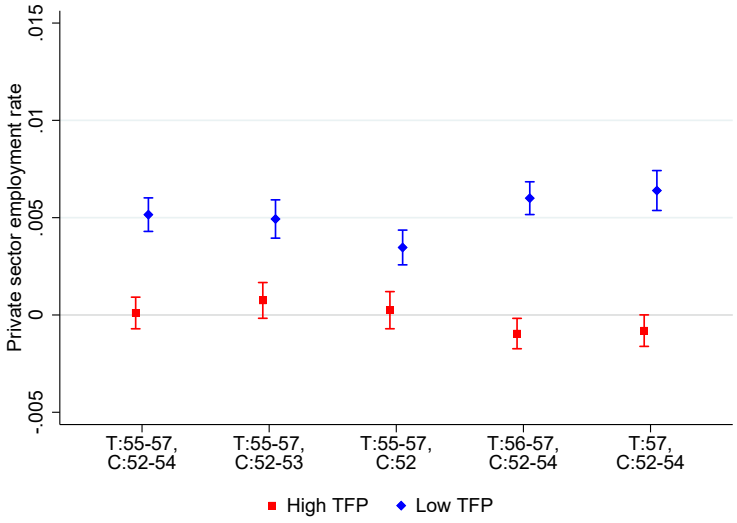
Note: Figure shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (10) separately for above-median (in red) and below-median (in blue) TFP firms. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. The sample is restricted to men. 95% confidence intervals are reported with standard errors clustered at the age × period level.

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

(a) All



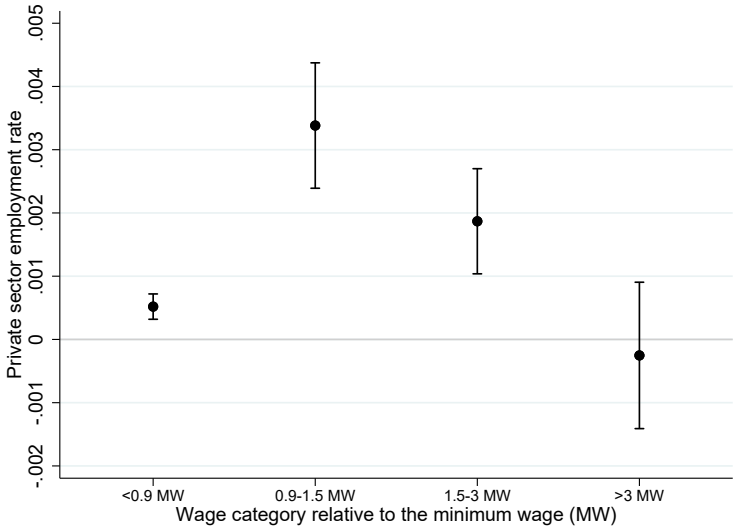
(b) By TFP



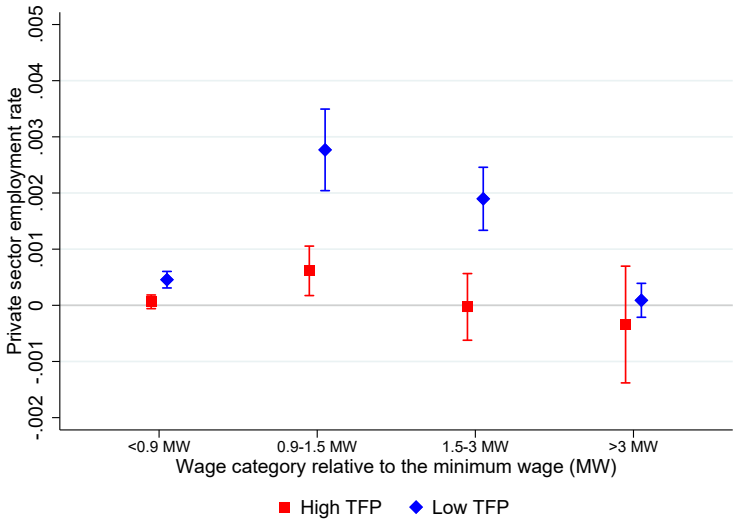
Note: Figure shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) for alternative control and treatment definitions. We compare the change in employment in the treatment age group with the change in employment in the control age group that was not affected by the tax subsidy. Panel (a) shows overall estimates and Panel (b) shows estimates separately for above-median (in red) and below-median (in blue) TFP firms. In both panels, the first estimate replicates our baseline results and the subsequent estimates change the age cutoffs for the control ("C") or treatment ("T") groups. The sample is restricted to men. 95% confidence intervals are reported with standard errors clustered at the age × period level.

Figure 6: Employment in Private Sector Companies: Heterogeneity by Wage

(a) All



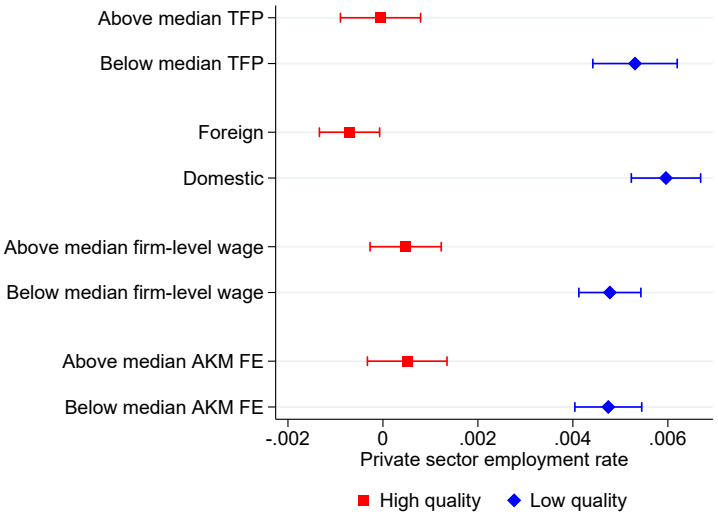
(b) By TFP



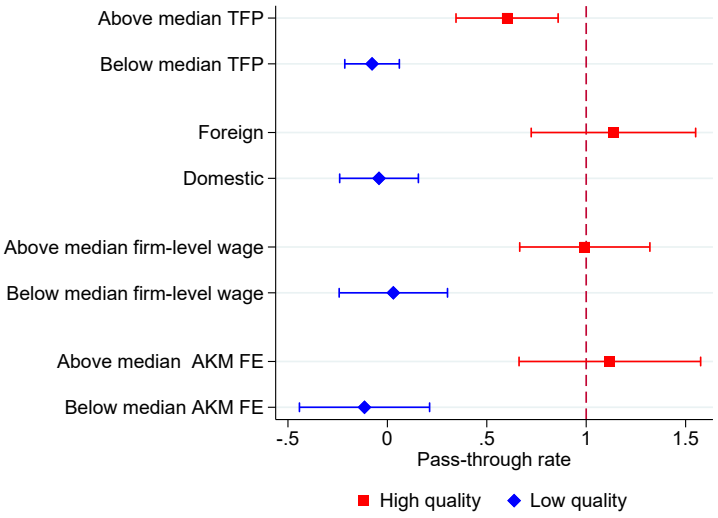
Note: Figure shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) by wage. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. Panel (a) shows overall estimates and Panel (b) shows estimates separately for above-median (in red) and below-median (in blue) TFP firms. In both panels, we present separate estimates by wage level. The sample is restricted to men. 95% confidence intervals are reported with standard errors clustered at the age × period level.

Figure 7: Employment and Wages in Private Sector Companies: Alternative Firm Quality Measures

(a) Employment



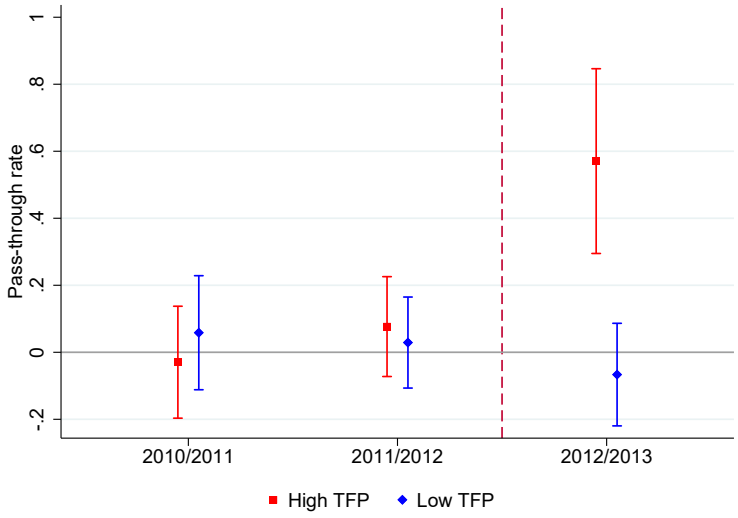
(b) Wage



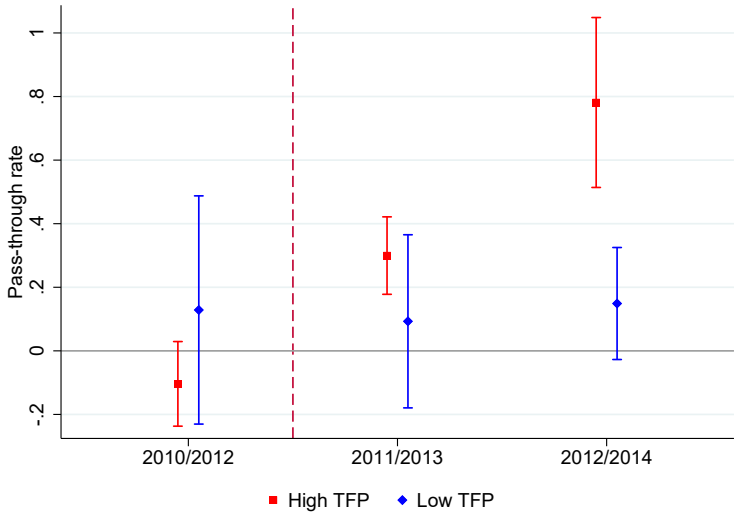
Note: Figure shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) and wages based on estimating equation (13) by alternative measures of firm quality. We compare the change in employment and wages among the over-55 age group that was affected by the payroll tax subsidy with the change in employment and wages among the 52 to 54 age group that was not affected by the tax subsidy. Panel (a) shows employment estimates and Panel (b) shows wage estimates (pass-through rate) separately for above-median (in red) and below-median (in blue) quality firms. The pass-through rate at below-median firms is β_1, the pass-through rate at above-median firms is the sum of β_1 and β_3 in equation (13). (In the case of foreign ownership a binary indicator of foreign ownership being above 50% is used.) In both panels, the first estimate replicates our baseline results and the subsequent estimates change the measure of firm quality used. The sample is restricted to men. 95% confidence intervals are reported with standard errors clustered at the age $\times$ period level.

