Firm Heterogeneity and the Impact of Payroll Taxes*

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Abstract

We study the impact of a large payroll tax cut for both younger and older workers in Hungary. Motivated by the prediction of standard equilibrium job search models we also study the heterogeneous impact of the policy by firm quality. We find that employment increases in response to the payroll tax cut, but the effects differ by firm characteristics. Employment increases most at low-productivity firms, which tend to hire from unemployment, while the effects are more muted for high-productivity firms, especially for older workers. At the same time, we find a significant increase in wages for older workers at high-productivity firms. These results point to important heterogeneity in the incidence of payroll tax subsidies by firm type and highlight that payroll taxes can change the composition of jobs in the labor market.

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1 Introduction

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

According to the neoclassical view of labor markets, the impact of tax cuts is driven by market-level labor supply and demand (see e.g. Rothstein, 2010). Firms are price takers and so the incidence of the policy is the same across firms. Nevertheless, recent empirical evidence highlight that wage premium differences across firms are important for understanding key phenomenon on the labor market such as rising wage inequality (Card, Heining and Kline, 2013; Song et al., 2019). In this paper, we explore how the presence of firm heterogeneity and wage setting power alter the impact of payroll taxes on employment and wages.

We start our analysis by introducing payroll tax subsidy into a standard search and matching model with heterogeneous firms (e.g. Bagger and Lentz, 2019; Mortensen and Pissarides, 2003). We obtain a number of novel and intuitive results. First, the impact of payroll taxes will depend on whether a worker is hired from unemployment or poached from another firm. Since the unemployed workers' outside option is unaffected by the the payroll tax cut, firms are able to claim the surplus when they are hiring from unemployment. At the same time, when a worker comes from employment, both firms benefit from the tax cut and a competition between firms will drive up the worker's wage. The model, therefore, predicts that the incidence of the payroll tax cut will be mainly on firms if the worker is hired from unemployment, and it will be on workers if the worker is poached.

Second, since more productive firms poach larger fraction of their workers the impact of the policy will be heterogeneous across firms. Low-productive firms, which tend to hire from unemployment, will benefit disproportionately more from the payroll tax cut than high-productive firms, which tend to poach from other firms. Therefore the model predicts that the share of jobs at low-productive firms will increase in response to the policy, while wages will increase at high-productive firms.

Third, the search model also highlights an important difference in how the policy impacts the younger and older workers. For young workers who are just entering the labor market and tend to move in and out of the labor force, the model predicts that the employment responses will dominate and the incidence will be mainly on firms. Nevertheless, for older workers, who are already higher up the job ladder and have more stable jobs, the subsidy will affect both hiring intensity and wages.

We illustrate the empirical relevance of these predictions by studying the impact of an age-specific payroll tax cut among younger (below 25) and older (above 55) workers in Hungary. We focus on a 2013 tax reform which decreased the social security contribution rate from 28.5% to 14% for all private sector employees who were younger than age 25 and older than age 54. This policy allows us to compare the impact of payroll tax cuts on the two age groups and also to study heterogeneity across different firms and jobs.

We utilize rich administrative data to estimate the impact of the policy in a difference-in-differences framework, comparing younger workers below the 25-year cutoff to workers just above and comparing older workers above the 55-year cutoff to workers just below. We find a large and significant increase in employment in response to the policy for both age group. The elasticity of employment with respect to the percent change in firm's labor cost is around 0.77 for the young and 0.40 for the elderly three years after the policy change.

We also find substantial heterogeneity across firm types for the elderly workers. For a variety of measures of firm quality—including the poaching index (Bagger and Lentz, 2019), total factor productivity, and firm fixed effects measured in the AKM framework (Abowd,

Kramarz and Margolis, 1999)—the employment-increasing effect of the policy mainly comes from lower-quality jobs and lower-quality firms, while the employment of older workers in higher-quality firms is unchanged. For most young workers, we find little evidence of heterogeneity as predicted by the model for workers entering to the labor market. Nevertheless, if we restrict the attention to young workers who are more attached to the labor force even at younger age, then we find a similar pattern as for the older: employment increases mainly at low quality firms.

We present several additional evidence to better understand the impact of payroll taxes. First, we vary the control group definition to make sure that our main estimates are not muted or exaggerated by potential spillovers to the control group. We find some clear, but qualitatively small indication that there are some spillover effects to workers close to the age cut off (age 26 for the young and age 54 for the old). Nevertheless, the main conclusions are unaffected by the choice of the control group.

Second, we show that firms that tend to hire more subsidized workers do not seem to decrease the hire of non-subsidized ones. This suggests that the employment creation at the targeted groups did not come at the expense of non-targeted workers.

Third, we document 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, while we find no indication for substantial change in employment at the upper part of the wage distribution where the change in labor cost was limited.[1] This evidence further corroborates that our estimates picked up the effect of the payroll tax cut and not something else. Furthermore, it is worth highlighting that the estimated change in employment is not concentrated at the minimum wage. Even two times above the minimum wage we find a significant increase in employment. In addition to that, we find no indication for differences in the impact of the policy in the tradable sector, where minimum wage seems to lead substantial job losses in Hungary (see Harasztosi

[1]The maximum amount of the tax cut was capped, which implies that at higher wages the percent change in labor cost was small.

and Lindner, 2019), and in the non-tradable sector, where minimum wage does not seem to impact employment. This latter suggests that the employment change does not simply reflect that some low quality jobs become viable in response to the payroll tax cut.

Fourth, we find that the employment increases mainly come from non-employment and inactivity. There is no change in self-employment or public sector employment – in the sectors where there was no payroll tax cut.

Finally, we also study the impact of the policy on wages. The model predicts that wages should increase for incumbent workers at high-productivity firms. For the elderly workers this is exactly what we find: there is a significant increase in wages for them at high-productive firms, but we find no change in wages for workers at low-productive firms.

These empirical findings together with our theoretical framework point to interesting (and so far undocumented) heterogeneity in the incidence of tax subsidies. Older workers employed by productive firms are able to extract more of the surplus from the subsidy and so the incidence of the subsidy (partly) falls on them. At the same time, younger, and 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.

Related Literature. This paper is closely related to studies of age-based employment subsidies (Kramarz and Philippon, 2001; Boockmann, Zwick, Ammermüller and Maier, 2012; Huttunen, Pirttilä and Uusitalo, 2013; Egebark and Kaunitz, 2018; Saez, Schoefer and Seim, 2019; Svraka, 2019). This literature is overall inconclusive on the impacts and effectiveness of these subsidies. We contribute to this literature in several ways: we analyze the differential impact of payroll tax cuts on workers in different age groups and focus on heterogeneity across firm and worker types. 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 age groups, as well as 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 of workers at high risk of early retirement, workers employed by firms with high shares of low-wage workers, and workers in small firms. Similarly, Laun (2017) report that an earned income tax credit and a payroll tax credit in Sweden for workers above age 65 increased participation mostly among low- and middle-income earners. The effect of the policy change was insignificant for workers in the top income quintile. Breda, Haywood and Wang (2019) study the impact of payroll tax subsidies on low-wage work in a search-and-matching framework using French administrative data and point to increased employment among low-skill workers but negative spillovers for high-skill workers.

Our study also relates to the literature on payroll tax incidence in general. Studies using payroll tax reforms to analyze incidence provide mixed evidence. Some studies find that the burden of the payroll tax is shifted on the workers (Gruber, 1997; Anderson and Meyer, 2000). However, some later studies find that the burden of the payroll tax is mostly borne by the employer (Benzarti and Harju, 2021; Kugler and Kugler, 2009; Saez, Matsaganis and Tsakloglou, 2012; Saez, Schoefer and Seim, 2019)[2]. Evaluating the incidence of business tax credits, Carbonnier, Malgouyres, Py and Urvoy (2021) find that the incidence of wage gains is on high-skill workers.

The remainder of this paper proceeds as follows. Section 2 introduces the search model with heterogenous firms. In Section 3, we provide background on the payroll tax reform we study. We describe our data in Section 4 and present our results in Section 5. Section 6 concludes.

2 Tax Subsidy in Search Models

We study the impact of payroll taxes through the lens of a standard search and matching model. We introduce tax subsidy in a framework with random search, heterogeneous firms

[2]Bozio, Breda and Grenet (2019) reconcile these seemingly conflicting results by the tax-benefit-linkage explanation.

and sequential bargaining on wages a la (Postel-Vinay and Robin, 2002*a*). We study how changing the tax subsidy affects employment, wages, and the composition of job types in equilibrium. In our framework, we abstract away from the age-specific nature of the tax cut, as we aim to highlight the heterogeneous nature of payroll tax cuts in general and not for the specific policy implemented in Hungary. We also abstract away from worker heterogeneity and assume job search is exogenous. These latter two assumptions are not necessary and can easily be relaxed.

2.1 Set-up

Firms are heterogeneous and characterized by productivity $y \in [0, 1]$, with cumulative distribution function $\Psi()$. The production function is $f()$, increasing in y.

Workers are homogeneous. Workers are either unemployed or employed. If unemployed, they receive b value of leisure 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 $\delta \in [0, 1]$.[3]

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

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

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 based on sequential auction a la Postel-Vinay and Robin (2002*b*). When an employed worker contacts an open vacancy, the prospective poacher and the incumbent

[3]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 advanced notice lay-offs 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).

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.[4]

2.2 Bellman equations

The value of unemployment when firms extract all the rents from unemployed workers 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 is not shown up in the above equation as a result of full rent extraction. Even if the unemployed gets a job offer, it will not make them better off. The maximum value the firm is willing to promise to deliver to the worker is:

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

where τ is the employment subsidy. Here, we use that if no outside offers arrive then the continuation value 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 $V(y,\tau)$ (Moscarini and Postel-Vinay, 2018).

Firms need to post vacancies to find workers. When firms can extract all the rents, the

[4]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.

value of posting ν vacancies will be the following:

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

where $\Gamma(y) = \dfrac{\int_0^y \nu(y',\tau)d\Psi(y')}{\int_0^1 \nu(y',\tau)d\Psi(y')}$ is the vacancy distribution function and $P(u) = \dfrac{u}{u+(1-\delta)s(1-u)}$ is the probability that a randomly drawn applicant is unemployed. The first part of the equation captures the cost of vacancy posting, $-\kappa(\nu(y,\tau))$. The second and the third part show the benefits of posting: more vacancies increases the chance of being matched to a worker, which is $\nu(y,\tau)\dfrac{\phi(\theta)}{\theta}$. The second part captures the benefits of matching to an unemployed. The third part shows the benefit of matched to an employed workers. Notice that the integral goes only to y as there is no benefit from applicants coming from better firms.

It is worth plugging in $V(y,\tau)$ (equation (3)) and V_u (equation (2)) into this expression, which leads to

$$V_v(y,\tau,\nu) = -\kappa(\nu(y,\tau)) + \beta\nu(y,\tau)\frac{\phi(\theta)}{\theta}P(u)\left[\frac{f(y)+\tau+\dfrac{\delta\beta b}{1-\beta}}{1-\beta+\delta\beta} - V_u\right] + \\ + \beta\nu(y,\tau)\frac{\phi(\theta)}{\theta}(1-P(u))\int_0^y \left[\frac{f(y)-f(y')}{1-\beta+\delta\beta}\right]d\Gamma(y'), \tag{5}$$

This equation highlights the key channels through payroll taxes affect vacancy posting and employment. Tax subsidies only appear in the second part of this equation, which reflect 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: the tax subsidy has no effect on hiring from employment as all firms receive the tax subsidy and the competition for workers will imply that the surplus will be enjoyed by the worker.

2.3 Equilibrium

Equilibrium is where firms optimally post vacancies up to the point where the marginal value of a posting vacancy equals to its cost – they maximize equation. Furthermore, market tightness, Θ, and the distribution of vacancies, $\Gamma(y)$, is consistent with firms choice of vacancies. 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 less productive firms can mainly fill their vacancies with unemployed as they cannot really poach the workers from more productive firms. In addition to that, the poaching index, the fraction of workers hired from other firms (instead of unemployment) will reveal firm quality (see Bagger and Lentz, 2019). Motivated by this we will present heterogeneity by the poaching index in the empirical section.