Figure 8: Wages in Private Sector Companies by Year: Heterogeneity by TFP

(a) One-Year Wage Effects



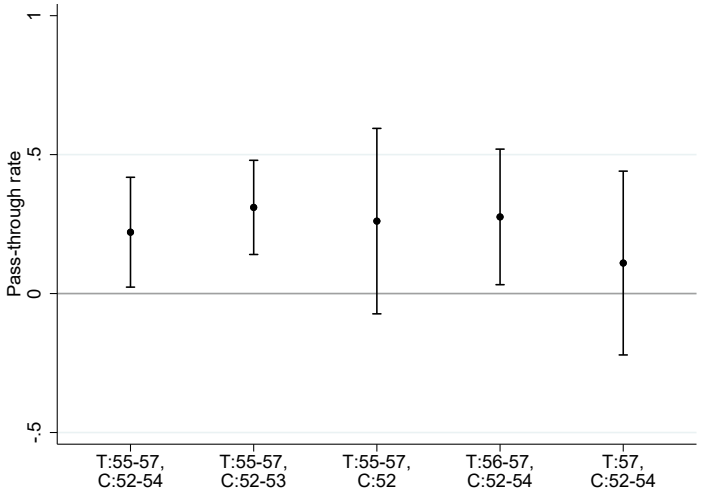
(b) Two-Year Wage Effects



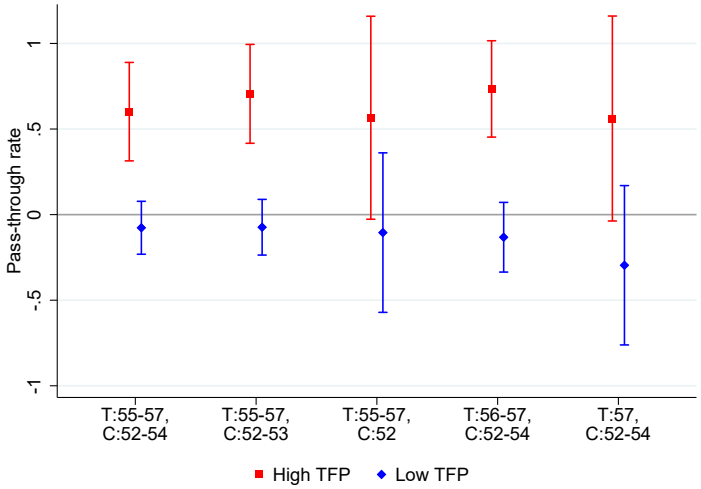
Note: Figure shows difference-in-differences estimates of the impact (pass-through rate) of the payroll tax subsidy on private sector wages based on estimating equation (13). We compare the change in wages among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. The pass-through rate at low TFP firms (blue diamonds on the figure) is β_1, the pass-through rate at high TFP firms (red squares on the figure) is the sum of β_1 and β_3 in equation (13). Each result is based on the two-year sample indicated on the x-axis. Panel (a) shows changes over one-year intervals and Panel (b) shows changes over two-year intervals. The sample is restricted to men. 95% confidence intervals are reported with standard errors clustered at the age $\times$ period level.

Figure 9: Wages in Private Sector Companies: Alternative Control and Treatment Ages

(a) All

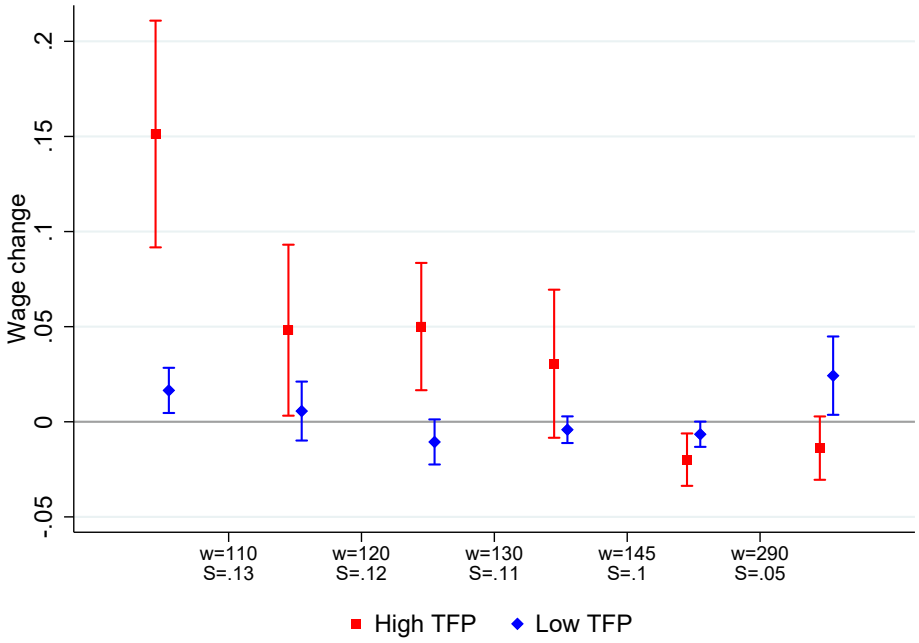


(b) By TFP



Note: Figure shows difference-in-differences estimates of the impact (pass-through rate) of the payroll tax subsidy on private sector wages based on estimating equations (12) and (13) for alternative control and treatment definitions. We compare the change in wages in the treatment age group that was affected by the payroll tax subsidy with the change in employment in the control age group that was not affected by the tax subsidy. Panel (a) shows overall estimates and Panel (b) shows estimates separately for above-median (in red) and below-median (in blue) TFP firms. In both panels, the first estimate replicates our baseline results and the subsequent estimates change the age cutoffs for the control ("C") or treatment ("T") groups. In Panel (a), the pass-through rate is β_1 in equation (12). In Panel (b), the pass-through rate at low TFP firms (blue diamonds on the figure) is β_1, the pass-through rate at high TFP firms (red squares on the figure) is the sum of β_1 and β_3 in equation (13). The sample is restricted to men. 95% confidence intervals are reported with standard errors clustered at the age $\times$ period level, except for the third and fifth estimation points (T:55-57, C:52 and T:57, C:52-54), where we do not cluster the standard errors as one cluster would capture the entire treatment or control age group.

Figure 10: Wages in Private Sector Companies: Heterogeneity by Subsidy Rate



Note: Figure shows difference-in-differences estimates of the payroll tax subsidy on private sector wages separately for above-median (in red) and below-median (in blue) TFP firms by lagged wage (w) and subsidy rate (S). We display the change in log wages among the over-55 age group that was affected by the payroll tax subsidy relative to the change in log wages among the 52 to 54 age group that was not affected by the tax subsidy. 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, with $S_{it-1} = 14,500/w_{it-1}$. The sample is restricted to men. 95% confidence intervals are reported with standard errors clustered at the age $\times$ period level.

Table 1: Summary Statistics

	(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
Number of individuals	123,154	141,875

Note: Table shows summary statistics for the treatment and and control groups in 2012. The treatment group comprises ages 55-57 and the control group comprises ages 52-54. Panel A shows labor market characteristics, including the share of workers employed in the private sector, the average monthly (full-time equivalent) wage in the private sector of these workers, and the share of these workers in white collar jobs. Panel B shows measures of firm quality composition, including the share of workers at firms with above-median firm quality and at foreign-owned firms. We calculate the median quality (measured by TFP, AKM firm effects, and average wage) based on all prime age workers. Firms are categorized as foreign-owned if foreign ownership is at least 50%. Further details on quality measures 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]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms in Panel A. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. 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 treatment and control age groups in 2012, the year before the introduction of the payroll tax subsidy. The second row adds to that baseline the estimated change from Panel A. The third row shows the percent change in employment relative to the baseline. Panel C calculates the percent change in labor cost using an analogous difference-in-differences estimation for tax rates. The first row shows the average unsubsidized labor cost in the control group between 2013-2015. The second row calculates the average labor cost for the treatment group taking into account the tax cut. Panel D calculates the implied employment elasticity with respect to the wage change by taking the ratio of the percent change in employment (Panel B) and labor cost (Panel C). The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. ($N = 9,003,984$ individual-months)

Table 3: Employment Effects of the Tax Cut by Employment Categories

Employment at private sector firms (including single-entry and double-entry bookkeping firms)	0.0096***
	[0.0006]
	⟨0.409⟩
Public sector	0.0016***
	[0.0003]
	⟨0.062⟩
Self-employed	-0.0014***
	[0.0003]
	⟨0.097⟩
Inactive/unemployed	-0.0101***
	[0.0007]
	⟨0.419⟩

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) for all private sector firms (extending the baseline category of private sector firms with single-entry bookkeping firms and firms with more than 10,000 workers, see Appendix Table B2 for separate estimates for these firm categories), as well as similar difference-in-differences estimates of the impact on the tax subsidy on public sector employment, self-employment and unemployment. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. For reference, we show mean outcomes in May 2012 in angle brackets. ($N = 9,003,984$ individual-months)

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***	-0.43	-0.48	-0.31
	[0.0005]	[0.0004]	[0.0002]	[0.06]	[0.06]	[0.09]
	{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 occupations	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⟩			

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms, as well as the implied employment elasticity with respect to the wage for all firms (column 4) and separately for below-median (column 5) and above-median (column 6) TFP firms. Panel A considers employment in jobs paying above vs below 150% of the minimum wage. Panel B considers employment in occupations with above- vs below-median average wages in 2012. Panel C considers employment in occupations requiring different levels of education, defined as the modal level of education in the Labor Force Survey in 2012. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. For reference, we show the share of individuals in each category in curly brackets and mean outcomes in May 2012 in angle brackets. ($N = 9,003,984$ individual-months)

Table 5: Employment Effects of the Tax Cut by Employment History and Firm Age

	(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⟩

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms. In Panel A, the outcome is employment conditional on less than 12 months employment the past 12 months (new entrants) vs employment conditional on 12 months employment the past 12 months (incumbents). In panel B, the outcome is employment at firms that existed the previous calendar year (old firms) vs employment at firms that did not exist or the previous calendar year (new firms). In panel C, the outcome is employment at a firms that existed in 2012 vs employment at firms that was established after 2012. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. For reference, we show mean outcomes in May 2012 in angle brackets. ($N = 9,003,984$ individual-months)

Table 6: Wage Effects of the Tax Cut

Outcome variable: log(wage)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	New entrants Baseline	Incumbents Baseline	Incumbents Baseline	Incumbents Same firm	Incumbents Different firm	Incumbents With part-time	Incumbents Two-year effects	Incumbents Extended
Treatment × After	0.022	-0.019*	0.008	0.007	-0.026	0.021**	-0.021**	0.011
	[0.018]	[0.010]	[0.007]	[0.006]	[0.113]	[0.009]	[0.009]	[0.016]
Treatment × After × Subsidy Rate		0.221**	-0.077	-0.071	0.249	-0.191**	0.149*	-0.129
		[0.090]	[0.070]	[0.053]	[0.925]	[0.085]	[0.081]	[0.215]
High TFP × Treatment × After			-0.046***	-0.041***	-0.068	-0.040***	-0.045***	-0.053**
			[0.013]	[0.011]	[0.118]	[0.006]	[0.014]	[0.021]
High TFP × Treatment × After × Subsidy Rate			0.678***	0.602***	0.905	0.600***	0.632***	0.780***
			[0.137]	[0.104]	[1.032]	[0.038]	[0.163]	[0.242]
Windfall × Treatment × After								0.546*
								[0.277]
Windfall × Treatment × After × Subsidy Rate								-5.979**
								[2.588]
Pass-through rate								
All firms		0.221**						
		[0.090]						
Low TFP			-0.077	-0.071	0.249	-0.191**	0.149*	-0.129
			[0.070]	[0.053]	[0.925]	[0.085]	[0.081]	[0.215]
High TFP			0.602***	0.531***	1.154**	0.409***	0.781***	0.651***
			[0.131]	[0.110]	[0.425]	[0.107]	[0.121]	[0.097]
Observations	13,429	97,789	97,789	93,666	4,123	112,713	82,910	97,789

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector wages based on estimating equations (12) and (13). We compare the change in wages among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. Except for column (6), the sample is restricted to full-time workers. Except for column (1), the sample is restricted to individuals with continuous employment during the preceding 12 months. In column (1), the sample is restricted to individuals without continuous employment during the preceding 12 months. In column (4), the sample is restricted to workers who were employed at the same firm the year before. In column (5), the sample is restricted to workers who were employed at a different firm the year before. In column (7), the wage effects are estimated over 2012 and 2014. In column (8), 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. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level.

Table 7: Wage Effects of the Tax Cut: Heterogeneity by Firm Quality Indicators

Outcome variable: log(wage)	(1) TFP	(2) Foreign ownership	(3) Mean firm wage	(4) AKM FE
Treatment × After	0.008	0.003	-0.004	0.010
	[0.007]	[0.011]	[0.016]	[0.020]
Treatment × After × Subsidy rate	-0.077	-0.042	0.030	-0.115
	[0.070]	[0.101]	[0.139]	[0.167]
High quality × Treatment × After	-0.046***	-0.068***	-0.054***	-0.072***
	[0.013]	[0.014]	[0.008]	[0.014]
High quality × Treatment × After × Subsidy rate	0.678***	1.179***	0.963***	1.235***
	[0.137]	[0.211]	[0.051]	[0.160]
Pass-through rate				
Low quality	-0.077	-0.042	0.030	-0.115
	[0.070]	[0.101]	[0.139]	[0.167]
High quality	0.602***	1.137***	0.993***	1.119***
	[0.131]	[0.211]	[0.167]	[0.233]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector wages based on estimating equation (13). We compare the change in wages among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. In each column, we interact all coefficients with an indicator for whether the firm is above-median with respect to the given quality measure. (In the case of foreign ownership a binary indicator of foreign ownership being above 50% is used.) The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. ($N = 97,789$ individual-months)

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	-159	1437
(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	-14	1433
(1)+(4)-(3) Willingness to pay (WTP), workers and firms	5297	2547	2770
Marginal value of public funds (MVPF), workers only	0.27	-0.01	0.51
MVPF, workers and firms	1.22	1.52	0.99

Note: Table shows estimates of the marginal value of public funds (MVPF) associated with the payroll tax subsidy. In each row, we report per worker average monthly amounts in HUF for workers aged 55 and above. Row 1 reports the direct cost defined as the subsidy amount multiplied by the employment rate of the subsidized group. Row 2 reports the subsidy amount received by workers based on the wage effect results reported in Table 6. Row 3 reports the benefits that non-employed individuals who become employed would have received based on the estimated employment effect of the reform and the average unemployment benefit amount. Row 4 reports the additional net wages received by non-employed individuals who become employed is based on estimated employment effect by wage categories. Row 5 reports the additional tax revenue defined as the total estimated income tax and social security contributions paid for workers who become employed.

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 $\Psi(\cdot)$. A job offer is a draw of a firm productivity from the vacancy distribution $\Gamma(\cdot)$ with probability distribution function $\gamma(\cdot)$. 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 $\kappa(\cdot)$. 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.[31]

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,

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

[31] 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 (20) 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 $L(\cdot)$, 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 (15), (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 $\xi(\cdot)$ 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,

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

After rearrangement and plugging in the equilibrium value of u,

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

Differentiation of this term with respect to y gives:

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

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:

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

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 $\kappa(\cdot)$ 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 $\kappa(\cdot)$ 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 $\kappa(\cdot)$ 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 $\lim_{y\to\infty} \frac{\gamma(0)}{\gamma(y)}$ 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 $\lim_{y\to\infty} \frac{l(0)}{l(y)}$ 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 (8), 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 (8), 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, younger 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 higher-productivity firms hire from unemployment to the same extent, thus lower-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 older 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:

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

- $\beta = 0.95$, 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

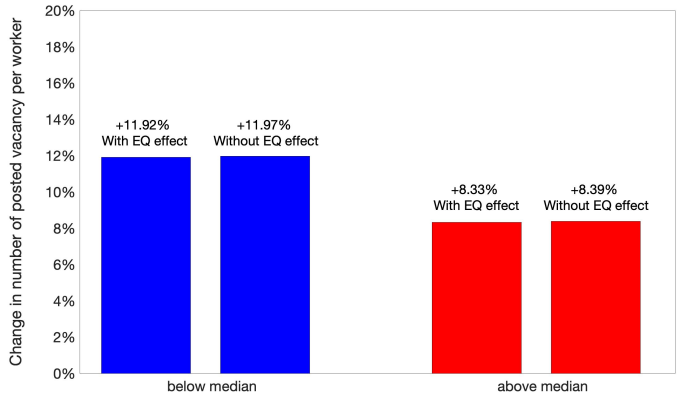
	(1)	(2)	(3)
Tax subsidy rate	0%	6%	Δ (15%)
Unemployment	0.223	0.206	-0.017
Job market tightness (θ)	1.935	2.380	0.445
Job finding rate	0.348	0.386	0.038

Note: 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%.

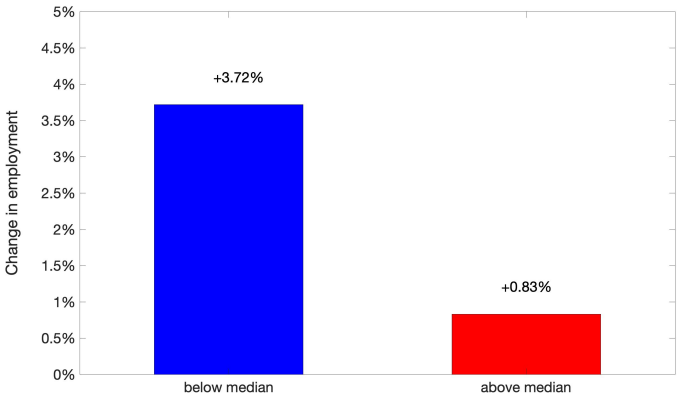
Turning to wages (Figure A3), the wage impact of the tax subsidy for workers who were not employed the previous period is essentially zero, while it is 2.3% for the rest of the workers ("incumbents"). Finally, among incumbent workers, the wage effect is small (0.8%) at less productive firms, whereas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

Appendix Figure A1: Simulation Results: Vacancies



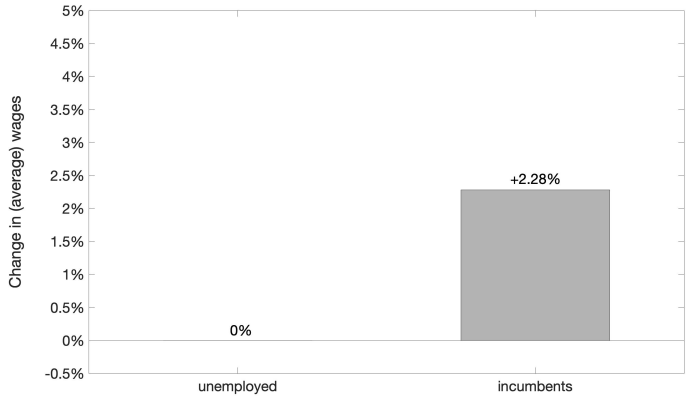
Note: Figure shows the simulated effect of a tax subsidy that is 6% of the average wage in the economy. Figure shows the impact of the subsidy on the number of posted vacancies per worker by productivity category of the employer (below or above median productivity). For each productivity category, the left bars show the impact with equilibrium (EQ) effects, the right bars show the impact without equilibrium effects.