2.4 Effects of the employment subsidy

We study now the effect of changing the tax subsidy. We describe what happens to the new steady state equilibrium when we raise the subsidy amount. In Appendix Section A we also provide further details and proofs. In this section, we simply focus on the intuition.

First, we prove (see Lemma 2) that vacancy posting increases in response to the subsidy, which increases market tightness (θ) and lowers the equilibrium unemployment rate u. Second, we show that the effect of employment subsidy on the value of vacancy decreases with firm productivity (see Proposition 1). This latter is a simple consequence of equation (16) where we discussed that firms can only get the surplus from the tax subsidy if they hire from unemployment.[5]

[5]Proposition 1 shows the direct effect of the tax subsidy on vacancy positing without taking into account the equilibrium effects of changes in unemployment rate and market tightness. The equilibrium effects dampen some of the direct effects on vacancy posting. Nevertheless, those effects will be small. We show

Third, the effect of the employment subsidy on wages increases with firm productivity (see Proposition 2). This is a consequence of the sequential bargaining over wages. If a worker receives an outside offer from another firm, she can use the firm's productivity with the tax subsidy, $f(y) + \tau$ as threat point in bargaining. Since all firms receive the tax subsidy, the competition between firms will drive up wages and the tax subsidy will be built into wages for workers getting an outside offer from another firm.

Furthermore, workers at low productive firms are exactly those one who are hired from unemployment and so far did not get any outside offer. At the same time workers at more productive firms are those who were poached from other firms, or might have received a job offer from less productive ones. This implies that tax subsidy will mainly affect wages at the more productive firms.

Finally, we also show that the employment subsidy does not have wage effects on new entrants (see Proposition 3). This is again a simple consequence of the fact that new entrants are without any outside offer from another firm and so all the rents (including the tax subsidy) will be extracted by the firms.

3 Background

Motivated by these theoretical insights, we study the impact of a radical payroll tax cut implemented in Hungary. In 2013, the government halved social security contributions paid by private sector employers from 28.5% to 14% for private sector employees who were younger than 25 years old and older than 55 years old.[6]. The cut applied to both new and ongoing jobs and it was available for private sector employers. Public sector workers and self- employed were not eligible for the cut.

this by solving the model by applying the functional form and parameter values usually applied in the literature

[6]The amount of the allowance was 14.5% of the gross wage but the base was capped at HUF 100,000 (453 USD on January 1, 2013), so the allowance was capped at 14,500 HUF (63 USD) per month. The cap of HUF 100,000 corresponded to the minimum wage at the time (though the level of the allowance did not follow the slight increases in the minimum wage in later years). For context, the average monthly gross wage was HUF 230,000 ($1042) and the average net monthly wage was HUF 151,000 ($684) in 2013.

Besides the young and the old workers there were certain workers who were eligible for the tax cut independently of their age. Workers in elementary occupations experienced the tax cut even if they were aged between 25 and 55. Furthermore, long-term unemployed re-entering the labor market; people returning to work after child-care leave; and career starters were also eligible for the tax cut for three years. In the first two years upon reemployment faced with a 100% tax cut, while in the third year the tax cut was 50%. In our primary analysis we include all these workers in our sample, but our results are robust to the exclusion of the non-age-specific subsidized groups from the sample.

The tax cut was first publicly discussed in Parliament on July 2, 2012 and the corresponding accepted law was officially announced on October 15, 2012. The tax change was effective from January 1, 2013. Due to the relatively short period of time between the discussion 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.

We will study the impact of the reform between 2010 and 2015. Throughout this period there was no other major labor market policy changes that affected the young or older workers.[7] Around this period the overall employment rate in Hungary was 64%, slightly below the OECD average (66%). The employment rate of older people was only 46%, substantially below the OECD average (58%), which suggests that the labor market functioned particularly poorly for older workers. In contrast, the NEET (neither in education nor employment or training) rate of youth was virtually identical with the OECD average of 16.5% (OECD, 2016).

Besides the social security taxes paid by the employers, employees pay a flat-rate tax of

[7] A new pension policy for women introduced in 2011, which grants an early retirement option for women with 40 years eligibility period, regardless of age. To make sure our results are not driven by this policy change we focuse on men in the main analysis. Nevertheless, we find a very similar result for women. Furthermore, in 2015, the government introduced the Youth Guarantee Programme, which was targeted workers younger than age 25. 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 Programme does not affect our results.

16% in 2013, and a social security contributions paid by the employee (18.5% in 2013). As a result, labor income is taxed heavily in Hungary – the average tax wedge was 49% in 2019, much higher than the OECD average of 36.5% (OECD, 2019).

4 Data and Sample

We use linked employer-employee administrative data from Hungary, covering years 2010–2015 on a random 50% sample.[8] For employment, we use monthly data, while for wages, we use data from a representative month (May) of each year. We restrict the sample to males because for females there was a pension rule change affecting only women throughout the period studied here. Nevertheless, we find very similar estimate for women than for men.

An individual is defined to be a private sector employee if the pension authority records employment on the 15th of a month at a private sector firm. We exclude those employees who work fewer than 20 hours per week. We also exclude the very large firms from the analysis (with firm size above 10,000). We observe gross wage, which includes all income that enter the pension benefit calculations.

We generate a set of firm-specific indicators that we use in the heterogeneity analyses. We follow Bagger and Lentz (2019) to calculate a poaching index. Using data from a representative month (May) of 2011-2012, for each firm we calculate the fraction of hires that are poached from other firms (i.e., where the newly hired worker was employed at another firm the year before). We impute missing values of the poaching index using other firm-specific indicators (average wage, logarithm of firm size, foreign ownership) and then extrapolate the poaching index to years 2013-2015. As further indicators of firm quality, we calculate firm-level average wage, total factor productivity (TFP) and also classify firms as foreign-owned if foreign ownership is above 50%. We also perform an Abowd, Kramarz, Margolis (AKM)

[8]The monthly labor force status and wage indicators originate from the Central Administration of National Pension Insurance. The demographic indicators originate from the National Health Insurance Fund Administration. The firm-specific indicators originate from the National Tax and Customs Administration of Hungary.

style decomposition of wages (Abowd, Kramarz and Margolis, 1999) and calculate firm and individual fixed effects.

In our main empirical analysis, we restrict the sample to males, use workers aged between 22-27 (with workers aged 22-24 comprising the treatment group and workers aged 25-27 serving as the control group) and workers between 52-57 (with workers aged 52-54 serving as the control group and workers aged 55-57 comprising the treatment group). Table 1 provides summary statistics on our data.

5 Results

5.1 Average payroll tax rate by age

Figure 1 depicts the average effective payroll tax rate paid by the employees by age before and after the payroll tax subsidy was implemented. It shows the discontinuities at ages 25 and 55 after the policy (in blue) compared to the constant rate of 28.5% before (in black). After the policy the average tax rate is lower than 28.5% (rate without subsidy) at all ages due to the fact that some workers could get the tax subsidies independently of age (e.g. those working in elementary occupations).

Furthermore, there is a jump from 17% to 23% from age 24 to 25 and a drop from 26% to 20% from age 54 to 55. (There are no discontinuities in the fraction of the other beneficiary groups at age 25 and 55, see Figure B1.) The higher than 14% tax rates among the treated are due to the cap at 14,500 HUF per month. The lower than 14% tax rates among the treated young below age 25 are the result of the larger tax cut of 27% for career starters in 2013 and 2014. The strong increase in average payroll tax rates by age among workers under age 25 is driven by the steady decline of career starters as worker age increases. Overall, this figure indicates that young and old workers are subsidized according to the aims of the age-dependent tax policy with a larger average effective tax cut for workers below age 25 than for workers above 55 between 2013-2015.

5.2 Effect on Employment

5.2.1 Descriptive Evidence

Figure 2 shows employment in private sector companies for males 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. 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. Pre-reform, they dropped quite quickly starting at age 53. (The low employment of older workers was one of the rationales for the policy.) 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 some divergence below 26 and above 55.

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 between 25 and 55 changes in employment rates were close to zero. This panel also suggests that below the age 25 and above the age 55 cutoffs, age-specific employment levels strongly diverge between the pre-reform and the post-reform periods. Among younger workers, 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. Similarly, among older workers, a 55-year-old worker was 1 percentage points more likely to be employed shortly after the policy was introduced. The difference widens with age. Overall, this figure suggests that the payroll tax cut had a positive employment effect among both younger and older workers, concentrated in the specific age ranges targeted by the reform.[9]

[9]Appendix Figure C1 shows employment rates by year and age. The figure shows that at ages 21-24, compared to 2012, the employment gap increased over years 2013-2015 and widened with age. Similarly, at ages 55-57, compared to 2012, the employment difference increased over years 2013-2015 and widened with age. Panel (b) suggests the presence of spillovers to control individuals in case of the young target group for years when control individuals have been in the treatment group previously (e.g. a 25-year-old in 2014 was treated in 2013). In other words, the employment effects seem to be persistent.

5.2.2 Baseline Results

To study the impact of the payroll tax subsidy in a difference-in-differences framework, we focus on two groups of treated workers. The first is younger workers under 25. We use workers aged 22-24 as our treated group and workers aged 25-27 as our control group. The second is older workers above 55. We use workers aged 55-57 as our treated group and workers aged 52-54 as our control group. Table 1 shows summary statistics on the two treatment and control groups. It suggests that they are relatively comparable, with the only major differences arising between treated and control individuals for private sector employment and wages among younger workers. Our empirical strategy controls for level differences in outcomes between the treatment and control groups. Relative to the before-after comparison above, this strategy has the advantage of allowing us to assess trends over time.

We estimate the equation

$$y_{it} = \alpha_a + \beta_q + \sum_q \delta_q Treated_{it} + \varepsilon_{it} \tag{7}$$

where y_{it} is the indicator of private sector employment of individual i in month t. The a age index runs from 22 to 27 for the younger workers and from 52 to 57 for the older workers. The quarterly date q index runs between $2010 - 2015$. The binary indicator $Treated$ is one for ages under 25 (young treated) or for ages at and above 55 (old treated). The δ_q terms are quarter-specific dummies. In all regression models, we cluster the standard errors at the individual level.

We also estimate a pooled version of equation (7) where we replace the quarter-specific δ_t terms with a binary indicator $After$ which equals zero for year 2012 and one for years $2013 - 2015$, i.e. after the tax cut:

$$y_{it} = \alpha_a + \beta_q + \delta After_t Treated_{it} + \varepsilon_{it}. \tag{8}$$

Our coefficients of interest are the δ_q and δ terms in the two equations, respectively.

The identifying assumption is that besides age and quarterly date effects, the employment rate in the treated and control groups would have evolved similarly without the policy. Consequently, the remaining differences between treated and control workers in employment are only due to the policy.

Figure 3 shows the quarter-specific effect of the tax cut on employment in private sector companies. This figure suggests that prior to the introduction of the policy, the employment rates of treated and control workers evolved similarly, suggesting that the control workers are likely good counterfactuals for the treatment workers. The impact on employment increased gradually over 2013-2015 from 1 percentage point to 2.5 percentage points among younger workers and from 0.5 percentage point to 1 percentage point among older workers. This gradual increase is consistent with other studies in the literature (see e.g. (Saez, Schoefer and Seim, 2019) and could be because employer-employee matches may take time to form even if firms are incentivized to hire more workers from the subsidized age group. Since the outcome is private-sector employment in a given month, it also reflects the cumulative effect of increased hiring on the rate of employment. Table 4 decomposes our main employment effect into employment where the person was not employed a year ago (new entrant) and employment where the person was already employed a year ago (incumbent). Thus, the new entrant and incumbent effects add up to our main effect. The table show that about two-third of the employment effect stems from new entrants among the young and about one-third of the employment effect stems from new entrants among the old. Table 5 further shows that as employment at private sector firms increased due to the policy, the stock of inactive/unemployed people decreased by about the same magnitude, suggesting that the employment effect comes from new hiring.