Appendix Figure A2: Simulation Results: Employment



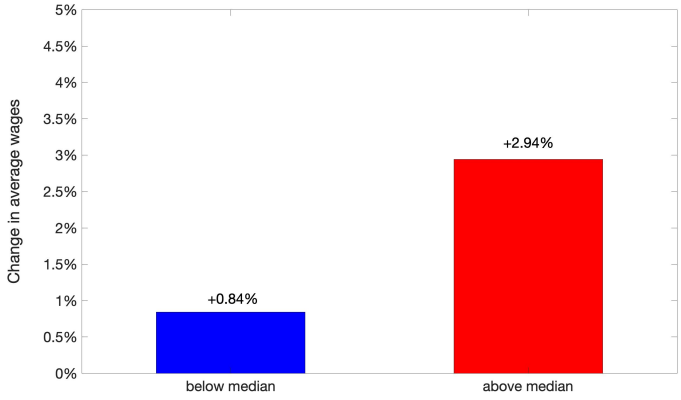
Note: Figure shows the simulated effect of a tax subsidy that is 6% of the average wage in the economy. Figure shows the impact of the subsidy on employment in percentage terms by productivity category of the employer (below- or above-median productivity).

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



Note: Figure shows the simulated effect of a tax subsidy that is 6% of the average wage in the economy. 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 Incumbent Workers

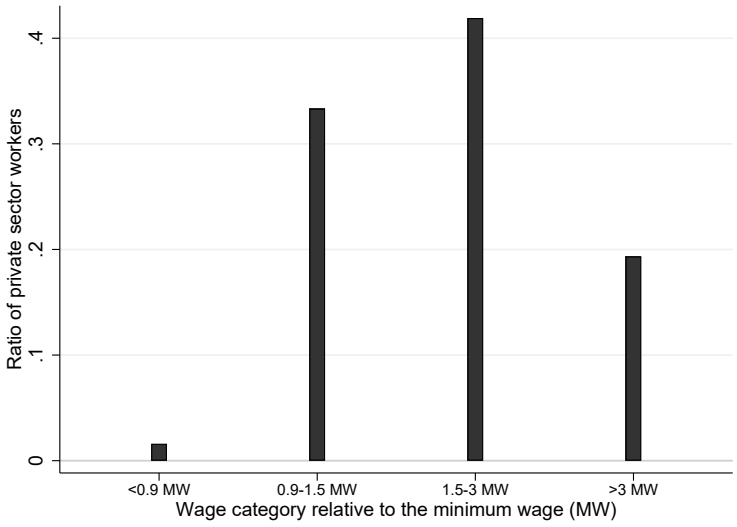


Note: Figure shows the simulated effect of a tax subsidy that is 6% of the average wage in the economy. Figure shows the impact of the subsidy on the wage of incumbent workers by productivity category of the current employer (below- or above-median productivity). The group of incumbent workers consists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

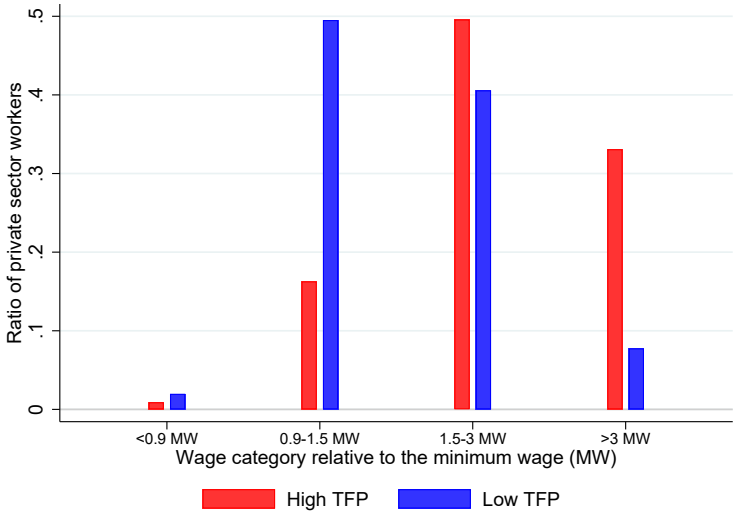
B Additional Figures and Tables

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

(a) All



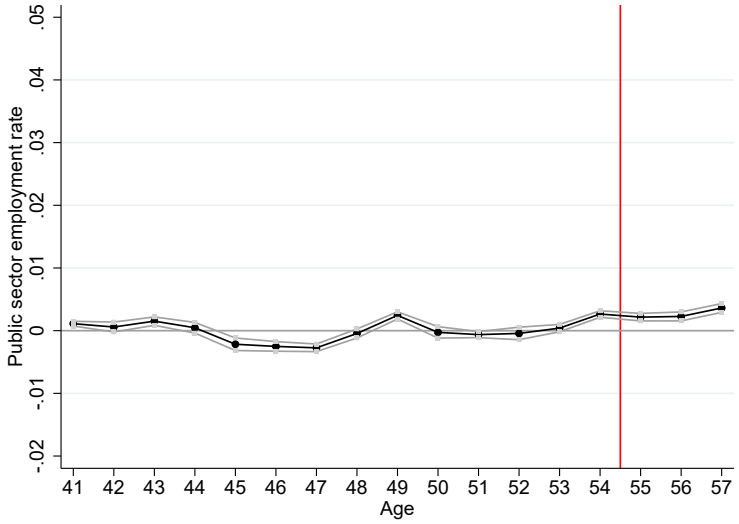
(b) By TFP



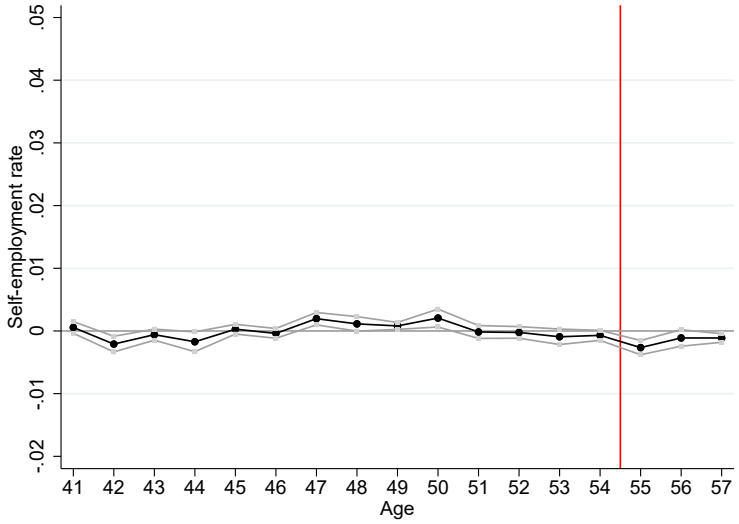
Note: Figure shows the distribution of male workers aged 52-57 by wage categories in 2012-2015. Panel (a) shows the distribution across all firms. Panel (b) shows the distribution separately across above-median (in red) and below-median (in blue) TFP firms.

Appendix Figure B2: Change in Employment Rate in Placebo Groups

(a) Public Sector Employment Rate

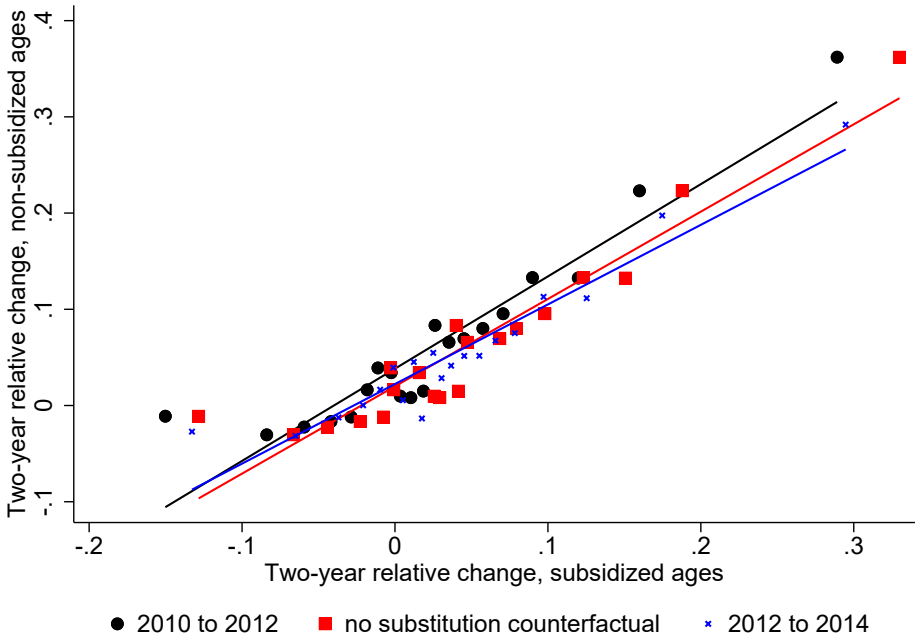


(b) Self-Employment Rate



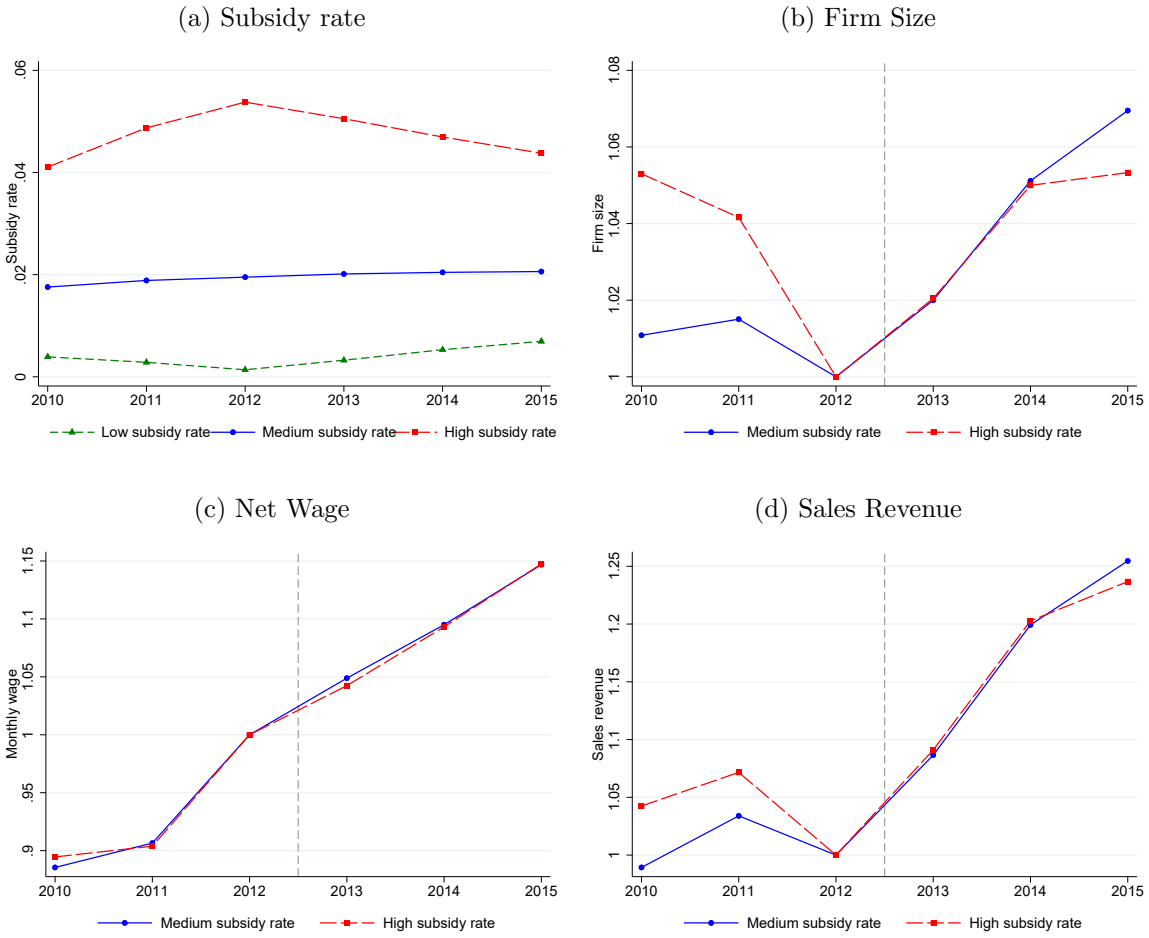
Note: Figure shows the difference in employment rates between years 2013-2015 and 2012, adjusted to mean zero over ages 41-54, with the 95% confidence interval (standard errors clustered at the age × period level) for two groups that were not eligible for the payroll tax subsidy. Panel (a) shows employment in the public sector and Panel (b) shows self-employment. The vertical red line shows the age threshold where the tax subsidy became effective from 2013. The sample is restricted to men.

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



Note: Figure shows the firm-level change in employment during the two-year period before the introduction of the payroll tax subsidy (2010 to 2012, in black) and during the two-year period after the introduction of the payroll tax subsidy (2012 to 2014, in blue) among the subsidized and non-subsidized age groups. 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 comprising 50% of the population, on average). After this restriction, we also exclude those firms that are not in the sample throughout years 2010-2014. We show a binned scatter 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: Figure replicates the basic results of Saez, Schoefer and Seim (2019). Using 2012 data, we calculate the firm-level windfall defined as the total subsidy payable based on workers aged 55 and above at the firm relative to the total payroll of the firm. We calculate the quartiles of the subsidy rate, excluding firms with zero subsidy rate, and group firms into four categories. "Low subsidy rate" firms have either zero subsidy rate or belong to the bottom quartile. "Medium subsidy rate" firms belong to the middle two quartiles. "High subsidy rate" firms belong to the the top quartile. We compare the outcomes of these groups, focusing on the medium and high subsidy rate groups. Panel (a) shows the average subsidy rate. Panel (b) shows firm size. Panel (c) shows average net wage. Panel (d) shows sales revenue.

Appendix Table B1: Comparing the Employment Measures in the Administrative Data with Official Statistics

	(1) Administrative data	(2) Labor Force Survey
Panel A: Private and public sector		
Including self-employment	60.1%	61.6%
Excluding self-employment	49.4%	51.8%
Panel B: Private sector (excluding self-employment)		
All private sector firms	43.1%	
Double-entry bookkeeping firms	37.5%	
Double-entry bookkeeping firms with at most 10,000 employees	35.5%	

Note: Table reports employment rates in the non-retired population of men aged 52-57 in 2012. Column (1) reports employment rates based on the linked employer-employee administrative data used in this paper. Column (2) reports employment rates based on the Labor Force Survey (LFS) of the Hungarian Central Statistical Office. Panel A shows employment rates in the private and public sectors with and without the self-employed. Panel B shows private sector employment in all firms, double-entry bookkeeping firms, and double-entry bookkeeping firms with at most 10,000 employees. It displays statistics only based on the administrative data because civil servants and the type of the firm cannot be identified in the LFS. The employment category in the last row corresponds to the employment definition we use in this paper. Employment under the public work program is excluded from all employment categories. Part-time workers are included; in this table we do not adjust employment by working hours.

Appendix Table B2: Employment Effects of the Tax Cut by Firm Categories

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Double-entry bookkeeping firms, excluding firms with more than 10,000 workers	0.0053***	0.0053***	-0.0001
	[0.0005]	[0.0005]	[0.0004]
	⟨0.330⟩	⟨0.167⟩	⟨0.163⟩
Panel B: All firms, including single-entry bookkeping firms and firms with more than 10,000 workers	0.0096***	0.0094***	0.0003
	[0.0006]	[0.0006]	[0.0004]
	⟨0.409⟩	⟨0.227⟩	⟨0.181⟩

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9). We show the results for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms. In Panel A, we report the results for the baseline category of private sector employment (excluding firms with more than 10,000 workers). In Panel B, we report the results for all firms, assuming that all single-entry bookkeping firms (for which firms the TFP is not observed) are below-median TFP firms and all firms with more than 10,000 workers are above-median TFP firms (which is in line with what we observe in the data). We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. For reference, we show mean outcomes in May 2012 in angle brackets. ($N = 9,003,984$ individual-months)

Appendix Table B3: Employment Effects of the Tax Cut: Binary Indicator of Employment

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Change in the probability of employment			
— $After \times Treated$	0.0054***	0.0053***	0.0001
	[0.0005]	[0.0005]	[0.0004]
Panel B: Percent change in employment			
—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%
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	-0.50	-0.01
	[0.03]	[0.05]	[0.06]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating 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. We show the results for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms in Panel A. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. 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 treatment and control age groups in 2012, the year before the introduction of the payroll tax subsidy. The second row adds to that baseline the estimated change from Panel A. The third row shows the percent change in employment relative to the baseline. Panel C calculates the percent change in labor cost using an analogous difference-in-differences estimation for tax rates. The first row shows the average unsubsidized labor cost in the control group between 2013-2015. The second row calculates the average labor cost for the treatment group taking into account the tax cut. Panel D calculates the implied employment elasticity with respect to the wage change by taking the ratio of the percent change in employment (Panel B) and labor cost (Panel C). The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. ($N = 9,003,984$ individual-months)

Appendix Table B4: Employment Effects of the Tax Cut: Sample Definitions

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Baseline sample (retirees excluded)			
Employment effect	0.0053***	0.0053***	-0.0001
	[0.0005]	[0.0005]	[0.0004]
Implied elasticity	-0.30	-0.53	0.01
	[0.03]	[0.05]	[0.06]
Panel B: Extended sample (retirees included)			
Employment effect	0.0065***	0.0062***	-0.0001
	[0.0005]	[0.0004]	[0.0004]
Implied elasticity	-0.37	-0.64	0.01
	[0.03]	[0.05]	[0.06]
Panel C: Baseline sample (retirees excluded)			
Employment effect, excluding elementary occupations	0.0063***	0.0063***	-0.0000
	[0.0006]	[0.0005]	[0.0005]
Implied elasticity	-0.41	-0.73	0.00
	[0.04]	[0.06]	[0.07]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. Panel A replicates the baseline results with retirees excluded from the sample. Panel B shows results with retirees included in the sample. Panel C shows results with retirees excluded from the sample and excluding elementary occupations from the definition of private sector employment (but keeping them in the sample). The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. ($N = 9,003,984$ individual-months in Panels A and C, $N = 9,482,667$ individual-months in Panel B)

Appendix Table B5: Employment Effects of the Tax Cut: Short-Run Estimates (2012-2013)

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

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9), restricting the sample to 2012-2013. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. Column (1) shows short-run estimates for all firms. Columns (2) to (5) show estimates separately for above-median and below-median quality firms. (In the case of foreign ownership a binary indicator of foreign ownership being above 50% is used.) The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. ($N = 4,711,215$ individual-months)

Appendix Table B6: Employment and Wage Effects of the Tax Cut: Firm Quality Indicators Defined in 2012

	(1)	(2)	(3)	(4)
Panel A: 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]
Panel B: Log(wage), pass-through rate				
Firm quality uses post-reform years	TFP	Foreign ownership	Mean firm wage	AKM FE
Low quality	-0.077	-0.042	0.030	-0.115
	[0.070]	[0.101]	[0.139]	[0.167]
High quality	0.602***	1.137***	0.993***	1.119***
	[0.131]	[0.211]	[0.167]	[0.233]
Firm quality uses pre-reform years only	TFP	Foreign ownership	Mean firm wage	AKM FE
Low quality	-0.094	-0.105	0.219*	-0.113
	[0.119]	[0.139]	[0.113]	[0.078]
High quality	0.547***	1.236***	1.103***	1.019***
	[0.108]	[0.180]	[0.200]	[0.224]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) and wages based on estimating equation (13) by alternative measures of firm quality. We compare the change in employment and wages among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. Panel A shows employment estimates and Panel B shows wage estimates separately for above-median and below-median quality firms. (In the case of foreign ownership a binary indicator of foreign ownership being above 50% is used.) In both panels, the first part replicates our baseline result with all years included in the definition of the firm quality measures and the second part shows results with only years before the introduction of the payroll tax subsidy used in the definition of firm quality measures. 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. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level.