The first part of Table 2 shows the baseline difference-in-differences results, corresponding to equation (8). The effective tax cut of an average 8.3 percentage points (young) or 6.2 percentage points (old) is estimated to increase the private sector employment of people aged 22-24 by 1.69 percentage points (5.19%) and of people aged 55-57 by 0.66 percentage

points (1.95%) over the years 2013-2015. The implied labor demand elasticity is 0.77 (s.e. 0.096) for the young (young employment increases by 5.19%, labor cost decreases by 6.7%) and 0.40 (SE 0.126) for the old (old employment increases by 1.95%, labor cost decreases by 4.9%). As summarized in Table 7, the short-run demand elasticity is 0.31 (s.e. 0.087) for the young and 0.25 (s.e. 0.108) for the old. The short-run elasticity estimates are about 1-1.5 times higher than the demand elasticities estimated by Saez, Schoefer and Seim (2019) (0.23), Laun (2017) (0.22) and Egebark and Kaunitz (2018) (0.3). Huttunen, Pirttilä and Uusitalo (2013) estimate even lower elasticities (0.07-0.13).

Robustness Analysis. We conduct robustness analysis by estimating the same equations as equation (8), but using ages as control group that are further from the cutoff age: 26-28, 27-29, ..., 30-32 among young workers and 47-49, 48-50, ..., 51-53 among older workers. The results are reported in panels (a) and (c) of Figure 4. Our baseline point estimates are shown as the first estimate in panel (a) and the last estimate in panel (c). We see similar or slightly higher results in the robustness estimations compared to them, suggesting that we might underestimate the true effects.

Spillover and Placebo Analyses. We conduct a sequence of additional analyses, where we assume that the cutoff age for treatment is different than the actual cutoff ages (28, 29, 30, 31, 32 or 33 for the young and 49, 50, 51, 52, or 53 for the old), but otherwise we run the same regressions as equation (8). In these regressions the treatment ages are defined as 25-27, 26-28, ..., 30-32 among younger workers (using ages 28-30, 29-31, ..., 33-35 as control) and 47-49, 48-50, ..., 52-54 among older workers (using ages 44-46, 45-47, ..., 49-51 as control). This is similar in spirit to the estimates of age-specific changes in Section 5.2.1. The results are reported in panels (b) and (d) of Figure 4. The results with this new treatment definitions are close to zero and mostly statistically insignificant. In fact, the only significant estimate is for the treatment group definition age 25-27 with ages 28-30 used as controls. This result likely reflects spillover effects of the policy that is apparent in years

where control individuals have been treated in earlier years. This is shown in Appendix Figure C1 and is in line with the findings of Saez, Schoefer and Seim (2021).

Substitution and windfall effects. A common concern about targeted tax cuts is whether firms substitute non-subsidized individuals with subsidized ones. To assess substitution effects we estimate the firm level relative growth in employment by age groups before and after the tax subsidy. Appendix Figure H1 shows the two-year relative change in employment for both the subsidized and non-subsidized ages prior to (between 2010 and 2012) and after the tax cut (between 2012 and 2014, with the subsidy enacted in 2013). We also add a counterfactual scenario of the period 2012-2014, where the relative change in employment shifts only for the subsidized age group (by the estimated average rate of increase) but remains the same as before the tax cut for the non-subsidized ages. The actual and counterfactual scenarios following the tax cut are closely aligned, which suggests that the substitution between the subsidized and non-subsidized age groups were limited.

In addition to that we also study whether windfall affects to firms tend to hire subsidized workers could grow faster as a result of the subsidy (**?**). Appendix Figure H2 indicates that the trends of firm size and average wage are similar for firms with high and low subsidy rates. This suggests that windfall effects must have been limited in our context.

5.2.3 Heterogeneity

In our heterogeneity analyses, we investigate if the treatment effect differs by firm and job characteristics. Here, we again use equation (8) but replace the outcome variable with a binary indicator of employment in a given type of job or firm.

In Table 2, we show how the impact of the payroll tax cut varies by firm characteristics. Overall, we see positive employment effects for most firm types and for both affected age groups, although some of the estimated effects are statistically and economically insignificant. However, we see major differences in the results between the two age groups. As a

consequence of the tax cut, the private sector employment of older workers increases significantly only at worse quality firms —firms with below median poaching index, domestic firms, firms with below-median TFP, and worse paying firms (based on the average wage). Among younger workers, the treatment effects are more balanced across firm types.

Next, in Table 3, we investigate whether the impact of the payroll tax cut on employment in private sector companies differs by job characteristics. As a results of the payroll tax cut, among younger workers, the employment probability increases both in blue collar jobs and — slightly less — in white collar jobs but among older workers, we see a significant increase only for blue collar jobs.

5.3 Effect on Wages

The theoretical model presented in Section 2 suggested that wages, especially at high productive firms, should increase in response to the policy. Nevertheless, studying the impact on wages is not straightforward in the presence of a large change in employment as a simple comparison of wages between treated and untreated workers could simple reflect a composition change.

Therefore, to assess the impact on wages we focus on workers who also worked during the previous year and so we exclude new entrants.[10] We also restrict the attention to the wage changes around the policy change (for the years in 2012 and 2013) to make sure that we only look at workers who had a job before the policy change. We estimate the following regression:

$$\ln w_{it} = \xi_a + \eta_t + \phi_a \ln w_{it-1} + \theta_t Treated_{it} + \nu_{it}, \tag{9}$$

where w_{it} is the monthly wage adjusted for working hours of individual i at time t (May of years 2012-2013). We include age dummies (ξ_a) and calendar year effects (η_t) in the model.

[10] Among younger private sector workers (age 22-27), 31% are new entrants. Among older private sector workers (age 52-57), 12% are new entrants.

We also include the age-specific effect of the one-year lag of wage ($\ln w_{it-1}$). The rationale for this specification is that lagged wage can capture the best available outside option of the worker in the baseline (beside worker's unobserved characteristics). The coefficient of interest is θ_t, which is the year-specific coefficient of treatment age ($Treated_{it}$).

The results of the wage regressions are reported in the first row of Table 6. On average, the payroll tax subsidy decreases wages at younger ages by 0.82% (significant at the 10% level), while at older ages the effect on wage is a statistically insignificant 0.42% increase.

In addition to estimating the average treatment effect on wages, we also allow the treatment effect to vary with indicators of firm quality. These results are reported in Table 6 starting from the second row. Heterogeneity results by the poaching index, TFP, firm fixed effects, and firm-level average wage show that among older workers, the positive wage effect is stronger at higher-quality firms. These results are in line with the predictions of search model. Among younger workers, the heterogeneity patterns are mixed, which might be due to the fact that a higher share of young workers are new entrants or have limited employment histories, leading to lower bargaining power. Appendix Table G3 in fact shows, that focusing on workers who entered to the labor market at age 18 leads to a similar results as for the old, albeit the estimates are rather noisy and statistically insignificant.

We report further wage regression results in Appendix F. The results shows the estimates when all workers (including new entrants) are in the sample. In that case, the estimated wage effects for the old are negative, which suggest that workers entering to the labor market (and also workers who more likely to keep their job in response to the policy) are negatively selected. This negative selection is consistent with the model prediction as workers entering from unemployment should have lower wages than incumbent workers.[11].

Once we control for lagged wages we see a clear and consistent pattern in the data: wages

[11]The model presented in Section 2 does not directly capture the change in incumbents. Nevertheless, one could easily explain the impact of the policy on incumbents by incorporating that some workers might get an advance notice before lay-off (see e.g. Bagger and Lentz, 2019). In that case, the tax subsidy makes sure that workers with advance notice finds job earlier and become less likely to unemployed. If the change in incumbents are driven by such advance notice, then we would expect that the incumbents will be negatively selected – their wages will be lower then the wages of the pre-policy incumbents

increase for old worker at high productive firms. This results we get even if was assume that the coefficient on the lagged variable is one (e.g. running a first difference version of equation (9)) or if do not vary the affect of the lagged dependent variable by age.

6 Conclusion

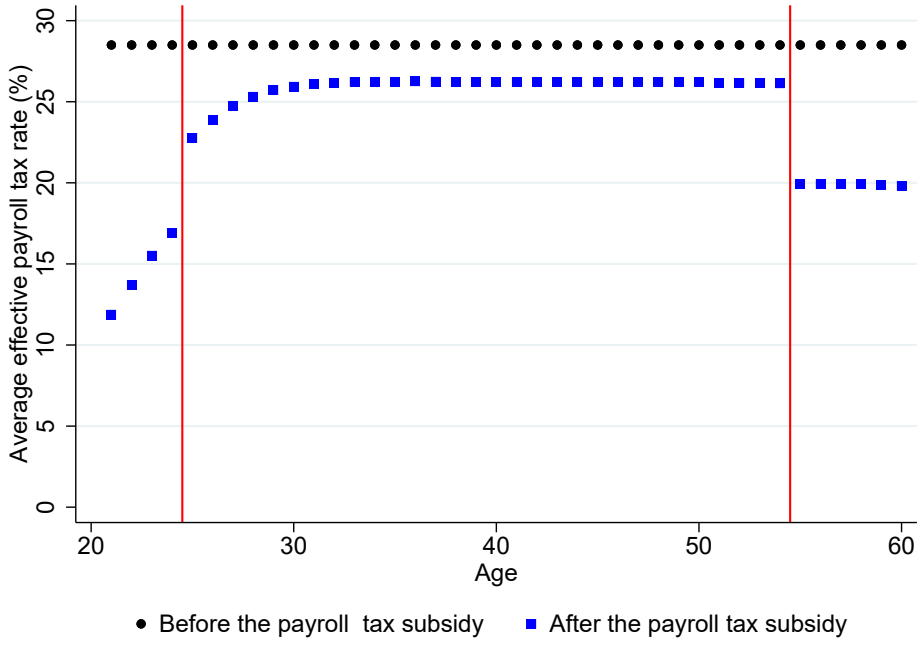
This paper provides theoretical and empirical evidence for heterogeneities in the impact of payroll tax subsidies on employment and wage by firm types. Based on an equilibrium search model we show that the effect of a payroll tax subsidy is positive on employment but this effect decreases with firm productivity. On the other hand, the positive effect on wages increases with firm productivity. The model also implies that the subsidy does not affect the wages of new entrants.

Based on 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 among both younger and older workers and small positive wage effects among older workers. However, there are substantial heterogeneities across firm types among older workers, but less so among younger workers. Among older workers, the positive effect of the payroll tax cut on employment is much at lower-quality firms, while the wage effect is stronger at higher-quality firms. A similar pattern emerged for the younger, but exprienced workers.

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.

Figures and Tables

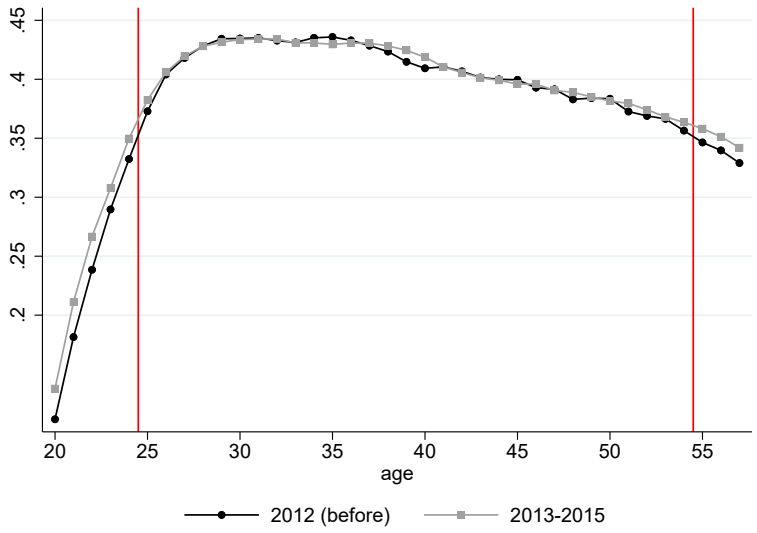
Figure 1: Average payroll tax rate (males)



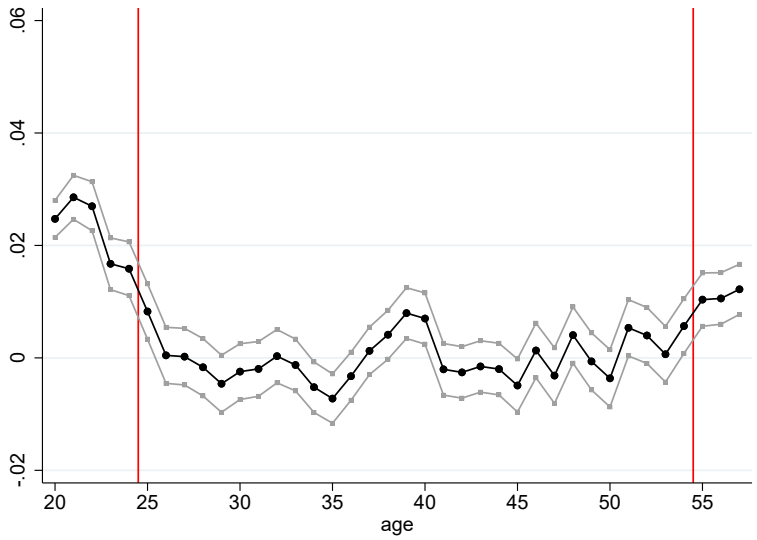
Notes: This figure shows the average payroll tax rate by worker age. Before the implementation of the payroll tax subsidy, the payroll tax rate was a flat 28.5%. Between 2013-2017 (after the implementation of the subsidy), the payroll tax rate was 28.5% minus the subsidy. Using the observed gross wages in years 2013-2015 and the prevalence of beneficiaries, we calculate the effective payroll tax rate. We consider the following beneficiary groups: ages below 25 and 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. Figure B1 shows the prevalence of the beneficiary groups by age. The age-specific subsidy and the subsidy of elementary occupations was 14.5% but capped at 14,500 HUF per month. The subsidy of career starters, long-term unemployed and people returning to work after a child-care leave was 27% but capped at 27,000 HUF per month. The capped 27% subsidy could be claimed only for two years.

Figure 2: Employment in private sector companies by age (males)

(a) Employment Rate



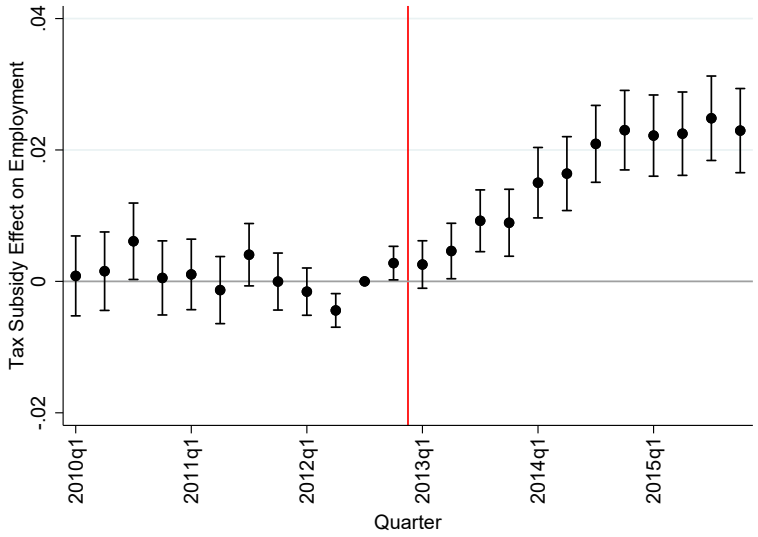
(b) Change in Employment Rate



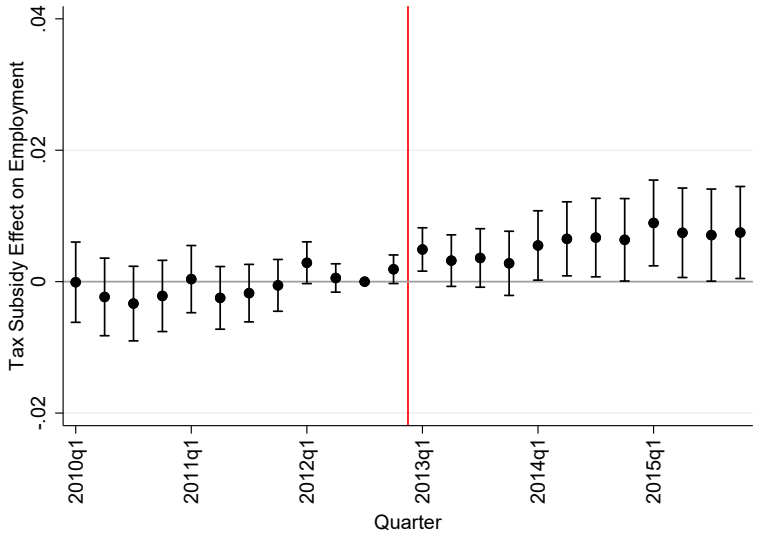
Note: Panel (a) shows the employment rate in private sector companies by age separately for year 2012 (before the implementation of the payroll tax subsidy) and for years 2013-2015 (after the implementation of the payroll tax subsidy). Panel (b) shows the differences between years 2013-2015 and 2012, net of age and quarterly date effects, with the 95% confidence interval. The vertical red lines shows the age thresholds where the tax subsidy became effective from 2013. The subsidy affected workers younger than 25 and older than 55.

Figure 3: Effect of the Payroll Tax Subsidy on Employment in Private Sector Companies (males)

(a) Young

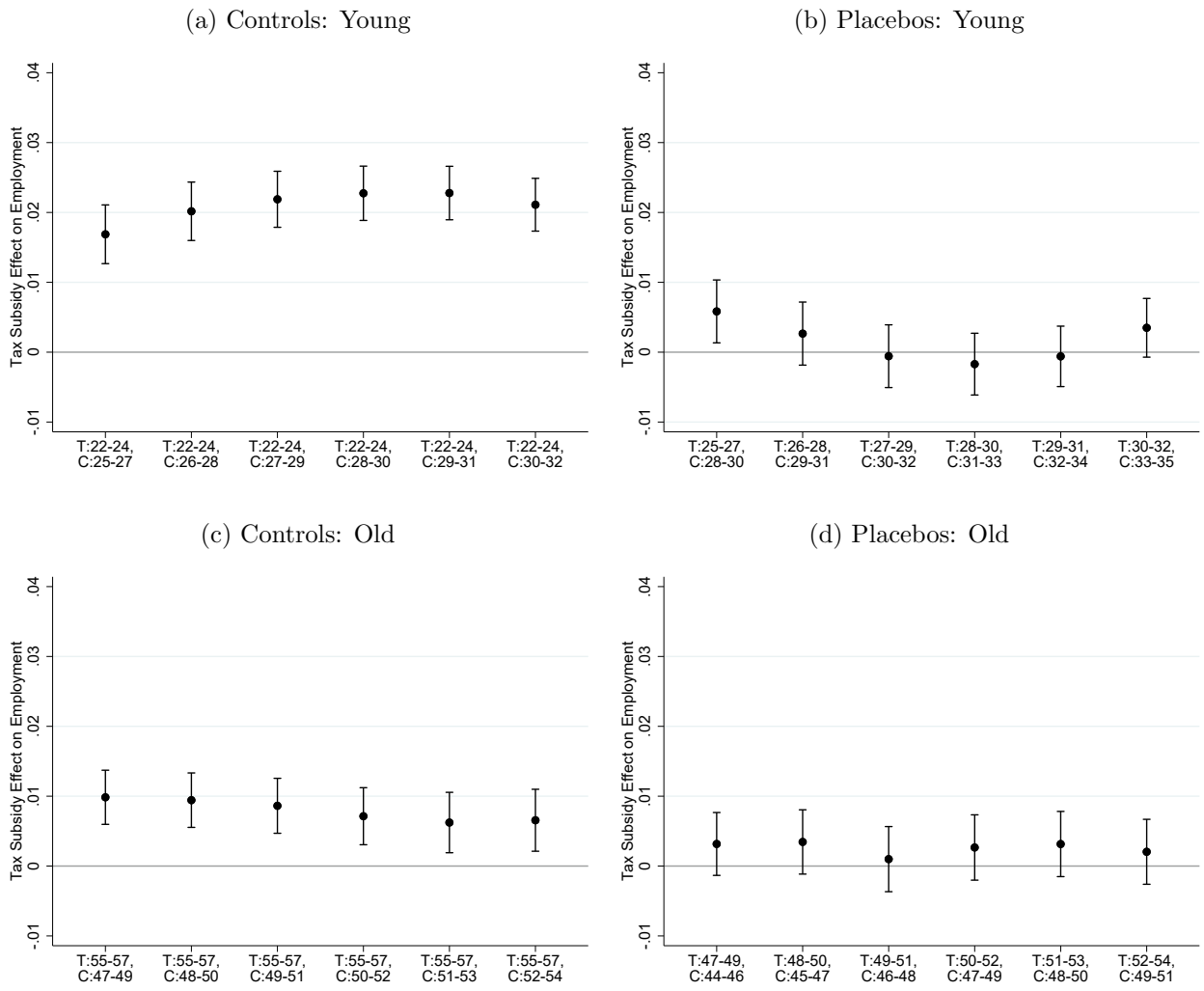


(b) Old



Notes: The figures show the change in employment for treated age groups (affected by the payroll tax subsidy) relative to the control age groups (similar age group, but unaffected by the tax subsidy) before and after the reform. In particular, we plot the δ_q from equation (7). Panel (a) shows the estimates for the young where the treated individuals are aged between 22 and 24, while the control individuals are aged 25 to 27. Panel (b) shows the estimates for the old, where the treated individuals are aged 55 to 57, while the control individuals are aged 52 to 54. The 95% confidence intervals are reported, where the standard errors are clustered at the worker-level.

Figure 4: Private sector employment: alternative control ages and placebo analyses (males)



Notes: The figures show estimated employment effects with the corresponding 95% confidence intervals, based on equation (8). These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period (after the introduction of the payroll tax subsidy in 2013). "T" denotes treatment group ages, "C" denotes control group ages. In panels (a) and (c), we keep the treatment group the same and vary the control group. In panels (b) and (d), we vary both the control and the treatment groups.

Table 1: Summary statistics (males)

	young		old	
	age 22-24 (treated)	age 25-27 (control)	age 52-54 (control)	age 55-57 age (treated)
Private sector employment	0.303	0.394	0.360	0.342
Monthly private sector wage (HUF)	158,665	195,471	228,165	224,586
Education level (1-primary to 3-tertiary)			1.826	1.786
Firm quality (private sector workers)				
Above median poaching index	0.476	0.523	0.548	0.545
Above median TFP	0.556	0.596	0.520	0.508
Above median firm FE	0.503	0.543	0.499	0.502
Above median firm level average wage	0.484	0.546	0.518	0.520
Foreign ownership	0.318	0.348	0.208	0.195
Industry (private sector workers)				
Agriculture	0.039	0.034	0.076	0.083
Manufacturing	0.380	0.361	0.329	0.329
Construction	0.081	0.076	0.106	0.106
Wholesale and retail trade	0.145	0.147	0.119	0.111
Accommodation and food service	0.056	0.042	0.017	0.016
Transportation and storage	0.054	0.063	0.113	0.109
Information and communication	0.011	0.017	0.012	0.010
Financial and insurance activities	0.186	0.208	0.121	0.125
Other	0.048	0.052	0.107	0.111
Total number of observations	4,370,704	4,385,834	4,965,949	4,516,718

Note: The table shows summary statistics for treated and and control group workers over years 2012-2015. Among younger workers, the treated group comprises ages 22-24 and the control group group comprises ages 25-27. Among older workers, the treated group comprises ages 55-57 and the control group comprises ages 52-54. The poaching index is calculated following Bagger and Lentz (2019). Using data from a representative month (May) of 2011-2012, for each firm we calculate the fraction of hires that are poached from other firms (i.e., where the newly hired worker was employed at another firm the year before). We impute missing values of the poaching index using other firm-specific indicators (average wage, logarithm of firm size, foreign ownership) and then extrapolate the poaching index to years 2013-2015. We calculate revenue-based total factor productivity (TFP) by regressing the log of net turnover on log wage bill and log material cost; the estimated TFP is the sum of the firm fixed effects and residuals. We calculate firm fixed effects from the Abowd-Kramarz-Margolis (AKM) model described in Abowd, Kramarz and Margolis (1999). We regress wages on individual and firm fixed effects, controlling for year fixed effects, using individuals who move between firms to identify the fixed effects. Foreign ownership refers to companies that are more than 50% owned by a foreign entity.