Appendix Table B7: Employment Effects of the Tax Cut: Heterogeneity by Firm Size

	(1) All firms	(2) Low TFP	(3) High TFP
Employment at firms with 1-49 workers	0.0015***	0.0015***	0.0001
	[0.0004]	[0.0003]	[0.0002]
	{39%}	{33%}	{6%}
	⟨0.1272⟩	⟨0.1074⟩	⟨0.0198⟩
Employment at firms with 50+ workers	0.0036***	0.0035***	0.0001
	[0.0005]	[0.0003]	[0.0005]
	{61%}	{18%}	{43%}
	⟨0.2021⟩	⟨0.0608⟩	⟨0.1413⟩

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) separately for all firms (column 1), below-median (column 2) and above-median (column 3) TFP firms by firm size. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. For reference, we show the share of individuals in each category in curly brackets and mean outcomes in May 2012 in angle brackets. ($N = 9,003,984$ individual-months)

Appendix Table B8: Employment Effects of the Tax Cut: Heterogeneity by Local Labor Market Conditions

	(1) Low TFP	(2) High TFP
Panel A: By unemployment rate		
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
Panel B: By change in labor market conditions		
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
Panel C: By Share of Older Workers		
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

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) separately for below-median (column 1) and above-median (column 2) TFP firms. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. 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. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. For reference, we mean outcomes in May 2012 in angle brackets.

Appendix Table B9: Robustness Check: Private Sector Wage Effects, Heterogeneity by Lagged Firm Quality

Outcome variable: log(wage)	(1)	(2)	(3)	(4)	(5)	(6)
	New entrants Baseline	Incumbents Baseline	Incumbents Baseline	Incumbents With part-time	Incumbents Two-year effects	Incumbents Extended
Treatment × 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]
Treatment × 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 × Treatment × After			-0.044***	-0.036***	-0.038***	-0.051**
			[0.023]	[0.008]	[0.017]	[0.018]
High TFP × Treatment × After × Subsidy rate			0.587***	0.484***	0.540***	0.687***
			[0.122]	[0.064]	[0.076]	[0.201]
Windfall × Treatment × After						0.561*
						[0.309]
Windfall × Treatment × After × Subsidy rate						-6.324**
						[2.601]
Observations	13,429	97,789	97,789	112,713	82,910	97,789

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector wages based on estimating equations (12) and (13), with the difference that lagged firm quality ($Q_{j(it-1)}$) is used. We compare the change in wages among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. 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). 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. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level.

Appendix Table B10: Private Sector Wage Effects: Heterogeneity by Education Categories of Jobs

Outcome variable: log(wage)	(1)	(2)	(3)
	Incumbents, education category of jobs:		
	Primary and lower-secondary	Upper secondary	Tertiary
Treatment × After	0.032***	-0.028	-0.014
	[0.010]	[0.019]	[0.012]
Treatment × After × Subsidy rate	-0.285***	0.353*	0.221*
	[0.084]	[0.190]	[0.109]
High TFP × Treatment × After	-0.053***	-0.014	-0.045
	[0.013]	[0.023]	[0.033]
High TFP × Treatment × After × Subsidy rate	0.643***	0.598**	0.742
	[0.130]	[0.240]	[0.683]
Pass-through rate			
Low TFP	-0.285***	0.353*	0.221*
	[0.084]	[0.190]	[0.109]
High TFP	0.358*	0.951***	0.963
	[0.164]	[0.140]	[0.585]
Observations	66,180	15,070	15,724

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector wages based on estimating equation (13). We compare the change in wages among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. The sample is split by education categories of jobs (measured at the previous year), which 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. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level.

Appendix Table B11: Private Sector Wage Effects: Heterogeneity by Firm Size

Outcome variable: log(wage)	(1) Incumbents 1-49 workers	(2) Incumbents 50+ workers
Treatment × After	-0.002	0.020
	[0.016]	[0.016]
Treatment × After × Subsidy rate	0.028	-0.234
	[0.136]	[0.174]
High TFP × Treatment × After	-0.027	-0.061***
	[0.034]	[0.017]
High TFP × Treatment × After × Subsidy rate	0.422	0.889***
	[0.285]	[0.167]
Pass-through rate		
Low TFP	0.028	-0.234
	[0.136]	[0.174]
High TFP	0.450	0.653***
	[0.290]	[0.128]
Observations	35,862	61,861

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector wages based on estimating equation (13). We compare the change in wages among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. In column (1), the sample is restricted to workers employed at firms with 1-49 employees. In column (2), the sample is restricted to workers employed at firms with 50+ employees. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level.

Appendix Table B12: 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
Treatment × After	0.008	0.011	0.003	0.006	-0.0004	0.011	0.010	0.025
	[0.007]	[0.016]	[0.011]	[0.008]	[0.016]	[0.019]	[0.020]	[0.024]
Treatment × After × Subsidy rate	-0.077	-0.129	-0.042	-0.101	0.030	-0.094	-0.115	-0.272
	[0.070]	[0.215]	[0.101]	[0.129]	[0.139]	[0.189]	[0.167]	[0.237]
High quality × Treatment × After	-0.046***	-0.053**	-0.068***	-0.070***	-0.054***	-0.065***	-0.072***	-0.082***
	[0.032]	[0.021]	[0.014]	[0.013]	[0.008]	[0.012]	[0.014]	[0.021]
High quality × Treatment × After × Subsidy rate	0.678***	0.780***	1.137***	1.222***	0.993***	1.073***	1.119***	1.345***
	[0.131]	[0.242]	[0.211]	[0.235]	[0.167]	[0.109]	[0.233]	[0.255]
Windfall × Treatment × After		0.546*		0.391		-0.111		-0.150
		[0.277]		[0.286]		[0.257]		[0.268]
Windfall × Treatment × After × Subsidy rate		-5.979**		-4.141**		-0.412		0.588
		[2.588]		[1.716]		[1.208]		[2.247]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector wages based on estimating equation (13). We compare the change in wages among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. 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. The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. ($N = 97,789$ individual-months)

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 lower-TFP or at a higher-TFP firm. We apply exactly the same definition for higher- and lower-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. higher-TFP firms. This translates to a -0.48 (s.e. 0.07) employment elasticity at lower-TFP firms and a -0.29 (s.e. 0.10) at higher-TFP ones. Therefore there is a clear and statistically significant difference in the employment responses at higher- and lower-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 lower-productivity firms increased by 0.21 percentage points in low paying occupations while the change in employment at higher-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 higher-productivity firms (0.37 vs. 0.01 for men). Overall, the findings highlight that the tax cut among women is smaller at higher-productivity firms than at lower-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 the effective subsidy rate. The patterns of wage effects are similar for women and men (see Figure 10 for men). Wages increase only at higher-TFP firms and lower wages with a higher corresponding effective subsidy rate increase more. However, the wage increase we see at higher-productivity firms is somewhat smaller for women.

Appendix Table C1: Elasticity of Employment: Women

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Change in the probability of employment			
— $After \times Treated$	0.0051***	0.0037***	0.0014***
	[0.0007]	[0.0005]	[0.0005]
Panel B: Percent change in employment			
—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%
Panel C: Percent change in 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%
Panel D: Implied elasticity (Panel B/Panel C)			
— Elasticity	-0.40	-0.48	-0.29
	[0.06]	[0.07]	[0.10]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms in Panel A. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. 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 treatment and control age groups in 2012, the year before the introduction of the payroll tax subsidy. The second row adds to that baseline the estimated change from Panel A. The third row shows the percent change in employment relative to the baseline. Panel C calculates the percent change in labor cost using an analogous difference-in-differences estimation for tax rates. The first row shows the average unsubsidized labor cost in the control group between 2013-2015. The second row calculates the average labor cost for the treatment group taking into account the tax cut. Panel D calculates the implied employment elasticity with respect to the wage change by taking the ratio of the percent change in employment (Panel B) and labor cost (Panel C). The sample is restricted to women. Standard errors are reported in brackets, clustered at the age × period level. ($N = 9,529,124$ individual-months)

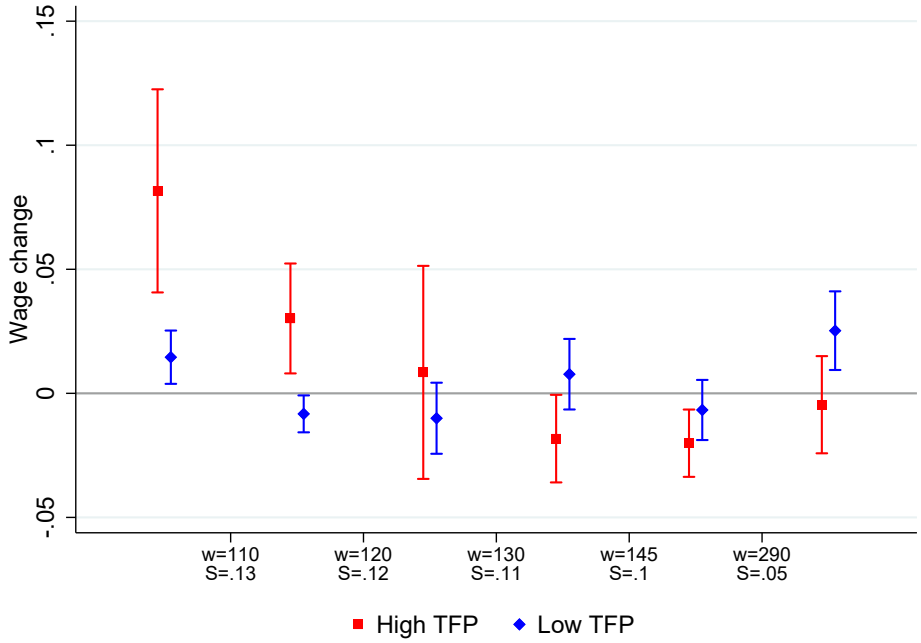
Appendix Table C2: Employment Effects of the Tax Cut by Various Subgroups for Men and Women

	(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]
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%}	{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%}
Panel B: By occupation						
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%}
Panel C: By education						
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%}

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms, as well as the implied employment elasticity with respect to the wage for all firms (column 4) and separately for below-median (column 5) and above-median (column 6) TFP firms. Panel A considers employment in jobs paying above vs below 150% of the minimum wage. Panel B considers employment in occupations with above- vs below-median average wages in 2012. Panel C considers employment in occupations requiring different levels of education, defined as the modal level of education in the Labor Force Survey in 2012. We compare the change in employment among the over-55 age group that was affected by the payroll tax subsidy with the change in employment among the 52 to 54 age group that was not affected by the tax subsidy. Standard errors are reported in brackets, clustered at the age × period level. For reference, we show the share of individuals in each category in curly brackets and mean outcomes in May 2012 in angle brackets.