Table 2: Heterogeneity by firm characteristics: Impact on employment in private sector companies

	Employment of Young Age 22-27	Employment of Old Age 52-57
Employment at private firms	0.0169***	0.0066***
	[0.0021]	[0.0023]
Poaching index (PI)		
Below median PI, treatment effect	0.0056***	0.0055***
	[0.0018]	[0.0018]
Above median PI, treatment effect	0.0101***	0.0012
	[0.0017]	[0.0019]
TFP		
Below median TFP, treatment effect	0.0087***	0.0048***
	[0.0015]	[0.0017]
Above median TFP, treatment effect	0.0060***	-0.0004
	[0.0018]	[0.0018]
Firm FE (based on AKM decomposition)		
Below median FE, treatment effect	0.0112***	0.0046***
	[0.0016]	[0.0017]
Above median FE, treatment effect	0.0037**	0.0013
	[0.0017]	[0.0018]
Firm level average wage		
Below median, treatment effect	0.0103***	0.0054***
	[0.0016]	[0.0017]
Above median, treatment effect	0.0051***	0.0006
	[0.0017]	[0.0019]
Foreign ownership		
Domestic firm, treatment effect	0.0151***	0.0075***
	[0.0019]	[0.0021]
Foreign firm, treatment effect	0.0017	-0.0007
	[0.0015]	[0.0013]

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

Note: The table shows estimates from the model in equation (8). These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. Among younger workers, the treated group comprises ages 22-24 and the control group group comprises ages 25-27. Among older workers, the treated group comprises ages 55-57 and the control group comprises ages 52-54. Cluster robust standard errors in brackets. In each regression, the outcome is the binary indicator of private sector employment at a firm with the given characteristic. In each regression, we control for age and quarterly date effects.

Table 3: Heterogeneity by job characteristics: Impact on employment in private sector companies

	Employment of Young Age 22-27	Employment of Old Age 52-57
Skill		
Blue collar job, treatment effect	0.0094***	0.0063***
	[0.0019]	[0.0021]
White collar job, treatment effect	0.0061***	-0.0005
	[0.0014]	[0.0014]

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

Note: The table shows estimates from the model in equation (8). These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. Among younger workers, the treated group comprises ages 22-24 and the control group group comprises ages 25-27. Among older workers, the treated group comprises ages 55-57 and the control group comprises ages 52-54. Cluster robust standard errors in brackets. In each regression, we control for age and quarterly date effects.

Table 4: Employment Effects: New Entrants and Incumbents

	Young (age 22-27)	Old (age 52-57)
All	0.0169***	0.0066***
	[0.0021]	[0.0023]
New entrants	0.0110***	0.0019**
	[0.0011]	[0.0008]
Incumbents	0.0059***	0.0047**
	[0.0019]	[0.0022]

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

Note: The outcome is either employment with no private sector employment a year ago (new entrants) or employment with private sector employment a year ago (incumbents). The data is restricted to May of each year. These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. Among younger workers, the treated group comprises ages 22-24 and the control group group comprises ages 25-27. Among older workers, the treated group comprises ages 55-57 and the control group comprises ages 52-54. In each regression, we control for age and quarterly date effects. Cluster robust standard errors in brackets.

Table 5: Effect of the payroll tax cut by employment categories

	Young (age 22-27)	Old (age 52-57)
Employment at private firms	0.0169***	0.0066***
	[0.0021]	[0.0023]
Public sector	-0.0001	0.0011
	[0.0010]	[0.0012]
Self-employed	-0.0002	-0.0013
	[0.0007]	[0.0014]
Inactive/unemployed	-0.0152***	-0.056**
	[0.0022]	[0.0023]

Note: Cluster robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1. The table shows estimates from the model in equation (8). These are difference-in-differences estimates that compare the change in the employment in the given category between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. Among younger workers, the treated group comprises ages 22-24 and the control group group comprises ages 25-27. Among older workers, the treated group comprises ages 55-57 and the control group comprises ages 52-54.

Table 6: Average treatment effect on log wage by firm quality indicators

	Log Wage of Young Age 22-27	Log Wage of Old Age 52-57
Average treatment effect	-0.0082*	0.0042
	[0.0045]	[0.0026]
Poaching index (PI)		
Below median PI, treatment effect	-0.0126**	-0.0018
	[0.0052]	[0.0031]
Above median PI, treatment effect	-0.0039	0.0098***
	[0.0053]	[0.0030]
p-value of equality test	0.101	0.000
TFP		
Below median TFP, treatment effect	-0.0255***	-0.0057**
	[0.0049]	[0.0029]
Above median TFP, treatment effect	0.0082	0.0148**
	[0.0057]	[0.0034]
p-value of equality test	0.000	0.000
Firm FE (based on AKM decomposition)		
Below median FE, treatment effect	-0.0255***	-0.0107***
	[0.0046]	[0.0029]
Above median FE, treatment effect	0.0168***	0.0189***
	[0.0059]	[0.0035]
p-value of equality test	0.000	0.000
Firm level average wage		
Below median, treatment effect	-0.0171***	-0.0153***
	[0.0044]	[0.0028]
Above median, treatment effect	0.0110*	0.0239***
	[0.0059]	[0.0034]
p-value of equality test	0.000	0.000
Foreign ownership		
Domestic firm, treatment effect	-0.0182***	0.0004
	[0.0046]	[0.0026]
Foreign firm, treatment effect	0.0168**	0.0232***
	[0.0066]	[0.0049]
p-value of equality test	0.000	0.000

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

Note: The table shows estimates of the model in equation equation (9). We report difference-in-differences estimates of wages allowed to vary with firm quality indicators. The sample is restricted to years 2012 (before the introduction of the payroll tax subsidy) and 2013 (after the introduction of the payroll tax subsidy). Firm quality indicators are measured in 2013. Among younger workers, the treated group comprises ages 22-24 and the control group group comprises ages 25-27. Among older workers, the treated group comprises ages 55-57 and the control group comprises ages 52-54. Cluster robust standard errors in brackets. The equality of the coefficients is tested with a t-test.

Table 7: Elasticity Calculation

	Short run (2013)		Long run (2013-2015)	
	Young	Old	Young	Old
Labor cost $(1+\tau_{ss})$				
—Without subsidy	1.24	1.26	1.24	1.26
—With subsidy	1.15	1.19	1.15	1.19
—Percent change in labor cost	-6.73%	-4.92%	-6.73%	-4.92%
Employment rate				
—Without subsidy	0.326	0.339	0.326	0.339
—With subsidy	0.333	0.343	0.343	0.345
—Percent change in employment	2.09%	1.24%	5.19%	1.95%
Implied elasticity	0.31	0.25	0.77	0.40

Note: The long run elasticity estimations are based on our baseline specification that is reported in Table 2. An estimation using the same setup including only years 2012 and 2013 is used for the short run elasticity calculation.

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Appendix

A The Effect of Tax Subsidy in Search Models

A.1 Set-up

Firms are heterogeneous characterized by productivity $y \in [0,1]$, with cumulative distribution function $\Psi()$. A job offer is a draw of a firm productivity from the vacancy distribution $\Gamma()$ with probability distribution function $\gamma()$. The production function is $f()$, increasing in y.

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

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

$$\theta = \frac{v}{u + s(1-\delta)(1-u)}. \tag{10}$$

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*b*). 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.[12]

[12]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.

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:

$$V_u = b + \beta V_u, \tag{11}$$

where β is the discount factor. Thus,

$$V_u = \frac{b}{1-\beta}. \tag{12}$$

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

$$V(y,\tau) = f(y) + \tau + \delta\beta V_u + (1-\delta)\beta V(y,\tau), \tag{13}$$

where τ is the employment subsidy. Here, we use that if no outside offers arrive then the continuation value 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 $V(y,\tau)$ (Moscarini and Postel-Vinay, 2018).

After rearrangement:

$$(1-\beta+\delta\beta)V(y,\tau) = f(y) + \tau + \frac{\delta\beta b}{1-\beta}. \tag{14}$$

The value of posting ν vacancies is, using full rent extraction by the firm:

$$\begin{aligned} V_v(y,\tau,\nu) = -\kappa(\nu(y,\tau)) + \beta\nu(y,\tau)\frac{\phi(\theta)}{\theta}P(u)\Big[V(y,\tau) - V_u\Big] + \\ + \beta\nu(y,\tau)\frac{\phi(\theta)}{\theta}(1-P(u))\int_0^y \Big[V(y,\tau) - V(y',\tau)\Big]d\Gamma(y'). \end{aligned} \tag{15}$$

Using equation (14), equation (4) can be rewritten:

$$V_v(y,\tau,\nu) = -\kappa(\nu(y,\tau)) + \beta\nu(y,\tau)\frac{\phi(\theta)}{\theta}P(u)\left[\frac{f(y)+\tau+\frac{\delta\beta b}{1-\beta}}{1-\beta+\delta\beta} - V_u\right] + \\ + \beta\nu(y,\tau)\frac{\phi(\theta)}{\theta}(1-P(u))\int_0^y \left[\frac{f(y)-f(y')}{1-\beta+\delta\beta}\right]d\Gamma(y'), \tag{16}$$

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

$$P(u) = \frac{u}{u+(1-\delta)s(1-u)}. \tag{17}$$

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

$$\Gamma(y) = \frac{\int_0^y \nu(y',\tau)d\Psi(y')}{\int_0^1 \nu(y',\tau)d\Psi(y')}. \tag{18}$$

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

A.3 Equilibrium

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

$$L(y) = (1-\delta)\Big[1 - s\phi(\theta)(1-\Gamma(y))\Big]L(y) + \phi(\theta)\Gamma(y)u. \tag{19}$$

Employment at firms with productivity y is:

$$l(y) = (1-\delta)\left[\Big[1 - s\phi(\theta)(1-\Gamma(y))\Big]l(y) + s\phi(\theta)\gamma(y)\int_0^y l(y')dy'\right] + \phi(\theta)\gamma(y)u. \tag{20}$$

The steady state rate of unemployment is:

$$u = (1-\phi(\theta))u + \delta(1-u). \tag{21}$$

Thus,

$$u = \frac{\delta}{\delta + \phi(\theta)}. \tag{22}$$

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

$$\kappa'(\nu(y,\tau)) = \beta\frac{\phi(\theta)}{\theta}P(u)\left[\frac{f(y)+\tau+\dfrac{\delta\beta b}{1-\beta}}{1-\beta+\delta\beta} - V_u\right] + \\ + \beta\frac{\phi(\theta)}{\theta}(1-P(u))\int_0^y \left[\frac{f(y)-f(y')}{1-\beta+\delta\beta}\right] d\Gamma(y'). \tag{23}$$

The equilibrium solution of Θ and $\Gamma(y)$ satisfies equations (1), (17), (18), (19), (22) and (23).

A.4 Effects of the employment subsidy

Lemma 1 *Hiring intensity is increasing in firm productivity ($\nu_y(y,\tau) > 0$).*

Proof. As in Bagger and Lentz (2019), hiring intensity increases in firm productivity y because both the output $f(y)$ and the acceptance rate increase with y in the right hand side of equation (23). This proves the lemma, using that $\kappa()$ is increasing in ν. ■

A.5 Effects of the employment subsidy

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

Proof. The proof follows directly from equation (23), using that $\kappa()$ is increasing and convex in the amount of vacancies. ■

Lemma 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.

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

Proof. It follows directly from equation (22) that the impact of the tax subsidy on the rate of unemployment is negative, using that θ increases in τ and $\phi(\theta)$ increases in θ. Equation (17) can be rewritten as:

$$P(u) = \frac{\delta}{\delta + (1-\delta)\phi(\theta)}. \tag{24}$$

Using that $\phi(\theta)$ increases in θ, which in turn increases in τ, the proof immediately follows. ■

Proposition 1 *The direct effect of employment subsidy on the value of vacancy decreases with firm productivity.*

Proof. The proof follows from equation (23). The direct effect of the subsidy (τ) on the right hand side of the equation is the same for all firms. Based on the convexity of the vacancy cost function $\kappa()$ and using that $\nu(y,\tau)$ increases in y, it follows that the increase in vacancies ($\nu(y,\tau)$) is smaller at higher values of y. ■

Proposition 2 *The effect of the employment subsidy on wages increases with firm productivity.*

Proof. Contracts can be renegotiated by mutual consent. Assume that employment subsidy $\tau > 0$ is introduced. 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 $V(y',\tau) > V(y,\tau)$ 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 $V_e(y,w,0) < V(y',\tau) < V(y,\tau)$. Since $V(y,\tau)$ is increasing in τ, 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. The probability of poaching a worker (case 1) increases in firm productivity (and is zero at the firm with the lowest productivity), thus this channel of the wage-increasing effect of τ is more likely to be realized at higher-productivity firms. The probability of wage renegotiation (case 2) also increases in τ. However, the positive effect of τ on wages due to wage renegotiation is zero at the firm with the lowest productivity. Thus, overall, the employment subsidy is more likely to have a positive wage effect at more productive firms both due to the higher probability of poaching and to the higher probability of wage renegotiation. ■

Proposition 3 *The employment subsidy does not have wage effects on new entrants.*

Proof. Young workers enter the labor market as non-employed, thus, essentially, poaching and wage renegotiation (cases 1 and 2 under the proof of Proposition 2) 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. ■

A.6 Simulations

The functional forms used in the model are the following.

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

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

- The job-finding rate is similar to Moscarini and Postel-Vinay (2018): $\phi(\theta) = A\theta^{\alpha}$

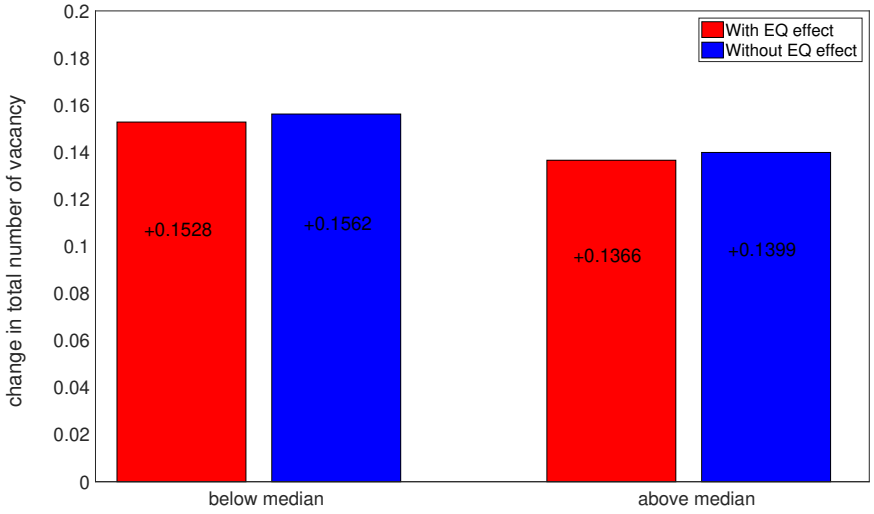
- The distribution of the firm's productivity is using the widely used Pareto distribution

$$f(y) \sim Pareto(\lambda)$$

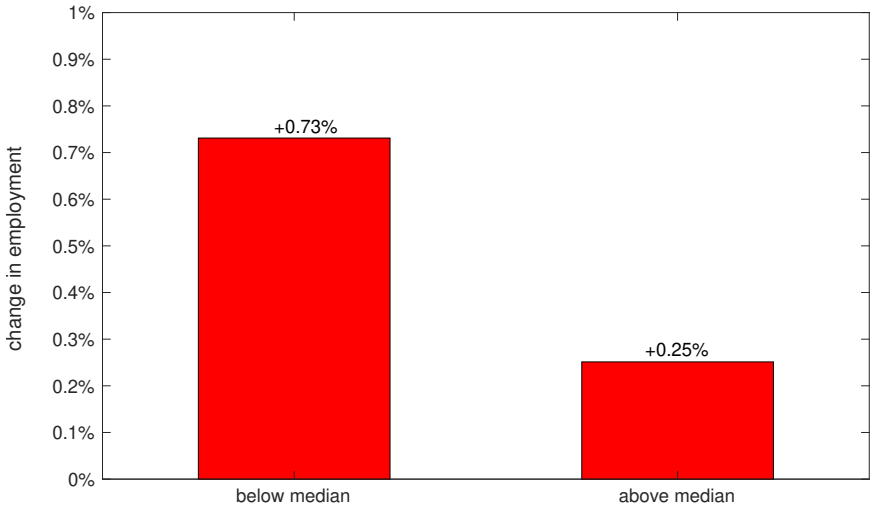
The parameters used during the simulations are the following.

- $\beta = 0.95^{1/12}$
- $A = 1/4$
- $\alpha = 1/3$ - as the 'classical' specification in Moscarini and Postel-Vinay (2018)
- $\delta = 0.024$ - as in Moscarini and Postel-Vinay (2018)
- $f(y)$ - $Pareto(\lambda)$, where $\lambda = 1.25$ - due to computational issues
- I scale $f(y)$ to have $E(f(y)) = 1$
- $b = 0$
- $c_v = 0.02$ - (in Bagger and Lentz, 2019: 0.006)
- $EE^{target} = 0.02$
- $\tau = E(f(y)) \times 0.15$

Appendix Figure A1: Simulation results: vacancies

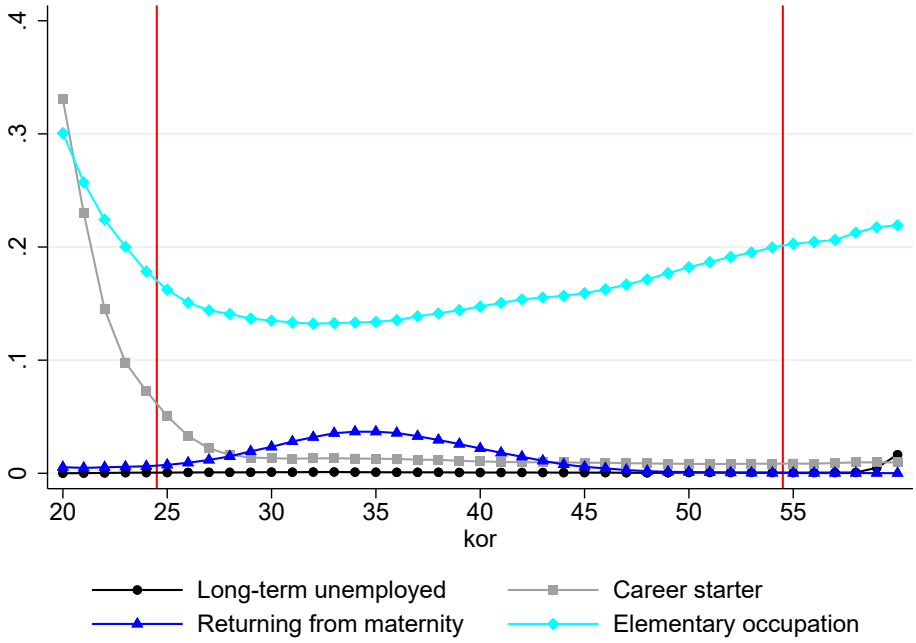


Appendix Figure A2: Simulation results: employment



B Not age-dependent beneficiary groups

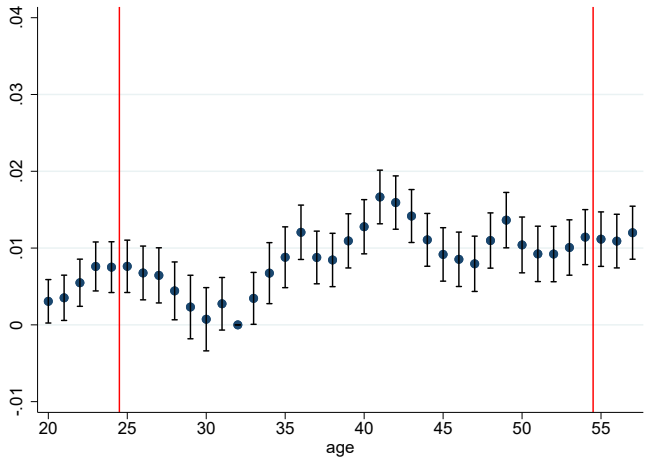
Appendix Figure B1: Fraction of not age-dependent beneficiary groups among private sector workers



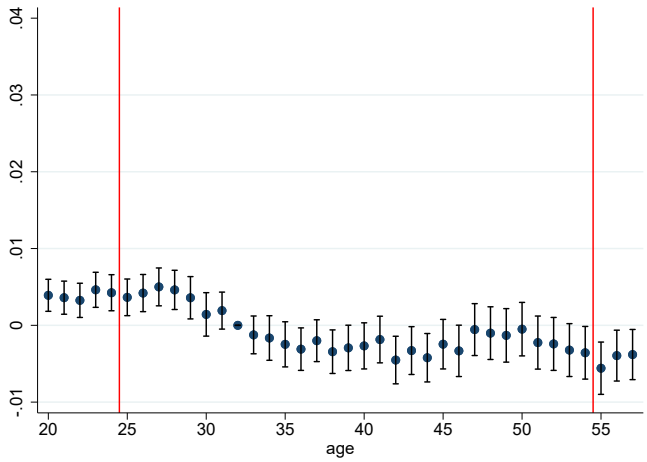
Note: The table shows the fraction of not age-dependent beneficiary groups among private sector workers over years 2013-2015 both for males and females. The long-term unemployed were registered unemployed for at least 6 months during the previous 9 months. Career starters had at most 180 days of prior employment. People returning from maternity are those who start an employment after receiving maternity payments. Elementary occupations correspond to ISCO code 9.

Appendix Figure B2: Change in employment rate in placebo groups

(a) Public Sector Employment Rate



(b) Self-Employment Rate

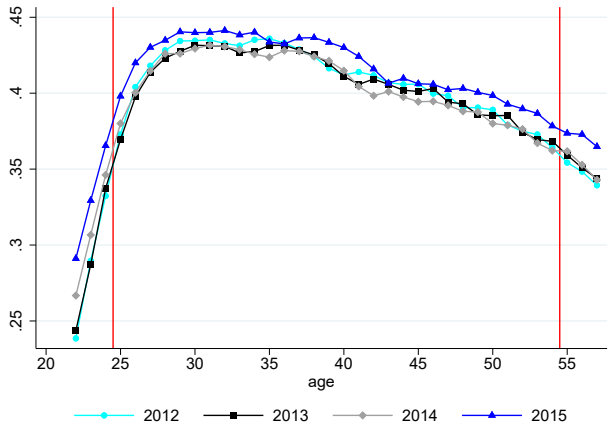


Note: Panel (a) refers to employment in the public sector where the payroll tax subsidy did not apply. Panel (b) refers to the self-employed who were not eligible for payroll tax subsidy. Both panels show the difference between years 2013-2015 and 2012, net of age (age 32 as reference) and quarterly date effects, with the 95% confidence interval. The vertical red lines shows the age thresholds where the tax subsidy became effective from 2013. The subsidy affected workers younger than 25 and older than 55.