Appendix Figure C1: Wages of Women in Private Sector Companies: Heterogeneity by Subsidy Rate



Note: Figure shows difference-in-differences estimates of the payroll tax subsidy on private sector wages separately for above-median (in red) and below-median (in blue) TFP firms by lagged wage (w) and subsidy rate (S). We display the change in log wages among the over-55 age group that was affected by the payroll tax subsidy relative to the change in log wages among the 52 to 54 age group that was not affected by the tax subsidy. 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, with $S_{it-1} = 14,500/w_{it-1}$. The sample is restricted to women. 95% confidence intervals are reported with standard errors clustered at the age $\times$ period level.

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 younger workers 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 younger 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 older workers. We show the elasticity of employment in Table

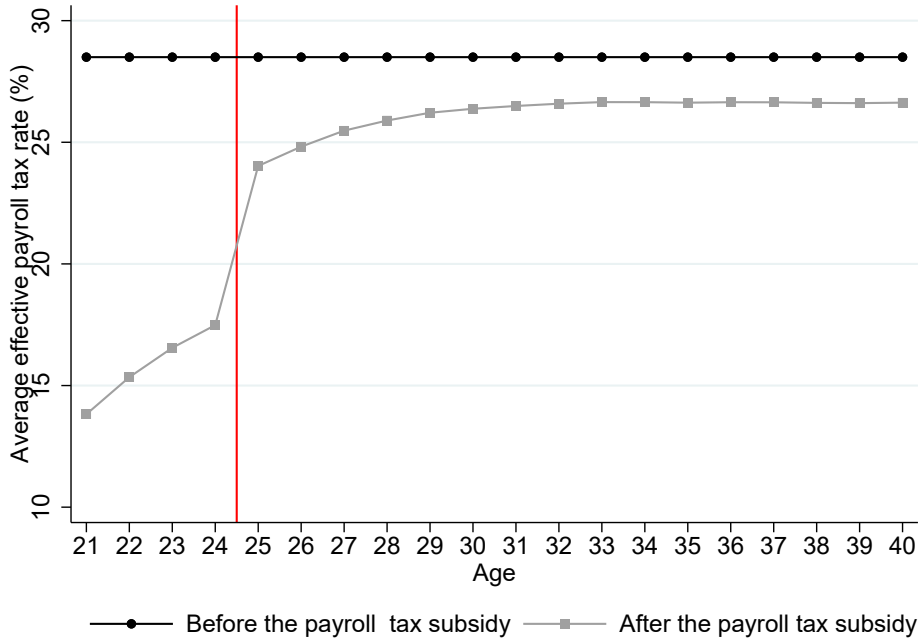
D1. The 1.6 percentage points (5.1%) increase in employment and the 6.6% decrease in labor costs for the 22-24 age group over years 2013-2015 imply a labor demand elasticity of -0.77. Overall, the employment effect is larger and labor demand is more elastic for younger workers.

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 younger workers 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 lower-TFP firms, whereas the impact on younger workers is positive both at low and higher-TFP companies (0.9 and 0.7 percentage points). The annual employment effects over 2012-2015 (Figure D4) are also similar at low and higher-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 younger 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 younger 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 net 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 younger 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 younger workers are new entrants or have limited employment histories, among whom the theoretical model predicts little or even negative wage effects of the subsidy.

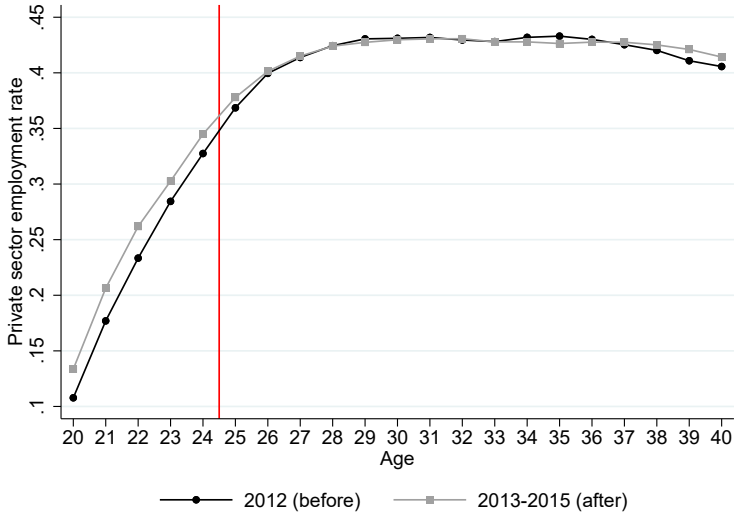
Appendix Figure D1: Average Payroll Tax Rate by Age (Age 20-40)



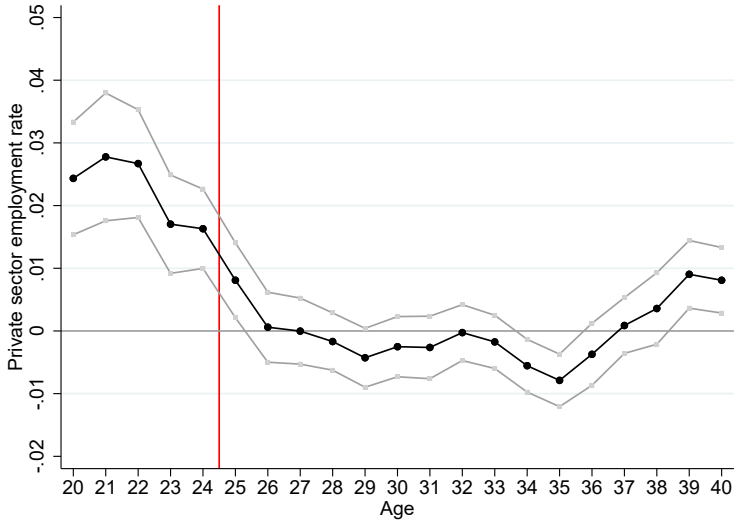
Note: Figure shows the average payroll tax rate by worker age for male workers. Before the implementation of the payroll tax subsidy, the payroll tax rate was a flat 28.5%. Between 2013-2015 (after the implementation of the subsidy), the payroll tax rate was 28.5% minus the subsidy. Using the observed net 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 HUF 14,500 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 HUF 28,500 per month for two years and 14.5% for a third year (then capped at HUF 14,500).

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

(a) Employment Rate

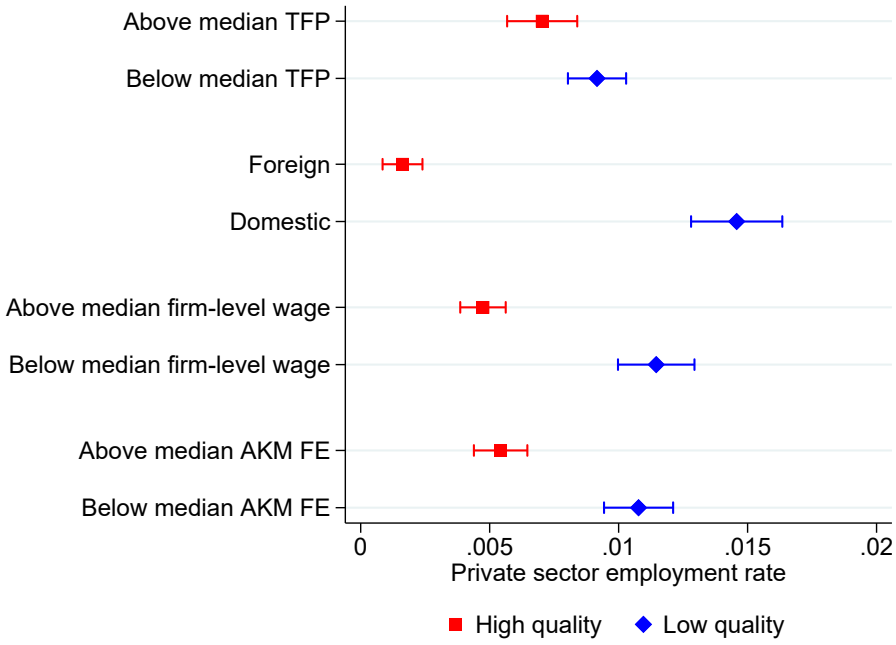


(b) Change in Employment Rate



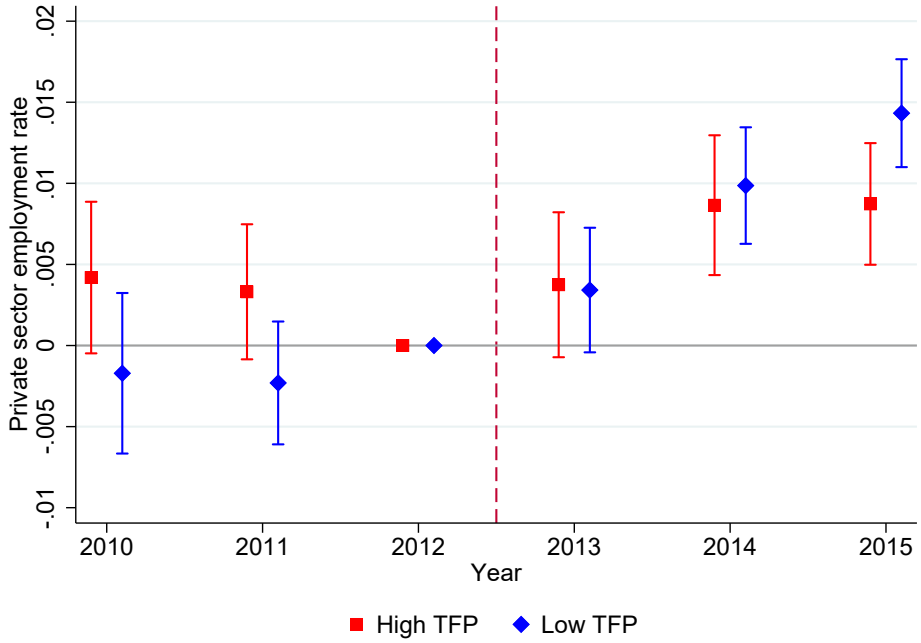
Note: Figure shows changes in private sector employment between the periods before and after the introduction of the age-specific payroll tax subsidy. Panel (a) shows the employment rate in private sector companies by age separately for year 2012 in black (before the implementation of the payroll tax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