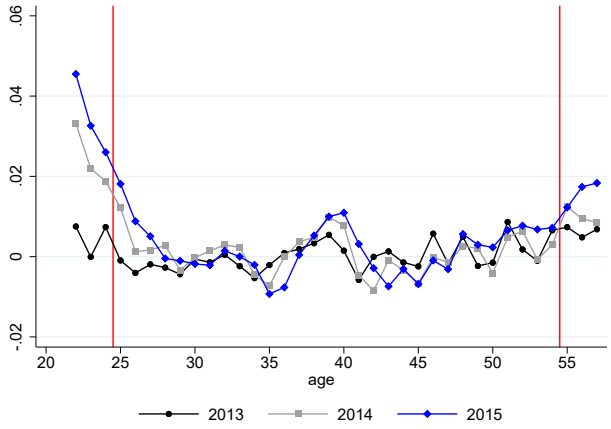
C Employment over years

Appendix Figure C1: Employment in private sector companies by age (males)

(a) Employment Rate



(b) Change in Employment Rate

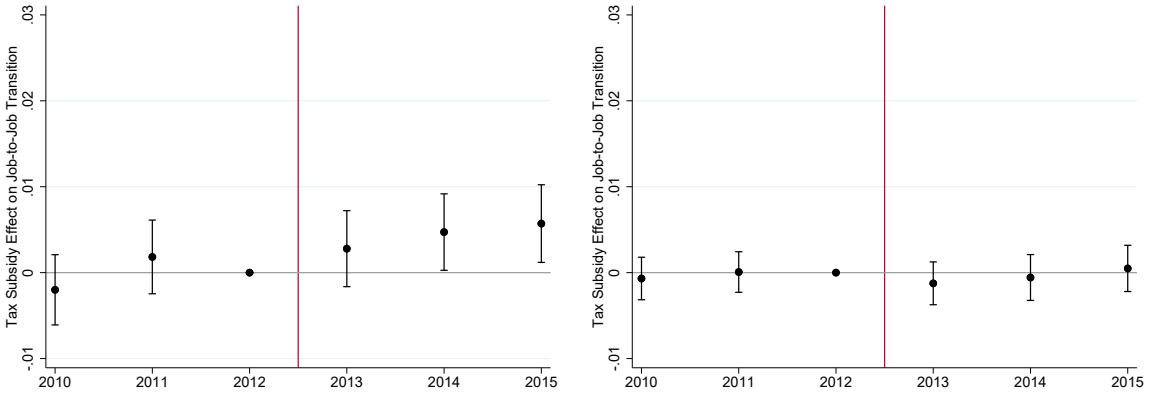


Note: Panel (a) shows the employment rate in private sector companies by age for years 2012-2015. Panel (b) shows the difference between years 2013-2015 and 2012, adjusted to mean zero at ages 28-51 (i.e., at ages that are neither in the treatment nor in the control groups in the difference-in-differences estimations). The vertical red lines shows the age thresholds where the tax subsidy became effective from 2013. The subsidy affected workers younger than 25 and older than 55.

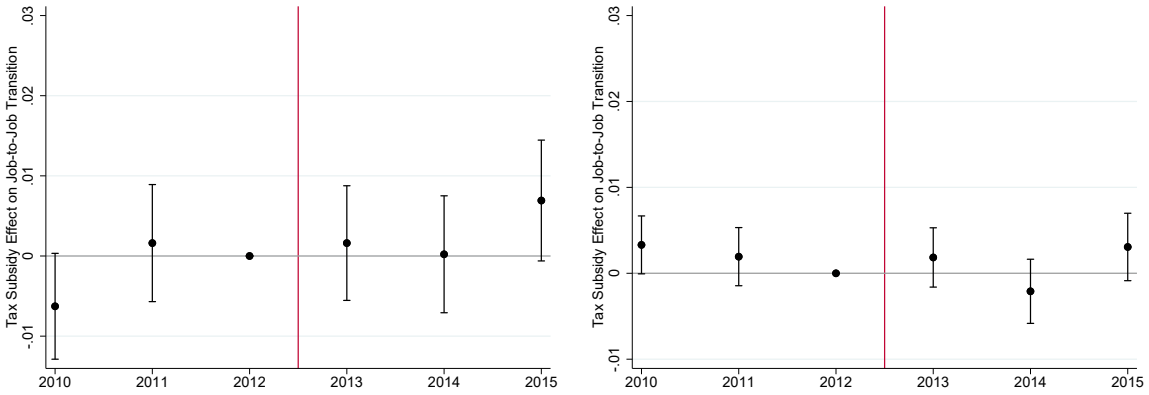
D Effect on employment transitions

Appendix Figure D1: Job-to-Job Transitions by Wage Differences

(a) Young, age 22-27, transition to lower wage job (b) Old, age 52-57, transition to lower wage job



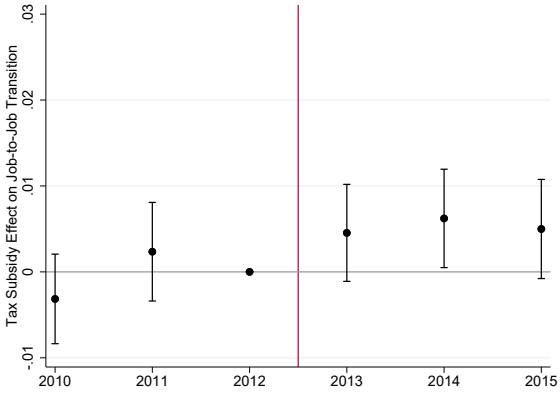
(c) Young, age 22-27, transition to higher wage job (d) Old, age 52-57, transition to higher wage job



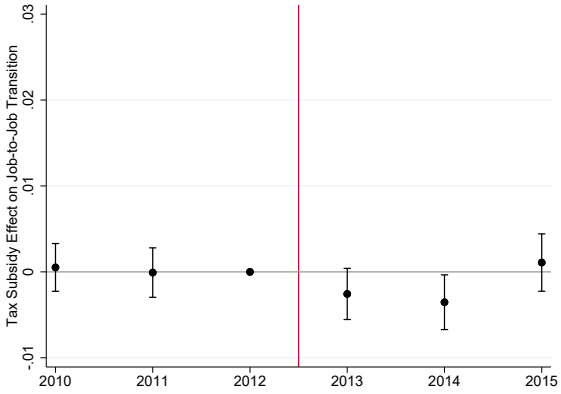
Note: The data is restricted to May of each year and to workers who worked in the private sector both the current year and a year before. Job-to-job transition is defined as working at a different firm this year than a year before. We also require no gaps in employment for job-to-job transition (based on monthly data). Coefficients of interaction term between annual date (reference: 2012) and treatment age are displayed with 95% CI.

Appendix Figure D2: Job-to-Job Transitions by Poaching Index (PI) Differences

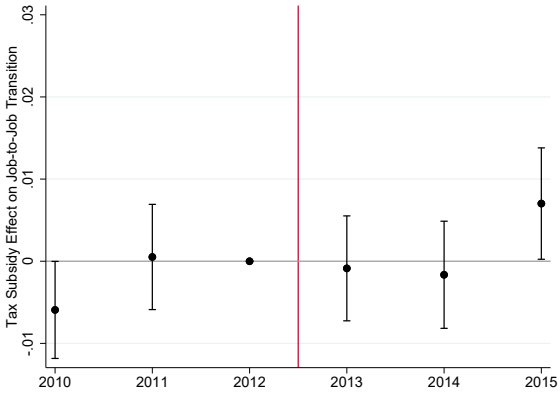
(a) Young, age 22-27, transition to lower PI firm



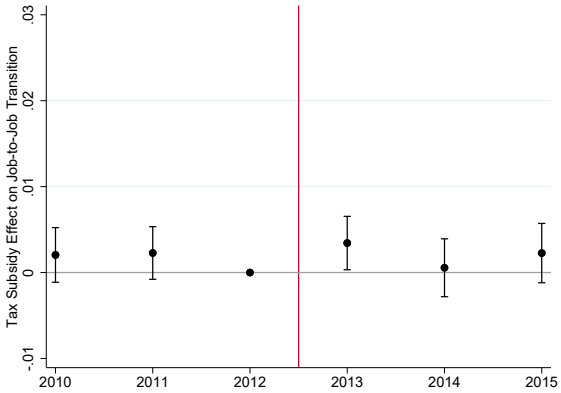
(b) Old, age 52-57, transition to lower PI firm



(c) Young, age 22-27, transition to higher PI firm



(d) Old, age 52-57, transition to higher PI firm



Note: The data is restricted to May of each year and to workers who worked in the private sector both the current year and a year before. Job-to-job transition is defined as working at a different firm this year than a year before. We also require no gaps in employment for job-to-job transition (based on monthly data). Coefficients of interaction term between annual date (reference: 2012) and treatment age are displayed with 95% CI.

E Role of the minimum wage

In this section, we provide indicative evidence that the estimated employment effects of the payroll tax subsidy is not a consequence of the presence of a minimum wage in Hungary.

Harasztosi and Lindner (2019) show that the disemployment effects of the minimum wage in Hungary are considerably larger in the tradable and in the exporting sectors than in the non-tradable or service sectors. To check if the impact of the tax subsidy is larger in sectors more affected by the minimum wage, we estimate the treatment effects by NACE industry categories. We report the estimated effects only for those industries where the effect is at least 0.1 percentage point in either of the analyzed age groups. The results reported in Table E1 indicate that the relative impact of the subsidy is smaller in manufacturing (a mainly tradable sector) than in the non-tradable sectors such as wholesale and retail trade, accommodation and food service. Thus, these results do not support that the role of the minimum wage drives the estimated effects.

Similarly, the estimated effect of the subsidy is smaller at exporting firms (Table E2, top panel), that are more affected by the minimum wage (Harasztosi and Lindner, 2019). Also, at least among the young, the impact of the subsidy is smaller at firms that employ a higher fraction of workers at the minimum wage (Table E2, bottom panel).

Appendix Table E1: Effect of the payroll tax cuts on employment in private sector companies by industry

	Young (age 22-27)			Old (age 52-57)		
	Employment effect	% of population	% change in employment	Employment effect	% of population	% change in employment
Agriculture	-0.0004 [0.0005]	1.311	-3.051	0.0015* [0.0008]	3.049	4.920
Manufacturing	0.0030* [0.0015]	11.517	2.605	-0.0034** [0.0015]	11.311	-3.006
Construction	0.0017** [0.0007]	2.695	6.308	0.0002 [0.0009]	3.634	0.550
Wholesale and retail trade	0.0045*** [0.0010]	4.962	9.069	0.0015 [0.0009]	3.870	3.876
Accommodation and food service	0.0018*** [0.0006]	1.512	11.905	0.0002 [0.0004]	0.547	3.656
Transportation and storage	0.0015** [0.0006]	1.994	7.523	0.0060*** [0.0009]	3.851	15.580
Information and communication	0.0020*** [0.0003]	0.599	33.389	-0.0005 [0.0003]	0.374	-13.369

Note: Robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1. The table shows estimates from the model in equation (8). These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. We estimate a separate linear probability model for the employment in each industry category. In each model, we control for age and quarterly date effects. The % of population indicator is measured in 2012. The % change in employment shows the increase in employment relative to the population employed in the given industry.