Appendix Figure D3: Employment in Private Sector Companies (Age 22-27): Alternative Firm Quality Measures



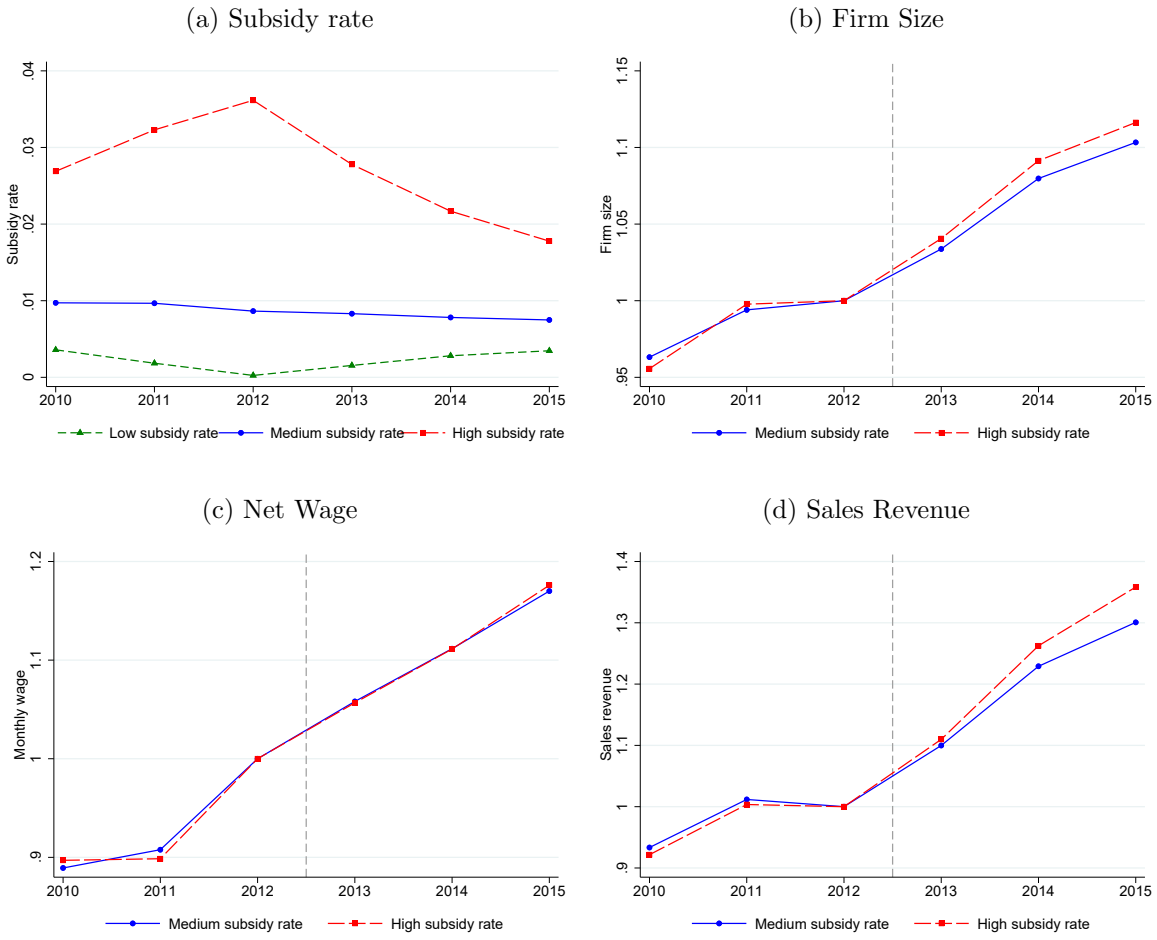
Note: Figure shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) by alternative measures of firm quality. We compare the change in employment among the under-24 age group that was affected by the payroll tax subsidy with the change in employment among the 25 to 27 age group that was not affected by the tax subsidy. The figure shows employment estimates separately for above-median (in red) and below-median (in blue) quality firms. (In the case of foreign ownership a binary indicator of foreign ownership being above 50% is used.) The first estimate replicates our baseline results and the subsequent estimates change the measure of firm quality used. The sample is restricted to men. 95% confidence intervals are reported with standard errors clustered at the age × period level.($N = 8,611,542$ individual-months)

Appendix Figure D4: Employment in Private Sector Companies by Year: Heterogeneity by TFP (Age 22-27)



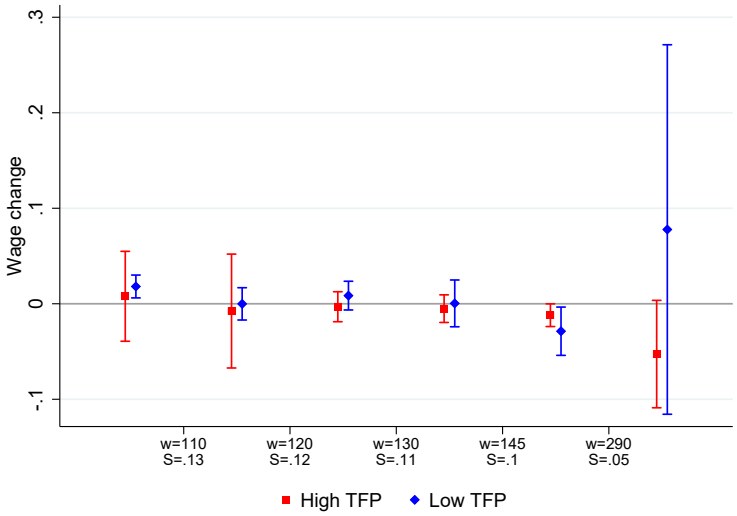
Note: Figure shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (10) separately for above-median (in red) and below-median (in blue) TFP firms. We compare the change in employment among the under-24 age group that was affected by the payroll tax subsidy with the change in employment among the 25 to 27 age group that was not affected by the tax subsidy. The sample is restricted to men. 95% confidence intervals are reported with standard errors clustered at the age × period level.

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



Note: Figure replicates the basic results of Saez, Schoefer and Seim (2019). Using 2012 data, we calculate the firm-level windfall defined as the total subsidy payable based on workers aged up to 24 at the firm relative to the total payroll of the firm. We calculate the quartiles of the subsidy rate, excluding firms with zero subsidy rate, and group firms into four categories. "Low subsidy rate" firms have either zero subsidy rate or belong to the bottom quartile. "Medium subsidy rate" firms belong to the middle two quartiles. "High subsidy rate" firms belong to the the top quartile. We compare the outcomes of these groups, focusing on the medium and high subsidy rate groups. Panel (a) shows the average subsidy rate. Panel (b) shows firm size. Panel (c) shows average net wage. Panel (d) shows sales revenue.

Appendix Figure D6: Wages of Younger Workers in Private Sector Companies: Heterogeneity by Subsidy Rate



Note: Figure shows difference-in-differences estimates of the payroll tax subsidy on private sector wages separately for above-median (in red) and below-median (in blue) TFP firms by lagged wage (w) and subsidy rate (S). We display the change in log wages among the under-24 age group that was affected by the payroll tax subsidy relative to the change in log wages among the 25 to 27 age group that was not affected by the tax subsidy. 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, with $S_{it-1} = 14,500/w_{it-1}$. The sample is restricted to men. 95% confidence intervals are reported with standard errors clustered at the age $\times$ period level.

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

	(1) All firms	(2) Low TFP	(3) High TFP
Panel A: Change in the probability of employment			
— $After \times Treated$	0.0162***	0.0092***	0.0070***
	[0.0011]	[0.0006]	[0.0007]
Panel B: Percent change in employment			
—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%
Panel C: Percent change in 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%
Panel D: Implied elasticity (Panel B/Panel C)			
— Elasticity	-0.77	-0.92	-0.67
	[0.05]	[0.06]	[0.07]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms in Panel A. We compare the change in employment among the under-24 age group that was affected by the payroll tax subsidy with the change in employment among the 25 to 27 age group that was not affected by the tax subsidy. 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 treatment and control age groups in 2012, the year before the introduction of the payroll tax subsidy. The second row adds to that baseline the estimated change from Panel A. The third row shows the percent change in employment relative to the baseline. Panel C calculates the percent change in labor cost using an analogous difference-in-differences estimation for tax rates. The first row shows the average unsubsidized labor cost in the control group between 2013-2015. The second row calculates the average labor cost for the treatment group taking into account the tax cut. Panel D calculates the implied employment elasticity with respect to the wage change by taking the ratio of the percent change in employment (Panel B) and labor cost (Panel C). The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level.

Appendix Table D2: Impact on Employment by Experience

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

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows difference-in-differences estimates of the impact of the payroll tax subsidy on private sector employment based on estimating equation (9) for all firms (column 1) and separately for below-median (column 2) and above-median (column 3) TFP firms. We compare the change in employment among the under-24 age group that was affected by the payroll tax subsidy with the change in employment among the 25 to 27 age group that was not affected by the tax subsidy. The sample is further split by working at least 6 months at ages 18-19 ("experienced" versus "non-experienced"). The sample is restricted to men. Standard errors are reported in brackets, clustered at the age × period level. For reference, we show the mean outcomes in May 2012 in angle brackets. Total number of observations, young: 8,611,542, experienced young: 707,259, non-experienced young: 8,004,351.