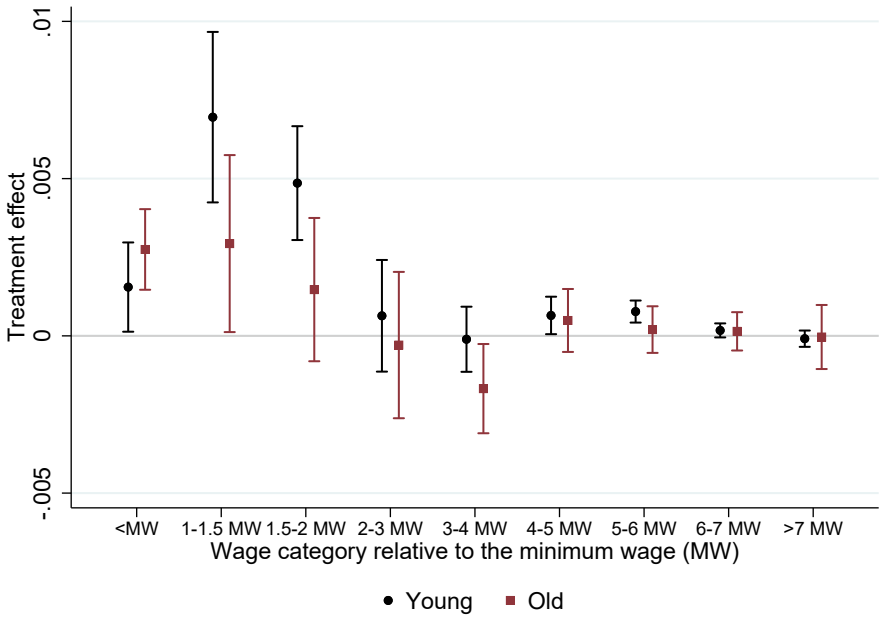
Appendix Table E2: Treatment effect on employment in private sector companies by some firm level indicators

	Employment of young Age 22-27	Employment of old Age 52-57
Treatment effect, not exporting firm	0.0116***	0.0056***
	[0.0016]	[0.0018]
Treatment effect, exporting firm	0.0039**	0.0001
	[0.0018]	[0.0018]
Treatment effect, ratio MW earners below median	0.0154***	0.0055**
	[0.0011]	[0.0022]
Treatment effect, ratio MW earners above median	0.0005	0.0003**
	[0.0001]	[0.0001]

Note: Robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1. The table shows estimates from the model in equation (8). These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period after the 2013 introduction of the payroll tax subsidy. In each regression, the outcome is the binary indicator of private sector employment at a firm with the given characteristic. In each regression, we control for age and quarterly date effects. When calculating the ration of minimum wage (MW) earners on the firm level, we consider a window of plus/minus 5,000 HUF around the year specific minimum wage level.

Figure E1 shows the employment effect of the payroll tax cut by wage categories. It can be seen that there are non-negligible employment effects even at jobs paying above 150% of the minimum wage.

Appendix Figure E1: Effect of the payroll tax cuts on employment in private sector companies by wage categories



Note: The figure shows the estimated employment effects with the corresponding 95% confidence intervals, based on Equation (8). These are difference-in-differences estimates that compare the change in employment between year 2012 and the 2013-2015 period (after the introduction of the payroll tax subsidy in 2013).

F Further wage regression results

Appendix Table G1: Average Treatment Effect on Log Wage of the Old

	All Workers	New Entrants	Incumbents	Incumbents	Incumbents	Incumbents
All Firms	-0.0114***	-0.0316*	-0.0115***	0.0011	0.0042	0.0040
	[0.0039]	[0.0167]	[0.0040]	[0.0026]	[0.0026]	[0.0026]
Low Poaching	-0.0245***	-0.0219	-0.0147***	-0.0048	-0.0018	-0.0016
	[0.0047]	[0.0201]	[0.0047]	[0.0030]	[0.0031]	[0.0031]
High Poaching	0.0007	-0.0333*	-0.0081	0.0065**	0.0098***	0.0091***
	[0.0044]	[0.0188]	[0.0048]	[0.0030]	[0.0030]	[0.0030]
Lagged dependent	no	no	no	yes	yes	yes
Type of lagged dependent				equals to 1	age	age + time

Note: Years 2012-2013, individuals aged 52-57. Incumbents had continuous employment at private sector companies the preceding 12 months. New entrants were not employed at a private sector company 12 months ago. Cluster robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table G2: Average Treatment Effect on Log Wage of the Young

	All Workers	New Entrants	Incumbents	Incumbents	Incumbents	Incumbents
All Firms	0.0091**	0.0001	0.0099**	-0.0082*	-0.0082*	-0.0021
	[0.0045]	[0.0114]	[0.0050]	[0.0044]	[0.0045]	[0.0047]
Low Poaching	0.0024	0.0010	0.0058	-0.0060	-0.0126**	0.0027
	[0.0053]	[0.0133]	[0.0059]	[0.0052]	[0.0052]	[0.0055]
High Poaching	0.0198***	0.0057	0.0169 ***	-0.0109**	-0.0039	-0.0076
	[0.0054]	[0.0128]	[0.0060]	[0.0051]	[0.0055]	[0.0053]
Lagged dependent	no	no	no	yes	yes	yes
Type of lagged dependent				equals to 1	age	age + time

Note: Years 2012-2013, individuals aged 22-27. Incumbents had continuous employment at private sector companies the preceding 12 months. New entrants were not employed at a private sector company 12 months ago. Cluster robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1.

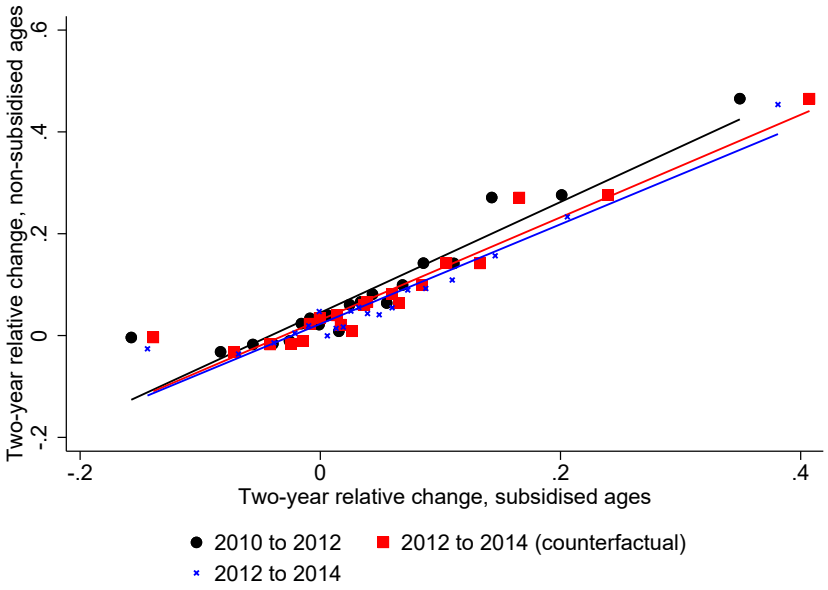
Appendix Table G3: Average Treatment Effect on Log Wage of the Young with at Least 6 Months Work Experience at Ages 18-19

	Incumbents	Incumbents	Incumbents	Incumbents
All Firms	-0.0052	0.0014	0.0036	0.0024
	[0.0135]	[0.0113]	[0.0119]	[0.0120]
Low Poaching	-0.0147***	-0.0030	-0.0009	-0.0023
	[0.0047]	[0.0133]	[0.0138]	[0.0138]
High Poaching	-0.0081*	0.0097	0.0123	0.0111
	[0.0048]	[0.0132]	[0.0139]	[0.0139]
Lagged dependent	no	yes	yes	yes
Type of lagged dependent		equals to 1	age	age + time

Note: Years 2012-2013, individuals aged 22-27. Incumbents had continuous employment at private sector companies the preceding 12 months. Cluster robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1.

G Firm level evidence for substitution and windfall effects

Appendix Figure H1: Firm level relative growth in employment by age groups



Note: On the x-axis, we indicate the two-year change from year t to year $t+2$ in the number of workers aged up to 24 or at least 55 (subsidized ages) relative to the observed firm size in year t. On the y-axis, we indicate the same two-year relative change in the number of workers aged 25-54 (non-subsidized ages). We exclude firms with less than 10 registered workers (5 workers in our sample, on average). After this restriction, we also exclude those firms that are not in the sample throughout years 2010-2014. We report binscatter plot of the observations with a linear fitted regression line. The black dots and line refer to relative change from 2010 to 2012 (i.e., before the introduction of the tax subsidy). The blue dots and line refer to relative change from 2012 to 2014 (with the tax subsidy being introduced in 2013). The red dots and line correspond to a counterfactual scenario under which the 2010-2012 relative change in employment rate in the subsidised age groups is increased by 4.3%, which is the estimated average rate of increase, while the 2010-2012 change in employment rate in the non-subsidised ages is left at its observed value.

The direct replication of the basic results of Saez, Schoefer and Seim (2019) is not straightforward for multiple reasons. First, firms that employ many subsidized young or old workers have different characteristics and have different growth patterns than firms with fewer workers at the subsidized ages. Second, if we compare, for example, firms that employ many young workers to firms that employ fewer young workers (as control group), it might still be the case that there is a high fraction of other subsidized individuals in the control group. Third, due to the cap on the subsidy, the number of subsidized individuals (e.g., workers aged under 25) does not capture how the amount of subsidy relates to the total payroll.

Due to these limitations, we apply the following firm-level approach.

1. Sample: We drop those firm-year observations where the size of the firm is less than 10 or more than 5,000 (based on the size recorded by the tax authority, not the firm size generated from the sample.) After this restriction, we keep firms that existed throughout 2010-2014.

2. We calculate the effective payroll tax subsidy rate in 2013, considering the age and occupation based subsidies only. We neglect the subsidies payable to long-term unemployed, career starters and people returning from maternity leave as the fraction of such workers is less persistent over years and the majority of career starters are likely captured by the age based subsidy.

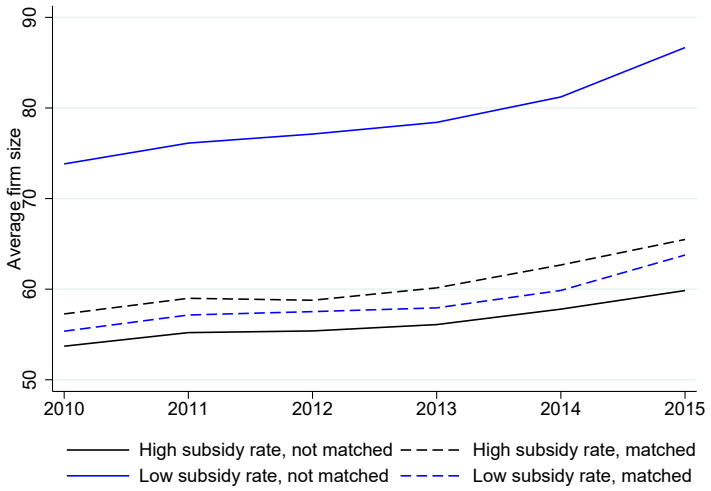
3. Based on the effective subsidy rate in 2013, we group firms into two categories according to whether the subsidy rate was below or above its median in 2013. We extrapolate these groups to the other sample years. The treatment (control) group is the group with the above (below) median subsidy rate.

4. We perform propensity score matching in year 2012. The variables used in the logit model are firm size, firm level average wage and two-digit industry code dummies. We do 1:1 nearest neighbor matching with no replacement and with caliper 0.01 (which is lower than usual).

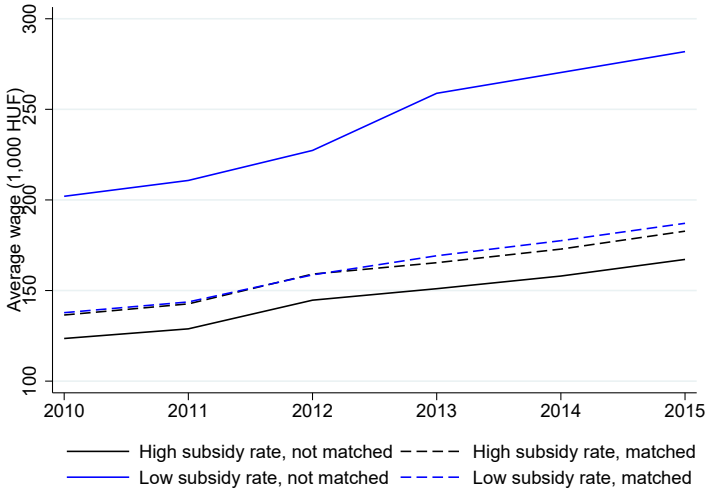
Figure H2 shows the matching results for firm size and firm level average wage. The reported plots do not provide evidence that the payroll tax subsidy had a windfall effect on firms employing a higher share of workers in the subsidized ages.

Appendix Figure H2: Evidence for windfall effects

(a) Firm size



(b) Average wage



Note: The differences between the high subsidy rate group and the matched low subsidy rate group are statistically not significant. Total sample size: 17,825 firms. Sample size in the matched sample: 10,016 firms.